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Disentangling the Mechanisms of the Boundary Restriction Effect

by

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Thesis Submitted to Flinders University
for the degree of

Doctor of Philosophy

College of Education, Psychology and Social Work

24th February 2020

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Summary

The role of visual memory is not to make a complete picture of the world, but rather to understand the meaning of what we see so that we can navigate the world. Thus, our visual memory is prone to errors. One such visual memory error is boundary extension, whereby people misremember seeing more of the periphery of a scene than they actually saw. This visual memory error is common and robust. However, the opposite memory error—whereby people remember seeing less periphery of a scene than they actually saw—can also occur. This visual memory error, boundary restriction, seems to occur after viewing negative visual stimuli. However, boundary restriction is not as robust as boundary extension, and some studies have actually failed to find the effect. Few studies have attempted to disentangle the mechanisms behind boundary restriction to determine why it occurs only after viewing some, but not all, negatively valenced stimuli. I seek to address this gap by investigating the possible mechanisms that may induce the boundary restriction effect. More specifically, I investigated a number of possible mechanisms in isolation; specifically, whether boundary restriction could be induced by (1) the presence of negative visual imagery (2) valence in isolation from negative visual imagery (3) attention both in the presence of, and isolated from, negative visual imagery, and (4) arousal in isolation from negative visual imagery.

Overall, my experiments revealed three important findings. First, valence appears to contribute little to inducing boundary restriction errors. I found that both the presence of negative stimuli, and priming ambiguous images to be negatively valenced did not induce boundary restriction. Second, I found that short presentation times and focussed attention induced boundary restriction errors. However, using an incidental measure of attention — relying on participants' natural tendency to over-attend to the left hemifield—did not induce

boundary restriction. Third, I found that aversive arousal can both induce boundary restriction errors, and decrease boundary extension errors.

My findings may explain why the literature surrounding boundary restriction is so mixed. Boundary restriction seems to occur not due to the negativity of visual stimuli, but rather due to the arousing and/or attention-grabbing nature of the stimuli instead. Thus, I conclude that boundary restriction may be a natural visual memory error, but one that occurs only under specific circumstances, and not one that occurs for all negative visual scenes. Boundary extension remains the more common visual memory error, and is only reversed under specific, highly arousing circumstances. Future research should further test this finding by investigating the way boundary restriction is measured (i.e., investigating different test types) or by employing materials that specifically heighten arousal and capture attention, rather than focussing on negative stimuli to induce the effect.

Declaration

I certify that this thesis does not incorporate without my acknowledgement any material previously submitted for a degree or diploma in any university; and to the best of my knowledge and belief, does not contain any material previously published or written by another person except where due reference is made in the text.

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Acknowledgments

To my supervisor Dr. Melanie Takarangi. You fostered my growth as a PhD student, taught me so many things outside of your actual responsibilities, and helped me to cope with life as well as the academic rigours that accompany completing this huge body of work. You have developed a lab which supports and uplifts everyone working in it. You helped to make the lonely process of my PhD one of collaboration and support, and I am forever grateful for that. Thank you for your wisdom, advice, and numerous opportunities you have offered me during my candidature. I am forever grateful for everything you have done for me and with me.

To the Takarangi lab, what an amazing, powerful, strong group of women. I am so grateful to all of you for making this time enjoyable. To Ella, Di, Victoria, Taylor, Jacinta and Sasha; working, socialising, and coffee-ing with all of you made my work so much more enjoyable. Thank you for being my conference travel buddies, my research sounding boards, my confidantes and my confidence. In particular to Ella. Thank you for being my PhD travel buddy, my constant lab companion, and my amazing colleague. You always went above and beyond to help me, and I am eternally grateful I got to work with you for all these years.

Thank you to my friends and family outside of my academic bubble. To my mum, dad, Carina, Daimon, and Angela. Thanks for understanding when I missed almost every family dinner for the last 5 years! Thank you for meals, and favours, and chats when I needed them. Your constant support and understanding was amazing. Grazie a mia nonna Teresa Nesci per aver vegliato su di me per tutto questo. To my friends Ellie, Abbey, Chelsea, and Janelle. Thank you for late night chats, for listening when I needed to talk, for sometimes only communicating in gifs. You kept me sane throughout this process.

Last, to my wonderful husband Nick. Thank you for everything. I cannot list all of the things you have done for me here, because there is not enough room in the universe to contain such a list. Thank you for making me laugh, for taking me for drives, for little surprise notes that tell me I am loved. Thank you especially for getting me through the toughest things I have ever had to cope with during this time. You have grieved with me, and travelled with me. You have written with me, and walked with me. I am eternally grateful for your love and support.

1 Literature Review

The role of visual memory is not to make a complete picture of the world, but rather to understand the meaning of what we see so that we can navigate the world. Humans are remarkably good at image recognition (Shepard, 1967), and can make "old" or "new" judgements for previously viewed images very accurately, even on thousands of trials (Standing, 1973). However, despite being quite good at recognising specific images they have seen, people are not very accurate when it comes to remembering the details of those images (e.g., Mandler & Ritchey, 1977). This poor memory for details may be because memory is reconstructive, with visual scenes being pieced back together in our mind's eye using schemas (e.g., how something fits into the world at large; Fiske & Linville, 1980), imagination, and previous experience of the world (Loftus & Palmer, 1974). Furthermore, because we cannot attend to all the information that bombards our visual system—due to limited attentional resources—some details will never be encoded.

What our attentional resources are devoted to at any one time is influenced by many factors, such as goal orientation (Simons, 2000), threat and arousal (see Yiend, 2010 for a review), and even neurological biases (e.g., pseudoneglect; Jewell & McCourt, 2000). One example of our imperfect visual attention system is inattentional blindness—the failure to notice a visible but wholly unexpected object (e.g., a man in a gorilla suit—attention-grabbing stimulus) when attention is focussed on another task (e.g., counting ball passes—current goal; Simons, 2000; Simons & Chabris, 1999). This sort of error is particularly pertinent when it occurs during an event where visual memory accuracy has real consequences. One such example is a crime, where eyewitnesses may fail to detect a crime occurring because they are engaged in another task (e.g., Chabris, Weinberger, Fontaine, &

Simons, 2011), or fail to recall particular details about the crime because their attention is focused on only one aspect of it (e.g., a weapon; Loftus, Loftus, & Messo, 1987). Therefore, even when we do attend to a scene, if a particular stimulus within the scene is not essential to fulfilling our current goal(s), we are liable to reject, not encode, and/or not remember ever seeing it. Taken together, we know that visual memory is prone to errors that can affect important decisions such as eyewitness identifications.

My thesis is focussed on one such visual memory error: *Boundary Extension*. Typically, when people remember visual scenes they tend to remember the boundaries as more expansive than they actually were; in essence, adding details to the unseen periphery of that scene. Intraub and Richardson (1989) first demonstrated boundary extension by showing participants a series of images and having them draw the images from memory. The majority of participants drew the images with more expansive boundaries than the images actually had (Intraub, & Richardson, 1989). Since this original study, boundary extension has proven to be a common, robustly replicated phenomenon (Beighley, Sacco, Bauer, Hayes & Intraub, 2018; Beighley & Intraub, 2016; Bertamini, Jones, Spooner, & Hecht, 2005; Blazhenkova, 2017; Candel, Merckelbach, & Zandbergen, 2003; Chadwick, Mullally, & Maguire, 2013; Chapman, Ropar, Mitchell, & Ackroyd, 2005; Daniels & Intraub, 2006; Dickinson & Intraub, 2008; Dickinson & Intraub, 2009; Dickinson & LaCombe Jr, 2014; Gagnier, Dickinson, & Intraub, 2013; Gagnier & Intraub, 2012; Gagnier, Intraub, Oliva, & Wolfe, 2011; Gottesman, 2011; Gottesman & Intraub, 1999; 2002; 2003; Hale, Brown & McDunn, 2016; Hale, Brown, McDunn & Siddiqui, 2015; Hubbard, Hutchinson, & Courtney, 2010; Intraub, 2010; Intraub, Bender, & Mangels, 1992; Intraub & Berkowits, 1996; Intraub & Bodamer, 1993; Intraub, Daniels, Horowitz, & Wolfe, 2008; Intraub & Dickinson, 2008; Intraub, Gottesman, & Bills, 1998; Intraub, Gottesman, Willey, & Zuk, 1996; Intraub, Hoffman, Wetherhold, & Stoehs, 2006; Kreindel & Intraub, 2017; Maguire, 2012; Liu, Yang, & Intraub, 2016; Mamus & Boduroglu, 2018; McDunn, Brown, Hale & Siddiqui, 2016; McDunn, Siddiqui & Brown, 2014; Ménétrier, Diddiejean, & Barbe, 2018; Ménétrier, Didiejean & Robin, 2017; Ménétrier, Diddiejean, & Viellard, 2013; Ménétrier, Iralde & Le Bohec, 2019; Mullally, Intraub, & Maguire, 2012; Park et al., 2007; Munger & Multhraup, 2016).

Boundary extension occurs across a variety of conditions. For example, it occurs across age and gender (Seamon, Schlegel, Hiester, Landau, & Blumenthal, 2002); with and without warning of the boundary extension phenomenon (Gagnier, Dickinson, & Intraub, 2013; Intraub & Bodamer, 1993); with both naturalistic shapes (Intraub et al, 1996; Intraub & Bodamer, 1993), abstract shapes (McDunn Siddiqui, & Brown 2014; Mamus & Boduroglu, 2018); and film clips (Ménétrier, Didierjean, & Vieillard, 2013). Boundary extension also occurs across a variety of test formats, including camera distance ratings (e.g., Intraub et al, 1992); forced choice (e.g., Candel et al., 2003); boundary adjustment tests (e.g., Dickinson & Intraub, 2009); and drawing from memory (e.g., Intraub & Berkowits, 1996). Typically, participants view either a close-up or wide-angled view of a scene, then at test view a scene that is either identical to the encoded view, or the opposite view. This procedure usually results in some or all of four conditions: close-ups tested by close-ups (CC), wide-angled tested by wide-angled (WW), close-up tested by wide-angled (CW), and wide-angled tested by close-up (WC). Participants often rate test images on a camera distance scale (e.g., from much closer-up to much more wide-angled; Intraub & Berkowits, 1996; Intraub, et al., 1998; Seamon et al., 2002). Alternatively, forced choice tests have participants choose which image they originally saw from two or more close-up or wide-angled versions (e.g., Candel et al., 2003; Mathews & Mackintosh, 2004; Safer, Christianson, Autry, & Osterland, 1998). Boundary adjustment tests ask participants to move the boundaries of a test image until it matches the image in their memory (e.g., Chapman, et al., 2005), and drawing tests require participants to draw the image from memory (e.g., Intraub et al., 1998). Boundary extension is clearly a robust, easily replicable effect, but why does it occur? There are several proposed mechanisms—unlikely to be mutually exclusive, which I consider next.

1.1 Boundary Extension and Imagination

Gottesman and Intraub (1999) hypothesised that boundary extension occurs because our visual system is accustomed to extrapolating beyond the scene periphery to contextualise brief eye fixations (saccades) into whole, complete images. In other words, boundary extension arises when people *imagine what is happening beyond the scene boundaries*, to contextualise information into our perceptual schema—our internal representation of the world. Several studies provide support for this hypothesis (e.g., Gottesman, & Intraub, 2003; Intraub, 2002; Intraub, 2004; Intraub, et al., 1998). In one series of experiments, Intraub et al. (1998) showed participants closely cropped images of objects, and asked some participants to imagine what was beyond the view. Participants who were asked to imagine beyond the view made more boundary extension errors compared to participants who viewed the whole scene, and participants who were not asked to imagine beyond the scene. We also know that images with little boundary at the edges—i.e., images that are highly zoomed in—tend to increase boundary extension, presumably because these images encourage people to assume—or imagine—more of what is beyond the boundaries (Intraub & Berkowits, 1996). Conversely, Bertamini and Jones (2004) found that occlusion at the borders of an image—indicating that part of an object within the image extends beyond the boundaries (i.e., a view of a shape bisected by the border)—*reduced* boundary extension. It is possible that occlusion draws attention to the missing parts of the object and hence *restricts* imagination beyond the boundaries, which reduces the boundary extension effect. Scenes with extremely wide angled views and a small central object are also less likely to lead to boundary extension than close-up views, presumably because these scenes are less likely to prompt imagining beyond the boundaries (McDunn et al., 2014). Taken together, these findings support the idea that imagining beyond the image boundaries makes boundary extension more likely.

But merely imagining and/or extrapolating beyond the boundaries of an image may not, on its own, entirely account for boundary extension. In one example, participants were either asked to imagine beyond the view, or simply remember scenes, including the background (Gagnier, 2010). The viewed scenes had the periphery slowly fading into view. Boundary extension occurred equally for both imagine and remember only conditions, suggesting that boundary extension is not due only to imagining beyond the boundaries. In another example, Munger and Multhaup (2016) had some participants imagine the smells and sounds that would accompany each scene as they viewed it; other participants simply viewed the image and tried to remember as much visual detail as possible. Again, imagining beyond the scene—in this case smells and sounds—did not exacerbate boundary extension. Thus, it could be the *type* of imagination beyond a scene that exacerbates boundary extension. In this study, participants also either completed the Vividness of Visual Imagery Questionnaire (VVIQ) or the Object–Spatial Imagery Questionnaire (OSIQ; Munger & Multhaup, 2016). The VVIQ measures visual ability (imagining images that are not actually seen), which correlates highly with recall accuracy for images (Marks, 1973). The OSIQ has two subscales, one that measures image recall ability; people's accuracy at recalling viewed images from long-term memory, and one that measures spatial imagery ability; people's ability to recall relative spatial positions of objects in images and to imagine the details of objects and places in their mind's eye (Blajenkova, Kozhevnikov, & Motes, 2006). Indeed, Munger and Multhaup found that *spatial* imagery ability (OSIQ) specifically—not image recall or visual ability (VVIQ)—was positively associated with boundary extension. Taken together, the evidence from these studies indicates that imagination alone does not necessarily affect boundary extension. Boundary extension may be the artefact of an adaptive process, whereby people imagine beyond boundaries so they are more aware of their surroundings. Thus, if there is no need to extrapolate beyond the boundaries of an image, or if spatial ability is poor, then extrapolation—and therefore boundary extension—may be less likely to occur.

Other research also points to spatial ability and its importance for inducing boundary extension. It is well known that the hippocampus plays a vital role in memory, and more specifically in spatial memory (see Burgess, Maguire, & O'Keefe, 2002 for a review). Thus, the role the hippocampus plays in processing scenes may be important for boundary extension (Maguire & Mullally, 2013). To test this idea, Mullally et al. (2012) compared participants with hippocampal damage and neurotypical participants. Participants completed a series of boundary extension experiments. First, they viewed images in a modified rapid serial visual presentation (250ms scene presentation), and then made a forced-choice judgement about each image they saw. Second, participants viewed an image of an object on a neutral background, and then drew the image from memory. Although neurotypical participants consistently made boundary extension errors—on both the forced-choice recognition and drawing from memory task—hippocampal damage participants made fewer boundary extension errors. This boundary extension attenuation observed in people with impaired hippocampal functionality lends additional support to the notion that spatial ability is essential for boundary extension to occur.

1.2 Boundary Extension and Attention

Boundary extension is also affected by attention; specifically, diffuse attention appears to exacerbate the boundary extension effect. Intraub and Berkowits (1996) first investigated this idea by having participants view inverted or upright images. Intraub and Berkowits posited that because it is more difficult to comprehend inverted images, participants would devote more attention to these images. At test, participants drew images they had seen earlier from memory and—contrary to the predictions—showed similar boundary extension for both the inverted and upright images. On the one hand, this result could indicate that attention does not affect boundary extension. On the other hand, there was no manipulation check to confirm that the inverted images *did* lead to an increase in attention. Therefore, it is not possible to conclude from these results whether attention affects boundary extension. In another example, Dickinson and Intraub (2009) used the pseudoneglect phenomenon to manipulate attention. Neurotypical right-handed people tend to pay more attention to the left than the right side of space (Jewel & McCourt, 2000; Kinsbourne, 1970). Due to contralateral processing, the left visual hemifield is initially processed by the right hemisphere, which is dominant for visuospatial attention (Bourne, 2009). Thus, people attend more to stimuli that appear in the left visual hemifield than stimuli that appear in the right hemifield. Dickinson and Intraub (2009) used this natural lateralisation to investigate the role of attention in boundary extension by presenting scenes (with a neutral object on both sides of the scene) that spanned participants' entire visual field. Participants then rated image boundaries for both the left and right sides of the images, using a boundary adjustment task. Despite maintaining fixation on a central fixation point (confirmed via eye-tracking), participants made more expansive boundary extension errors in the *right* hemifield (where less attention was paid), than in the left hemifield (where more attention was paid). In other words, less attention resulted in greater boundary extension.

Others have more directly investigated the effect of attention on boundary extension. One paradigm involves having participants do concurrent tasks, which results in rapid task-switching (e.g., Intraub et al., 2008). Thus, participants can only pay attention to one stimulus at a time and attention is diffuse for stimuli in both tasks. In an example study, participants viewed scenes while concurrently counting along with an auditory tone (divided attention) or not (selective attention; see Hubbard et al., 2010 for a review). Participants in the divided attention condition, where attention was diffuse, drew the scenes with more expansive boundaries than participants in the selective attention condition. In a similar study, participants looked for target digits while viewing images (divided attention) or rated how easy it was to see an image behind superimposed numbers (focused attention; Intraub et al., 2008). At encoding, participants viewed either close-up or wide-angled images then made boundary judgement ratings (using the boundary rating scale developed by Intraub & Richardson, 1989). Participants in the divided attention condition were more likely to show boundary extension than those in the focussed attention condition. Taken together, these results support the idea that divided or diffuse attention away from a scene—or part of a scene—can exacerbate boundary extension, while focussed attention to a scene—or part of a scene—can attenuate boundary extension.

1.3 **Source Monitoring Errors**

Some researchers suggest that boundary extension is due to source monitoring errors (Intraub, 2012; McDunn et al., 2014). Source monitoring errors—where internally generated information (e.g., imagining or dreaming), external information (e.g., confusing details of multiple events) or multiple similar pieces of information, are confused for the actual stimulus—are common (Johnson, Hashtroudi, & Lindsay, 1993). Indeed, source monitoring errors occur often in visual memory; when after viewing an image, a person is unable to separate the original visual information from internally generated and/or externally suggested details about the image (Johnson et al., 1993). Researchers propose that this same process occurs when participants erroneously recall images as having wider boundaries than they actually did: participants contextualise the images to the wider world, and then there is confusion between what is seen, and what is imagined, resulting in boundary extension. It is likely that imagination and attention processes contribute to source monitoring failures. When attention to a stimulus is diffuse, less visual information is available, and the mind may need to "fill in the gaps" left by the visual system with internally generated information—such as previous knowledge, imagination, or schemas—in much the same way as source monitoring errors occur. Limited attention presumably exacerbates the difficulty in distinguishing between remembered information and internally generated information.

To summarise, participants consistently remember more beyond the image boundaries than they actually saw (boundary extension). Boundary extension seems to occur because of confusion between what was seen and what was imagined (e.g., source monitoring errors), and it seems to be exacerbated by diffuse attention. However, what about the opposite phenomenon, boundary restriction? There is evidence that viewing negative stimuli can reverse the boundary extension effect, which I explore next.

1.4 Boundary Restriction

In some circumstances, instead of remembering *more* than they actually saw, people sometimes remember *less* than they actually saw. This effect can be either a memory narrowing or tunnel memory effect, whereby people focus more on the central objects than on the periphery, or a *boundary restriction* effect, whereby people remember the central object as closer to them than it actually was. These effects have been demonstrated when participants recall details of an emotional personal experience (Berntsen, 2002; Talarico, Berntsen, & Rubin, 2009); emotional scenes (Christianson & Loftus, 1987; Christianson, Loftus, Hoffman, & Geoffrey, 1991; Ménétrier et al., 2013; Safer, et al., 1998); and negative images (Fawcett, Russell, Peace, & Christie, 2011; Kramer, Buckhout, & Eugenio, 1990; Mathews & Mackintosh, 2004; Pickel, Ross, & Truelove, 2006; Takarangi, Oulton, Green, & Strange 2015; Waring, Payne, Schacter, & Kensinger, 2010). These findings suggest that negative emotion influences boundary extension.

To my knowledge, only a handful of studies have demonstrated boundary restriction using negative images. In one example, Safer et al. (1998) presented participants with a series of images, culminating in either a highly negative or neutral image. After a delay, participants made a forced choice selection of the image they had originally seen. Participants who viewed the neutral target images tended to select the images with extended boundaries—making boundary extension errors. Participants who viewed the negative target images tended to select the images with restricted boundaries—making boundary restriction errors. Likewise, Mathews and Mackintosh (2004) presented participants with a series of negative and neutral images, and then tested participants' memory for these images with wider-angled and cropped versions of the same scenes. Participants were asked to select the original image. Participants who were high in trait anxiety displayed boundary restriction, but only for a subset of highly negative images.

There is also evidence that negative valence can attenuate boundary extension, potentially identifying the mechanism by which the effect might be entirely reversed (i.e. boundary restriction). In an example, participants watched 16 one-second film clips with actors displaying one of four emotions: happiness, pleasure, anger, or irritation (Ménétrier et al., 2013). Participants re-watched the same films again immediately after the first viewing and evaluated whether the camera was closer to or farther from the scenes compared to before. Participants judged the film clips displaying positive emotions as being farther away—a boundary extension effect—despite there being no actual change to the clips. However, participants did not judge the film clips displaying negative emotions as being more expansive than they actually were—meaning there was no boundary extension effect for negative material. In short, the film clips displaying negative emotions demonstrated a noticeable attenuation of boundary extension, an otherwise robust effect.

Negative valence may attenuate boundary extension by narrowing participants' recall to central (physically central) or salient (meaningful to the theme) details of an event (Christianson & Loftus, 1987). Participants wrote down details of an emotional or non-emotional image series immediately after it was shown to them, and later recalled the original images. The emotional images all contained traumatic central details (e.g., an injured person on a car bonnet) and peripheral details (e.g., witnesses standing nearby). The non-emotional images also contained both central (e.g., a woman picking a flower) and peripheral (e.g., other flowers growing nearby) details. Participants who saw the negatively valenced images correctly remembered more physically central details, and fewer physically peripheral details. Conversely, participants who saw the neutral versions of the same scenes tended to recall more physically peripheral detail, and less information about each image's physical central focus. In another series of studies, Berntsen (2002) demonstrated how negative valence can narrow memory recall in a pattern akin to boundary restriction. Participants identified their most negative or positive life event, and recalled as many details as possible about that event. Independent raters (and, in a separate experiment, participants themselves) classified these details as central (if the detail was related to what was shocking to the participant and/or it could not be left out or replaced without changing the event), peripheral (did not meet the central criteria), or neither (was not related to the event). After a break, participants recalled a memory of the opposite valence (if negative first, then positive, and vice versa) and repeated the same procedure. Participants remembered the details classified as central better for the negative experience, but not for the positive experience. Although this study was not about images nor boundary ratings per se, the result suggests that the negative valence may narrow participants' recall to central or salient details of that event.

In summary, there is evidence that boundary extension may be reversed—or attenuated—by negative valence. However, this evidence is mixed. For example, Candel et al. (2003), were unable to replicate Safer et al.'s findings. In Candel et al. (2003), participants viewed neutral or negative scenes, and then either drew the from memory (Experiment 1) or rated identical images at test as "farther than the original", to "closer than the original" (Experiment 2). Participants tended to make boundary extension errors, regardless of valence. Likewise, other studies have failed to find boundary restriction for negative versus neutral images. Beighley et al. (2018) used images containing faces displaying either a happy or sad emotion, thus creating a set of images which were identical except for the emotion displayed. Participants then saw the same images again and rated them as "the same," "closer-up," or "farther away". Contrary to predictions, participants did not make more boundary restriction errors for the negative images, and in fact consistently made boundary extension errors, regardless of the emotion of the images. In a study that used a much higher number of negative images than typical studies (50), Takarangi et al. (2015) presented negative images then gave a forced choice (Experiments 1, 2, & 3) or a camera distance test (Experiment 4). Contrary to the prediction that participants would tend to remember negative images as having narrower boundaries, participants mostly made boundary extension errors. Taken together, these studies demonstrate that boundary extension is the more robust error, and may overshadow any subtle boundary restriction errors that may otherwise occur.

One explanation for the mixed findings is that it is not *negative valence* per se that reverses boundary extension, but rather some other factor(s) that are closely associated, or tend to co-occur, with valence. For example, negative valence can be difficult to capture without also capturing arousal. Arousal is state of mental and bodily preparation for intense activity (Russell, Weiss, & Mendelsohn, 1989) and it can be both negative (aversive) or positive (pleasant). Heightened arousal can lead to both impairment in memory (e.g., Kassin, Ellsworth, & Smith, 1989), and enhancement in memory (e.g., Heuer & Reisberg, 1990). Despite these contradictions, it seems that overall, emotional arousal (whether negative or positive) does receive some preferential processing, and thus may lead to an enhancement in memory, specifically for the salient aspects of that memory (see Christianson 1992 for a review). Indeed, negative images may narrow a person's focus because the aversive arousal they tend to bring with them activates a flight-or-flight response, making imagination and extrapolation beyond the boundaries of those images less likely to occur. A related possibility for why negative valence does not consistently induce boundary restriction is that negative stimuli induce boundary restriction only when they capture and hold attention. Recall that diffuse or limited attention can exacerbate boundary extension, thus focussed attention may induce boundary restriction. When attention is captured and held by a negative stimulus—for example, due to high arousal—there is again less opportunity for imagination and extrapolation beyond the boundaries, and thus less chance of a source monitoring error. Instead, people's focus—and therefore their memory—is narrowed. It is possible that all three factors—valence, attention, and arousal—contribute to the boundary restriction effect. I consider these factors next.

1.5 Boundary Restriction and Attention

Negative images may lead people to preferentially attend to negative images' central details. Indeed, we know our limited visual system preferentially attends to highly arousing, threatening stimuli, (Lang & Bradley, 2010; Loftus, Loftus, & Messo, 1987; Mather, 2007). For example, in Christianson, Loftus, Hoffman, and Loftus (1991), participants viewed target images that were negative (a woman with a head injury), neutral (woman riding a bike) and unusual images. Participants made more eye fixations on the negative target images' central details (when compared to the neutral and unusual events), suggesting more they paid more attention to these details (Christianson, Loftus, Hoffman, & Loftus, 1991). Participants' memory for central details was better for the negative images compared to the unusual images, suggesting that the difference in attention lead to an enhanced memory for central details. Thus, when attentional capture occurs due to a negative, salient stimulus, it can lead to a memory advantage.

This attentional capture may be due to a type of implicit goal setting, whereby attention is directed by emotionally salient information which affects a person's goals. Goals powerfully direct attention by motivating what a person pays attention to, and thus goals can affect what is committed to memory (Levine & Edelstein, 2009). For example, fear may elicit a goal of wanting to escape, and thus attention is then focussed on escape options, whereas a positive emotion may elicit a goal of enjoyment, whereby the person seeks only to enjoy the memory, with no specific attentional focus. This goal setting can be the result of negative or positive valence, but the way attention is directed does not seem to depend on valence alone.

Indeed some studies have demonstrated that attentional capture can occur regardless of valence, and may be due instead to implicit goals set by other factors. For example, Pickel (1998) found that attentional capture led to a focus on *unusual* objects (an example of memory narrowing). Pickel used a video with four conditions (man walks into a hairdresser with a threatening, unusual object (gun), a nonthreatening unusual object (chicken), a threatening usual object (scissors) or a nonthreatening usual object (wallet). The unusualness of the object reduced memory accuracy for peripheral details regardless of threat level, presumably as a result of captured attention. In a similar study, Mitchell, Livosky, and Mather (1998) showed participants a scene with no object, an unusual object (celery), or an unusual threatening object (gun). Mitchell et al. found results similar to Pickel (1998): participants' memory for peripheral details was less accurate when they viewed an unusual object, compared to no object. Taken together, these data support the assertion that when an object captures attention—regardless of whether it is negative or not—participants may make memory narrowing errors. However, one limitation of these experiments is that the threat implied by the objects is not directed at the participants themselves, unlike viewing negative images that imply a threat of danger, disease, or personal anxiety. Indeed, Mitchell et al. also found that participants' negative affect was no different in the threatening object group compared to the non-threatening object and no object groups. Additionally, overall negative affect was very low. Thus, we do not know what would happen if participants were confronted with an actual threat. Indeed, there may be some situations where a negative stimulus *is* more likely to capture attention than a neutral but unusual stimulus. For example, if stress and arousal were at the level that we would expect in a real armed robbery situation (i.e., very high), we may see a pattern of remembering akin to boundary restriction.

1.6 Boundary Restriction and Arousal

There is some evidence that a combination of negative valence, attention, and arousal leads to memory narrowing. In another weapon focus study, Pickel et al. (2006) created a “real life” threat situation (as opposed to the laboratory model in the study by Pickel, 1998) to heighten participants’ arousal levels. Participants watched a lecture during which a confederate entered the room and shouted aggressively whilst wielding either a weapon, or a textbook. Participants in the weapon-present condition recalled more incorrect details of the perpetrator (a peripheral detail to the gun) than those in the non-weapon present condition. Furthermore, participants recalled central details better in the weapon-present condition than the textbook-present condition. In a second experiment, the researchers raised participants’ arousal—measured by their heart rate—by telling them that they would be giving a talk at the end of the experiment, a manipulation akin to the Trier Social Stress Test (TSST; Kirschbaum, Pirke, Hellhammer, 1993). The TSST successfully raises arousal levels (measured by cortisol blood levels), psychosocial stress, and general anxiety, due to the feelings of uncontrollability, and fear of social judgement raised by the task (Vors, Marqueste, & Mascret, 2018). Despite this elevated arousal, the same results occurred: in the weapon-present condition, participants’ attention was grabbed by the threatening object, and thus they remembered less about the perpetrator and more about the object than in the textbook-present condition—irrespective of arousal level (Pickel et al., 2006). However, Pickel et al.’s (2006) results may not generalise outside of arousal elicited by social anxiety; other forms of arousal encountered in real-life threat situations (i.e., fear) may not yield the same results. Yet, these results do suggest that arousal affects memory; specifically, it seems to improve memory for central, threatening, attention-grabbing objects.

In general, arousal also improves memory accuracy for scenes. In one example, participants watched a series of neutral slides, which culminated in either a negative (i.e., a friend contemplating suicide) or neutral (i.e., a friend with a hangover) outcome (Laney, Campbell, Heuer & Reisberg, 2004). Participants watched the slides, accompanied by a narration that told either a negative unfolding story or a neutral unfolding story. Participants were asked to empathise with the characters on screen, to elicit emotional arousal. Arousal was measured as a change from participants' own baseline heartrate to their heart rate at the end of the slide presentation. Greater change in arousal improved memory accuracy for both the peripheral details and the central details of the target scenes (details rated by the researchers as central—the objects were central to what the character was doing; removing a loaf of bread from a bag—or peripheral—objects in the background; a table in the background). There was no evidence of memory narrowing. However, it should be noted here that the slides themselves were not highly arousing, and did not contain a central, salient object to capture attention. Had a central, salient object been present as well as the heightened arousal established by the narration, it is possible that attention could have been captured by the central object, leading to a rejection of the periphery: boundary restriction.

There may be a biological basis for the fact that arousal improves memory accuracy for scenes. Arousal activates memory processes which aid in the consolidation of memory. Emotional arousal results in a series of physiological responses known as an "arousal cascade" (see Kalman & Grahn, 2004 for a review). Specifically, arousal increases blood glucose levels, and the release of endogenous adrenaline (epinephrine), and this heightened blood glucose results in improved memory consolidation (e.g., Gold & van Buskirk, 1975; Hall, Gonder-Frederick, Chewning, Silveira, & Gold, 1989; Mohanty & Flint, 2001). Additionally, the arousal cascade results in activation of the amygdala, which results in better memory for emotionally arousing scenes than for neutral scenes (see McGaugh, Cahill, &

Roozendaal, 1996 for a review). Indeed, the activation of the amygdala increases with the intensity of the emotional arousal, suggesting that the level and type of arousal play a critical role in memory (Canli, Zhao, Brewer, Gabrieli,& Cahill, 2000).

Other studies have more directly demonstrated how arousal can induce boundary restriction. Recall that in Mathews and Mackintosh (2004), people with high trait anxiety—who are more prone to high state arousal than those with low trait anxiety (Mathews & MacLeod, 1994)—tended to make more boundary restriction errors for highly negative, highly arousing images. Waring et al. (2010) also found a pattern akin to boundary restriction; participants high in trait anxiety had better memory accuracy for central objects than the peripheral background. Thus anxiety—which may be a proxy for arousal—appears to contribute to the boundary restriction effect, perhaps because anxious individuals find it harder to disengage from arousing stimuli (Koster, Crombez, Verschuere, Van Damme, & Wiersema, 2006). The inability to disengage from emotionally arousing stimuli means that people are more likely to rehearse and recall the information. In fact, there is evidence that rehearsing emotionally arousing material involuntarily is related to boundary restriction. Recall that Takarangi et al. (2015) found that participants tended to overestimate the image boundaries of traumatic images. However, for participants who did make boundary restriction errors, these errors were positively correlated with the severity of participants' reaction to the images (as measured by analogue symptom ratings on the PTSD Checklist: PCL-5, a standardised self-report rating scale for PTSD; Blevins, Weathers, Davis, Witte, & Domino, 2015). Taken together, it seems that arousal may attenuate boundary extension—particularly for negative images or scenes—by heightening attention to negative objects and reducing imagination and extrapolation beyond the scene's boundaries. Thus, there seems to be an interplay between arousal, attention and negative valence, which warrants further investigation.

In sum, people often extrapolate beyond the boundaries of a scene, making boundary extension errors. Boundary extension can be exacerbated by imagination, diffuse attention (e.g., Intraub et al., 2008), and source monitoring errors. Despite the robustness of this effect, the research outlined here demonstrates that negative valence may attenuate or reverse boundary extension. However, the findings for these mechanisms have been mixed, leading to the question: Does boundary restriction reliably occur? Is the underlying mechanism of boundary restriction negative valence, or rather the attention-grabbing or arousing nature of these images—or even a combination of all three of these factors? My thesis aims to answer these questions, and to clarify which elements of negative images are responsible for the boundary restriction effect. Specifically, I aim to isolate valence, attention, and arousal, and explore how these factors boundary judgement errors, using a variety of testing paradigms to measure boundary judgement errors. By isolating these factors, my thesis sheds light on the underlying mechanism of boundary restriction.

2 Overview of Methods and Research Objectives

As outlined in Chapter 1, prior research on boundary restriction has yielded mixed results, with some researchers suggesting that it is a subtle phenomenon, and may be overshadowed by the more robust boundary extension phenomenon (e.g., Beighley, Sacco, Bauer, Hayes & Intraub, 2018; Candel, Merckelbach, & Zandbergen, 2003; Intraub, Bender, & Mangels, 1992). The overarching aim of my thesis is to investigate the possible mechanisms underlying boundary restriction. Specifically, I aim to investigate the role of valence and attention (using negative images and presentation time; Experiment 1), valence alone (using negative and positive priming on neutral images; Experiments 2a-2e); attention alone (using pseudoneglect; Experiments 3a-3b; using dot-probes; Experiment 4), and arousal (using a noise stressor or image type; Experiments 5, 6a-6b, and 7a-7b). The following sections provide a brief overview of the thesis structure and each chapter's objective.

2.1 Chapter 3 – Experiment 1

This chapter aims to examine whether the potential combination of image valence and attention induce boundary restriction. To address this aim, I presented either neutral or negative images for a shorter (250ms) or a longer presentation time (2000ms). After a ten-minute delay, participants undertook a forced-choice recognition test, where they selected the image they had seen during encoding from two options: a closer-up version of the original, and the original; or a wide-view version of the original, and the original.

I found that overall boundary extension was the most common error, regardless of valence, even when investigated with an extreme negative subset of our images. This experiment revealed that shorter presentation time increased the rate of boundary restriction errors, but—contrary to our prediction—boundary extension errors were not affected. These findings provide some evidence that attention may influence boundary restriction, and that valence may not affect boundary judgement errors.

2.2 Chapter 4 – Experiments 2a-2e

This chapter aims to determine whether valence—in the absence of other factors (such as attention-grabbing objects, or incidental arousal)—plays a role in the boundary restriction effect. To address this aim, I used a set of neutral images, and primed the valence of these images using a procedure adapted from Porter, ten Brinke, Riley, and Baker (2014). I presented six images (Experiments 2a–2d) or 30 images (Experiment 2e) to participants. Each image was accompanied by either a negative, positive, or no prime. After a ten-minute delay, participants saw the same images again, and rated how much the image had changed using a camera distance rating (Experiments 2a & 2b), or a slider rating (Experiments 2c-2e).

I found that, overwhelmingly—and despite successfully manipulating image valence—participants made boundary extension errors regardless of valence prime. These results indicate that valence alone may not be enough to induce boundary restriction.

2.3 Chapter 5 – Experiments 3a and 3b

Because the results from Experiment 1 indicated that attention may be one of the factors involved in inducing boundary restriction, this chapter aims to determine whether attention—in isolation of negative valence—plays a role in the boundary restriction effect. Here we relied on an incidental visual attention bias: specifically, we know that neurotypical people pay slightly more attention to the left than the right side of space (Bowers & Heilman, 1980), an effect known as pseudoneglect. As demonstrated by Dickinson and Intraub (2009) diffuse attention to the right side of space (where attention is typically less focused) exacerbates boundary extension. I expanded on Dickinson and Intraub's (2009) study by investigating whether this pattern would occur for images presented in each visual hemifield (rather than images across the entire visual field with individual left-and right-located items). In Experiment 3a, I presented images one at a time (on either the left or right visual hemifield), and then tested participants' memory for those images. I used a forced-choice recognition test, where participants selected the image they had seen during encoding from an option of two (presented top and bottom): a zoomed in version of the original, and the original; or a zoomed out version of the original, and the original. In Experiment 3b, I presented images in pairs (one on the left, and one on the right visual hemifield), and then tested participants' memory for *one* of the two presented images (target image). In both experiments, and consistent with most boundary judgment studies, participants were more likely to make boundary extension errors than boundary restriction errors. Furthermore, side of presentation (left, right) did not interact with the type (restriction, extension) of boundary judgement errors. Presenting images in isolated visual hemifields may not affect boundary judgement errors in the same way as images presented over the whole visual field. Further, pseudoneglect leads people to perceive that there is *more space* on the left, a pattern at odds with the typical relationship between attention and boundary extension, where participants make more expansive boundary extension errors when their attention is diffuse (e.g., Intraub, Daniels, Horowitz, & Wolfe, 2008). Finally, an incidental attention bias may not be the best way to measure the effect of attention on boundary judgement errors, and a direct manipulation of attention may be more effective.

2.4 Chapter 6 – Experiment 4

This chapter uses a modified dot-probe paradigm to directly investigate whether attention alone (i.e., irrespective of valence) plays a role in the boundary restriction effect. I manipulated subjects' attention by presenting a dot-probe either where a neutral image subsequently appeared (congruent trials – attention focussed) or opposite to where the image subsequently appeared (incongruent trials – attention distracted). I then tested participants' boundary judgements by presenting the same image again at test. Participants were told that some images had been altered so that the camera was either slightly closer to, or slightly further away from, the main object(s) in the image (Candel et al., 2003; Intraub et al., 1992; Safer et al., 1998; Takarangi, Oulton, Green, & Strange, 2016). Participants then judged whether the "camera distance" matched the camera distance of the image that they saw earlier, using a slider scale, for the congruent and incongruent trial images.

Participants made more boundary extension errors for incongruent trials—i.e., when their attention was drawn away from the images—than for congruent trials—where their attention was drawn to the images. This pattern is consistent with other boundary extension studies that have found that decreasing attention to images increases boundary extension errors (e.g., Dickinson & Intraub, 2009; Intraub et al., 2008; also see Hubbard, Hutchison, & Courtney, 2010 for a review). These results demonstrate that attention—rather than the negative valence itself—may affect boundary judgement errors, leading to boundary restriction errors, and thus may be an important underlying mechanism of boundary restriction.

2.5 Chapter 7 – Experiments 5a, 5b and 6

This chapter aims to investigate another aspect of negative images: that they are usually accompanied by incidental arousal. One speculation is that boundary restriction occurs because of focussed attention given to negative stimuli, which is induced by the incidental arousal that often accompanies these images. Indeed we know that our visual system prioritises attention to highly arousing or stressful stimuli, at the expense of neutral, non-stressful stimuli (Sutherland & Mather, 2012; Mather, 2007), and we know that—for highly negative images—trait arousal can lead to boundary restriction (Mathews & Mackintosh, 2004).

To isolate arousal from image content and image valence, I manipulated arousal using an external stimulus (noise blasts). Across three experiments, participants viewed cropped or uncropped versions of images either with or without a noise. In Experiments 5a and 5b, participants completed a forced-choice recognition test, where participants selected the image they had seen during encoding from an option of two: a zoomed in version of the original, and the original; or a zoomed out version of the original, and the original. In Experiment 6, I altered the test to a camera-distance paradigm. In Experiment 5a, images presented with noise led to more boundary restriction errors and fewer boundary extension errors (compared to images without noise). In Experiment 5b, noise led to more boundary restriction errors, and in Experiment 6, noise attenuated boundary extension. Taken together, these data suggest that heightened arousal can lead to both a reduction in boundary extension, and an increase in boundary restriction. These results demonstrate that arousal—rather than negative valence—can affect boundary judgement errors, leading to boundary restriction errors, and thus may be an important underlying mechanism of boundary restriction.

2.6 Chapter 8 – Experiments 7a and 7b

This chapter aims to investigate some of the limitations in Experiments 5a, 5b, and 6. To investigate whether conditioning arousal to one category of images was necessary, in Experiment 7a I used 24 images from a single category (rather than two categories of images), presented with or without a stress-inducing noise. At test, participants selected the image they had seen during encoding. Participants made boundary restriction errors and boundary extension errors regardless of noise condition, suggesting that conditioning the noise stimulus to an image category (in Experiments 5a, 5b and 6) was necessary.

To examine the effect of arousal arising specifically from composition differences between image categories, Experiment 7b compared the same image categories used in Experiment 5a, but in the absence of the arousing noise stimulus. Participants undertook the same procedure as in Experiment 5a. The absence of the arousal manipulation reduced overall boundary restriction errors, when compared to Experiments 5a, 5b, 6, and 7a. Thus, arousal appears necessary for inducing some boundary restriction. Last, despite nature images being rated as more arousing and more pleasant than object images, there was no difference in boundary judgement errors, suggesting the external arousal manipulation in conjunction with the nature images seemed to combine to induce boundary restriction. In order to induce boundary restriction, the arousal stimuli may need to be aversive (e.g., noise stressor) rather than positive (e.g., nature images).

3 Does Emotion Modify the Boundary Extension Effect?

3.1 Abstract

Visual memory is important for all aspects of day-to-day life. Yet visual memory, particularly memory for scenes, is prone to errors. Ordinarily, when people view scenes, they tend to recall the boundaries as more expansive than they actually were, a phenomenon known as boundary extension. However, the opposite memory error can also occur. When people view negative scenes, they sometimes recall the boundaries as *narrower* than they were, a phenomenon known as *boundary restriction*. One explanation for boundary restriction is that negative valence captures attention, resulting in a survival-driven attention bias towards the scene's central details, and rejection of peripheral details. As a result, people remember a restricted version of the scene. However, negative images alone do not result in consistent boundary restriction errors (e.g., Candel et al., 2003), possibly due to the incidental attention-grabbing nature of the images, rather than the negative valence of the images. Therefore, we aimed to test whether attention and valence combine to induce boundary restriction. We manipulated attention by presenting attention-grabbing and non attention-grabbing images (within-subject: negative, neutral images) in two presentation times (between-subjects: 250ms, 2000ms). Participants then picked the image they had originally seen from either a more expansive version, or a cropped version of the same image. Participants who viewed images for 2000ms made fewer boundary restriction errors, but presentation time did not affect boundary extension errors. Valence did not affect boundary error type or rate. These data suggest that the attention-grabbing nature of negative stimuli may be an underlying mechanism of boundary restriction.

3.2 Introduction

When people view scenes, they generally recall the boundaries as being more expansive than they actually were (Intraub & Richardson, 1989). This common and easily replicable memory error is known as boundary extension (e.g., Chapman, Ropar, Mitchell, & Ackroyd, 2005; Daniels & Intraub, 2006; Dickinson & Intraub, 2009; Intraub & Bodamer, 1993; Intraub et al, 1996; Intraub, 2004; Intraub, Hoffman, Wetherhold, & Stoehs, 2006; McDunn Siddiqui, Brown, 2013). However, negative scenes can result in the opposite pattern, whereby people remember the boundaries of a scene as narrower than they actually were, known as *boundary restriction* (e.g., Mathews & Mackintosh, 2004; Safer, Christianson, Autry, & Österlund, 1998). Yet several other studies have found no boundary restriction effect for negative images (e.g., Beighley, Sacco, Bauer, Hayes & Intraub, 2018; Candel, Merckelbach, & Zandbergen, 2003). One explanation for these mixed findings is that boundary restriction is actually driven by the attention-grabbing nature of negative images, rather than negative images themselves. For example, the negative content at the centre of a scene may act as an "attention magnet" (Laney, Campbell, Heuer, & Reisberg, 2004). This attention magnet leads to better encoding of that information, at the expense of peripheral information (e.g., Mather, 2007), and likewise eliminates imagining beyond the boundary (see Levine & Edelstein, 2009 for a review). We posit that attention capture by central information would be exacerbated when participants have less time to attend to an image, because they would preferentially attend to the salient aspects of that image, ultimately leading to a greater chance of boundary restriction. However, to the extent that neutral images do not contain these salient, attention-grabbing aspects, we would not expect presentation time to affect boundary judgments. To test these ideas, we presented images with attention-grabbing stimuli (negative), or without attention-grabbing stimuli (neutral) for either 250ms or 2000ms. First, we aimed to replicate the finding that negative valence induces boundary restriction, and second, to examine whether this effect would be exacerbated by shorter presentation times.

Several studies show that negative images can both attenuate and/or reverse the boundary extension effect (boundary restriction; Christianson & Loftus 1987; Mathews & Mackintosh, 2004; Ménétrier, Didierjean, & Vieillard, 2013; Safer et al., 1998; Takarangi, Oulton, Green, & Strange, 2015) or improve memory for central versus peripheral details (Waring, Payne, Schacter, & Kensinger, 2010). In one example, participants viewed a series of images from the International Affective Picture System (IAPS; Lang, Bradley, & Cuthbert, 2008) for five seconds each (as estimated by the participants themselves; Mathews & Mackintosh, 2004). The images were negative and neutral, based on the standardised IAPS ratings. Participants then viewed two closer-up versions, the original, and two wider-angled versions of that same image (five options in total). Participants' task was to pick the original image from the array. In general, participants made boundary extension errors, picking one of the wider angled versions of the image. However, when the researchers split the images into subsets—to investigate the pattern of boundary judgements for highly negative, highly arousing images in particular—participants displayed more boundary restriction for images in this subset than for the other images in the study. In a second example, Safer et al. (1998) used a slide series with a target slide at the end of the series that depicted either a negative outcome (e.g., a man's gloved hand holding a bloody knife; a woman with her throat being cut; a woman lying in the grass covered in blood) or a neutral outcome (e.g., a man near a car; a man's gloved hand holding keys; a woman picking flowers). Similar to Mathews and Mackintosh's findings, participants tended to make boundary restriction errors for the negative scenes, and boundary extension errors for the neutral scenes.

However, several other studies have found no memory narrowing or boundary restriction for negative images (Candel et al 2004; Davies, Smith, & Bilcoe, 2007; Intraub et al., 1992; Kensinger, Garoff-Eaton, & Schacter, 2006; Steinmetz & Kensinger, 2013). For example, Candel et al. (2003) had participants encode neutral images or highly negative images from the IAPS, then at test draw the images from memory (Experiment 1) or rate the camera distance of identical images (Experiment 2). In Experiment 1 (Candel et al., 2003), participants drew the images from memory, and they tended to make boundary extension errors regardless of image valence. In Experiment 2 (Candel et al., 2003), participants rated the camera distance of the images at test (rating how much they had changed from “farther than the original”, to “closer than the original”, with an option of “no change”) and participants tended to be accurate, rating the images as no change 67-80% of the time. However, like Experiment 1, when participants did make errors, they tended to be boundary extension errors, regardless of image valence.

One obvious discrepancy between studies that did vs. did not find boundary restriction is test format (drawing from memory and camera distance rating; Candel et al, 2003; forced choice; Mathews & Macintosh, 2004). A second important methodological difference is the stimuli used. Although the images in Candel et al. were *similar* (i.e., in valence) to the ones Mathews and Mackintosh used, it is possible that the images differed on other elements (such as their attention-grabbing content). Indeed, one explanation for the mixed findings is that the mechanism driving boundary restriction is not the *valence* of the images, but rather the attention-grabbing nature of negative images. To our knowledge, no prior research has investigated whether boundary restriction is the result of attention (induced by negative stimuli), rather than the negative stimuli themselves.

The idea that attention is the underlying mechanism behind boundary restriction fits with how attention affects boundary *extension* errors. We know that diffuse attention can exacerbate boundary extension errors. For example, in Intraub, Daniels, Horowitz, and Wolfe (2008) participants viewed close up or wide angled images superimposed with numbers. Participants either performed a visual search task—where they counted the number of "5s" amongst digit "2" distractors—or simply rated how difficult it was to see the images behind the superimposed digits. Thus, there were two conditions: the diffuse attention group (visual search task) and the focussed attention group (difficulty rating task). Participants then rated their memory for the image boundaries, for images that were the same, closer up, or wider angled by rating them as "a lot closer-up", to "a lot farther away". Participants in the diffuse attention group tended to make more expansive boundary extension errors than the participants in the focussed attention group. This finding suggests drawing attention away from the image, by having participants count the digits, increased extrapolation beyond the boundaries, which we know increases boundary extension (McDunn et al., 2014). The lack of boundary extension errors in the focussed attention group is also meaningful. Where participants focussed on the images, they presumably had less opportunity to extrapolate/imagine beyond the boundaries, and thus made fewer boundary extension errors. Put simply, the attentional capture seems to attenuate boundary extension, which suggests it may also induce boundary restriction.

To address this possibility, in the present study we used negative images, but limited the presentation time in order to ensure that attention was grabbed quickly—without the opportunity to imagine beyond the boundaries—for all participants in the study. A question arises however: What is the ideal presentation time to observe boundary restriction errors? Although boundary extension has been observed at a large variety of presentation times, boundary restriction has been observed only at relatively short presentation times. Boundary extension has been observed at presentation times of 250ms to 15 s (Chapman et al., 2005; DeLucia & Maldia, 2006; Dickinson & Intraub, 2008; Dickinson & Intraub, 2009; Gottesman & Intraub, 2002; Intraub, Gottesman, Willey & Zuk, 1996; Intraub et al., 2006; Intraub et al., 1992; Intraub et al., 1998; Munger, Owens, & Conway, 2005). Conversely, despite being studied in a similarly wide range of presentation times (250ms to 15 seconds; Candel et al., 2003; Intraub, Bender, & Mangels, 1992; Kensinger, Garoff-Eaton, Schacter, 2006; Mathews & Mackintosh, 2004; Safer et al., 1998; Touryan, Marian, & Shimamura, 2007), boundary restriction has only been observed in presentation times between 250ms to 5000ms (Kensinger et al., 2006; Mathews & Mackintosh, 2004; Safer et al., 1998). We know that humans can differentiate between emotional and non-emotional stimuli quickly, in as little as 100ms (Sabatinelli, Lang, Bradley, Costa, & Keil, 2009); thus even in the short presentation times within this range, participants will be able to quickly determine the valence of the image, and thus devote cognitive resources to the highly salient content. With focus on the salient aspects, less imagination and extrapolation should occur, leading to a decrease in boundary extension, and an increase in boundary restriction—but only for the negative images.

We based our experimental procedure on Mathews and Mackintosh (2004), with three key methodological changes. First, Mathews and Mackintosh relied on participants' own estimates of viewing time for each image during encoding. For the present experiment, we administered all phases on a computer, to ensure that each image was viewed for precisely the set presentation time. Second, we increased the number of images from 24 to 48 to provide more opportunities for participants to make boundary errors. Third, because of limited backgrounds on some of our images, we reduced the forced choice test of boundary judgements to two options, instead of four. Last, based on pilot testing showing ceiling effects for accuracy, we reduced presentation time from 5000ms to 2000ms in one of our presentation time conditions; in the other condition we used 250ms. Thus, we presented negative or neutral images for either 250ms or 2000ms, and then tested participants' memory for those images. Our aims in this study were twofold. First, we aimed to replicate the finding that negative valence modifies boundary judgments, such that boundary extension errors are less likely for negative compared to neutral stimuli. Second, we aimed to test whether this effect would be exacerbated by shorter presentation times.

3.3 Experiment 1 Method

Participants. To estimate required sample size, we conducted a power analysis (G*Power) assuming a small interaction effect size (η_{p2} = .02) for the three way interaction (boundary error differences (encoding type: cropped, uncropped) on differently valenced images (image valence: negative, neutral), for different presentation times (presentation time: 2000ms, 250ms)). We chose this effect size due to the small sized differences between encoding type, valence, and arousal observed in previous boundary restriction studies (e.g., Mathews and Mackintosh, 2004). We needed at least 180 participants to maintain 80% power at the .05 significance level (repeated measures, within/between effects). To maintain counterbalancing, we increased this sample size to 186. We recruited 186 participants with normal to corrected-to-normal vision from an undergraduate research participation pool, and the wider community. The majority of participants were female (69%) and ranged in age from 18-40 (*M* = 21.11, *SD* = 4.71). The majority identified their ethnicity as Caucasian (including "White"; 69%). Others identified as Asian (22%), European (1.5%), Indian (.5%), Indigenous Australian (.5%), Middle Eastern (.5%), mixed ethnic origin (5%); the remaining participants did not report ethnicity (1.5%). Participants received course credit, or payment

(AUD$10) for their time.

Design. We used a 2 (encoding type: cropped, uncropped) x 2 (image valence: negative, neutral) x 2 (presentation time: 2000ms, 250ms) mixed design. The within-subject factors were error type and image valence, and the between-subjects factor was presentation time.

Materials and Procedure

The Flinders University Social and Behavioural Research Ethics Committees approved this research. Participants completed the experiment on an Apple iMac desktop computer using Superlab. Participants were situated approximately 40cm from the screen. We told participants that our study would allow us to evaluate the impact of graphic visual images on mock juror decision-making and advised them that their participation in the study would involve exposure to highly negative images.

Stimuli. We selected 48 images from the International Affective Picture System, 24 negative, and 24 neutral (IAPS; Lang et al., 2008). Each IAPS image has normed emotional valence ratings (1 = very unpleasant, 9 = very pleasant). We selected images on the basis of these ratings; our negative images had valence ratings below 3 (range = 1.31 – 2.73, M = 2.03, SD = 1.41; arousal ratings: range = 4.79 – 7.16, M = 6.11, SD = 2.03) and neutral images had valence ratings around 6 (range = 4.38-8.21, M = 6.05, SD = 1.44; arousal ratings: range = 3.04 – 5.61, M = 4.19, SD = 2.12). We cropped all 48 images to 75% of their original size, resulting in two image versions (75% and 100%). We counterbalanced the presentation of cropped and uncropped images versions, so that all images were presented in both sizes and equally often, with the order of presentation randomised for each participant.

Encoding phase. Participants viewed each image and then rated its unpleasantness on a Likert scale, from 1 = very pleasant to 7 = very unpleasant. After viewing and rating the 48 images, participants completed an unrelated 10-minute filler task (to ensure we were measuring memory rather than perceptual errors; as per Safer et al. 1998).

Testing phase. At test, participants viewed the same image again, alongside the opposite version—75% cropped or 100% uncropped—for each of the 48 images. We randomised the order of the test slides, and the position of the cropped and uncropped versions, so that sometimes participants saw the cropped version on the right, and sometimes they saw the cropped version on the left. We asked participants to indicate the image that they were originally shown, and to rate how confident they were in their answer from 1 = not at all certain to 5 = absolutely certain, with a separate option "completely guessing". We then thanked participants, and gave them full debriefing information.

3.4 Experiment 1 Results and Discussion

Analysis of valence manipulation. First, we examined whether participants rated negative images as more unpleasant than neutral images. We ran a 2 (valence: negative, neutral) x 2 (presentation time: 2000ms, 250ms) mixed ANOVA on pleasantness ratings. As expected, participants rated the negative images as more unpleasant (M = 5.73, SD = 0.67), than neutral images (M = 2.16, SD = 0.69), regardless of presentation time, $F(1, 184)$ = 3114.85, $p < .001$, $\eta_p^2 = .94$. There was no interaction between valence and presentation time, and no main effect of presentation time (Fs < 1).

Analysis of boundary errors. Examining the proportion of participants' errors overall, we found that participants were inaccurate—essentially at chance—at identifying the correct image (M = .44, SD = 0.08). We next investigated whether the valence of the images or presentation time affected the frequency and type of boundary judgement errors. We conducted a 2 (encoding type: cropped, uncropped) x 2 (valence: negative, neutral) x 2 (presentation time: 2000ms, 250ms) repeated measures mixed ANOVA to test our primary research questions: does negative valence reduce boundary extension and induce boundary restriction, and are these effects exacerbated by short presentation times?

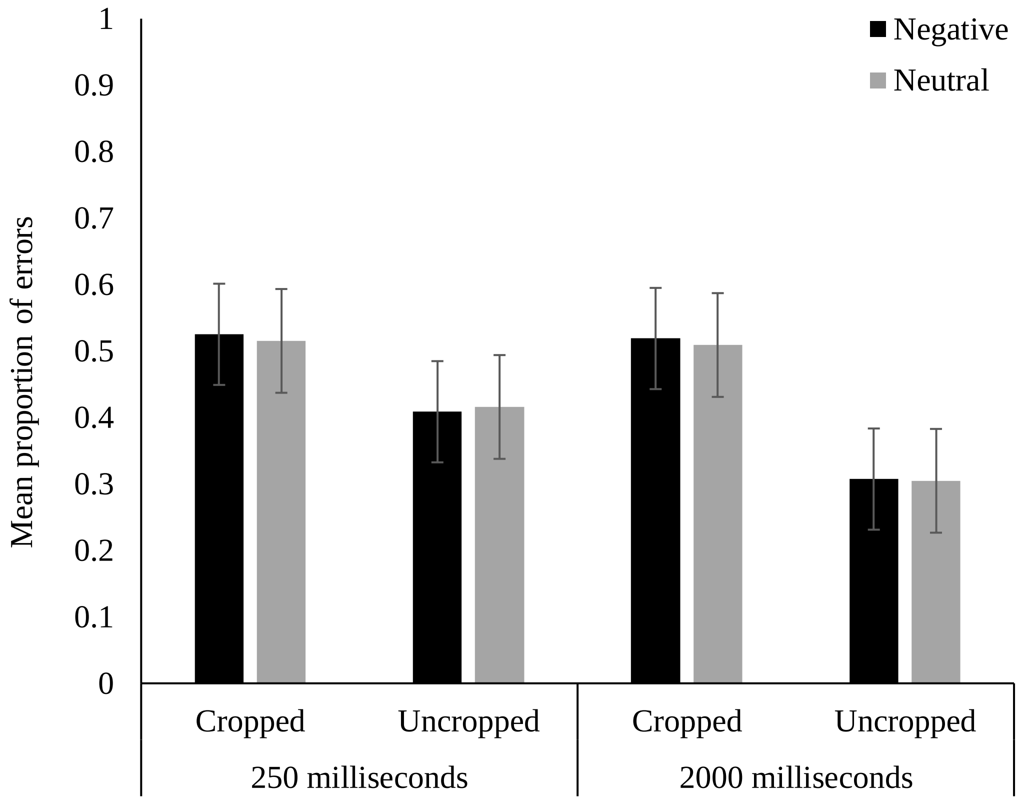

Figure 3.1. Mean proportion of errors by encoding type, valence and presentation time. Error bars represent 95% confidence intervals

Consistent with previous research, we found a main effect of encoding type, with participants making more errors for cropped images (boundary extension; *M* = 0.52, *SD* = 0.17) than uncropped images (boundary restriction; *M* = 0.36, *SD* = 0.15), $F(1,184) = 64.16$, $p < .001$, $\eta_{p}^{2} = .25$. Participants made more errors—of both types—in the 250ms presentation time condition (*M* = 0.47, *SD* = 0.15) than in the 2000ms presentation time condition (*M* =

0.41, *SD* = 0.16), *F*(1,184) = 23.12, *p* < .001, η_p^2 = .11—a main effect of presentation time. There was also a significant interaction between encoding type and presentation time, *F*(1,184) = 6.42, *p* < .01, η_p^2 =.03. This interaction was driven by a difference in boundary restriction errors; though participants in both presentation time conditions made a similar number of boundary extension errors (*t*(184) = 0.25, *p* = .80, *d* = 0.04 95% CI [0.25, 0.32]), participants made more boundary restriction errors in the 250ms presentation time condition, than in the 2000ms presentation time condition (*t*(184) = 5.11, *p* < .001, *d* = 0.75 95% CI [0.45, 1.05]; see Figure 3.1). There was no significant interaction between valence and presentation time, *F* < 1. Surprisingly, and contrary to our hypothesis, we did not see a difference in boundary restriction errors for negative versus neutral images, i.e., no significant interaction between encoding type and valence, *F* <1.

Valence investigation. We did not find the expected difference in boundary restriction errors for negative versus neutral images. However, it is possible that the valence of the negative images bled over to the neutral images, meaning that as the experiment went on, the effect of valence became less distinct to the negative image category. To investigate this possibility, we split the data into 3 time periods corresponding to the first 16 images the participants saw (image group 1), the next 16 (image group 2), and the last 16 images (image group 3). We ran a 3 (group: image group 1, image group 2, image group 3) x 2 (valence: negative, neutral) x 2 (presentation time: 250ms, 2000ms) mixed ANOVA for valence ratings. Consistent with earlier analyses, we found a main effect of valence, with participants rating the negative images as more unpleasant (*M* = 5.73, *SD* = 0.77) than the neutral images (*M* = 2.16, *SD* = 0.78), *F*(1, 184) = 3114.85, *p* < .001, η_p^2 = .94. There was also a main effect of group, whereby participants' valence ratings—for both types of images, in both conditions— became more unpleasant over time (image group 1: *M* = 3.86, *SD* = 0.79; image group 2: *M* = 3.98, *SD* = 0.74; image group 3: *M* = 4.00, *SD* = 0.79) $F(2, 368) = 10.26$, $p < .001$, $\eta_p^2 = .05$. There was no main effect of presentation time, $F < 1$. There was a significant interaction between group and presentation time, $F(2, 368) = 5.82$, $p = .03$, $\eta_p^2 = .03$, with unpleasantness ratings increasing over time in both presentation time conditions (see Table 3.1 for descriptive statistics). There was also a significant interaction between valence and image group, $F(2, 368) = 16.79$, $p < .001$, $\eta_p^2 = .08$ (see Table 3.1 for descriptive statistics). This interaction was qualified by the fact that there was a change in valence ratings over time for the negative images ($F(2, 370) = 24.31$, $p < .001$, $\eta_p^2 = .12$), but not for neutral images ($F < 1$). Consistent with the earlier analysis, there was no interaction between valence and presentation time ($F < 1$), meaning that participants rated the negative and neutral images as unpleasant and pleasant respectively across conditions. There was no three-way interaction between group, valence and presentation time $F(2, 368) = 1.33$, $p = .27$, $\eta_p^2 = .01$. These data demonstrate that the negative images became more negative over time, but this effect did not bleed over to the neutral images.

Table 3.1. Valence ratings over time (group: image group 1, image group 2, image group 3), by between-subjects factor (presentation time: 250ms, 2000ms)

	250ms presentation M(SD)	**2000ms presentation M(SD)**	**Negative images M(SD)**	**Neutral images M(SD)**
Image group 1	3.89 (0.79)	3.83 (0.78)	5.53 (0.80)	2.19 (0.77)
Image group 2	3.94 (0.70)	4.02 (0.79)	5.80 (0.72)	2.17 (0.76)
Image group 3	3.91 (0.74)	4.08 (0.85)	5.86 (0.76)	2.14 (0.79)

Recall that we were particularly interested in how negative images affect boundary judgement errors. Previous studies have indicated that boundary restriction is a subtle effect that may be overshadowed by the more robust boundary extension effect. Indeed, a strength of the present study was that we gave participants equal opportunity to make boundary extension and boundary restriction errors (in contrast to Safer et al., 1998). It is possible that boundary extension and boundary restriction exist on a continuum, with correct memory existing somewhere in between these two extremes. Thus, with boundary extension being the more robust error, boundary restriction is unlikely to occur. However, in the present study, participants were only able to make a boundary restriction error or be correct in their answer (or make a boundary extension error or be correct in their answer), thus there was no direct competition of the two types of boundary judgement error. Despite this methodological strength, we were still unable to induce boundary restriction errors, suggesting that additional methodological issues are at play.

Recall that Mathews and Mackintosh (2004) only found a boundary restriction effect for their highly negative and highly arousing images. To investigate the possibility that only highly negative, highly arousing images would induce boundary restriction, we analysed a subset of the data. We first divided our negative images into two groups: 12 low negative, low arousal images and 12 extreme negative, high arousal images (based on the original IAPS ratings where low valence indicates more negativity; see Table 3.2 for the descriptive statistics. Our subset of extreme negative images were comparable to Mathews and Mackintosh's (2004) extreme negative images.

Table 3.2. Subsets of negative images (valence: 1 = very unpleasant to 9 = very pleasant and arousal: 1 = least arousing to 9 = most arousing) from the present study compared to Mathews and Mackintosh (2004) extreme negative valence ratings.

	Valence *M*(SD) (extreme negative)	Arousal *M*(SD) (extreme negative)	Valence *M*(SD) (low negative)	Arousal *M*(SD) (low negative)
Present study	1.74 (1.29)	6.50 (2.31)	2.31 (1.54)	5.73 (2.30)
Mathews and Mackintosh (2004)	1.76 (1.34)	6.99 (2.08)		

Next we ran a 2 (encoding type: cropped, uncropped) x 2 (subset: extreme negative, low negative) x 2 (presentation time: 250ms, 2000ms) mixed ANOVA on proportion of errors. Consistent with earlier analyses, and despite the fact that all of these images were negative, participants still made more boundary extension errors (M = 0.40, SD = 0.21) than boundary restriction errors (M = 0.27, SD = 0.19), a main effect of encoding type, $F(1, 184) = 61.77$, $p < .001$, $\eta_{p2} = .25$. Participants also made more boundary errors (overall) for the extreme negative images subset (M = 0.44, SD = 0.23), than for the low negative image subset (M = 0.24, SD = 0.18) a main effect of image subset, $F(1, 184) = 173.41$, $p < .001$, $\eta_{p2} = .49$. There was no main effect of presentation time and no interaction between subset and presentation time, Fs < 1; there was also no interaction between encoding type and presentation time condition, $F(1, 184) = 1.05$, $p = .31$, $\eta_{p2} = .006$. There was, however, an interaction between encoding type, subset and presentation time $F(1, 184) = 13.46$, $p < .001$, $\eta_{p2} = .07$ (see Figure 3.2).

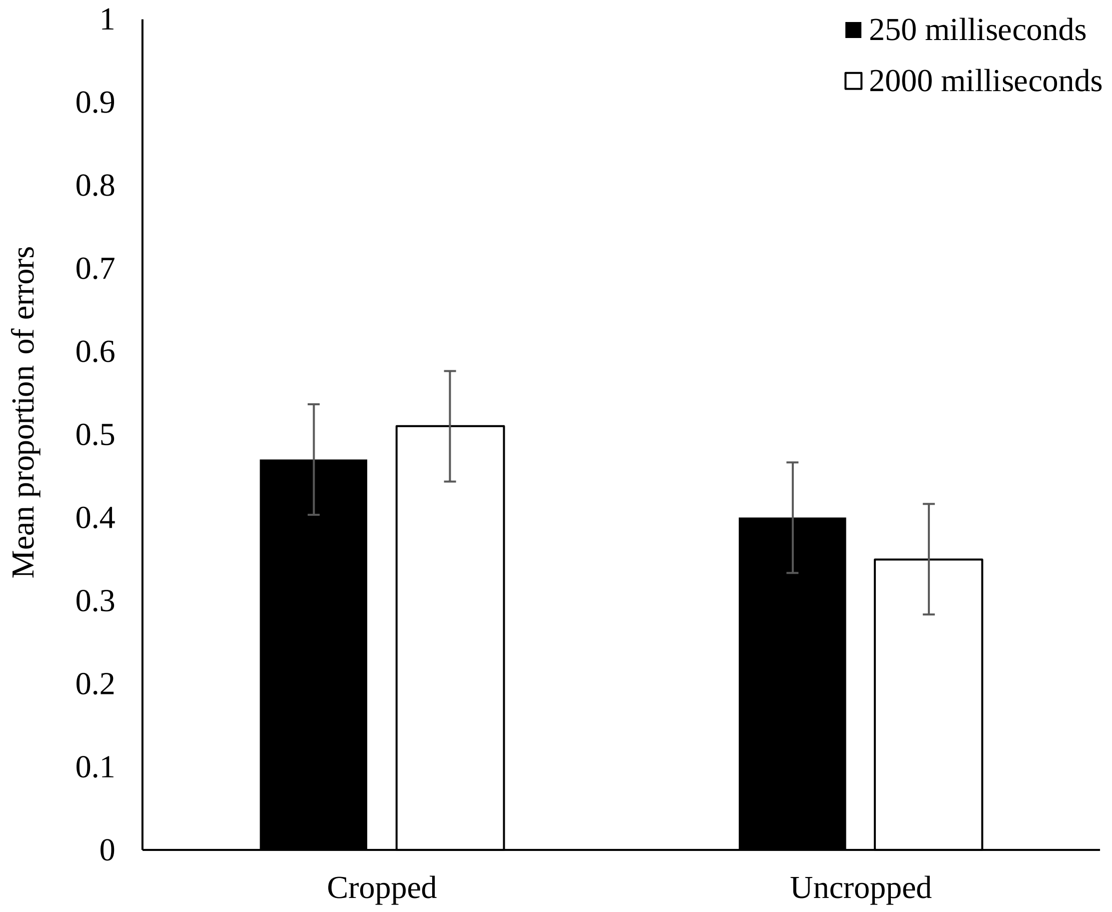

Figure 3.2. Mean proportion of errors by encoding type and presentation time for extreme negative subset. Error bars represent 95% confidence intervals

This interaction was clarified by a difference in boundary restriction errors between conditions; specifically, participants who saw the images for 250ms made more boundary restriction errors, and participants who saw the images for 2000ms made fewer boundary extension errors. Participants made fewer boundary restriction errors in the 2000ms presentation time condition than in the 250ms presentation time condition, $t(184) = 2.87$, $p = .005$, $d = 0.42$ 95% CI [0.13, 0.71] while participants made similar number of boundary extension errors in the 2000ms presentation time condition as in the 250ms presentation time condition *t*(184) = 2.16, *p* = .03, *d* = 0.32 [0.03, 0.61]. This pattern is the same as the one we found for all of the images (with participants making more boundary restriction errors in the 250ms version) but unlike our initial analysis, the difference in this subset was in this case significant.

To summarise, our data reveal several important findings related to the boundary restriction phenomenon. First, although presentation time did not affect boundary *extension* errors, it did affect the rate of boundary *restriction* errors. Shorter presentation times increased boundary restriction errors, whereas longer presentation times did not affect boundary extension errors. Participants made more boundary errors overall for the extreme negative subset than for the low negative subset. Thus, this finding lends partial support to the hypothesis that negative emotional valence increases the tendency for boundary restriction errors, yet we cannot definitively make that claim, because the pattern occurs for both error types and both high and low negative images. Our data suggests that valence may not be the only factor at play in inducing boundary restriction, and that attention-capture induced here by a short presentation time may explain why boundary extension occurs. Second, we found that participants made both types of errors, but the most common error type was boundary extension. Last, we found that valence did not affect error type, which was consistent with some research (Candel et al., 2003), but inconsistent with other research (Mathews & Mackintosh, 2004; Safer et al., 1998).

Despite some inconsistencies, some of our data confirms earlier research. Recall that Mathews and Mackintosh (2004) only found their boundary restriction effect for images that were highly negative and highly arousing. Following this finding, we divided our negative images into low negative and extreme negative groups. Analysis of this extreme negative subset of images revealed that participants who saw images for 250ms made more boundary restriction errors than participants who saw images for 2000ms. This finding is in line with our prediction that negative images would capture and hold attention, and this effect was stronger for the shorter timeframe. We posit that participants made more boundary restriction errors—regardless of valence—for shorter presentation time, because they had limited time to encode and imagine/extrapolate beyond the boundaries, thus, they did not activate their perceptual schema, making boundary restriction more likely.

Our experiment has some key limitations, which provide pathways for future research. First, we reasoned that at shorter presentation times, attention would be grabbed quickly and focused on the negative stimuli. Indeed, we know that valence can be recognised rapidly (in as little as 100ms; Sabatinelli, Lang, Bradley, Costa, & Keil, 2009), and that our visual system preferentially attends to negative over neutral stimuli (e.g., Williams, Moss, Bradshaw, & Mattingley, 2011). There is evidence that this preferential attention can persist even during divided attention tasks (when people are engaged by concurrent tasks; Clark-Foos & Marsh, 2008). Nevertheless, using presentation time to manipulate attention relied on participants responding to the short presentation time by making eye fixations primarily on the central, salient parts of the images. However, without eye tracking data, we cannot definitively conclude that the paradigm was effective at either (a) initially capturing attention, or (b) holding attention on the central, salient parts of the images once captured. Further, because participants had little time to attend to the images, they may have had to engage *more* imagination and extrapolation to fill in parts of the missing visual information, and this could have washed out any boundary restriction effects. Indeed, perhaps longer presentation times led to participants paying *more* attention overall to the image (than for the short presentation time) because they have more time to attend to the image.

Future studies could focus on manipulating attention using a paradigm to draw attention to or away from images, or use eye tracking to confirm where participants' attention is directed. Second, to measure boundary judgement errors, we used a forced choice test paradigm. We did so because of the success that previous studies have had at finding a boundary restriction effect using this test paradigm (e.g., Mathews & Mackintosh, 2004; Safer et al., 1998). However, this paradigm is a coarse-grained measure that requires the participant's memory to match one of the choices shown to them, and therefore may not capture subtle boundary judgement errors. Furthermore, this test paradigm does not allow us to measure the extent to which a participant makes an error. For example, participants could be making both types of errors regardless of valence, but they may be making more *expansive* boundary extension errors for neutral images, than negative images and/or more *expansive* boundary restriction errors for negative than neutral images. Indeed, capturing boundary restriction may require a more fine-grained measure (e.g., camera distance paradigm; Safer et al., 1998). Finally, as previously noted, negative images bring with them many other complex factors that may or may not exacerbate attentional capture. One additional example is that the same negative stimulus may be more or less salient depending on the personal attributes—such as trauma history—personal fears and even clinical phobias—of each participant (Alpers, Gerdes, Lagarie, Tabbert, Vaitl, & Stark, 2008; Lundh & Öst, 1996; McIntyre & Graziano, 2016). It is possible that participants' own reactions to the images may also affect boundary judgement errors. Future studies could measure participants trauma history and/or phobias, or could use a more consistent manipulation of attention to ensure that attention is captured consistently across participants.In summary, we found that shorter presentation time increased the rate of boundary restriction errors, and this effect was stronger among extreme negative images. However, images valence alone did not affect boundary restriction errors.

Our findings provide evidence for the assertion that attention may be one of the key underlying mechanisms for the boundary restriction effect.

4 The Influence of Primed Positive and Negative Emotion on Boundary Errors for Neutral Images

4.1 Abstract

People tend to erroneously remember seeing beyond the edges of a scene, a memory error known as boundary extension. However, sometimes people "zoom in" when viewing negative stimuli, resulting in the opposite memory error: boundary restriction. Yet findings for boundary restriction are mixed, perhaps because emotional valence is often manipulated by comparing different scenes, which differ on many factors besides valence. In the present experiment series, we overcame this limitation by presenting the same images with different statements that primed negative, positive or neutral valence. We presented six images (Experiments 2a–2d) or 30 images (Experiment 2e), each accompanied by either a negative, positive, or no prime. After a ten minute delay, we tested participants' memory for the images. In all five experiments, participants selected the extent to which the images at test had restricted or extended boundaries compared to their memory of the original image. Despite the successful valence manipulation, participants tended to make boundary extension errors regardless of valence prime. These experiments demonstrate that boundary extension persists in the presence of negative valence. We propose that valence alone is not enough to induce boundary restriction and suggest that other factors that usually accompany negative stimuli—such as arousal and attention—may be necessary to induce boundary restriction.

4.2 Introduction

Boundary restriction—where the boundaries of an image are sometimes remembered as narrower than they actually were—is a memory error that occurs with negatively emotional stimuli. However, while some evidence shows that negatively valenced images can induce boundary restriction (Mathews & Macintosh, 2004; Safer, Christianson, Autry, & Österlund,1998), or errors akin to boundary restriction (e.g., weapon focus, see Fawcett, Russell, Peace, & Christie, 2013 for a review), other evidence shows that negative valence merely reduces boundary extension (Menetrier, Didierjean, & Vieillard, 2013; Touryan, Marian, & Shimamura, 2007) or does not affect boundary judgement errors at all (Beighley, Sacco, Bauer, Hayes, & Intraub, 2018; Candel, Merckelbach, & Zandbergen, 2003; Davies, Smith, & Blincoe, 2007; Intraub, Bender, & Mangels, 1992; Kensinger, Garoff-Eaton, Schacter, 2006; Steinmetz & Kensinger, 2013). Most previous research has investigated how valence affects boundary judgments by comparing negative images with positive or neutral images (see Beighley et al., 2018, for an exception). However, these image categories differ in aspects besides valence, including content, colour, and composition. Moreover, several other factors often go hand-in-hand with negative valence, such as unusual, threatening or attention-grabbing aspects of an image, and emotionally arousing content. Due to these differences, we cannot conclude that boundary restriction effects arise in relation to negative valence specifically. To assess whether negative valence alone induces boundary restriction and/or attenuates boundary extension, we isolated negative valence from the other components of an image—such as image aversiveness, composition, and content—by presenting the same neutral images accompanied by either a negative, positive or no prime statement.

Prior research suggests that the attention-grabbing and/or arousing nature of negative images—rather than their valence per se—could be responsible for boundary restriction. Indeed, when high levels of arousal are present—for example due to high trait anxiety or particularly arousing, negative images—boundary restriction appears to be more likely (Mathews & Mackintosh, 2004). It is possible that certain items present in, or compositional features of, negative images draw attention and raise arousal. But to empirically test the specific role of valence, we need to separate it from other factors such as arousal and attention.

Beighley et al. (2018) attempted to address this issue by using two versions of the same image, but manipulating whether the actor was displaying a happy or a sad face (using Photoshop). Participants viewed either a series of images with actors displaying negative emotions, or identical images with actors displaying positive emotions. Overwhelmingly, participants made boundary extension errors, regardless of the valence of the images they saw. On the basis of Beighley et al.'s results, we might conclude that valence *does not* affect boundary restriction. However, there are alternate explanations for the Beighley et al. findings. Despite controlling the content and composition of the differently valenced images (unlike prior research), Beighley et al. relied upon the emotion the images elicited, or mood (a fluctuation of emotional state; Frijda, 1988) rather than the inherent aversiveness (negative valence) or attractiveness (positive valence) of the image (Lang, Bradley, & Cuthbert, 2008). Put differently, participants needed to empathise with the actor in the negative image to experience negative emotion; the content of the image was not highly attention-grabbing or aversive like the images in Safer et al. (1998; e.g., a dead woman on the bonnet of a car). Nevertheless, participants who saw actors portraying negative emotions reported an overall decrease in positive mood—indicating that they *felt* negative after viewing the images. This shift in mood did not translate a change in boundary extension errors. Thus, from this study, we might conclude that *mood* does not affect boundary judgement errors. However, without a neutral condition, we cannot conclude that participants would have different boundary judgements for emotional and non-emotional images. Regardless, this study shows the importance of isolating valence, while keeping all other aspects of images identical, and our present series of experiments expands on this idea. With this methodological finding in mind, the present study aimed to address two issues: 1) isolating valence from the effect of mood and image aversiveness and 2) equating the images so that they are all *exactly* the same except for the valence.

One way to isolate valence from other aspects of the image is to use to the same images but to prime them with different valences. Here, we drew on the affective priming literature. When viewing a stimulus, semantic meaning—i.e., a snake rearing its fangs means death—happens almost instantly (Fazio, Sanbonmatsu, Powell, & Kardes, 1986). However, when a stimulus is ambiguous—e.g., a woman weeping could be crying in *sadness* or in *joy*—participants cannot process the semantic meaning as readily, and may instead rely on other internal processes such as mood, negative bias, or schemas (Ferguson, Bargh, Nayak, 2005). Thus, ambiguous stimuli can be interpreted differently depending on differently valenced primes, because their meaning is open to interpretation. For instance, participants are more likely to categorise ambiguous pictures of objects (could be interpreted as both positive and negative; e.g., cake), as positive after receiving positive primes (e.g., dessert), and negative after receiving negative primes (e.g., fat; Ferguson, Bargh, Nayak, 2005).

We know then that people will interpret ambiguous images differently depending on the background story associated with an image. In an example, Porter, ten Brinke, Riley, and Baker (2014) used ambiguous images preceded by a negative or positive statement about the image content to alter the valence of the images (Ambiguous Stimuli Affective Priming

(ASAP) paradigm; Porter et al., 2014). Participants rated the emotional valence and arousal of each image before and after the prime/image pair. They interpreted the ambiguous images as negative, positive or neutral according to the valence of the prime that had been paired with, it despite no changes to the actual stimulus itself.

In the present series of experiments, we used Porter et al.'s ASAP paradigm to alter the valence of images in a boundary judgement task, while keeping all other aspects of the image identical across conditions. We assessed whether the participants rated the images according to the prime valence by asking participants to make pleasantness and arousal ratings of each image. In other words, In Experiment 2a, we presented six images for 15 seconds each, accompanied by a negative, positive, or no prime, and tested boundary errors using a camera distance rating. In Experiment 2b, we replicated Experiment 2a, but in order to increase overall errors, we presented the images for *5 seconds* each. In Experiment 2c, we replicated Experiment 2b, but in order to capture subtle boundary judgement errors we tested boundary errors using a *slider rating*. Experiment 2d replicated Experiment 2c, but to increase the attention participants paid to the image at the time of encoding, we presented the image *following* the prime. In Experiment 2e, we replicated Experiment 2d, but to increase participants' opportunity to make both types of errors for multiple images in the same valence category, we used *30 images*.

4.3 Experiment 2a Method

Participants. Using G*Power, we calculated the sample size required to detect a medium within-subjects effect (f = .25) of valence (negative, positive, no-prime) on boundary judgements. We ran a repeated measures ANOVA with three levels in G*Power (Erdfelder, Faul, & Buchner, 1996), with f = .25, alpha = .05, power = .95, using a conservative estimate of zero correlation between repeated measures. The recommended sample size was 84. To allow for attrition, we increased the target sample size to 96. We recruited participants from Amazon Mechanical Turk's online research participation tool. Approximately half of participants (52%) were male, and participants identified their ethnicity as Caucasian (including "White" 69%), African American (14%), Latino (7%), Asian (4%), European (3%), and mixed (3%). The participants ranged in age from 21 to 71, with a mean age of 37.44 (*SD* = 11.81).

Design. We used a 3 (valence: negative, positive, no-prime) within-subjects design; the key dependent variable was mean camera distance. To analyse proportion of errors, we also used a 3 (valence: negative, positive, no-prime) x 2 (boundary error type: boundary extension, boundary restriction) within-subjects design.

Materials

Stimuli. We used the six target photographs and accompanying prime statements from Porter et al. (2014). The statements either depicted the event in the photograph negatively (e.g., *an amateur pilot screams S.O.S. into his microphone as the single engine in his plane dies and he loses control*), or positively (e.g., *an experienced pilot enjoys pulling "loop de loops" to the excitement of his grandchildren on the ground below*), or appeared without a statement. As fillers, we selected six additional neutral but not ambiguous images, from the International Affective Picture System (IAPS). We chose these images based on similar IAPS valence and arousal ratings to the target images. We generated a neutral statement (e.g., *Cruise liner arrives at Baltimore port in Maryland*) for each filler photo. We did not test participants' distance judgements for the filler images. For ease of narrative, we will refer to the different prime statements as "valence".

Procedure

The Flinders University Social and Behavioural Research Ethics Committees (SBREC) granted ethics approval. We used Qualtrics online software to administer the experiment.

Encoding Phase. Participants first completed the STAI–S, then viewed the six target images (and six filler images) for 15 seconds each, in a randomised order. Of the target images, two appeared with a negative prime, two appeared with positive primes, and two appeared without primes (no-prime). We counterbalanced presentation of the different primes with the different images, so that all of the images appeared with each prime type an equal number of times. To test whether the prime affected image valence and/or arousal, participants rated the how pleasant each image was (1 = very unpleasant, to 7 = very pleasant) and how emotionally arousing each image was (1 = not at all, to 7 = highly), on 7 point Likert scales (Porter et al., 2014), following image presentation. Participants then completed the STAI–S once more followed by an unrelated filler-task (a game of sudoku) for 10 minutes.

Test Phase. We next presented participants with the original images again, but we told them that the images had been altered (Ménétrier et al., 2013; Safer et al., 1998; Takarangi, Oulton, Green, & Strange, 2015). Specifically, our instructions were: *Now you will see pictures one at time. Some of these pictures have been altered so that the camera is either slightly closer to, or slightly further away from, the main object(s) in the picture. For each picture, you will be asked to judge whether the camera distance matches the camera distance of the picture that you saw earlier.*

We asked participants to rate the images as: -2 = much closer than the original, -1 = slightly closer than the original, 0 = no change, 1 = slightly farther than the original, or 2 = much farther than the original (note participants saw the anchors but not the numbers). When participants responded that the test photo was *much closer than the original* and *slightly closer than the original*, we classified their response as a boundary extension error. When participants responded that the test photo was *slightly farther than the original*, or *much farther than the original,* we classified their response as a boundary restriction error. We classified "no change" responses as correct (no error). We first demonstrated the task to participants by showing them three example picture pairs where one image was its original size and the other image was a version that was either closer, farther away, or at the same camera distance. Participants then completed a practice task with unrelated neutral pictures. Participants had to correctly rate two of each type (closer, same, farther) to remain in the study. If they failed this test, they saw a screen informing them that they were not eligible to continue in the study. For the test images, participants also rated how confident they were in their decision for each photo (1 = guessing, 2 = somewhat sure, 3 = definitely sure). Following the experiment, we fully debriefed all participants.

4.4 Experiment 2a Results and Discussion

Analysis of valence manipulation. First, we investigated whether the prime statements affected how participants interpreted the valence and arousal of the target images. A repeated measures ANOVA revealed a significant main effect of prime for pleasantness ratings, $F(2,190) = 40.41$, $p < .001$, $\eta_{p2} = .30$. As shown in Figure 4.1a, participants rated positive prime images as more pleasant than negative prime images, $t(95) = 9.09$, $p < .001$, $d = 0.93$ 95% CI [0.69, 1.17], and more pleasant than no-prime images, $t(95) = 6.41$, $p < .001$, $d = 0.65$ [0.43, 0.87]. Participants rated negative prime images as less pleasant than no-prime images, $t(95) = 3.17$, $p = .002$, $d = 0.32$ [0.12, 0.53].

We next investigated arousal ratings. A repeated measure ANOVA on arousal ratings revealed a significant main effect of prime, $F(2,190) = 5.86$, $p = .003$, $\eta_{p}^{2} = .06$. As shown in Figure 4.1b, surprisingly participants rated no-prime images as more arousing than negative prime images $t(95) = 2.44$, $p = .02$, $d = 0.25$ 95% CI [0.05, 0.45], and as more arousing than positive prime images, $t(95) = 2.55$, $p = .01$, $d = 0.26$ [0.06, 0.46]. Negative prime images and positive prime images were rated as similarly arousing, $t < 1$.

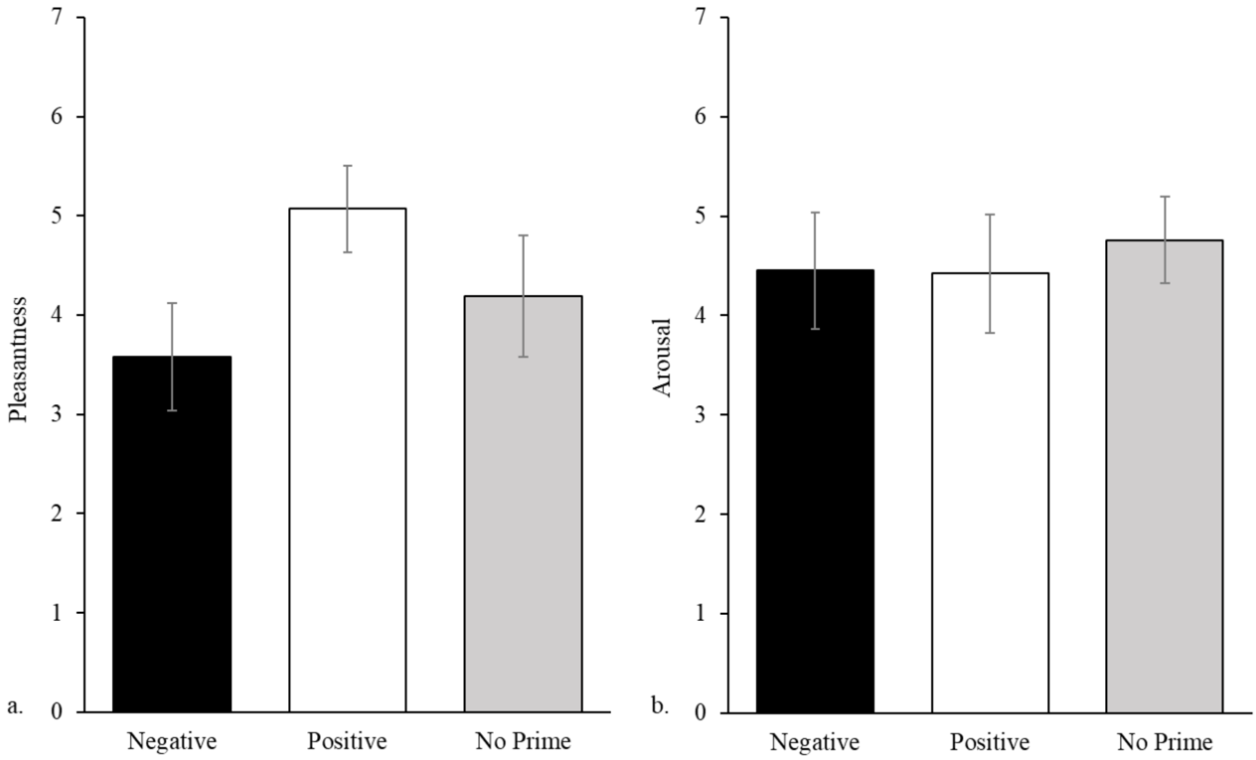

Figure 4.1a (left) and 4.1b (right). Mean pleasantness and arousal ratings for image primes for Experiment 2a. Error bars represent 95% confidence intervals.

Analysis of state anxiety scores. Participants' state anxiety (on the STAI-S) remained stable with no change from time 1—before encoding ($M = 42.00$, $SD = 9.45$) to time 2—after encoding ($M = 41.56$, $SD = 9.81$, $t(95) = 0.84$, $p = .40$, $d = 0.09$, 95% CI [-0.12, 0.29]), indicating that—throughout the experiment, participants' state anxiety remained low—despite being exposed to some negative statements. Thus, we managed to manipulate image valence without affecting participants' overall arousal levels. It should be noted that this arousal level refers to participants' rating of their *own feelings*, and not the arousal level of the image itself. Indeed, because we were using a mixture of negative, positive and neutral primes, this result could be the effect of the positive and neutral primes acting as a mood repair for the negative primes, and thus washing out any effect of the negative primes on participants' mood.

Analysis of boundary errors. On average, participants judged the images as much closer (2.60%), slightly closer (17.19%), no change (72.92%), slightly farther (5.73%) and much farther (1.56%). Thus, most of the time participants correctly recognised that the image was the same at test as it was at encoding. For each participant, we then calculated the mean reported camera distance on the −2 to +2 scale across all six target images ($M = -0.14$, $SD = 0.31$) and conducted a one-samples *t*-test. We found that the mean was significantly less than zero (where 0 indicates no change), $t(95) = 4.25$, $p < .001$, $d = 0.44$, 95% CI [0.23, 0.64]. Thus, when participants did make errors, those errors tended to be boundary extension.

We next turned to our primary research question: can negative valence induce boundary restriction or attenuate boundary extension? We analysed boundary judgement errors in two different ways. We first investigated whether participants' camera distance judgments at test depended on valence. We separated participants' mean camera distance by valence, and ran a repeated measures ANOVA. Distance judgements did not depend on valence, $F < 1$ (Figure 4.2). We next ran a within-subjects Bayesian ANOVA with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) and found $BF_{01} = 13.03$. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates strong evidence that there was no difference in camera distance judgement by valence.[1]

We next examined the proportion of errors participants made. We counted the total number of errors that participants made within each valence, and divided this number by their total errors for each valence. We conducted a 3 (valence: negative, positive, no-prime) x 2 (boundary error type: boundary restriction, boundary extension) repeated measures ANOVA on proportion of errors. Consistent with our earlier analyses, this pattern revealed that participants made more boundary extension errors (M = 0.18, SD = 0.29) than boundary restriction errors (M = 0.07, SD = 0.21), a main effect of error type, $F(1, 95) = 19.99$, $p < .001$, $\eta_p^2 = .17$. However, we note that the error rates overall were very low. Consistent with the mean camera distance measure, there was no main effect for valence, and no interaction between boundary error type and valence, Fs < 1.

[1] We asked participants to rate the certainty of their camera distance ratings and removed responses they reported as guesses. An ANOVA with guesses removed showed that boundary judgement errors did not depend on valence, $F < 1$.

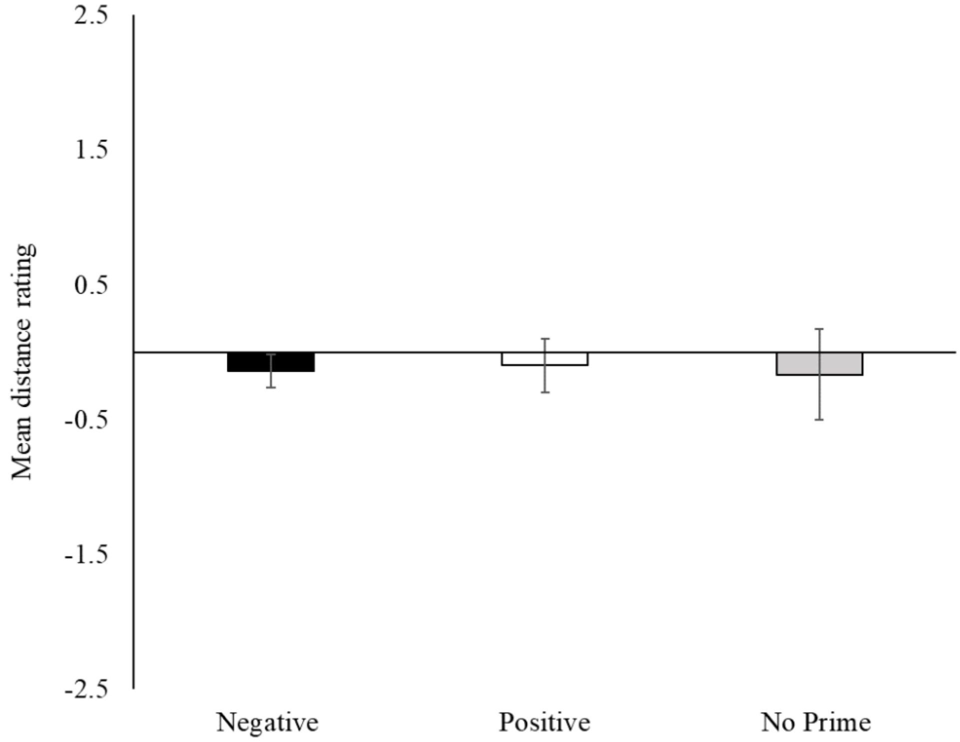

Figure 4.2. *Distance judgement ratings by valence for Experiment 2a. Error bars represent 95% confidence intervals.*

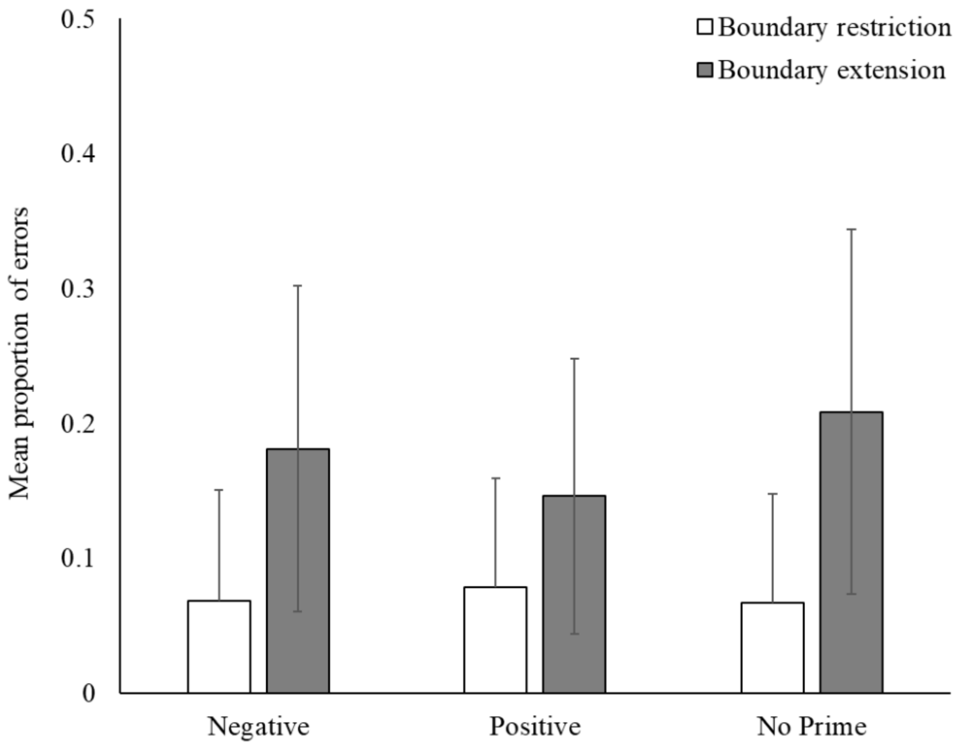

Figure 4.3. Proportion of boundary errors by error type and valence for Experiment 2a. Error bars represent 95% confidence intervals

To summarise, valence did not affect boundary error judgements at test, indicating that valence has no effect on the quantity, direction, or extent of boundary judgement errors. However, we note that participants made very few errors of either type; the most common response at test was “no change.” Thus, we had very few opportunities to examine the reason that participants made errors, on the rare occasions that they did. Additionally, recall that the exposure time for images at encoding was 15s. We used this relatively long exposure time for two reasons: one, it was the same as the exposure time Porter et al. (2014) used in their ASAP paradigm, and two, we reasoned that participants needed time to associate the prime with the image. However, these 15 seconds gave participants ample opportunity to not only encode the images, but to also extrapolate beyond the boundaries, making boundary extension errors more likely. Indeed, studies that have examined and successfully found boundary *restriction* have typically used shorter exposure times, around 5 seconds or less (Green et al, 2019; Mathews & Mackintosh, 2004; Safer et al., 1998). To fit with this literature, increase participants' overall error rate, and limit boundary extension errors, in Experiment 2b we reduced image encoding time to 5 seconds.

4.5 Experiment 2b Method

Participants. We again recruited 96 participants from Amazon Mechanical Turk's online research participation tool. Approximately half of the participants (58.3%) were female, and participants identified their ethnicity as Caucasian (including "White" 77%), African American (9%), Latino (9%), Asian (2%), European (4%), Native American (1%), and mixed (5%). The participants ranged in age from 20 to 70, with a mean age of 38.54 (SD = 12.05).

Design, Materials & Procedure. These were identical to Experiment 2a, except that we used a 5 second rather than 15 second encoding time.

4.6 Experiment 2b Results and Discussion

Analysis of valence manipulation. We again investigated whether the prime statements affected how participants rated the valence and arousal of the target images. A repeated measures ANOVA revealed a significant main effect of prime for pleasantness ratings, $F(2,190) = 58.30$, $p < .001$, $\eta_{p2} = .39$. As with Experiment 2a, participants rated positive prime images as more pleasant than negative prime images, $t(95) = 9.80$, $p < .001$, $d = 1.00$, 95% CI [0.75, 1.24]; and more pleasant than no-prime images, $t(95) = 3.70$, $p < .001$, $d = 0.38$ [0.17, 0.58]. Participants rated negative prime images as less pleasant than no-prime images, $t(95) = 6.97$, $p < .001$, $d = .71$ [0.49, 0.93].

In addition, a repeated measure ANOVA on arousal ratings revealed a significant main effect of prime, $F(2,190) = 15.23$, $p < .001$, $\eta_{p2} = .14$. As shown in Figure 4.4b, participants rated negative prime images as more arousing than with no-prime images, $t(95) = 4.92$, $p < .001$, $d = 0.50$ 95% CI [0.29, 0.71]; and negative prime images as more arousing than positive prime images, $t(95) = 4.31$, $p < .001$, $d = 0.44$ [0.65, 0.23]. No-prime images and positive prime images were rated as similarly arousing, $t(95) = 1.77$, $p = .08$, $d = 0.18$ [0.02, 0.38]. Note that finding is different to Experiment 2a, where we found that no-prime images were the most arousing. Although it is possible that the change to encoding time altered how participants rated the images, this explanation seems unlikely.

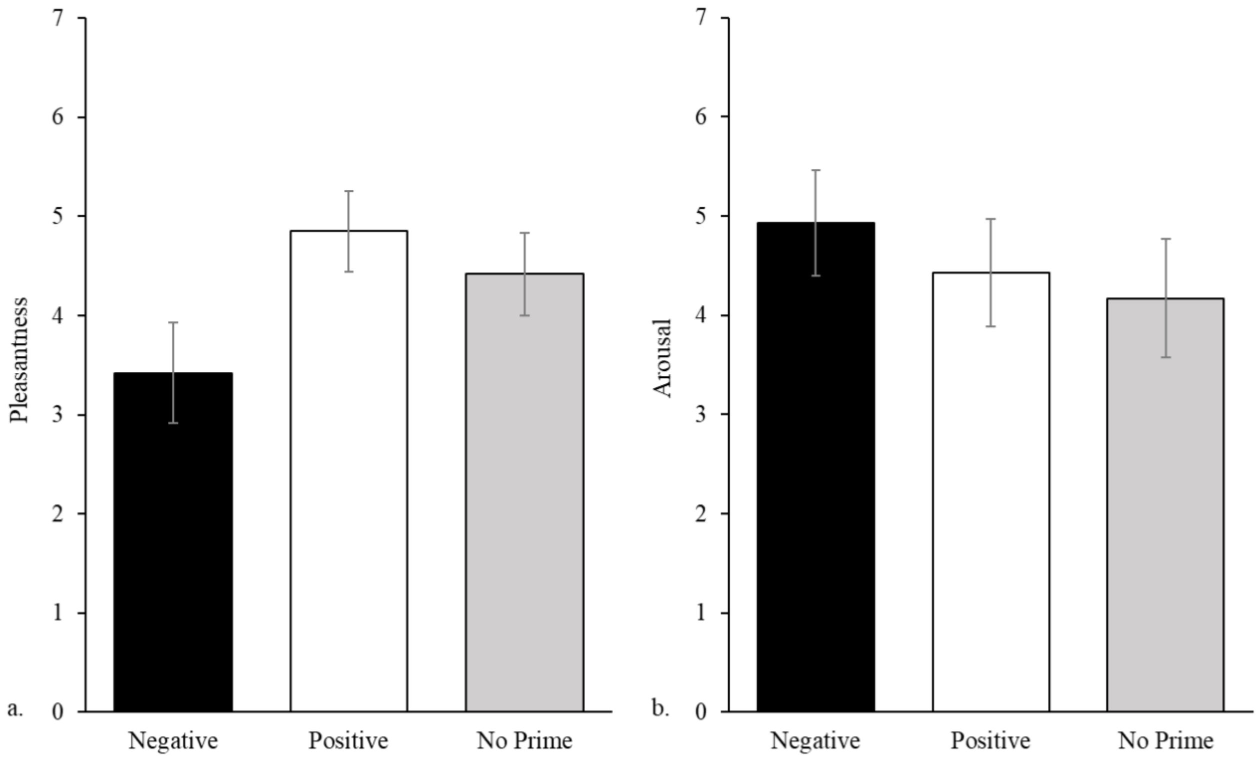

Figure 4.4a (left) and 4.4b (right). Mean pleasantness and arousal ratings for image primes for Experiment 2b. Error bars represent 95% confidence intervals.

Analysis of state anxiety scores. Consistent with Experiment 2a, participants' state anxiety (on the STAI-S) remained stable with no change from time 1—before encoding (M = 42.43, SD = 9.58) to time 2—after encoding (M = 42.91, SD = 9.49), $t(95) = 1.10$, $p = .27$, d = 0.11, 95% CI [0.08, 0.31].

Analysis of boundary errors. We again examined how often participants judged the images as much closer (7.12%), slightly closer (19.44%), no change (62.50%), slightly farther (9.72%) and much farther (1.22%). Like Experiment 2a, and despite our change to encoding time, participants were very good at recognising that the images at test were the same as they were at encoding. Note, however, that the error rates were slightly higher than in Experiment 2a, meaning that the lower exposure time did somewhat increase overall errors.

We next calculated the mean reported camera distance on the −2 to +2 scale across all images (M = −0.22, SD = 0.35) and conducted a one-samples t-test. We found that the mean was significantly less than zero (where 0 indicates no change). Therefore, consistent with Experiment 2a, when participants made errors those errors tended to be boundary extension, $t(95) = 6.04$, $p < .001$, $d = 0.62$, 95% CI [0.15, 0.29]. We next investigated whether participants' image distance judgments at test depended on valence. We separated participants' mean camera distance by valence and ran a repeated measures ANOVA. We found that boundary error distance did not depend on valence, $F < 1$ (Figure 4.5). We again ran a repeated-measures Bayesian ANOVA with default Cauchy prior and found BF_{01} = 13.72. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates strong evidence that there was no difference in boundary judgement errors by valence.[2]

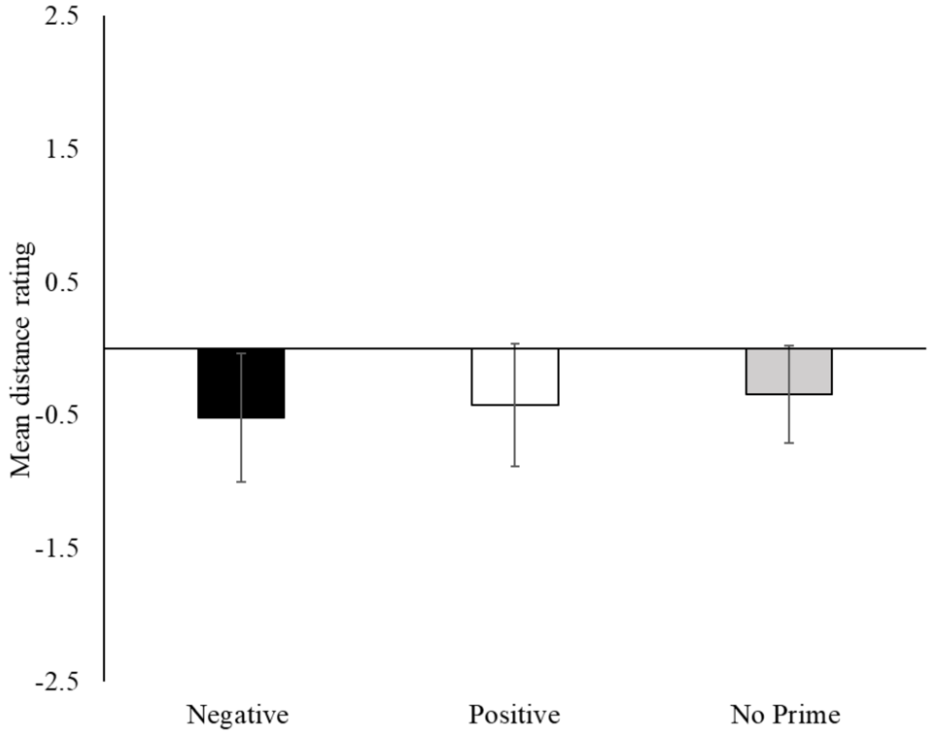

Figure 4.5. Distance judgement ratings by valence for Experiment 2b. Error bars represent 95% confidence intervals

We next conducted a 3 (prime valence: negative, positive, no-prime) x 2 (boundary error type: BR, BE) repeated measures ANOVA on proportion of errors. Consistent with Experiment 2a, participants made more boundary extension errors (*M* = 0.20, *SD* = 0.27) than boundary restriction errors (*M* = 0.08, *SD* = 0.19), a main effect of error type $F(1, 95) = 31.26$, $p < .001$, $\eta_p^2 = .25$. There was no main effect of valence, $F(2, 190) = 3.01$, $p = .05$, $\eta_p^2 = .03$, and no interaction between boundary error type and valence, $F < 1$ (see Figure 4.6).

[2] We asked participants to rate the certainty of their camera distance ratings and removed responses they reported as guesses. An ANOVA with guesses removed showed that boundary judgement errors did not depend on valence, $F < 1$.

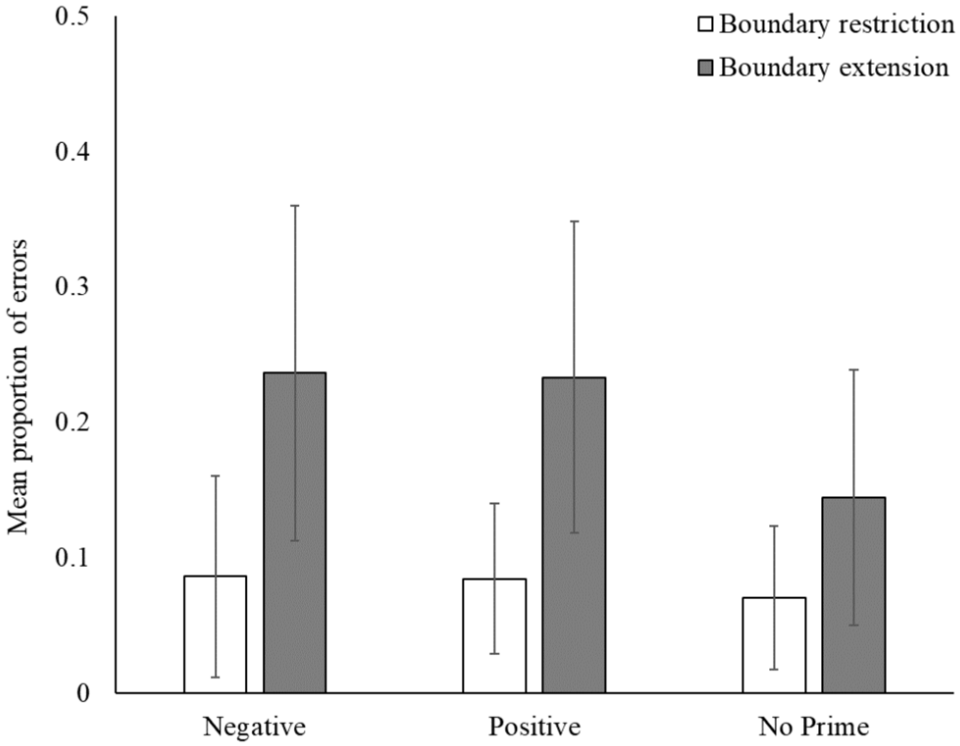

Figure 4.6. Proportion of boundary errors by error type and valence for Experiment 2b. Error bars represent 95% confidence intervals.

There are at least two key explanations for why Experiment 2a and Experiment 2b did not yield the expected results. First, these studies were within-subjects, thus the effect of each prime may have been washed out by the presence of the other primes; second, there may not have been enough opportunities for participants to make errors. To investigate the first possibility, we replicated Experiment 2b with a between-subjects design (n = 160), giving each participant the opportunity to make distance judgement errors on a total of six images for one valence (rather than only two images for each valence). The patterns of pleasantness and arousal ratings were consistent with

Experiment 2b. However, there was no significant difference between no-prime images and positive image pleasantness ratings ($p < .05$). Further, participants' judgments of image distance at test did not depend on valence, $F(2,159) = 1.54$, $p = .21$, $\eta_{p2} = .02$. Again we found that participants made more boundary extension errors ($M = 0.22$, $SD = 0.22$) than boundary restriction errors ($M = 0.09$, $SD = 0.15$, $t(159) = 5.56$, $p < .001$, $d = 0.44$ 95% CI [0.08, 0.17]). Two one-way ANOVAs on the proportion of boundary errors showed no effects of valence for boundary extension errors (Fs < 1) or for boundary restriction errors, $F(2,157) = 2.29$, $p = .10$, $\eta_{p2} = .03$). We can conclude then that the presence of all three primes in a within subject design washing out any effects is unlikely. In fact, the valence manipulation in the within-subjects design yielded more consistent results, with participants rating all three primed images in line with their prime in Experiments 2a – 2b, but not for our between-subjects paradigm.

To investigate the second possibility, we next replicated Experiment 2b with a different test paradigm. We hypothesised that participants may have felt that the camera distance Likert scale (with the closer to farther choices) did not have an option to indicate subtle (perceived) changes to the images. To increase error rates and give participants the opportunity to make distance judgements in finer gradations, we changed the camera distance Likert scale to a slider rating. We had 101 points across the slider, with -50 = much closer than the original, (0 = no change – not labelled), and +50 = much farther than the original. Note that participants saw the anchor labels but not the numbers. Additionally, there is evidence that participants tend to mark to the left of Likert scales more often than to the right, regardless of the anchor (e.g., Nicholls, Orr, Okubo & Loftus, 2006). Thus, in order to control for—and measure—the effect of this lateralised attentional bias, we counterbalanced the scale anchors: half the participants had the distance rating with "closer" (boundary extension error) on the left side, and the other half of the participants had the distance rating with “closer” on the right side.

4.7 Experiment 2c Method

Participants. We recruited 96 participants from Amazon Mechanical Turk’s online research participation tool. Approximately half of the participants (55.2%) were female, and participants identified their ethnicity as Caucasian (including “White” 71%), African American (9%), Latino (3%), Asian (9%), European (5%), and mixed (2%). The participants ranged in age from 21 to 67, with a mean age of 36.32 (*SD* = 11.70).

Design, Materials & Procedure. These were identical to Experiment 2b, except that we used a slider rating at test with rating of -50 to +50, instead of the camera distance Likert scale used in Experiments 2a and 2b.

4.8 Experiment 2c Results and Discussion

Analysis of valence manipulation. We again investigated whether the prime statements affected how participants rated the valence and arousal of the target images. As with Experiments 2a and 2b, a repeated measures ANOVA revealed a significant main effect of prime for pleasantness ratings, $F(2,190) = 82.13$, $p < .001$, $\eta_{p2} = .46$. Participants rated positive primes images as more pleasant than negative prime images, $t(95) = 11.13$, $p < .001$, $d = 1.14$ 95% CI [0.88, 1.39]; and more pleasant than no-prime images, $t(95) = 4.32$, $p < .001$, $d = 0.44$ [0.23, 0.65] (as seen in Figure 4.7a). Participants rated negative prime images as less pleasant than no-prime images $t(95) = 8.58$, $p < .001$, $d = 0.88$ [0.64, 1.11].

In addition, a repeated measure ANOVA on arousal ratings revealed a significant main effect of prime, $F(2,190) = 8.78$, $p < .001$, $\eta_{p2} = .09$. As shown in Figure 4.7b, participants rated negative prime images as more arousing than no-prime images $t(95) = 4.13$,

$p < .001$, $d = 0.42$ 95% CI [0.21, 0.63], and negative prime images as more arousing than positive prime images $t(95) = 2.28$, $p = .03$, $d = 0.23$ [0.03, 0.44], and positive prime images as more arousing than no-prime images $t(95) = 1.95$, $p = .05$, $d = 0.20$ [-0.40, 0.003].

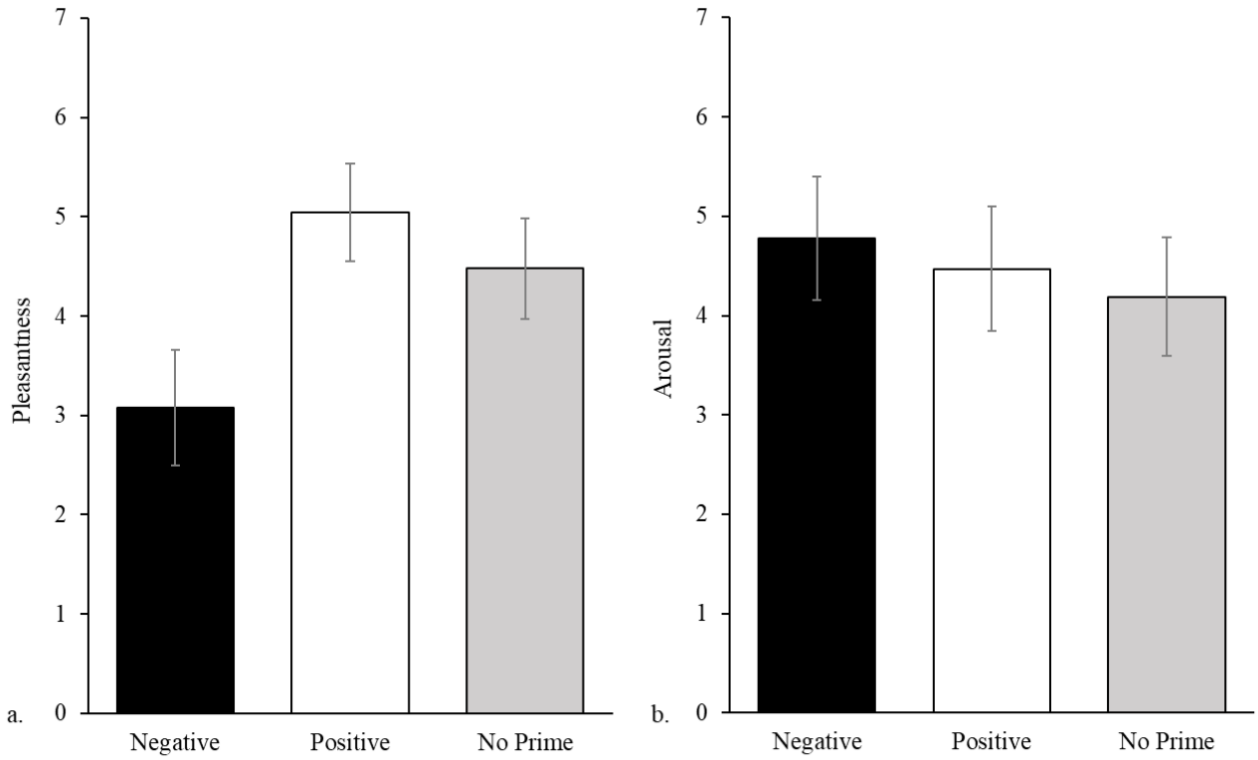

Figure 4.7a (left) and 4.7b (right). Mean pleasantness and arousal ratings for image primes for Experiment 2c. Error bars represent 95% confidence intervals.

Analysis of state anxiety scores. Consistent with Experiments 2a and 2b, participants' state anxiety (on the STAI-S) remained stable with no change from time 1—before encoding $M = 43.58$, $SD = 9.78$ to time 2—after encoding $M = 42.69$, $SD = 10.46$, $t(95) = 1.88$, $p = .06$, $d = 0.19$, 95% CI [-0.01, 0.39].

Analysis of boundary errors. Recall that we changed the camera distance Likert scale to a slider, to increase error rates. Indeed, consistent with our hypothesis, errors increased, with each participant making at least two errors (M = 5.51, *SD* = .98). Additionally, participants chose "no change" (i.e., did not move the slider from starting position) only 8.84% of the time, indicating that we had successfully increased error rates.

We next examined participants' average distance ratings at test (-50 to +50 by valence). Overwhelmingly, participants tended to make boundary extension errors, judging the images at test as closer than the original images (*grand mean* = -0.84, *SD* = 11.30). Consistent with Experiments 2a and 2b, participants' boundary judgement errors at test did not depend on valence (Figure 4.8), $F(2,190) = 1.19$, $p = .31$, $\eta_{p2} = .01$. A Bayesian ANOVA with default Cauchy prior and found $BF_{01} = 8.82$, again indicating substantial evidence that there was no difference in boundary judgement errors by valence.[3]

[3] We asked participants to rate the certainty of their camera distance ratings and removed responses they reported as guesses. An ANOVA with guesses removed showed that boundary judgement errors did not depend on valence, $F(2, 190) = 1.66$, $p = .19$, $\eta_{p2} = .02$.

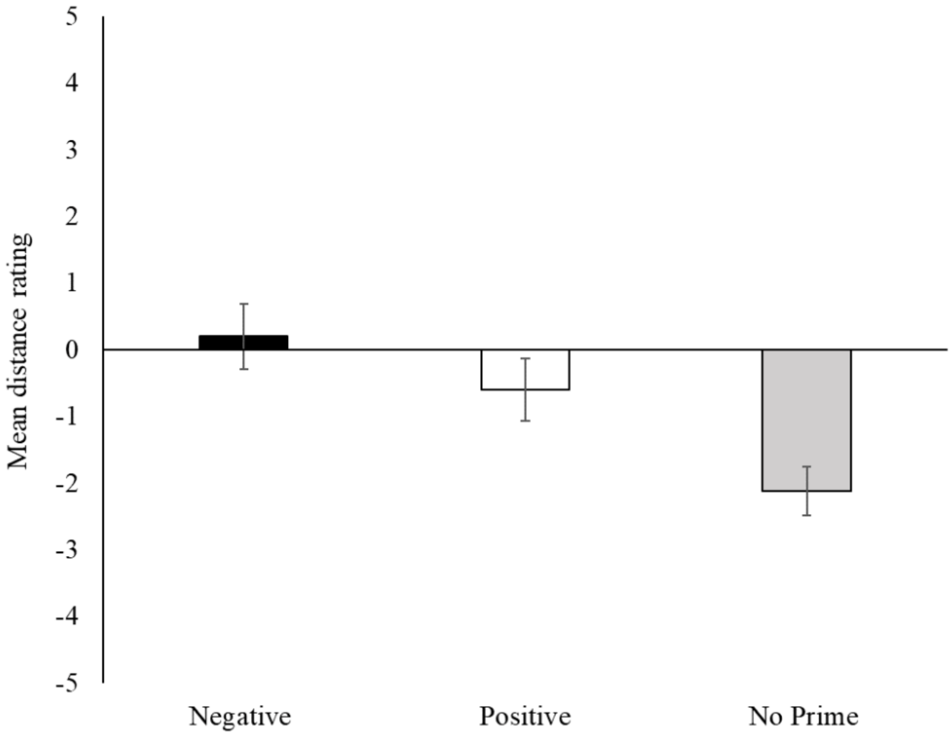

Figure 4.8. Mean judgement ratings by valence for Experiment 2c. Error bars represent 95% confidence intervals.

We next examined the proportion of boundary extension versus boundary restriction errors participants made. We conducted a 3 (valence: negative, positive, no-prime) x 2 (boundary error type: BR, BE) repeated measures ANOVA on proportion of errors. Consistent with Experiments 2a and 2b, participants made more boundary extension errors (M = 0.22, SD = 0.10) than boundary restriction errors (M = 0.11, SD = 0.10), a main effect of error type, $F(1, 95) = 27.10$, $p < .001$, $\eta_{p2} = .22$. There was no main effect of valence, $F(1, 95) = 1.66$, $p = .19$, $\eta_{p2} = .02$; and no interaction between valence and boundary error type $F < 1$ (see Figure 4.9).

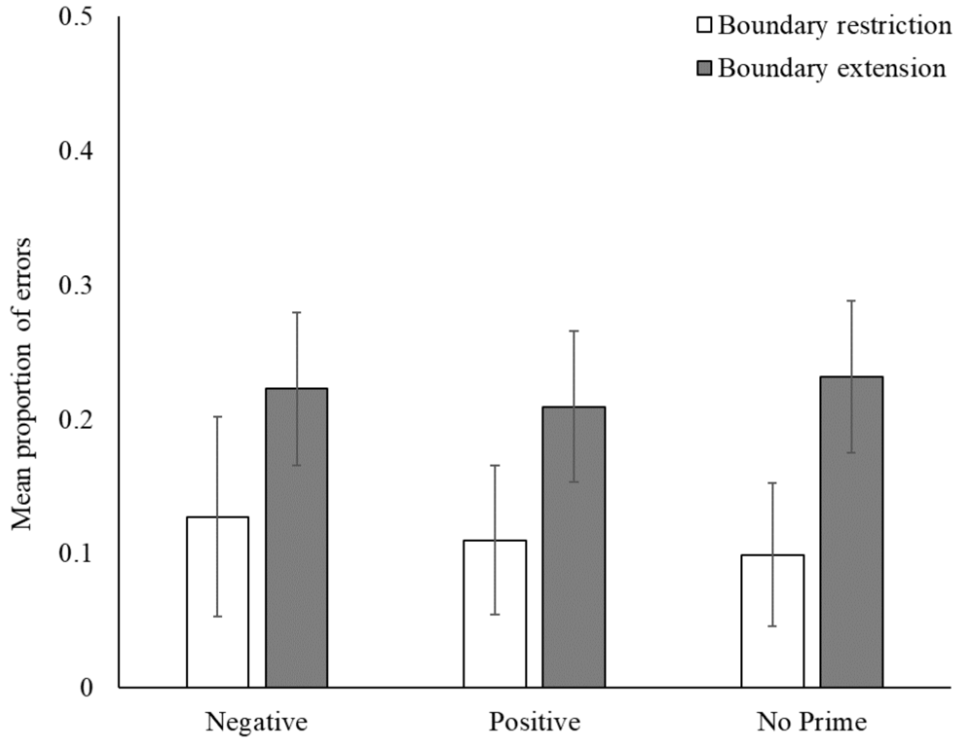

Figure 4.9. Proportion of boundary errors by error type and valence for Experiment 2c. Error bars represent 95% confidence intervals.

Recall that we counterbalanced the scale anchors in order to control for—and measure—the effect of lateralised attentional bias. We know that attention to the left and

right visual hemifields is lateralised—with more attention being paid to the left than the right side of space (Bowers & Heilman, 1980). Additionally, when making distance judgements, people tend to make less expansive boundary extension errors for the left compared to the right side of images (Dickinson & Intraub, 2009). Taken together, these findings indicate that the side to which participants indicate boundary judgement errors may affect the type of errors they make. Thus, we next analysed the effect of laterality on boundary errors.
Boundary restriction errors could be made by moving the slider to the left for half of the participants and boundary restriction errors could be made by moving the slider to the *right* for the other half of the participants. For ease of narrative, we will refer to the side on which the boundary extension error option ("closer") appeared. We ran a 2 (error type: boundary restriction, boundary extension) x 2 (side: left, right) mixed ANOVA with side as a between-subjects factor. Consistent with our earlier analysis, participants made more boundary extension errors than boundary restriction errors $F(1, 94) = 31.21$, $p < .001$, $\eta_{p2} = .25$. There was no main effect of side, $F < 1$. But there was an interaction between boundary error and side, $F(1, 94) = 23.71$, $p < .001$, $\eta_{p2} = .16$. When the boundary extension option appeared on the left, participants were more likely to make boundary extension errors than when the boundary extension option appeared on the right $t(94) = 4.87$, $p < .001$, $d = 1.00$, 95% CI [0.57, 1.42]. When the boundary extension option appeared on the left, they were less likely to make boundary restriction errors than when the boundary extension option appeared on the right, $t(94) = 4.87$, $p < .001$, $d = 1.00$, 95% CI [0.57, 1.42], see Figure 4.10.

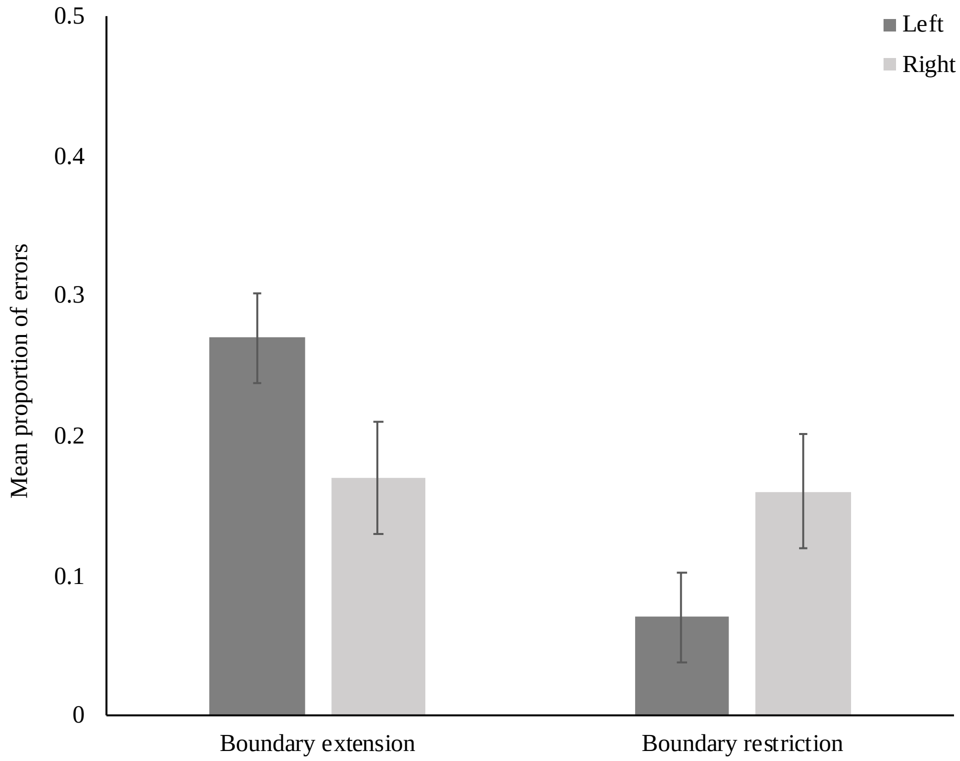

Figure 4.10. Proportion of boundary errors by error type and separated by the side on which the boundary extension option ("closer") appeared. Error bars represent 95% confidence intervals.

In Experiment 2c, we successfully increased the error rate, with participants making errors the majority of the time, rather than correctly rating the images as the same. Despite the increase however, and consistent with Experiments 2a and 2b, we found no difference for boundary judgement errors according to image valence. We did find laterality effects at test, where–for negatively primed images participants made more boundary extension errors when "closer" appeared on the right. This result is consistent with Dickinson and Intraub (2009), who found that participants make less expansive boundary extension errors for the left compared to the right side of images (Dickinson & Intraub, 2009). This finding is also consistent with pseudoneglect, where people pay more attention to the left than to the right side of space. Additionally, boundary extension research has found that attention allocation can affect boundary judgement errors: specifically, decreased attention can *exacerbate* the boundary extension effect. This research—together with our findings—indicates that attention allocation may play a role in attenuation of boundary extension.

However, one possibility is that participants made errors because they were attending to the primes rather than the images. To increase the chance that participants would carefully attend to the *images*, and to reduce the likelihood that they would guess, we presented the prime *before* the image. We kept all other aspects of the experiment the same, and aimed to investigate the effect of valence, and "side" on boundary judgement error rates and types.

4.9 Experiment 2d Method

Participants. We recruited 96 participants from Amazon Mechanical Turk's online research participation tool. Approximately half of the participants (56.3%) were female, and participants identified their ethnicity as Caucasian (including "White" 75%), African American (6%), Latino (5%), Asian (10%), European (1%), and mixed (3%). The participants ranged in age from 18 to 72, with a mean age of 37.68 (*SD* = 12.38).

Design, Materials & Procedure. These were identical to Experiment 2c, except that we separated the image and the prime, by presenting the prime *before* the image. Participants were asked to read the prime at their own pace, and then to click "next" to view the image.

4.10 Experiment 2d Results and Discussion

Analysis of manipulation of valence. A repeated measure ANOVA on pleasantness ratings revealed a significant main effect of prime, $F(2,190) = 57.62$, $p < .001$, $\eta_{p2} = .38$. As with Experiments 2a–2c, participants rated positive prime images as more pleasant than negative prime images $t(95) = 9.09$, $p < .001$, $d = 0.93$ 95% CI [0.69, 1.17], and more pleasant than no-prime images, $t(95) = 3.49$, $p < .001$, $d = 0.36$ [0.15, 0.56] (as seen in Figure 4.11a). Participants rated negative prime images as less pleasant than no-prime images $t(95) = 7.59$, $p < .001$, $d = 0.78$ [0.55, 1.00].

In addition, a repeated measure ANOVA on arousal ratings revealed a significant main effect of prime, $F(2,190) = 7.99$, $p < .001$, $\eta_{p2} = .08$. As shown in Figure 4.11b, participants rated negative primes images as more arousing than no-prime images $t(95) = 3.52$, $p < .001$, $d = 0.36$ 95% CI [0.15, 0.57]; and negative prime images as more arousing than positive prime images, $t(95) = 2.86$, $p = .01$, $d = 0.29$ [0.09, 0.50]. Participants rated positive prime images as similarly arousing to no-prime images, $t(95) = 1.08$, $p = .05$, $d = 0.11$ [0.09, 0.31].

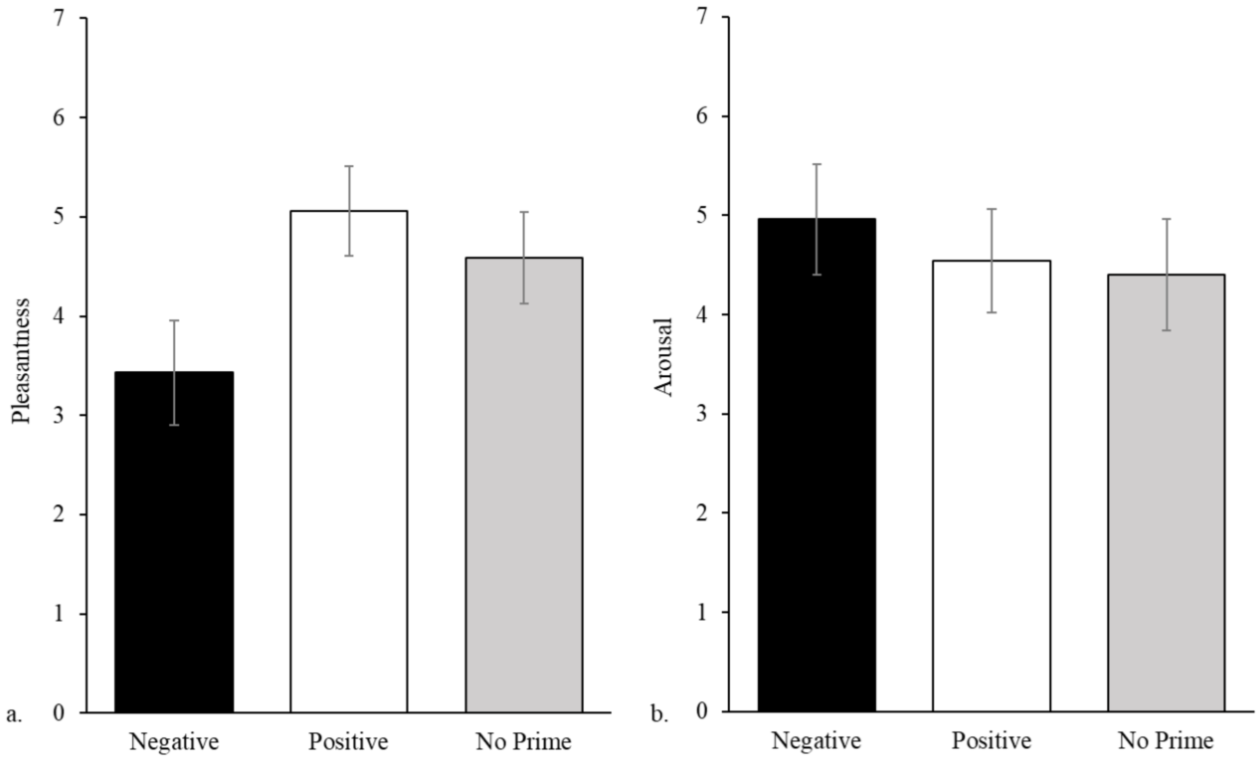

Figure 4.11a (left) and 4.11b (right). Mean pleasantness and arousal ratings for image primes for Experiment 2d.

Analysis of state anxiety scores. Consistent with Experiments 2a - 2c, participants' state anxiety (on the STAI-S) remained stable with no change from time 1—before encoding (M = 42.06, SD = 9.29) to time 2—after encoding (M = 41.44, SD = 9.29), $t(95) = 1.58$, p = .12, d = 0.16, 95% CI [-0.04, 0.36].

Analysis of boundary errors. Recall that for the present experiment, we presented the prime *before the image* to increase attention to the image itself. Consistent with Experiment 2c, errors overall were greater than in Experiments 2a - 2b, with the majority of participants (75%) now making errors on all six images (M = 5.57, SD = 1.01, errors ranged from 0% to 100%). Additionally, and consistent with Experiment 2c, participants chose "no change" (i.e., did not move the slider from starting position) only 7.12% of the time. We examined participants' average distance ratings at test (-50 to +50 by prime valence). Overwhelmingly, participants tended to make boundary extension errors, judging the images at test as closer than the original images were (*grand mean* = -3.48, *SD* = 9.86). However, contrary to our overarching hypothesis, participants' judgments of image distance at test did not depend on valence (see Figure 4.12), $F(2,190) = 1.05$, $p = .35$, $\eta_p^2 = .01$. We conducted a Bayesian ANOVA with default Cauchy prior and found $BF_{01} = 9.44$, again indicating substantial evidence that there was no difference in boundary judgement errors by valence.[4] Recall that we separated the presentation of the prime and the image in order to increase attention to the images, and to ensure that participants were accurately indicating memory errors, rather than just guessing. Note here that the rate of "guesses" reduced from Experiment 2c (83, 14%) to Experiment 2d (46, 8%). This result suggests that participants were relying more on their memory of the images, which in turn suggests that we successfully increased attention to the images.

[4] We asked participants to rate the certainty of their camera distance ratings and removed responses they reported as guesses. An ANOVA with guesses removed showed that boundary judgement errors did not depend on valence, $F(2, 190) = 1.04$, $p = .35$, $\eta_p^2 = .01$.

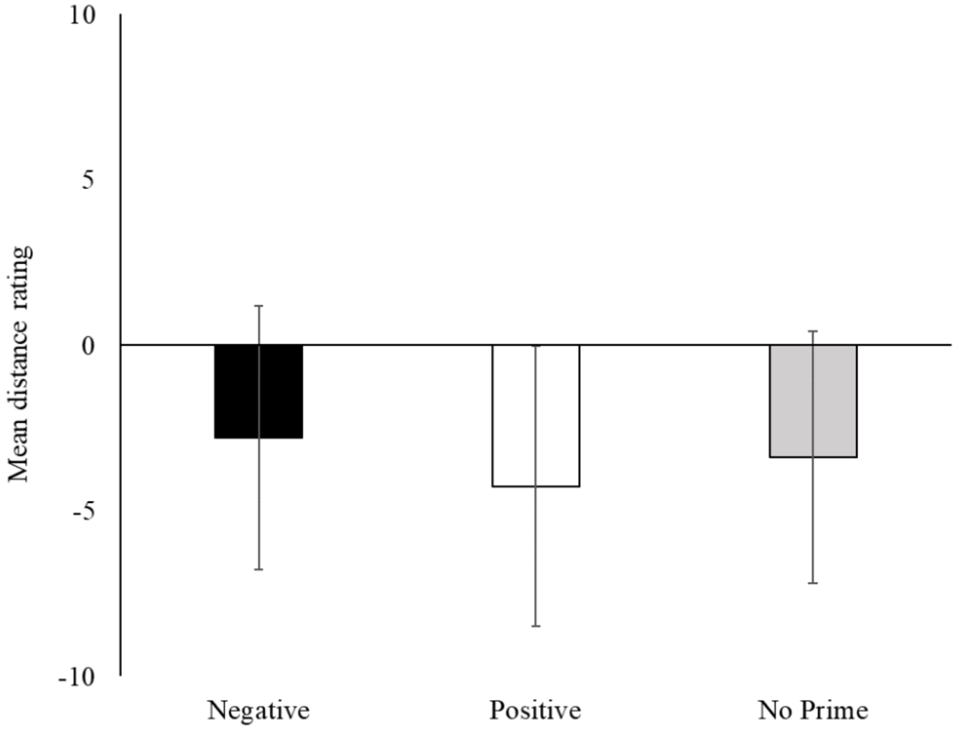

Figure 4.12. Distance judgements ratings by valence for Experiment 2d. Error bars represent 95% confidence intervals.

We next examined the proportion of boundary extension vs. boundary restriction errors participants made. We conducted a 3 (valence: negative, positive, no-prime) x 2 (boundary error type: boundary restriction, boundary extension) repeated measures ANOVA on proportion of errors. Consistent with Experiments 2a - 2c, participants made more boundary extension errors (*M* = 0.65, *SD* = 0.31) than boundary restriction errors (*M* = 0.28, *SD* = 0.29), a main effect of error type $F(1, 95) = 37.20$, $p < .001$, $\eta_{p2} = .28$. Consistent with Experiments 2a - 2c, there was no main effect of valence, $F(2, 190) = 1.37$, $p = .27$, $\eta_{p2} = .01$, and no interaction between valence and boundary error type, $F < 1$ (see Figure 4.13).

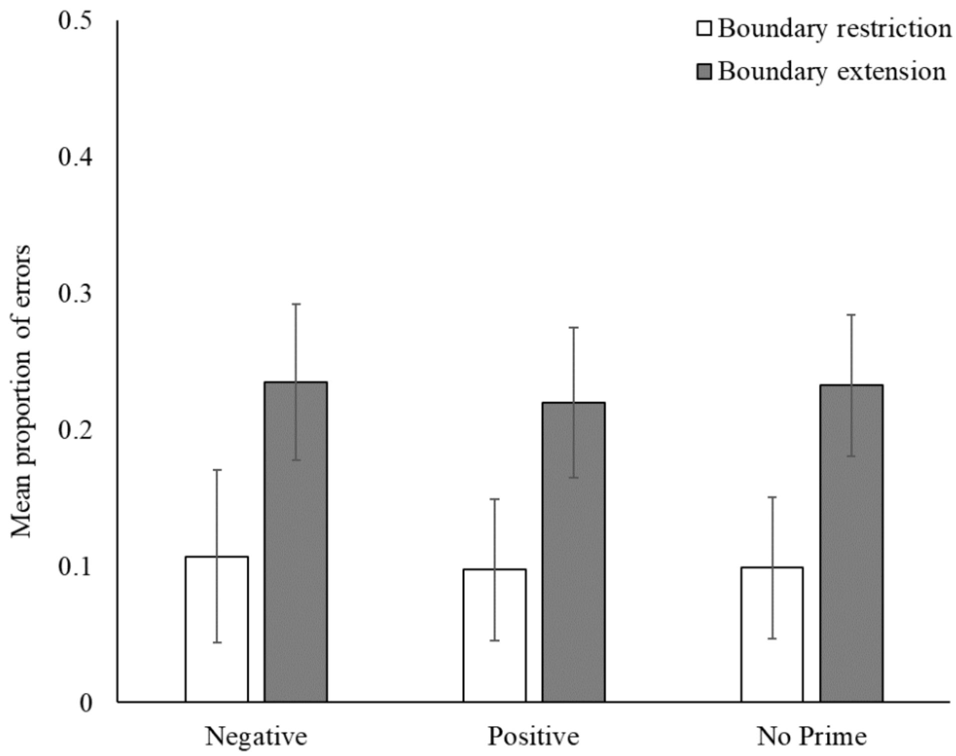

Figure 4.13. Proportion of boundary errors by error type and valence for Experiment 2d. Error bars represent 95% confidence intervals.

We next analysed the effect of laterality on boundary errors. We ran a 2 (error type: boundary restriction, boundary extension) x 2 (side: left, right) mixed ANOVA with side as a between-subjects factors. Consistent with our earlier analysis, there was a main effect of error type, with participants making more boundary extension errors than boundary restriction errors, $F(1, 94) = 44.23$, $p < .001$, $\eta_{p2} = .30$. There was no main effect of side $F < 1$. Consistent with Experiment 2c there was an interaction between boundary error and side, $F(1, 94) = 8.12$, $p = .005$, $\eta_{p2} = .06$. When the boundary extension option appeared on the left, participants were more likely to make boundary extension errors than when the boundary extension option appeared on the right, $t(94) = 2.45$, $p = .02$, $d = 0.50$, 95% CI [0.09, 0.91], When the boundary extension option appeared on the left, they were less likely to make boundary restriction errors than when the boundary extension option appeared on the right, $t(94) = 3.04$, $p = .003$, $d = 0.62$, 95% CI [0.21, 1.03] (see Figure 4.14).

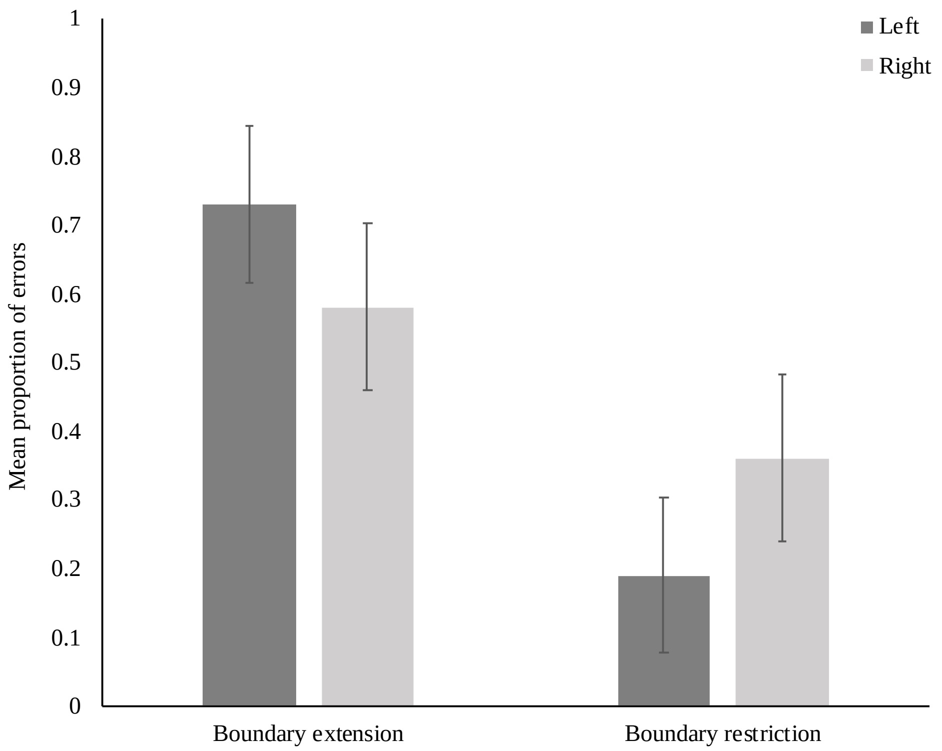

Figure 4.14. Proportion of boundary errors by error type and separated by the side on which the boundary extension option ("closer") appeared. Error bars represent 95% confidence intervals.

In Experiment 2d, we separated the prime statement from the image in order to increase participants' attention to the images. However, despite this change, and consistent with Experiments 2a – 2c, we found no difference on boundary judgement errors according to valence. Consistent with Experiment 2c, we also found laterality effects, where participants were more likely to make boundary extension errors, and less likely to make boundary restriction errors, when “closer” (the boundary extension option) appeared on the left, suggesting that participants were marking to the left of the scale, regardless of the type of errors they made.

Despite our success at increasing the error rate, and at increasing the attention paid to each image, we were concerned about the relatively low number of opportunities participants had to make boundary judgement errors on each valence type. In particular, the opportunity to make boundary restriction errors for negatively primed images was rare. To increase the opportunity for participants to make each type of error for each image valence images, we increased the number of images we used.

4.11 Pilot Study

Stimuli preparation

We chose 30 ambiguous neutral images from the IAPS database (Lang et al., 2008) with a normed emotional valence rating (4-6 on a scale where 1 = very unpleasant, 9 = very pleasant). We generated three negative (e.g., *Man in critical condition following horror crash*), and three positive (e.g., *Off-duty paramedic, trooper saves crash victims*) prime statements for each image. Four hundred and seven participants from Amazon Mechanical Turk rated the statements using the self-assessment manikin (SAM) for valence (Bradley & Lang, 1994). The SAM is a simple, quick, graphical method of assessing people’s reactions to stimuli (Bradley & Lang, 1994; see Figure 4.15). Participants rated the images from completely happy (e.g., pleased, satisfied, contented, hopeful) to unhappy (e.g., annoyed, unsatisfied, melancholic, despaired, bored). Participants were instructed to select the centre manikin if they felt completely neutral (e.g., neither happy nor sad).

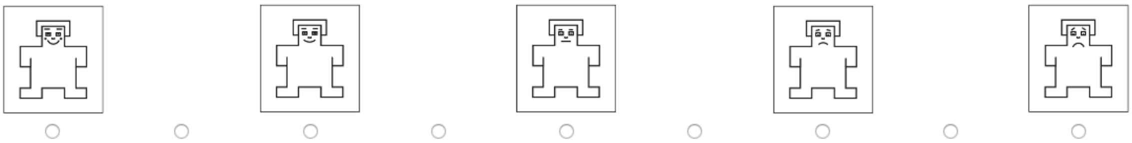

Figure 4.15. SAM scale for valence in the Pilot Study.

We calculated the SAM valence scores from 1 = positive to 9 = negative, and selected the highest (most negative) and lowest (most positive) rated statements for each image. Our selected negative statements were rated as high in negative valence, $M = 7.96$, $SD = 0.42$, and our selected positive statements were rated as low in negative valence, $M = 2.83$, $SD = 0.32$.

4.12 Experiment 2e Method

Participants. Recall that the previous studies all detected no within-subject differences for boundary judgement errors, when powered to a medium effect size, with few (6) trials. Thus, for the current study, we calculated the power needed to detect a small within-subjects effect. We ran a repeated measures ANOVA with three levels in G*Power (Erdfelder, Faul, & Buchner, 1996), with $f = .10$, alpha = .05, power = .95. We based our estimate of the correlation between repeated measures on the intercorrelation between error rates for negative, positive and no-prime images in our four previous studies (average correlation = .30). The recommended sample size was 362. We increased this number to 366 to allow for a counterbalance with 6 versions. We recruited 368 participants from Amazon Mechanical Turk's online research participation tool. Approximately half of the participants (55.4%) were male, and participants identified their ethnicity as Caucasian (including "White" 72%), African American (7.9%), Latino (7.6%), Asian (7.6%), Indian (0.5%), Middle Eastern (0.5%), Native American (1.1%), mixed (3%), and no answer (0.3%). The participants ranged in age from 19 to 73, with a mean age of 37.80 (*SD* = 11.50).

Design, Materials & Procedure. We pre-registered this study on the Open Science Framework (https://osf.io/k4vnj/). The materials and procedure were identical to Experiment 2d, except that we increased the number of images from six to 30 target images and accompanying prime statements outlined in the pilot study above.

4.13 Experiment 2e Results and Discussion

Analysis of manipulation of valence. A repeated measures ANOVA revealed a significant main effect of prime for pleasantness ratings, $F(2,734) = 260.56$, $p < .001$, $\eta_{p2} = .42$ (see Figure 4.16a). Participants rated positive prime images as more pleasant than negative prime images, $t(367) = 18.65$, $p < .001$, $d = 0.97$, 95% CI [0.85, 1.10]; and more pleasant than images with no-prime, $t(367) = 8.57$, $p < .001$, $d = 0.45$ [0.34, 0.55]. Negative prime images were rated as less pleasant than no-prime images, $t(367) = 16.20$, $p < .001$, $d = 0.84$ [0.73, 0.96].

In addition, a repeated measure ANOVA on arousal ratings revealed a significant main effect of prime, $F(2, 367) = 61.01$, $p < .001$, $\eta_{p2} = .14$. As shown in Figure 4.16b, participants rated positive prime images as more arousing than no-prime images, $t(367) = 9.46$, $p < .001$, $d = 0.52$ 95% CI [0.41, 0.63], and negative prime images as similarly arousing as positive prime images $t(367) = 1.74$, $p = .08$, $d = 0.09$ [-0.01, 0.19]. Participants rated negative prime images more arousing than no-prime images, $t(367) = 9.91$, $p < .001$, $d = 0.49$ [0.38, 0.60].

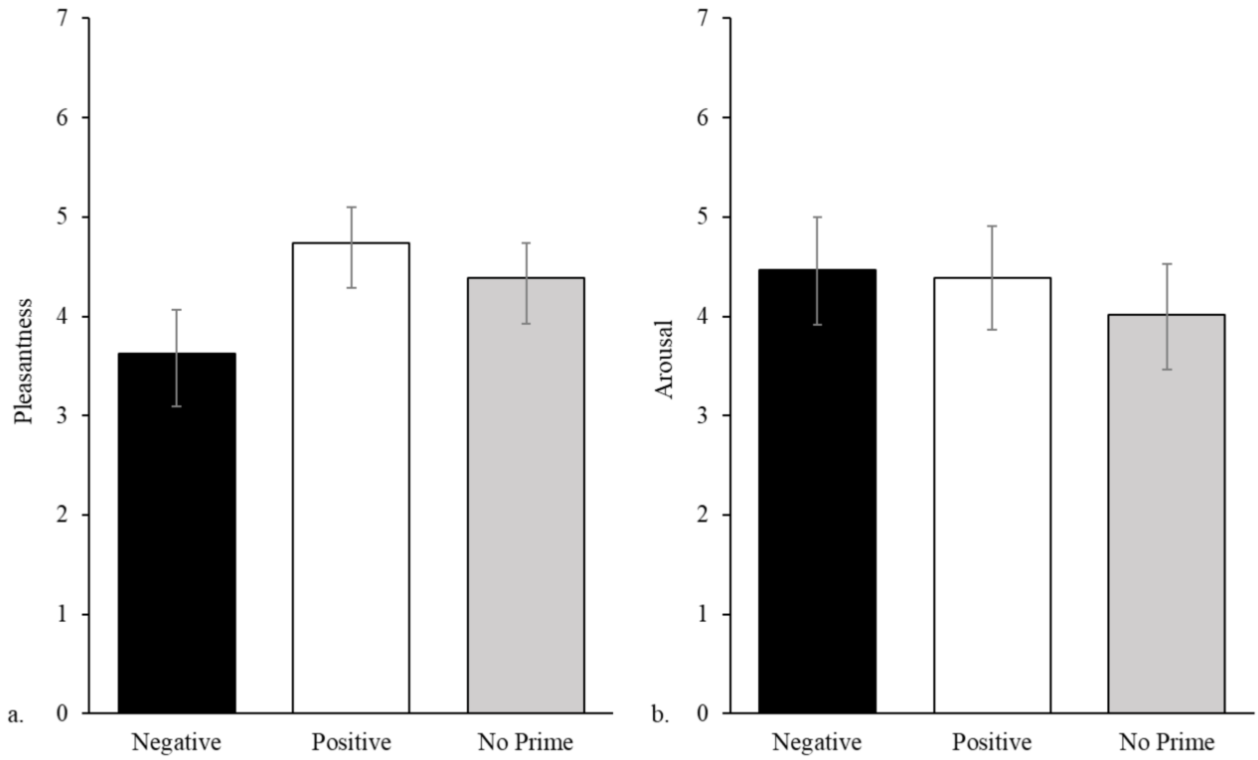

Figure 4.16a (left) and 4.16b (right). Mean pleasantness and arousal ratings for image primes for Experiment 2e. Error bars represent 95% confidence intervals.

Analysis of state anxiety scores. Consistent with Experiments 2a - 2d, participants' state anxiety (on the STAI-S) remained stable with no change from time 1—before encoding M = 43.15, SD = 12.03 to time 2—after encoding M = 43.36, SD = 9.70, $t(95) = 0.46$, p = .47, d = 0.02, 95% CI [0.08, 0.13].

Analysis of boundary errors. The present experiment had 30 images, meaning participants had the opportunity to make 10 errors for each valence. Consistent with Experiments 2c - 2d, participants made multiple errors, with the majority of participants (99.73%; i.e., all but one) making at least one error, and 23% of participants making the maximum number of errors (M = 0.77, *SD* = 0.25, errors ranged from 0% to 100%). Additionally, participants chose “no change” (i.e., did not move the slider from starting position) 22.93% of the time. We examined participants’ average distance ratings at test (-50 to +50 by valence). Overwhelmingly, participants still tended to make boundary extension errors, judging the images at test as closer than the original images (*grand mean* = -2.98, *SD* = 11.36, $t(368) = 5.04$, $p < .001$, $d = .26$, 95% CI [0.16, 0.37]). Despite our overarching hypothesis that participants would make more boundary restriction errors for negatively valenced images than positive or neutral images, participants’ judgments of image distance at test did not depend on valence (see Figure 4.17), $F(2, 734) = 5.71$, $p = .69$, $\eta_p^2 < .01$. Indeed, a Bayesian ANOVA with default Cauchy prior found $BF_{01} = 66.13$. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates very strong evidence that there was no difference in boundary judgement errors by valence.[5]

[5] We asked participants to rate the certainty of their camera distance ratings and removed responses they reported as guesses. An ANOVA with guesses removed showed that boundary judgement errors did not depend on valence, $F(2, 190) = 1.04$, $p = .35$, $\eta_p^2 = .01$.

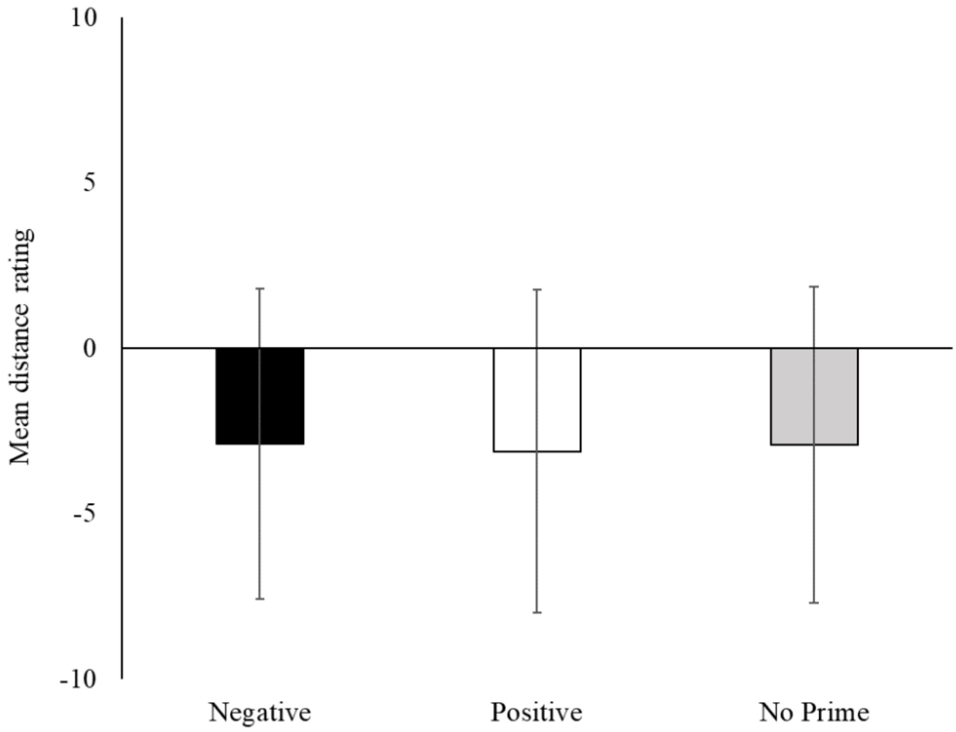

Figure 4.17. Distance judgement ratings by valence for Experiment 2e Error bars represent 95% confidence intervals.

We next examined the proportion of boundary extension versus boundary restriction errors participants made. We conducted a 3 (valence: negative, positive, no-prime) x 2 (boundary error type: BR, BE) repeated measures ANOVA on proportion of errors. Consistent with Experiments 2a - 2d, participants made more boundary extension errors (M = 0.16, SD = 0.11) than boundary restriction errors (M = 0.09, SD = 0.09), a main effect of error type, $F(1, 367) = 51.75$, $p < .001$, $\eta_{p2} = .12$. Consistent with Experiments 2a - 2d, there was no main effect of valence, $F(2, 734) = 1.37$, $p = .25$, $\eta_{p2} = .004$; and no interaction between valence and boundary error type, $F(2, 734) = 0.86$, $p = .42$, $\eta_{p2} = .002$ (see Figure 4.18). Thus, valence had no effect on the type or extent of errors participants made.

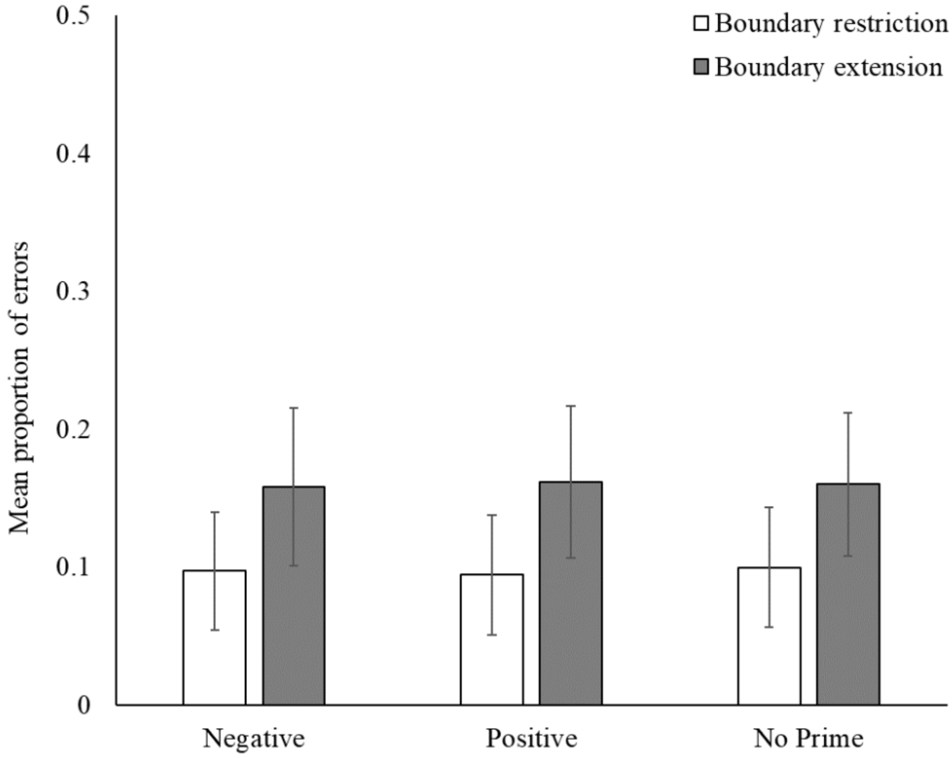

Figure 4.18. Proportion of boundary errors by error type and valence for Experiment 2e. Error bars represent 95% confidence intervals.

We next analysed the effect of laterality on boundary errors. We ran a 2 (error type: boundary restriction, boundary extension) x 2 (side: left, right) mixed ANOVA with side as a between-subjects factors. Consistent with our earlier analysis, participants made more boundary extension errors than boundary restriction errors $F(1, 366) = 51.87$, $p < .001$, η_{p2} = .12. There was no main effect of side $F < 1$. Contrary to Experiment 2c and 2d there was no interaction between boundary error and side, $F(1, 94) = 2.76$, $p = .09$, $\eta_{p2} = .01$. But note that the pattern of results are in the same direction as in Experiments 2c and 2d, with participants making more boundary extension errors when the boundary extension option appeared on the left than when the boundary extension option appeared on the right, $t(366) = 1.17$, $p = .24$, d

= 0.12, 95% CI [-0.08, 0.33]; and making fewer boundary restriction errors when the boundary extension option appeared on the left than when the boundary extension option appeared on the right, *t*(366) = 1.84, *p* = .07, *d* = 0.19, 95% CI [0.01, 0.40] (see Figure 4.19). Again these data suggest that participants were marking to the left of the scale, regardless of the type of errors they made.

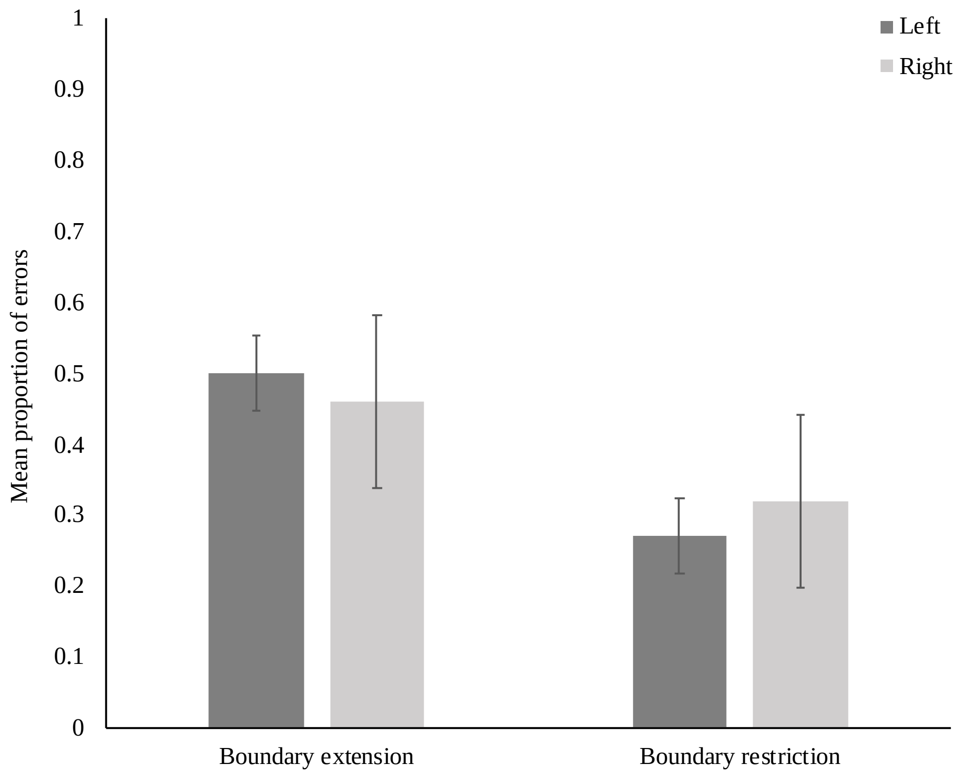

Figure 4.19. Proportion of boundary errors by error type and separated by the side on which the boundary extension error option ("closer") appeared. Error bars represent 95% confidence intervals.

In Experiment 2e, we replicated Experiment 2d, with a larger number of images. However, despite this increase, and consistent with Experiments 2a – 2d, we found no difference on boundary judgement errors according to valence. Consistent with Experiment 2c and 2d, we also found laterality effects, where participants were more likely to make boundary extension errors, and less likely to make boundary restriction errors, when "closer" (the boundary extension option) appeared on the left, suggesting that participants were marking to the left of the scale, regardless of the type of errors they made.

4.14 General Discussion

Our aim was to isolate valence from other features of an image that may accompany valence—such as image aversiveness, composition, and content—to investigate how valence alone contributes to boundary judgement errors. Over five experiments, there were five key findings. First, we found that valence—manipulated by priming—did not affect the rate, the type, nor the extent of participants' boundary judgements errors. Second, consistent with previous research, across all experiments, participants made more boundary extension errors than boundary restriction errors. Third, in Experiment 2c and 2d, we found that the side to which participants indicated boundary extension errors affected the type of error participants made. Specifically, when the boundary extension option appeared on the left, participants made more boundary extension errors, and fewer boundary restriction errors, than when the option appeared on the right. However, note that this interaction between boundary error type and the position of the anchor was not significant in Experiment 2e. Fourth, consistent with our hypotheses (and with Porter et al., 2014), participants rated the valence of the images in line with the valence of the primes. Last, across the five experiments, participants rated the arousal of the negatively primed images as consistently the most arousing, with one anomaly: participants in Experiment 2a rated the no-prime images as the most arousing. However, note that the size of this difference between negatively primed images and no-prime images was small (d = 0.25). In contrast, in Experiments 2b – 2e, these effect sizes ranged from small to medium (ds = 0.36 – 0.50).

The present experiments' findings demonstrate that in the absence of attention-grabbing and/or arousing negative stimuli, negative valence does not affect participants' boundary judgement errors. Recall that Beighley et al. (2018) isolated valence from the other elements of negative images by altering the facial expressions of the people featured in the images (either happy or sad expressions) thus creating nearly identical positively and negatively valenced version of images. Though participants rated the images in line with the photo subjects' facial expression, participants' boundary extension errors—which were common—were not affected by image valence. It is thus possible that emotion—whether positive or negative—does not affect boundary judgement errors. However, without a neutral image condition, Beighley et al. could not make this claim. Additionally, without a rating of how arousing/negative the images were (specific to the images) the negative and positive valence differences could not be attributed specifically to the mood of the participant or the aversiveness (or pleasantness) of the images themselves. The present series of experiments confirm and expand upon Beighley et al.'s study. We demonstrated that valence (measured by participants' ratings of image pleasantness) could be isolated from arousal (measured by participants' ratings of image arousal) and anxiety (as measure by change in the STAI). We found that these measures changed independently of one another, indicating that our manipulation altered the valence of the images, but not their emotional arousal, or participants' general affect. Past studies have used negative and neutral stimuli (e.g., Candel et al., 2003; Mathews & Mackintosh, 2004; Safer et al., 1998) a paradigm which was later refined by Beighley et al. (2018) using only emotional stimuli (negative and positive, with no neutral condition). Our series of experiments expanded further on Beighley et al. by including a non-emotional condition (neutral), allowing us to make similar comparisons to past research between emotional and neutral stimuli (e.g., Candel et al., 2003; Davies, et al., 2007;

Intraub et al., 1992; Kensinger et al., 2006; Steinmetz & Kensinger, 2013).

Our finding that emotional valence does not affect boundary judgement errors has both theoretical and methodological implications. When we removed all of the factors that accompany negative valence—such as image aversiveness, composition, and content—and manipulated valence in isolation, we found that negative valence did not induce boundary restriction. Our results suggest that prior research—which has demonstrated that negative images induce memory narrowing and boundary restriction more than neutral images (e.g., Kramer, Buckhout, & Eugino, 1990; Mathews & Mackintosh, 2004; Mitchell, Livosky, & Mather,1998; Pickel, 1998; Safer et al., 1998)—may have found boundary restriction because of these other elements of negative images that coincide with valence. Indeed, attention—rather than the negative valence per se—may be the underlying mechanism behind boundary restriction. In support of this idea, all of the studies that have successfully demonstrated boundary restriction have only found the effect when using negative images containing attention-grabbing negative aspects (e.g., Mathews & Mackintosh, 2004; Safer et al., 1998), highly novel/unusual stimuli (e.g., Mitchell et al.,1998; Kramer et al., 1990; Pickel, 1998) or threatening stimuli (Most, Chun, Widders, & Zald, 2005; Mogg et al., 2000; Yiend, 2010).

Boundary restriction may arise in response to attention-grabbing stimuli because people preferentially attend to negative images (see Yiend, 2010 for a review), and they have trouble disengaging from negative stimuli in favour of neutral stimuli (Most, Chun, Widders, & Zald, 2005; Mogg et al., 2000); there is even evidence that this attentional bias towards emotional stimuli happens at an unconscious, mechanical level (i.e., emotional induced blindness; Most, Chun, Johnson, & Kiehl, 2006; Most et al., 2005). In contrast, the images used in the present experiments may not have captured attentional focus in the way that images from other boundary restriction studies did, because there were no specific attention-grabbing stimuli to focus attention (e.g., Mathews & Mackintosh, 2004). Although we altered successfully altered the image valence, without such stimuli, peoples' capacity to extrapolate and imagine beyond the boundaries was not inhibited, and boundary extension occurred unabated. Thus, researchers trying to induce boundary restriction may need to either focus on inducing anxiety and altering state affect, or use more emotionally arousing content, attention-grabbing stimuli, or threatening/unusual objects rather than merely relying on negative valence.

Second, our finding that boundary judgement errors were affected by Likert anchor placement highlights the importance of counterbalancing Likert options on camera distance scales. Experiments 2c-2e demonstrated that participants tend to mark to the left of the scale, regardless of the error type. This finding is consistent with laterality studies which have found that participants tend to mark to the left of Likert scales more often than to the right (Nicholls, Orr, Okubo & Loftus, 2006). Often, when using camera distance scales, options run from "closer" (boundary extension) on the left to "farther" (boundary restriction) on the right. Thus, this tendency for participants to mark to the left of the scale may have been exacerbating the already robust boundary extension effect. To ensure that we control for this bias, camera distance scale anchors should always be counterbalanced. Alternatively, boundary adjustment tasks may be an important alternative to avoid this potential influence (e.g., Daniels & Intraub, 2006).

We must acknowledge several limitations of the present study. First, we cannot rule out that participants' pleasantness ratings reflected their perception of the prime statements alone, rather than the images. However, two methodological procedures make this possibility unlikely. We asked participants to rate the *image* pleasantness, rather rate the prime or their own feelings at the time of encoding. Moreover, in Experiments 2d and 2e, participants read the prime statement, and then separately viewed and rated the image in isolation of the prime. In both experiments, participants' valence ratings were comparable with Experiments 2a - 2c, where participants viewed the prime and image simultaneously: in all cases participants rated the images' valence consistent with the prime valence. These results suggest that participants were rating the *images* across experiments rather than the prime statements. Furthermore, previous research supports our assertion that the valence of the image was altered by the primes. When an image is ambiguous, participants will not automatically process the semantic meaning, and instead rely on other internal processes (e.g., negative bias; Ferguson, Bargh, Nayak, 2005). By presenting primes with the ambiguous images, we altered these internal processes, and induced a valence for the participant to feel, before they had interpreted—or, in Experiments 2d and 2e before they had even viewed—the image.

A second limitation is that we did not manipulate arousal in the present study, and we found that arousal ratings were inconsistent across the five experiments. A further limitation is that we asked participants to rate the arousal of the *image* at the time of encoding, rather than their own emotional arousal. Without a measure of participants' own arousal, it is difficult to draw from our findings any definitive conclusions about the effect participants' own arousal had on boundary judgement errors. Thus, in order to investigate the effect of arousal on boundary judgement errors future studies should ask participants how emotionally aroused they *felt* rather than how arousing the image was. Note that we address this limitation is Chapter 7, Experiment 6.

To summarise, our results reveal that the valence of an image had no effect on boundary judgement errors. Though we cannot categorically claim that negatively valenced images in-and-of themselves do not affect boundary judgement errors, our study does add to the growing body of literature that demonstrates that more than valence is at play in the boundary restriction effect. Possibly, it is the factors that *accompany* negative images—such as attention-grabbing objects, or arousing, threatening stimuli—that actually induce boundary restriction (e.g., Mathews & Mackintosh, 2004; Menetrier, Didierjean, & Vieillard, 2013; Safer et al., 1998; Touryan, Marian, & Shimamura, 2007). Next I investigate how attention—in isolation of negative valence—affects boundary restriction.

5 Using Attention Bias to Investigate Boundary Judgement Errors

5.1 Abstract

Pseudoneglect is a phenomenon whereby neurotypical people tend to pay slightly more attention to their left than to their right visual hemifield. It is a specific type of perceptual asymmetry that can affect our memory for visual information. Under ordinary conditions people exhibit boundary extension, resulting in a memory error of more periphery than we actually saw. There is some evidence that boundary extension is affected by pseudoneglect, with participants making more expansive boundary extension errors for visual information in their right visual hemifield as opposed to their left visual hemifield (Dickinson & Intraub, 2009). The current experiments expanded on Dickinson and Intraub's study by examining how attention for images presented in each visual hemifield (rather than a single centrally presented scene) affected boundary judgements. In Experiment 3a, participants viewed one single image (either 75% cropped, or 100% original) in either visual hemifield. Participants were then presented with a forced choice test item immediately after each trial. Each test item either consisted of the identical image (75% cropped) alongside the more expansive version (100% uncropped; boundary extension); or the identical image (100% uncropped) alongside the less expansive version (75% cropped; boundary restriction). In Experiment 3b, participants viewed image pairs, one in each visual hemifield, either cropped or uncropped. Participants then completed the same test items as in Experiment 3a, immediately after each trial. Across both experiments, we found that the hemifield in which participants viewed the image did not affect boundary error type, nor boundary error frequency.

5.2 Introduction

In Chapters 3 and 4, we found that valence in and of itself did not affect boundary judgement errors, but shorter encoding times did increase boundary restriction. This valence finding contrasts previous studies that have found that negative images induce boundary restriction (e.g., Mathews & Mackintosh, 2004), but is consistent with studies that have not been able to find the effect (e.g., Candel, Merckelbach, & Zandbergen, 2003). It is possible that boundary restriction occurs because of the attention-grabbing nature of negatively valenced images, rather than negative valence itself. Therefore, in the present study we aimed to isolate attention (using an incidental manipulation of attention; pseudoneglect)—from valence (using neutral images). There is some evidence that boundary extension is affected by pseudoneglect, with participants making more expansive boundary extension errors for visual information on the right side of a stimulus—where less attention is paid—compared to the left side (Dickinson & Intraub, 2009). Here, we expanded Dickinson and Intraub's study to investigate the effect of pseudoneglect on boundary judgement errors for individual images presented in discrete visual hemifields.

Visuospatial attention is lateralised, leading to a bias in attention allocation whereby most neurotypical people pay slightly more attention to the left side of space than to the right (Kinsbourne, 1970). This asymmetry is known as pseudoneglect, which is observable in several basic judgement tasks. In the Landmark task (Harvey, Milner, & Roberts, 1995), participants view a series of pre-bisected horizontal lines and identify which side of the line is longer (Learmonth, Gallagher, Gibson, Thut, & Harvey, 2015). In the manual line bisection task, participants view a horizontal line and manually indicate the centre point of the line (either with pen and paper or on a computer screen; Learmonth et al., 2015). In the greyscales task, participants view two bars that start as black and gradually fade to white (and vice versa), and select which side of the bar looks darker (or lighter; Mattingley, Bradshaw, Nettleton, & Bradshaw, 1994). All three tasks show an attentional bias to the left; participants judge the left side rather than the right side as longer more often in the Landmark task, consistently draw the “centre” marker to the left of true centre in manual line bisection, and tend to pick the target (darker or lighter) that appears on the left side of the bar in the greyscales task (Learmonth et al., 2015).

Pseudoneglect occurs because the right hemisphere is predominantly responsible for visuospatial attention (de Schotten et al., 2011), and visual information is initially processed contralaterally (Beaumont, 1983). This initial right hemisphere processing results in a small but consistent bias of attention to the left side of space, and slight neglect of the right side of space (e.g., Bowers & Heilman, 1980; and see Jewell & McCourt, 2000 for a review). The attentional bias appears to originate wherever participants focus their initial eye gaze. In particular, studies that have measured participants’ initial saccade (first eye-movement) when viewing a stimulus show that participants generally make their initial saccade towards their left visual hemifield in accordance with a leftward attentional bias (Bradshaw, Nettleton, Nathan & Wilson, 1985; Foulsham, Gray, Nasiopoulos, & Kingstone, 2013; Chokron & Imbert, 1995; Dickinson & Intraub, 2009; Ossandón, Onat, & König, 2014).

This leftward attentional bias can affect how people perceive image boundaries. In a key study, participants fixated on a central point on the screen, and then viewed a neutral image for 500ms (Dickinson & Intraub, 2009). These neutral images covered their entire visual field and had one salient (but neutral) object on either side of the fixation cross. After viewing each image, participants immediately completed a boundary adjustment task, whereby they viewed the image again, and adjusted the boundaries to conform with their memory of the image they just viewed. Across two experiments, Dickinson and Intraub

(2009; Experiments 1 & 3) demonstrated that boundary extension errors were more expansive to the right side of space than to the left.

Importantly, Dickinson and Intraub's (2009) paradigm had two salient objects within a common space (i.e., the left visual hemifield object had no distinct right-hand boundary, while the right visual hemifield object had no distinct left-hand boundary). This setup reflects our natural state of viewing; our visual field is not divided neatly into left and right hemifields with a central divide (Stone, Leicester, & Sherman, 1973). There is some crossover close to the centre of the visual field where ipsilateral (same side) processing occurs (e.g., left visual hemifield processed by the left hemisphere). The lateralisation of the visual system means that each hemi-retina—of each eye—perceives the opposing side of the visual field. Thus, in Dickinson and Intraub's study, when participants judged the boundaries of the sides of an image, they judged the position of the left boundary relative to the position of the right boundary, resulting in a perceptual error in the size of the image. However, from this experiment we do not know what would happen if stimuli were presented to one visual hemifield only, such that they would be received and initially processed by the contralateral hemisphere (i.e., an image presented in the left visual hemifield is processed by the opposite (right) hemisphere, and vice versa; Bourne, 2006). Using such a paradigm, we could isolate the influence of contralateral processing on boundary judgement errors for entire images. In particular, we expected that right hemisphere processing (i.e., images presented in the left hemifield) would lead to more attention to images presented on the left, and thus pseudoneglect—and more boundary extension, and less boundary restriction—of images presented on the right.

To examine whether lateralised attention (induced by contralateral processing) would attenuate boundary extension and/or induce boundary restriction, we partially replicated Dickinson and Intraub (2009), but presented a single image either side of a fixation cross (see Moeck, Thomas, and Takarangi, 2018) in a true divided visual field paradigm that ensured that 1) each image had distinct left and right boundaries, and 2) the initial processing was contralateral. We retained Dickinson and Intraub's 500ms encoding time for each image. We also used a recognition memory test, rather than a boundary adjustment task, based on the finding that recognition memory is better for images presented in the left visual hemifield than in the right visual hemifield (e.g., Moeck, Thomas, & Takarangi, 2018). This test is a coarse-grained measure with only two possible choices (boundary restriction / boundary extension, or correct), compared to the boundary adjustment paradigm, which is a fine-grained measure with multiple possible choices.

We predicted—based on Dickinson and Intraub's (2009) findings—that participants would make more boundary restriction errors for images presented on the left (where more attention is paid) than on the right (where less attention is paid) and participants would make more boundary extension errors on the right than on the left; an interaction between error type and target presentation side.

5.3 Experiment 3a Method

Participants. Dickinson and Intraub (2009) used 24 participants thus we aimed to recruit a similar number of participants. Our analyses focus on a collected sample of 32 participants. To maximise the likelihood that visuospatial attention was lateralised to the right hemisphere, we recruited only strongly right-handed participants (Hugdahl, 2000; Nicholls, Thomas, Loetscher, & Grimshaw, 2013) from an undergraduate research participation pool, and the wider community. The majority of participants were female (62.5%), and ranged in age from 18-67 (*M*= 30.53, *SD*= 12.18). Participants received course credit, or payment

(AUD$10) for their time.

Design. We used a 2 (target side: left, right) x 2 (encoded image: cropped, uncropped) within-subjects design. Our key dependent variable was proportion of errors.

Materials

Images. We selected 128 neutral images from the International Affective Image System (IAPS; Lang, Bradley, & Cuthbert, 2008). Each IAPS image has normed emotional valence ratings (1 = very unpleasant, 9 = very pleasant). Images were selected or edited (in Photoshop) to have one central object, on an expansive background. Fifty participants from Amazon Mechanical Turk rated the all of the 128 images according to how pleasant (from 1 = happy (e.g., pleased, satisfied, contented, hopeful) to 5 = unhappy (e.g., annoyed, unsatisfied, melancholic, despaired)), it made them feel (M = 2.84, SD = 0.64) and how emotionally arousing (from 1 = excited (e.g., stimulated, excited, frenzied, jittery, wide-awake, aroused) to 5 = calm (e.g., relaxed, calm, sluggish, dull, sleepy, unaroused)) it made them feel (M = 3.26, SD = 0.86). We cropped all 128 images to 75% of their original size, resulting in two versions of each image (75%, and 100%).

Handedness measure. Before the presentation of the images, participants rated their handedness using the Flinders Handedness Survey (FLANDERS; Nicholls et al., 2013). The FLANDERS scores from -10 (completely left-handed) to +10 (completely right-handed). The FLANDERS has 10 questions where participants indicate which hand (left, either, or right) they use to complete obvious (e.g., ‘with which hand do you write?’) and less obvious (e.g., ‘in which hand do you hold the peeler when peeling an apple?’) tasks. We excluded participants based on their FLANDERS scores before they participated in the experiment, only including participants who scored between +8 and +10 (*strongly* right-handed), M = 9.59, SD = 1.78.

Procedure

The Flinders University's Social and Behavioural Research Ethics Committees approved this research. Participants completed the experiment on an Apple iMac desktop computer using E-Prime software. Participants were situated approximately 40cm from a 20-inch screen, with their chin on a chin rest to limit head movement, and to maintain the visual angle at which the stimulus was presented (as recommended by Bourne, 2006). Although traditionally laterality studies present stimuli within a timeframe that is sufficiently short that eye movements are unlikely (e.g., between 150-180ms; Bourne, 2006), using such short presentation times is not viable for testing memory, because short presentations likely affect encoding such that memory accuracy falls below chance (Moeck et al., 2018). Thus, in the current experiments we balanced the need for contralateral processing with presentation times long enough for above-chance accuracy. It has been demonstrated that 500ms is an appropriate encoding time to investigate memory errors (e.g., Loftus & Kallman, 1979); boundary extension errors (e.g., Dickinson & Intraub, 2009); and the effects of leftward attentional bias on memory, but is short enough for participants to maintain central fixation (e.g., Moeck et al., 2018). The images (width: 14.93°, height: 11.19°) appeared 2.3° from the fixation cross with the opposing edge of the image aligned 1.7° from the edge of the screen. Each participant viewed one version of each image (i.e., 75% cropped or 100% uncropped). Half of the presented images were cropped, and half were uncropped. We counterbalanced the presentation of cropped and uncropped images versions, so that all images were presented in both sizes and equally often, with the order of presentation randomised for each participant.

Each trial was preceded by a black fixation cross in the centre of a white screen, for 500ms. The cross then accompanied a single image presented in one visual hemifield for

500ms. As per Dickinson and Intraub (2009) and Moeck et al. (2018), participants were instructed to maintain fixation on the cross during image presentation. Across 128 trials, we counterbalanced the position of the image (left or right) and counterbalanced the image type (75% or 100%), resulting in 64 trials for each visual field. Immediately after each trial, participants viewed both versions of the target image—75% cropped and 100% uncropped—presented at the top and bottom of the screen, and made a judgment about the image they had seen. We counterbalanced the position of the cropped and uncropped versions of the image, so that sometimes participants saw the cropped version on the top, and sometimes they saw the cropped version on the bottom. We also included mirror-reversed versions of each image, to rule out any compositional differences that may have affected boundary judgements (Dickinson & Intraub, 2009). Participants were instructed to select the image they had originally seen, and we recorded participant response times. We instructed participants to "please respond as quickly and as accurately as possible". When participants viewed an uncropped image at encoding (half the time), incorrectly selecting the cropped version at test was a boundary restriction error; when participants viewed a cropped image at encoding, incorrectly selecting the uncropped version at test was a boundary extension error. Correctly selecting the image they had actually seen was classified as no error. Participants then rated how confident they were in their (1 = not at all certain to 5 = absolutely certain, with the separate option of 0 = completely guessing). The whole session was completed in approximately 30 minutes (including debriefing).

5.4 Experiment 3a Results and Discussion

Analysis of boundary errors. We conducted a 2 (target side: left, right) x 2 (encoded image: cropped, uncropped) within-subjects repeated measures ANOVA on proportion of errors to test our primary research question: can pseudoneglect attenuate boundary extension errors, or induce boundary restriction errors? Overall, participants made a similar number of boundary extension (*M*= .37, *SD*= .15) and boundary restriction errors (*M* = .33, *SD* = .14), thus there was no main effect of encoded image, $F(1,31) = 1.19$, $p = .39$, $\eta_p^2 = .04$. This finding is contrary to most boundary restriction studies, which consistently find that boundary extension is the more common boundary judgement error. Participants made a similar number of errors when the image was presented in the right (*M* = .36, *SD* = .15) and the left (*M* = .35, *SD* = .15) hemifield, thus there was no main effect of target side, $F < 1$. Additionally, and contrary to our predictions, we found no interaction between error type and side presentation, $F < 1$, see Figure 5.1.

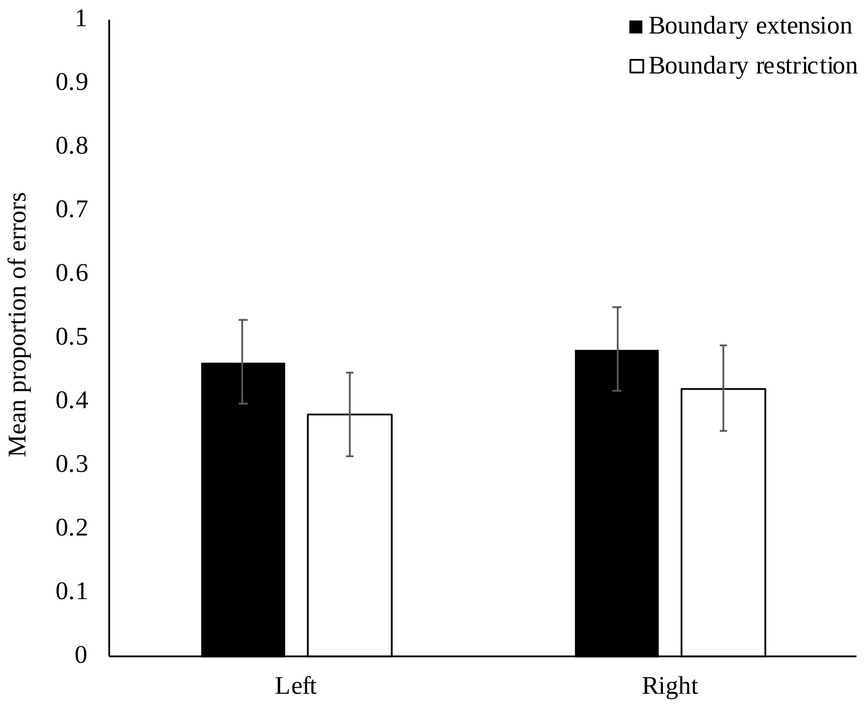

Figure 5.1. Mean proportion of boundary errors by error type and presentation side for Experiment 3a. Error bars represent 95% confidence intervals.

We next ran a paired-samples Bayesian t-test with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) on boundary restriction errors by side (left vs. right), and found BF_{10} = 0.27. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates no evidence for a difference in boundary restriction errors for images presented on the left versus the right. In other words, participants were not more likely to make boundary restriction errors for images presented on the left than on the right.

Recall that we asked participants to rate their confidence in their boundary judgements, and to indicate whether they were just guessing. We removed the ratings that participants rated as "guesses", and reanalysed the data, but found the same pattern of results as our initial analysis. Participants made a similar number of boundary extension (*M*= 0.37, *SD*= 0.15) and boundary restriction errors (*M* = 0.33, *SD* = 0.13), thus there was no main effect of encoded image, $F(1,31) = 1.40$, $p = .25$, $h_{p2} = .04$. Again, there was no main effect of target side, $F(1,31) = 1.02$, $p = .32$, $h_{p2} = .03$, and no interaction between error type and side presentation, $F < 1$.

We also wanted to examine whether target side or encoding type affected response time. Overall, the sample mean response time (in milliseconds) was 1500.41 (*SD* = 550.75). We conducted a 2 (target side: left, right) x 2 (encoded image: cropped, uncropped) within-subjects repeated measures ANOVA on average response time and found that neither target side (left: *M* = 1487.01, *SD* = 531.45; right: *M* = 1524.20, *SD* = 579.30, $F < 1$), nor encoding type (cropped: *M*= 1476.61, *SD* = 579.55; uncropped: *M*= 1513.80, *SD* = 545.50, $F(1,31) = 3.19$, $p = .08$, $h_{p2} = .09$), affected response times. Likewise, there was no interaction between encoding type and target side, $F(1,31) = 1.17$, $p = .29$, $h_{p2} = .04$, for average response times.

Experiment 3a demonstrated that—contrary to Dickinson and Intraub's (2009) findings—target side did not affect the number or the type of boundary judgement errors participants made. Importantly, our paradigm differed from Dickinson and Intraub in three key ways, each of which could explain why our pattern of results also differed: (1) we presented images to each visual hemifield separately, to restrict the possibility of ipsilateral processing; (2) we presented these images to only one hemifield at a time, rather than across both visual hemifields simultaneously (i.e., central unilateral presentation); and (3) we used a forced-choice recognition test. In Experiment 3b, we investigated the implications of single vs. simultaneous hemispheric processing, by presenting images in pairs. We continued to present *single* images in the left and right hemifields (rather than a single image across the visual field); we presented one per hemifield on each trial. This setup allowed us to continue to isolate contralateral processing, but mimic the more ecologically valid situation of having both hemispheres process information simultaneously. Further, participants can process images presented simultaneously at the same speed as images presented in one visual field (Rousselet, Fabre-Thorpe & Thorpe, 2002), but importantly for our purposes, this simultaneous presentation reduces accuracy due to increased cognitive load (Lavie, Hirst, Fockert, & Viding, 2004). Thus, participants would be more likely to make errors, and so we would have more opportunity to investigate the cause of each error. Finally, since we halved the trials, and presented the images in pairs, we doubled the sample size.

5.5 Experiment 3b Method

Participants. We recruited 64 participants from an undergraduate research participation pool, and the wider community. The majority of participants were female (71.9%) and ranged in age from 18-51 (M = 23.79, SD = 6.24). Participants received course credit, or payment (AUD$10) for their time. We again excluded participants based on their

FLANDERS scores before they participated in the experiment, only including participants who scored between +8 and +10 (*strongly* right-handed), $M = 9.70$, $SD = 0.68$.

Design. We conducted a 2 (target side: left, right) x 2 (encoded image: cropped, uncropped) within-subjects design, with proportion of errors the key dependent variable.

Materials. We used the same materials as Experiment 3a.

Procedure

We replicated the procedure in Experiment 3a, but here each participant viewed image pairs. The pairs consisted of the same 128 images from Experiment 3a, resulting in 64 pairs. One target image and one filler image were presented to each participant (either 75% cropped or 100% uncropped). We counterbalanced the position of the target image (left or right) and counterbalanced the image type (75% or 100%), resulting in 64 trials. We used the same test format as Experiment 3a.

5.6 Experiment 3b Results and Discussion

Analysis of boundary errors. We conducted the same 2 x 2 repeated measures ANOVA as Experiment 3a. Contrary to Experiment 3a, but consistent with previous research on boundary restriction, and with Chapters 3 and 4, participants made more boundary extension errors ($M = .47$, $SD = .17$) than boundary restriction errors ($M = .40$, $SD = .16$), a main effect of encoded image, $F(1,63) = 4.18$, $p = .045$, $\eta_{p2} = .06$. Also contrary to Experiment 3a, we found a main effect of target side, with participants making more errors when the target image appeared on the left ($M = .47$, $SD = .17$) than on the right ($M = .40$, $SD = .18$) $F(1,63) = 4.05$, $p = .048$, $\eta_{p2} = .06$. This finding is surprising; because the right hemisphere is predominately responsible for visuospatial attention we would expect that information viewed in the left hemifield would yield better memory. Indeed, most research finds a *better* memory for stimuli on the left than on the right (e.g., Dickinson & Intraub, 2009; McGeorge, Beschin, Colnaghi, Rusconi, & Della Sala, 2007; Moeck et al., 2018), thus the fact that our result here contradicts previous findings (coupled with the fact that it is only marginally significant) suggests this result is likely spurious. We found no interaction between error type and target side, $F < 1$.

Recall that our primary aim was to determine whether attention bias to the left side of space induced boundary restriction errors. We next ran a paired-samples Bayesian t-test with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) on boundary restriction errors by side (left vs. right), and found $BF_{10} = 0.77$. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates no evidence for a difference in boundary restriction errors for images presented on the left versus the right. In other words, participants were not more likely to make boundary restriction errors for images presented on the left than on the right.

In the current experiment, we again asked participants to rate their confidence in their boundary judgements, and to indicate whether they were just guessing. We removed the ratings that participants rated as "guesses", and re-analysed the data. When we removed errors that participants rated as guesses, participants still made more boundary extension errors ($M = 0.48$, $SD = 0.18$) than boundary restriction errors ($M = 0.40$, $SD = 0.18$), a main effect of encoding type, $F(1,63) = 4.43$, $p = .04$, $\eta_{p2} = .07$. Importantly however, removing guesses removed our effect of side, with participants making an equal number of errors for targets on the left and right (no main effect of side, $F(1, 63) = 3.71$, $p = .06$, $\eta_{p2} = .06$), demonstrating that the effect of side we found above may have been inflated by guesses, and was indeed likely a spurious finding. There was no interaction of encoding type and target side for proportion of errors, $F < 1$.

We also wanted to examine whether target side or encoding type affected response time. Overall, the sample mean response time (in milliseconds) was similar to Experiment 3a, $M = 1831.54$, $SD = 601.94$. We conducted a 2 (target side: left, right) x 2 (encoded image: cropped, uncropped) within-subjects repeated measures ANOVA on average response time and found that neither target side (left ($M = 1817.56$, $SD = 622.91$) right ($M = 1845.52$, $SD = 612.86$)), nor encoding type (cropped ($M = 1831.39$, $SD = 625.68$) uncropped ($M = 1831.68$, $SD = 626.2$)), affected response times Fs < 1. Likewise, there was no interaction between encoding type and target side, ($F < 1$) for average response times.

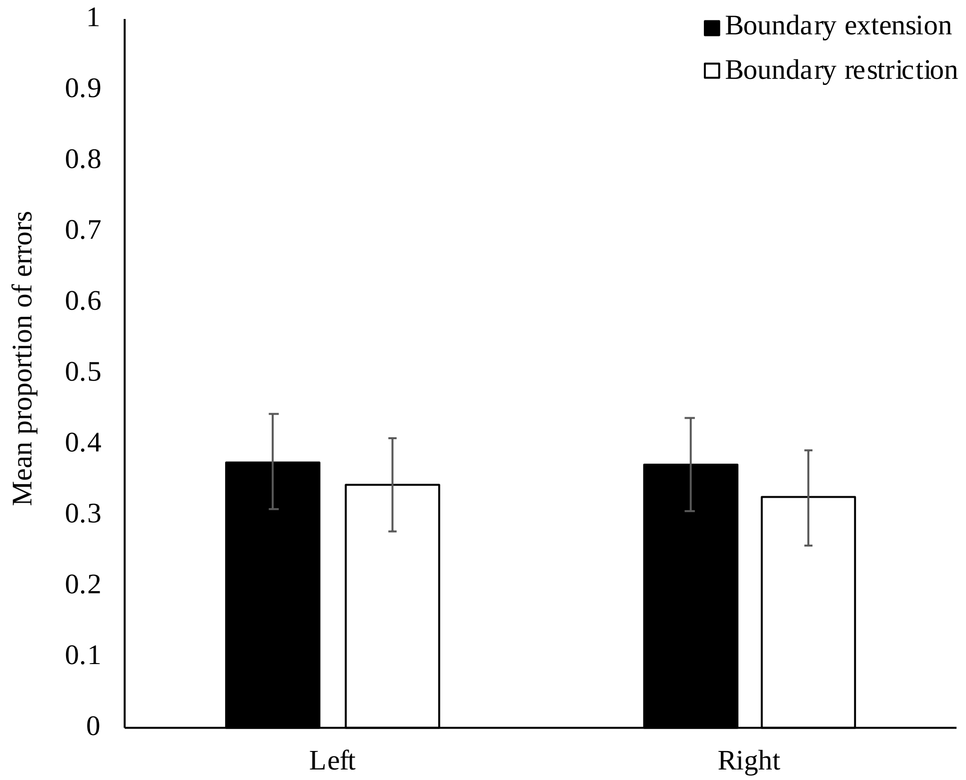

Figure 5.2. Mean proportion of boundary errors by error type and target side for Experiment 3b. Error bars represent 95% confidence intervals.

5.7 General Discussion

In these experiments pseudoneglect—our measure of attention—did not appear to affect boundary judgement errors. Recall we predicted that we would see increased boundary extension (and/or reduced boundary restriction) for images presented on the right, and increased boundary restriction (and/or reduced boundary extension) for images presented on the left. Such findings would have been consistent with Dickinson and Intraub (2009). However, there was no effect of target side, and no interaction with boundary error type. We also found, in Experiment 3a, that participants made an equal number of boundary extension and boundary restriction errors. In contrast, in Experiment 3b, participants made more boundary extension than boundary restriction errors. Across both experiments, response times were not affected by encoding type, nor by target side.

There are several possible explanations for these contradictory findings. Dickinson and Intraub (2009) found that participants did appear to display pseudoneglect: they paid more attention to the left side of images (indicated by more often making their first saccade to the left). However pseudoneglect also generally leads people to perceive that there is more space on the left, a pattern that is at odds with the typical relationship between attention and boundary extension, where people expand scenes from which their attention is diffuse (e.g., Intraub, Daniels, Horowitz, & Wolfe, 2008). These competing phenomena provide a possible explanation for our null findings; specifically, any boundary restriction effects we might have seen due to increased attention to images presented on the left could have been washed out by a general tendency to perceptually expand images on the left. These competing processes were less likely to arise in Dickinson and Intraub's paradigm, which was more consistent with a line bisection task, which has one continuous stimulus per trial. Specifically, even if participants' attention was biased to one side of an image, they could nonetheless perceive an entire image. For example, in judging the left and right sides of an image relative to one another, perhaps participants estimated the "centre" point of this image to be further on the left than it actually was, leading the right side of the image to seem more expansive and hence resulting in more expansive boundary extension errors on the right. By contrast, in the current experiments we presented images to each visual hemifield separately, such that participants had to immediately disengage their attention (but not their fixations) from the centre, and shift their attentional focus to the periphery, to perceive the entire image presented in one (Experiment 3a) or both (Experiment 3b) visual hemifields. In this situation, although our participants' attention may have been drawn more to the images in the left hemifield—an attentional capture that we would normally expect to result in boundary restriction—participants may also have perceived images in the left hemifield as more expansive, compared to images that appeared in the right hemifield, a process that would compete with boundary restriction.

Furthermore, the difference in test format likely played an additional role in this process. The boundary adjustment task Dickinson and Intraub (2009) used allowed participants to make both boundary extension and boundary restriction errors on both the left and right sides of a single image. In contrast, our test paradigm only allowed participants to make one type of error (either boundary extension or boundary restriction) or make no error for an entire image. Therefore, we were not able to measure the *extent* to which participants made boundary errors. Furthermore, participants' boundary judgement errors may have been subtle and were not reflected in the test choices. Recall that participants were given the choice to either make the correct judgement, or make an incorrect boundary judgement of +25% or -25%. Thus if the choices did not match the participants' memory for the images, they may have been conservative in their judgements (choosing correctly, rather than indicating a large error). Indeed, both pseudoneglect and boundary restriction are subtle effects (Mathews & Mackintosh, 2004; Dickinson & Intraub, 2009) which may require a sensitive test such as the boundary adjustment task to measure. To investigate this possibility, future research could replicate Experiment 3b, using a boundary adjustment paradigm.

To address the limitations in the current study, we propose two methodological changes to ensure that we are testing the effect of *attentional capture,* and not relying on a neurological process to drive attention, and to capture more subtle boundary judgement errors. We propose an attentional paradigm which allows us to observe the effect of attention on boundary judgement errors without the complexity of hemispheric processing—which can be difficult to isolate. Indeed, using a visual, automatic, bottom-up attentional capture (i.e., an abrupt visual onset stimulus) to capture attention in a consistent way, rather than relying on incidental attention capture would remove this complexity. Despite our findings here, there is converging evidence that attention affects boundary judgement errors (Intraub, Daniels, Horowitz, & Wolfe, 2008). If diffuse attention can exacerbate boundary extension, we expect that focussed attention can induce boundary restriction, which is an avenue of study that warrants further investigation. To test for more subtle boundary judgement errors, and to test for both boundary extension and boundary restriction simultaneously, we propose a camera distance paradigm. Last, to ensure we are measuring errors of memory—rather than perceptual errors—and to increase overall errors, we also want to increase the retention time between encoding and test (rather than testing immediately following the presentation of each image). In conclusion, pseudoneglect—as a measure of attention—did not affect boundary judgement errors. Next, we expand on the current findings to investigate the effect of attention on boundary judgement errors using a direct manipulation of attention.

6 Attention can Modify the Boundary Extension Effect

6.1 Abstract

For negative scenes, the “boundaries” are sometimes narrowed at retrieval, a phenomenon known as boundary restriction. This phenomenon is the opposite of the more common boundary extension error, whereby people remember images or scenes as more expansive than they actually were. However, evidence for boundary *restriction* is mixed. One explanation for these mixed findings is that the mechanism driving boundary restriction is not valence, but rather the attention-grabbing nature of the images. Thus, the studies that examined boundary restriction in the past may have used stimuli that were alike in valence but different in their attention-grabbing properties. We know diffuse attention (e.g., divided attention; Intraub et al., 2008) exacerbates boundary extension; thus, focussed attention could increase boundary restriction. To isolate attention from valence, we presented an abrupt onset stimulus (a dot), followed by a neutral image in the same place as (congruent) or in the opposite place to the dot (incongruent), across 40 trials. Participants then viewed the same images, but were told that some images had been changed. Participants judged how the image view had changed, from farther to closer. Consistent with our hypothesis that attention-grabbing stimuli induce boundary restriction, participants made more expansive boundary restriction errors and less expansive boundary extension errors for congruent—where their attention was drawn to the image—than incongruent trials. Similarly, participants made more expansive boundary extension errors and less expansive boundary restriction errors for incongruent—where their attention was drawn away from the image—than congruent trials.

6.2 Introduction

For negative scenes, people sometimes recall scene boundaries as narrower than they actually were (Mathews & Mackintosh, 2004; Safer, Christianson, Autry, & Österlund, 1998; Takarangi, Oulton, Green & Strange, 2016). This phenomenon is the opposite of the much more common boundary extension error, whereby people recall *more expansive* scene boundaries than they actually saw (e.g., Intraub, Bender & Mangels, 1992). Yet, evidence for boundary *restriction* is mixed, with some researchers unable to find the effect, even when replicating previous studies, or using stimuli matched on image compositional factors such as image layout (e.g., Candel, Merckelbach, & Zandbergen, 2003; Beighley, Sacco, Bauer, Hayes, Intraub, 2018). We posit that boundary restriction arises in response to the typically arousing and attention-grabbing objects that often appear in negative images (a bottom-up attention process), rather than the negative *valence*— "attractiveness" (i.e., positive valence) or "aversiveness" (i.e., negative valence; a top-down attention process)—of particular images. To investigate whether it is the attention-grabbing nature of the images that leads to boundary restriction, we wanted to remove all other aspects of negative images (including arousal). In Chapter 5 (Experiments 3a & 3b), we isolated attention from valence and arousal by using an incidental visual attention bias—pseudoneglect. However, we found that pseudoneglect did not affect the type or number of boundary judgement errors participants made. In the current experiment, we instead directly manipulated attention using an abrupt visual onset stimulus. Moreover, we used neutral images to control for the arousal/valence of the target images. We aimed to test whether increasing attention to an image would increase boundary restriction, and reduce boundary extension; and accordingly, whether reducing attention to an image would increase boundary extension and decrease boundary restriction.

Not all of the available evidence supports the idea that negative (versus neutral or positive) images induce boundary restriction (e.g., Candel et al., 2003; Safer et al., 1998). For example, Beighley et al. (2018) found that boundary restriction was difficult to induce using negative valence alone. Participants viewed images where scene structure was identical, but the expression displayed by the photograph's subject was either negative or positive. Participants then viewed the same scenes at test and rated how much the camera view had changed. Participants made boundary extension errors and few boundary restriction errors; neither were affected by valence. Consistent with Beighley et al., when we isolated valence by using neutral images accompanied by prime statements (Chapter 4), we also found that—overwhelmingly—participants made boundary extension errors, regardless of valence. Together, these findings demonstrated than boundary restriction may not be induced by valence alone.

The difficulty in producing boundary restriction using valence alone may be because—rather than valence—it is the attention-grabbing nature of negative images that leads to boundary restriction. Indeed, we know that when people's attention is not captured, boundary extension is more likely. In one example, participants looked for digits superimposed over a neutral image (divided attention) or rated how easy it was to see the image behind the superimposed numbers (focused attention; Intraub, Daniels, Horowitz, & Wolfe, 2008). Then participants viewed either an identical image, or an opposite view (close-up at encoding, wide-angled at test, wide-angled at encoding, close-up at test), and made a camera distance rating. Participants in the divided attention condition showed more boundary extension errors than those in the focussed attention condition. Thus, if diffuse attention exacerbates boundary extension, focussed attention may attenuate boundary extension and/or induce boundary restriction.

Attention can induce boundary restriction regardless of valence. In Chapter 3

(Experiment 1) we demonstrated that attentional capture—manipulated using varying presentation times—increased boundary restriction errors for both neutral and negative images. Similarly, Mitchell, Livosky and Mather (1998) demonstrated that captured attention —induced by a weapon or unusual object in the scene—led participants' memory for peripheral details to be less accurate, a pattern similar to boundary restriction. Other studies of this "weapon focus effect" have found that emotionally salient information focusses participants' attention such that peripheral details are ignored, not encoded, or are completely forgotten, a phenomenon known as "memory narrowing" (Fawcett, Russell, Peace, & Christie, 2013; Kramer, Buckhout, & Eugenio, 1990; Pickel, Ross, & Truelove, 2006).Taken together, these data suggest that when boundary restriction is observed, it may actually be the result of an object in an image capturing attention—due to restricted encoding time, unusualness, or salience—which leads to a rejection of the periphery and focus on the central object.

To summarise, evidence for negative valence inducing the boundary restriction effect is mixed, but the role of attention in determining boundary judgements may shed some light on these mixed findings. Thus, in the present study we wanted to investigate the effect of attention on boundary judgement errors, *in isolation of valence*. We used a more direct manipulation of attention, a modified dot-probe paradigm, rather than relying on an incidental attention manipulation (as in Experiments 3a and 3b). We presented an abrupt onset visual stimulus to either draw attention *to* or draw attention *away* from neutral images at encoding. We predicted that participants would make more expansive boundary extension errors and less expansive boundary restriction errors for incongruent trials: when images were presented in a different position to the dot probe. We also predicted that participants would make more expansive boundary restriction errors and less expansive boundary extension for congruent trials: when images were presented in the opposite position as the dot probe. Lastly, we predicted that participants would make more boundary restriction errors and fewer boundary extension errors for congruent trials than for incongruent trials.

6.3 Experiment 4 Method

Participants. We conducted an a priori power analysis using the program G*Power (Erdfelder, Faul, & Buchner, 1996) to determine the sample size necessary to detect a predicted small effect (*d* = 0.2) with a paired samples t-test. For *d* = 0.2, alpha = .05, power = .80, G*power suggested data from 156 participants. Counterbalancing required our sample to be divisible by 8, so we increased the number to 160, recruited from an undergraduate research pool. The majority of participants (78.80%) were female, identified their ethnicity as Caucasian (including “White” 73.80%), Asian (16.30%), European (3.80%), Hispanic (1.90%), African (2.50%) and mixed (1.90%). The participants ranged in age from 18 to 67 (*M* = 24.18, *SD* = 9.04). To ensure that we could accurately test the effects of laterality on the slider anchors, we only recruited participants who were strongly right-handed *M* = 9.89, *SD* = 0.35 *range* = +8 - +10 (The Flinders Handedness Survey, FLANDERS; Nicholls, Thomas, Loetscher, & Grimshaw, 2013). Participants were selected with normal to corrected to normal vision (self-reported). 160 eligible participants completed the experiment. Participants received course credit, or payment (AUD$10) for their time.

Design. We used a within-subjects design. Our key independent variable was probe congruence (congruent, incongruent). Our key dependent variable was mean distance judgements (-50 = boundary extension, +50 = boundary restriction, with 0 = no change, on a slider). To analyse proportion of errors, we also used a 2 (congruence: congruent, incongruent) x 2 (boundary error type: boundary extension, boundary restriction) within-subjects design.

Materials

Stimuli preparation. We ran several pilot studies in order to ensure that our new and edited images were within the neutral valence range (see Chapter 7). We selected or edited (using Photoshop) 128 neutral images (between 4-6 on a scale where 1 = very unpleasant, 9 = very pleasant) from the International Affective Picture System (IAPS; Lang, Bradley, & Cuthbert, 2008), and the Nencki Affective Picture System (NAPS; Marchewka, Zurawski, Jednoróg, & Grabowska, 2014). The pictures were selected or edited to have one central object on an expansive background. We asked 180 pilot participants to rate the images on valence (1 = very pleasant, to 5 = very unpleasant) and arousal (1 = relaxed/calm, 5 = aroused/excited). We asked a second sample of 180 participants to categorise the all of the images—including the new, edited, and the IAPS and NAPS images— into distinct categories and rate their category membership: “Nature”, “Everyday Objects”, “Tools”, “Landscapes”, and “Animals”. Participants were asked “*How well does this picture fit into the category …”* from 1 = very well, to 7 = not at all. We selected images with neutral valence ratings (M = 3.12, SD = 0.24). From the category group ratings, we determined that 40 images fit into one distinct category: Everyday objects (M = 1.67, SD = 0.26).

Manipulation of attention. We used an abrupt visual onset stimulus to attract participants’ attention to one side of the screen or the other (i.e., top or bottom). Early research into visual search and attentional capture have demonstrated the effectiveness of abrupt visual onset stimulus in capturing and holding attention in visual search tasks (e.g., Yantis & Jonides, 1984; Jonides & Yantis, 1988). This attention capture seems to be an evolutionarily driven need to identify new objects in the visual field, in order to assess their relevance/need for attention (Yantis & Hillstrom, 1994). Thus, this paradigm is an ideal way to capture and hold attention during an encoding task.

Procedure

We pre-registered this study on the Open Science Framework (https://osf.io/82e4r/). The Flinders University's Social and Behavioural Research Ethics Committees approved this research. Participants completed the experiment on an Apple iMac desktop computer using Superlab. Participants were situated approximately 40cm from the screen.

Encoding Phase. We presented the encoding phase using Superlab software, and the test phase on Qualtrics[6]. We divided images into four groups of 10 images each, matched on valence, arousal and category group membership. The four groups were then counterbalanced to presentation position (top, bottom), and probe position (congruent, incongruent). Each trial began with the display of a white fixation cross in the middle of a black background for 1000ms. Participants were instructed to fixate on the cross for the entire time that it appeared on the screen. The cross was then followed by a dot probe (a white dot on a black background; 500ms). The dot probe appeared either where the image would appear (congruent trials) or in the opposite position to where the image would appear (incongruent trials), which was counterbalanced across participants. Each image then appeared either at the top or bottom of the screen for 2000ms. The images appeared in a random order.

We measured participants' state anxiety before and after the encoding phase using the using the 20-item state subscale of the State-Trait Anxiety Inventory *(STAI-S;* Spielberger, Goruchm, & Lushene, 1970). Participants read a series of statements about how they felt *at the present time*, (e.g., "I feel calm") and rated how much they agreed with each statement on

[6] We used Qualtrics programming because Superlab does not yet have the capabilities of using a slider question. Likewise, we were unable to run the dot probe paradigm in Superlab.

a 4-point Likert scale. We measured state anxiety over time, because state anxiety is malleable (unlike trait anxiety). Overall, the STAI has good reliability and validity for both trait and state subscales (α = 0.88; Grös, Antony, Simms, & McCabe, 2007). We also found high internal consistency for STAI-S: α = 0.92 at time 1, and α = 0.94 at time 2.

Test Phase. Participants saw identical images to the encoding phase. However, we told them that the images were similar to the images originally presented, but that some had been altered so that the camera was either slightly closer to, or slightly further away from, the main object(s) in the picture (e.g., Candel et al., 2003; Takarangi et al., 2016). For each image, participants judged whether the "camera distance" matched the camera distance of the image that they saw earlier. Participants indicated the perceived change using a slider scale, from “Much farther than the original” to “Much closer than the original”. We counterbalanced the anchors such that half of the participants received “Much farther than the original” on the left, and the other half received “Much farther than the original” on the right[7]. We began the test phase by showing participants six examples of their task. Participants were shown pairs of images, one at a time. The image pairs were altered so that the camera was either slightly closer to, or slightly further away from, the main object(s) in the picture. For each pair, participants were told that the "camera distance" of the image on the right either matched, was more zoomed out, or was more zoomed in than the camera distance of the image on the left. Participants were then asked to complete some practice trials with images presented in this way (but without the caption). Participants were told: “In the example above, compared to the picture on the left, the camera has moved CLOSER in

[7] See Chapter 2, Experiment 2c – 2e for the laterality findings that informed this counterbalance procedure.

the picture on the right. So you can now see less background in the picture on the right.”. Participants were required to accurately rate two of each type of test (closer, same, farther) to remain in the study. None of our participants failed this practice task. Following the practice images, participants then completed the test with the original 40 images in a randomised order. Following the experiment, we fully debriefed all participants.

6.4 Experiment 4 Results and Discussion

We first measured participants’ state anxiety (on the STAI-S). We found that participants’ state anxiety remained stable from time 1 (M = 43.47, SD = 5.24), to time 2 (M = 43.09, SD = 5.92, $t(159)$ = 1.18, p = .24, d = 0.09, 95% CI [-0.06, 0.25]). This lack of change in the STAI demonstrates that the images did not alter participants’ state. We next turned to our main research aim, to examine whether participants would make more expansive boundary extension errors and less expansive boundary restriction errors for incongruent trials; and whether participants would make more expansive boundary restriction errors and less expansive boundary extension for congruent trials. We first examined the rate at which participants made errors at test. When we examined the proportion of participants’ errors overall, we found that participants were generally inaccurate (74.36% of the trials, on average) at identifying that the image had not changed. We calculated mean camera distance, from -50 (boundary extension) to +50 (boundary restriction), where 0 = no change for each participant. Overall, participants made slightly more boundary extension errors than boundary restrictions errors; the mean camera distance rating was M = -0.19 (SD = 5.64). However, this mean was not significantly different from zero, meaning participants did not make more boundary extension errors than correct ratings, $t(159)$ = 0.42, p = .67, d = .03 95% CI [0.12, 0.19]. This finding is contrary to most boundary restriction studies, with the majority finding that participants overwhelmingly make boundary extension errors.

We next examined our main aim, whether attention drawn to an image would induce boundary restriction. We compared camera distance judgements on congruent vs incongruent trials, using a paired samples t-test. On average we found that for congruent (where attention was drawn to the image) trials, participants rated the distance as above zero (boundary restriction) (*M* = 0.20, *SD* = 5.95), whereas for incongruent trials (where attention was drawn away from the image), participants rated the distance as below zero (boundary extension) (*M* = - 0.58, *SD* = 6.23), *t*(159) = 2.15, *p* = .03, *d* = 0.17 95% CI [0.01, 0.33], see Figure 6.1.

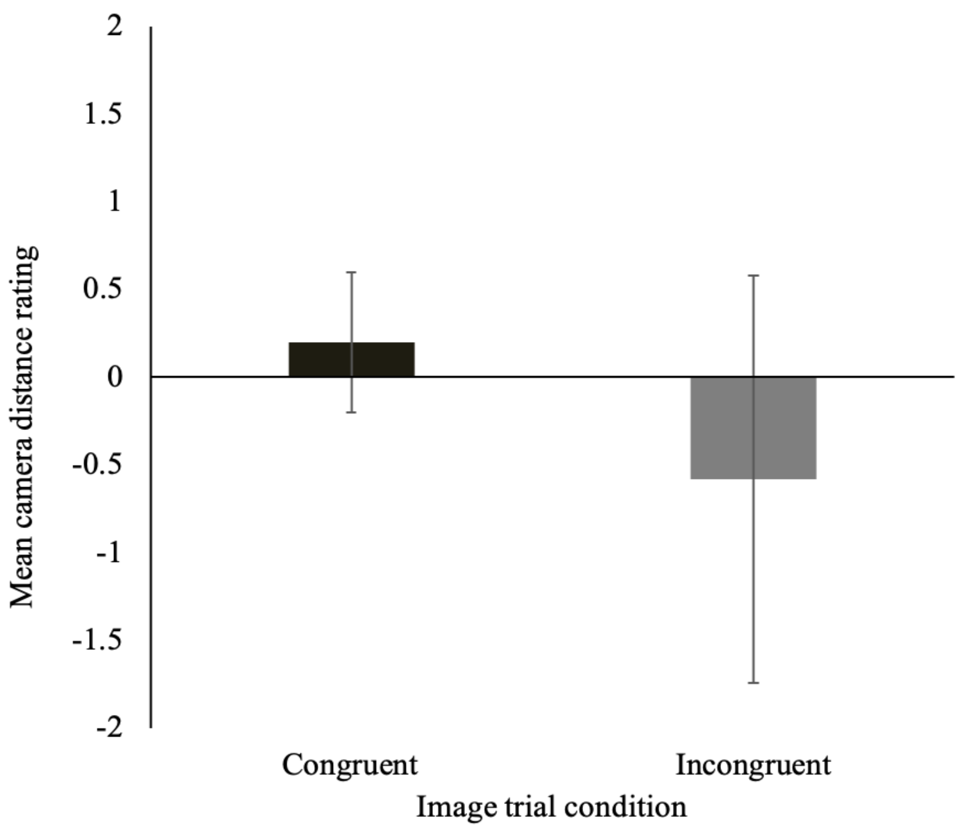

Figure 6.1. Mean camera distance ratings for congruent and incongruent trials. Error bars represent 95% confidence intervals.

We next ran a paired-samples Bayesian t-test with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) on boundary distance judgements by congruence (congruent vs. incongruent) and found $BF_{10} = 1.62$. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates only anecdotal evidence that this difference is present. In other words, participants were more likely to incorrectly judge images as having narrower boundaries when their attention was drawn *to* the image, and more likely to erroneously judge images as having more extensive boundaries when their attention was drawn *away from* the image, but there is weak evidence for this effect. On the one hand, this effect size is small, and coupled with the Bayesian interpretation, this finding should be interpreted with caution. On the other hand, it has been argued that effect sizes should be judged only in comparison to meaningful benchmarks—such as effect sizes found in comparable studies—rather than arbitrary benchmarks—such as qualifying them as "small", "medium" and "large" (Cohen, Cohen, Aiken, & West, 1999; Funder & Ozer, 2019). With this idea in mind, we can compare the current finding to our previous investigation of the effect of valence on boundary judgement errors. Recall that in Experiments 2a – 2de (Chapter 4) we found that the effect of valence (negative vs. neutral) on camera distance judgments were even smaller (d = 0.01 – 0.15), with no significant effects. Therefore, although the effect we found here—of attention on boundary judgement errors—is small, it is slightly larger than those found in previous, comparable studies and thus may be worthy of further investigation. Indeed, one way to determine whether this effect size is stable, is Bayesian inference, which involves collecting data until an effect the either proved or disproved, (Wetzels et al., 2011).

Next, we examined the proportion of errors participants made across their boundary judgements at test. We conducted a 2 (congruence: congruent, incongruent) x 2 (boundary error type: boundary restriction, boundary extension) repeated measures ANOVA on proportion of errors. There was no difference in the proportion of boundary extension vs. boundary restriction errors: i.e., no main effect of boundary error type, and no main effect of congruence, *F*s < 1. There was however, an interaction between congruence condition and boundary error type, $F(1, 159) = 7.11$, $p = .008$, $\eta_{p2} = .04$. Specifically, when attention was drawn *to* the image (i.e., congruent trials), participants were more likely to make boundary restriction errors (*M* = 0.39, *SD* = 0.23) than boundary extension errors (*M* = 0.35, SD = 0.20). But this difference was not significant, $t(159) = 1.89$, $p = .06$, $d = 0.15$, 95% CI [-0.01, 0.31], though note that the effect size is comparable to the one we observed for the camera distance measure (*d* = 0.17). When attention was drawn *away from* the image (i.e., incongruent trials), participants were equally likely to make boundary restriction errors (*M* = 0.37, *SD* = 0.21) and boundary extension errors (*M* = 0.37, *SD* = 0.22; $t(159) = 1.89$, $p = .99$, $d < .001$, 95% CI [0.15, 0.16], see Figure 6.2. The overall pattern is consistent with what we observed when we analysed the data as a distance judgement, and the effect sizes are similar. However, converting continuous data into categorical data by splitting the findings into boundary restriction versus boundary extension errors may still be problematic because it creates an artificial dichotomy that does not allow researchers to discriminate between different scores within the same group (DeCoster, Gallucci, & Iselin, 2011; MacCallum, Zhang, Preacher, & Rucker, 2002). For example, a participant who makes a minor boundary extension error (close to zero) is treated the same as a participant who is nearer the maximum boundary extension value (close to 50) on the scale. Thus, we propose that the average distance judgement is a more sensitive measure, because it takes into account small variations in distance judgement, rather than just the type of error participants made. So, although we found no difference in the proportion of errors, we argue that the camera distance findings are more meaningful.

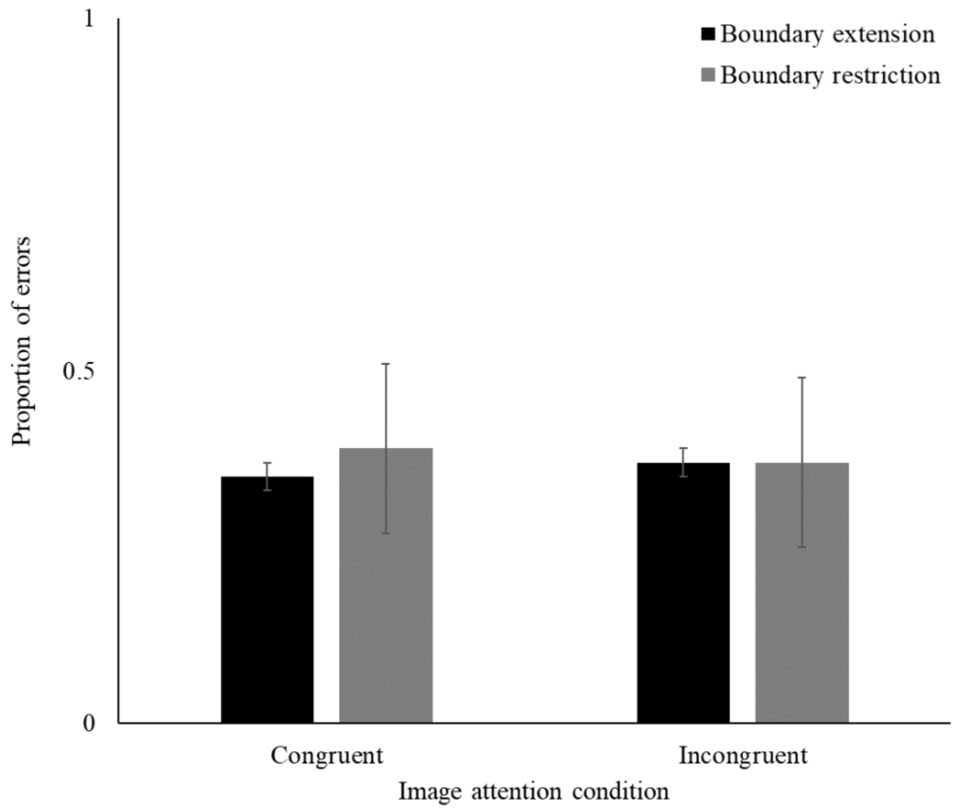

Figure 6.2. Proportion of errors for congruent and incongruent trials. Error bars represent 95% confidence intervals.

Recall that we counterbalanced the scale anchors in order to control for—and measure—the effect of lateralised attentional bias. We know that participants' own attentional bias to the left side of space can affect boundary judgement errors (See Chapter 5, Experiments 3a and 3b); thus, we next analysed the effect of laterality on boundary errors next. Half of the participants indicated boundary extension errors by moving the slider to the left and half of the participants indicated boundary restriction errors by moving the slider to the *right*. For ease of narrative, we will refer to the side on which the boundary extension error option ("closer") appeared. We ran an independent samples t-test on average camera distance with side (side: left, right) as a between-subjects factor. We found that when participants had the boundary extension option on their left, they made more expansive boundary extension errors (*M* = -1.07, *SD* = 5.81), and when participants had the boundary extension option on their right, they made more expansive boundary restriction errors (*M* = 0.67, *SD* = 5.37; *t*(158) = 1.98, *p* = .049, *d* = 0.31, 95% CI [0.001, 0.63], see Figure 6.3. In other words, participants tend to pick to the left of the scale, regardless of error type.

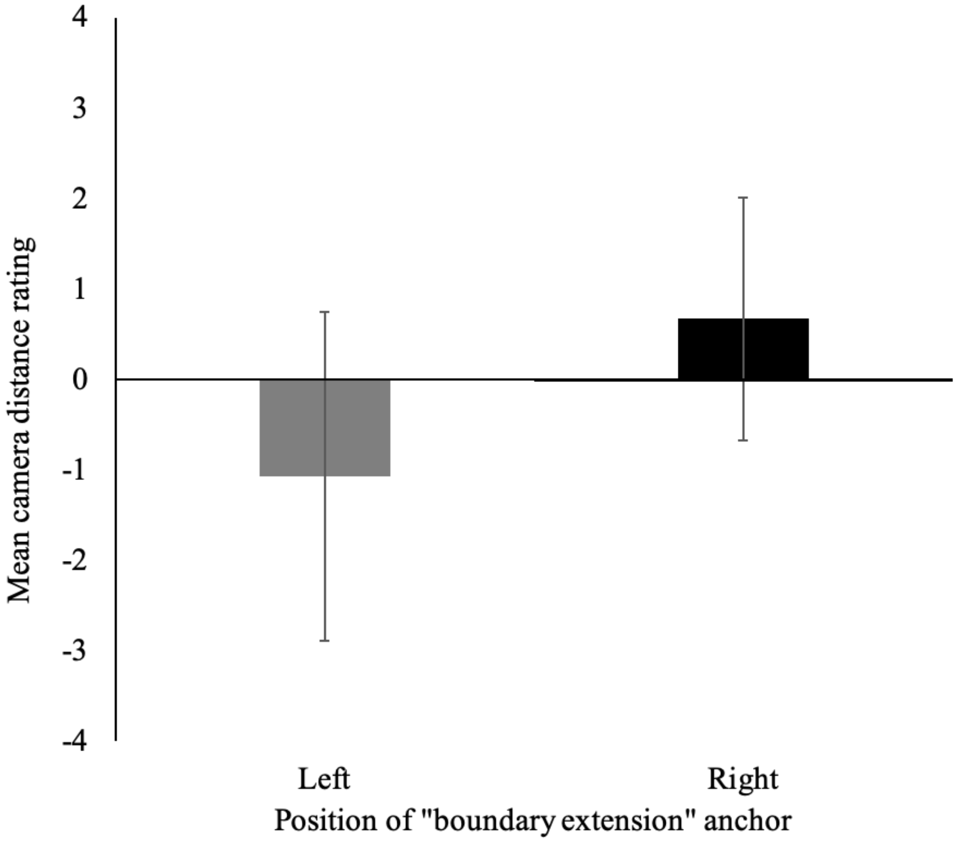

Figure 6.3. Mean camera distance rating for counterbalanced anchor labels. Error bars represent 95% confidence intervals.

In summary, drawing attention to or away from an image affected how participants remembered the boundaries of that image. Specifically, drawing attention to an image increased the expansiveness of boundary restriction, and reduced the expansiveness of boundary extension, whereas drawing attention away from an image increased the expansiveness of boundary extension and decreased the expansiveness of boundary restriction. This pattern was consistent with our predictions. However, when we quantified the distance errors that participants made as categorical rather than continuous, drawing attention to or away from an image did not significantly change the type of error participants made (though we note the consistent pattern). However, because these proportions create an artificial dichotomy, we believe our camera distance measure is more informative. Last, we found—consistent with Experiments 2a - 2e—participants tended to mark to the left of the scale, regardless of the type of errors they made.

Our findings are consistent with boundary extension studies that demonstrate that decreased attention to images increases the extent of boundary extension errors (e.g., Dickinson & Intraub, 2009; Intraub et al., 2008; also see Hubbard, Hutchison, & Courtney, 2010 for a review). We expanded on these studies by also measuring boundary restriction errors (rather than a reduction of boundary extension errors only); demonstrating that when participants' attention was focussed, boundary restriction increased, and when participants' attention was diffuse, boundary restriction decreased.

Our results have theoretical implications. Previous research has demonstrated that boundary restriction is induced by highly negative, highly arousing stimuli (e.g., Mathews & Mackintosh, 2004). One explanation for such stimuli causing boundary restriction is that they are a priority for our visual system—and therefore capture attention—compared to neutral, non-stressful stimuli (Mather, 2007). Thus, previous studies that found boundary restriction for negative stimuli may actually have been demonstrating the effect of the incidental attention focus elicited by negative images on boundary judgement errors, rather than the influence of negative valence per se. Where studies have *not* found boundary restriction in the context of highly negative or arousing stimuli (e.g., Candel et al., 2003; Davies, Smith, & Bilcoe, 2007; Intraub et al., 1992; Kensinger, Garoff-Eaton, & Schacter, 2006; Steinmetz & Kensinger, 2013) the stimuli may not have been activating the same attention-grabbing process, either because of features of the images themselves, or because of individual differences inherent to the participants themselves. Without attentional capture, imagination and extrapolation can occur unabated, and these imagined, extrapolated details may be confused with the original information from the actual stimulus—resulting in a source monitoring errors—observed as boundary extension errors. But when a stimulus successfully captures attention, there is limited opportunity for imagination and extrapolation, and thus no—or limited—internally-generated information to confuse with the original stimulus information.

Previous studies have demonstrated that boundary restriction can be induced by top-down attentional capture—where the salience of an image directs attention. More specifically, top-down attentional capture occurs when prior information (e.g., a known threat like a gun) or current goals (e.g., finding a friend's face in a crowd) direct attention. In the boundary judgment context, the salient information may be the image itself (Katsuki & Constantinidis, 2014; Mathews & Mackintosh, 2004; Safer et al., 1998). Furthermore, top-down attentional capture only occurs once a person has had a chance to assess and evaluate the stimulus (see Theeuwes, 2009 for a review). Previous boundary restriction studies have generally used top-down attentional capture, relying on the salience of negative images that participants view, to capture and hold attention. Thus, in this sort of paradigm, the attention participants pay to the salient (negative) stimulus is volitional (directed by the participants' own experiences and goals). Importantly however, salience may be determined by many internal and external factors (e.g., autobiographical history, or psychopathologies—such as phobias). By contrast, in the current study we used *bottom-up* attentional capture via an abrupt-visual onset stimulus. In this paradigm, attention is not volitional; attention was instead grabbed by a stimulus that was salient solely due to the differences between it and the background (i.e. contrast) (Katsuki & Constantinidis, 2014). This contrast driven salience captures attention consistently and regardless of the image's salience, threat or valence, and regardless of any individual participant factors. Indeed, in the present study, we demonstrated that we eliminated the effect of valence on participants' affect, as their STAI ratings remained stable from time 1 to time 2. The fact that this bottom-up attentional capture led to an increase in boundary restriction and a reduction in boundary extension in the current study supports the assertion that boundary restriction is a result of captured attention and can happen in the absence of valenced stimuli.

There are at least two methodological implications of our findings. First, this study demonstrated that boundary restriction can be induced using neutral images, eliminating the need to use negative images, and all of the complexity they bring with them. Second, our laterality finding is relevant to the way boundary restriction is studied. Often boundary restriction studies ask participants to rate their camera distance judgements on scales similar to Likert ratings. The findings here demonstrate the importance of counterbalancing the anchors of the scales, or indeed instead employing a boundary adjustment test. This finding is consistent with laterality studies which have found that participants tend to mark to the left of Likert scales more often than to the right (Nicholls, Orr, Okubo & Loftus, 2006), and consistent with our laterality findings in Chapter 4. Thus the anchors of such scales should always be counterbalanced.

It is important to acknowledge several limitations of the present study. First, although we were able to induce boundary restriction errors, these errors were very few, thus analysing the cause of such errors is difficult. Future research could focus on increasing the number of trials and the number of participants, and thus increasing *overall* errors. Note that we demonstrated that boundary restriction is a subtle effect, however, and subtle effects are difficult to detect when there are a small number of observations (e.g., Van De Schoot, Broere, Perryck, Zondervan-Zwijnenburg, & Van Loey, 2015). Indeed, a better approach to a replication may be to instead use Bayesian inference, whereby researchers collect data until an effect is either proved or disproved (Wetzels et al., 2011). Second, the present study, like other boundary restriction studies (e.g., Candel et al., 2003; Mathews & Macintosh, 2004; Safer et al., 1998) used basic stimuli, whereas in the real world, these effects would occur during complex, negative events. Thus this paradigm has may have limited generalisability to real world events with high levels of negative valence and arousal, such as being a victim or witness to an armed robbery.

To summarise, our results reveal that attention plays a role in image boundary judgements. Our study suggests that *any* attention-grabbing stimulus may result in boundary restriction errors, and may not require negative valence to induce such an effect. To investigate what would happen in a real-world situation, it is necessary to introduce each of the components that would be present in a real-life negative event. The results in the present study demonstrate that an attention-grabbing stimulus is likely to lead to boundary restriction. Thus far we have demonstrated that valence alone does not induce boundary restriction, but attention does. Could attentional capture induced by *arousal* irrespective of valence) induce boundary restriction? Indeed, past experiments that have found boundary restriction as a result of negative stimuli may have been demonstrating the effect of focussed attention on boundary judgement errors—induced by the incidental arousal that often accompanies negative stimuli. Thus, our next step is to investigate attention, by inducing arousal—in isolation of negative valence—to investigate this possible mechanism further.

7 The Role of Arousal in Boundary Judgement Errors[8]

7.1 Abstract

Eyewitnesses to a crime rely heavily on their visual memory, however, there are many ways that the details of visual scenes can be missed, or distorted. In particular, for emotional scenes, the “boundaries” are narrowed at retrieval, whereas central details – such as a weapon – are remembered in greater detail. This phenomenon is known as boundary restriction, the reverse of boundary extension whereby people tend to expand the boundaries of a neutral scene at retrieval. In the present series of experiments, we investigated whether arousal is the element of an emotional scene that leads to increased boundary restriction or reduced boundary extension. We presented neutral images to participants either with or without a stress inducing noise. In Experiment 5a and 5b, at test, participants viewed the image they originally viewed next to the same image but with narrower or wider boundaries and selected which of the two images they originally viewed. In Experiment 6, at test, participants viewed the identical image they originally viewed, but were told the boundaries had been changed. Participants selected the extent to which the images at test had restricted or extended boundaries compared to their memory of the original image. When the noise stressor was present, participants made more boundary restriction errors—selecting the image with narrower boundaries than the original—and fewer boundary extension, errors than when the noise was absent. Our data suggest that arousal plays a key role in boundary judgements.

[8] Green, D. M., Wilcock, J. A., & Takarangi, M. K. (2019). The role of arousal in boundary judgement errors. *Memory & cognition*, 1-15. doi: 10.3758/s13421-019-00914-8

7.2 Introduction

Eyewitnesses to a crime rely on visual memory—underpinned by where their attention was directed during the crime—when they recount what they saw. However, details of visual scenes can be missed, or distorted, affecting an eyewitness's memory report. In particular, for emotional scenes, sometimes central details—such as a weapon—are remembered better than peripheral details—such as other witnesses (Kramer, Buckhout, & Eugeniot, 1990). This pattern has been described as memory narrowing, tunnel vision, or weapon focus (Berntsen, 2002; Fawcett, Russell, Peace, & Christie, 2013; Pickel, 1999; Pickel, Ross, & Truelove, 2006). However, these memory phenomena all involve a rejection of peripheral details. Here, our focus is on *boundary restriction*, whereby people erroneously remember the boundaries of a scene as narrower than they actually were. One explanation for boundary restriction highlights enhanced attention or memory-processing resulting from increased arousal (Mathews & Mackintosh, 2004; Safer et al., 1998). Previous research has compared scenes rated as differing in valence and thus, presumably arousal. Of course, there are other qualities that could make an image inherently more or less memorable, such as content, context, or composition. Moreover, some studies fail to demonstrate boundary restriction, even using similar paradigms (e.g., Candel et al., 2003). Thus, in the present study we aimed to isolate arousal from the other qualities of a scene, to examine its influence on boundary restriction.

Memory for visual information is highly malleable. *Boundary extension*, the tendency to remember more expansive background of scenes, is one example of memory malleability. In an early study, Intraub, Gottesman, Willey, and Zuk (1996) showed participants an image of a teddy bear on a single step. When participants drew the scene from memory, they generally drew the teddy bear sitting on a flight of stairs. Boundary extension occurs across age and gender (see Seamon et al., 2002); despite prior warning (Intraub & Bodamer, 1993); with both real life images (Intraub et al., 1996; Intraub & Bodamer, 1993) and abstract shapes (McDunn, Siddiqui, & Brown, 2014); and when memory is assessed by drawing, forced-choice, or boundary adjustment tests (where participants adjust an image's boundaries; Chapman, Ropar, Mitchell, & Ackroyd, 2005; Daniels & Intraub, 2006; Dickinson & Intraub, 2009; Intraub, 2004; Intraub, Hoffman, Wetherhold, & Stoehs, 2006).

It was initially hypothesised that boundary extension occurs because we extrapolate beyond the periphery of a scene to contextualise scene information into the wider-world (e.g., Gottesman & Intraub 1999). However, more recent work suggests boundary extension is a source monitoring error (Intraub, 2010; Intraub, 2012). Specifically, people rely on internally generated mental representations instead of their sensory experience. Visual-sensory input is only available for a short duration, and because of constraints to the visual system—e.g., vision is pieced together from a series of quick eye movements—imagined details are then extrapolated from incomplete sensory inputs (Blackmore, Brelstaff, Nelson & Troscianko, 1995; Simons & Levin, 1998). Memory for this extrapolated information is difficult to distinguish from sensory information derived from the actual stimulus, resulting in a source monitoring error (Intraub, 2012). In Munger and Multhaup (2016), participants with higher (vs. lower) spatial abilities made more boundary extension errors; i.e., they judged images they had seen earlier as appearing more wide angled—remembering the images as having wider boundaries—even though the images were unchanged. These data suggest boundary errors could occur because of source monitoring errors; specifically because of internally generated mental representations that involve visuo-spatial elaboration.

Attention allocation also appears to affect boundary judgements. When attention is divided by a visual search task—and hence diverted away from the scene(s)—participants tend to rate test images as having *narrower* borders (a form of boundary extension error: participants remember the original images as having wider borders compared to test) compared to participants without an attentional distractor (Intraub, Daniels, Horowitz, & Wolfe, 2008). Furthermore, people tend to make less expansive boundary extension errors for the left compared to the right side of images—when maintaining central fixation on briefly presented images (Dickinson & Intraub, 2009). This finding is consistent with pseudoneglect, whereby neurotypical people pay less attention to the right (vs. left) side of their visual field (Bowers & Heilman, 1980). Together, these findings suggest decreased attention to scene content can increase boundary extension; by contrast, increased attention can attenuate boundary extension.

Interestingly, our visual system prioritises attention to highly arousing or stressful stimuli, at the expense of neutral, non-stressful stimuli (Lang, 2010; Mather, 2007). For example, we know that stimuli signalling threat—such as a weapon—can make people more attentive to the source of that threat (Gilbert, 2001; see also Fawcett et al., 2013; Kramer et al., 1990). When negative stimuli draw attention away from other non-negative information, this process may reduce or eliminate broader contextualisation of the scene, making it less likely that an internally generated mental representation will be confused with actual sensory information (Vuilleumier, 2005). Indeed, some research shows boundary extension is attenuated when the scenes are negative, such as when they depict threatening objects, physical violence, and death (Levine & Edelstein, 2009; Mathews & Mackintosh; 2004; Safer et al., 1998). Put differently, after viewing emotionally negative or traumatic images, people sometimes show boundary *restriction*, erroneously recall boundaries as *narrower* than they were—or indeed make fewer boundary extension errors (Mathews & Mackintosh, 2004; Ménétrier, Didierjean, & Vieillard, 2013; Safer et al., 1998; Takarangi, Oulton, Green, &

Strange, 2015).

In one example, participants viewed scenes of actors expressing particular emotions (Ménétrier et al., 2013). Participants then saw the same scenes again, but were told the camera distance had changed for some images. They indicated on a 5-point scale whether the image was more zoomed-in, more zoomed-out, or the same as the original image. Participants displayed more boundary extension errors—judging scenes as closer at test—for positively valenced emotions (Happiness, Pleasure), than for the negatively valenced emotions (Anger, Irritation). Thus, negative stimuli may attenuate boundary extension. However, in the absence of a neutral condition it is also possible that the positive images enhanced boundary extension. In another example, participants viewed either negative or neutral single images, or a series of slides depicting a story ending with a negative or neutral scene (Safer et al., 1998). Safer et al. used two different methods to test participants' memory for the images: a forced-choice paradigm, with the original image accompanied by three distracters (all more zoomed-in than the original, or two zoomed-in and one more zoomed-out than the original); and a camera distance judgement. On the forced-choice test, participants selected which image they originally viewed. Participants who viewed negative images made more boundary restriction errors—indicating the image was more zoomed-in than the one they originally saw—than boundary extension errors—indicating the image was more zoomed-out than the one they originally saw. Participants who viewed neutral images made more boundary extension errors than boundary restriction errors. Participants made fewer boundary extension errors for negative images than neutral images. However, they were equally likely to make boundary restriction errors for negative and neutral images.

Yet not all studies have found boundary restriction—or attenuated boundary extension—for negatively valenced stimuli across different test types. Similar to Safer et al., using the camera distance paradigm, Candel et al. (2003) found no difference in the rate of boundary extension, and no evidence for increased boundary restriction for negative vs. neutral images. In a second experiment, Candel et al. asked participants to draw the images from memory. Overwhelmingly, participants made boundary extension errors regardless of valence—drawing the images with the central object smaller and with more extensive boundaries than the original images. Using a camera distance paradigm, Beighley, Sacco, Bauer, Hayes, and Intraub (2018) also found that valence—operationalised here as positive (happiness, joy, pleasure, social connection) or negative (depression, grief, angst, social isolation) emotion expressed by an actor in each image—did not affect the rate of boundary extension errors.

Together, these studies show that evidence for boundary restriction—and attenuation of boundary extension—is inconsistent. One possibility is that *arousal*, rather than negative valence per se, affects boundary restriction, and previous results are mixed because relying on incidental arousal—e.g., to an existing stimulus, which could vary in content, context, or composition—rather than using a manipulation of arousal, means that arousal levels vary across studies. Indeed, there is converging evidence that attention narrows specifically when people are highly aroused. An early example demonstrated that a simulated state of arousal—where the effect of stress hormones was mimicked by methamphetamine administration—reduced participants' ability to encode peripheral information beyond their direct line of sight (Callaway & Dembo, 1958). Further, in a series of studies, participants viewed slide shows that ended in one of two ways: with either a neutral (e.g., woman riding a bicycle) or arousing critical slide (e.g., woman with a head injury; Christianson & Loftus, 1991; Christianson, Loftus, Hoffman, & Loftus, 1991). Participants who viewed the arousing slide recalled more central details than those who viewed the neutral slide. Additionally, Echterhoff and Wolf

(2012) manipulated arousal at encoding using a video (a stressful burglary or a non-stressful burglary). Participants in the stressful condition exhibited both a greater memory for central details, and a reduced memory for peripheral details. Together, these results suggest that arousal during encoding induces a focus of attention.

To date, there is only indirect evidence that arousal per se affects boundary errors. Mathews and Mackintosh (2004) found that high-anxious participants made more boundary restriction errors than low-anxious participants for highly negative pictures. People with high trait anxiety are more prone to higher states of arousal than those with low trait anxiety (Mathews & MacLeod, 1994). Thus, if participants with higher trait anxiety were more aroused at encoding, these data might suggest that arousal affects boundary judgments (albeit not independent of valence in this instance). Unfortunately, because the researchers did not report state arousal, we cannot confirm this supposition. Thus, no study has rigorously manipulated arousal to induce boundary judgment errors. Our studies address this important gap.

We manipulated arousal using an external stressor (noise blasts). This paradigm elicits a similar level of psychological and physiological arousal to a fear response (Rhudy & Meagher, 2001). Across three experiments, we used noise blasts to influence participants' encoding of neutral images. We used neutral images to rule out valence effects. Participants viewed cropped or uncropped versions of images either with or without a noise. In Experiments 5a and 5b, participants completed a forced-choice recognition test where they saw the image they originally viewed alongside a comparison version. If the participant saw a cropped image at encoding, the test comprised the original cropped image and a version that was zoomed-out with greater boundaries. Incorrectly selecting the zoomed-out version was a boundary extension error. Where participants saw an uncropped image at encoding, the test comprised the original uncropped image and a version that was zoomed-in with reduced boundaries. Incorrectly selecting the zoomed-in version was a boundary restriction error. In Experiment 6, we changed this test to a camera-distance paradigm. We predicted that boundary restriction errors were more likely to occur for images paired with noise than images without noise. We also predicted that boundary extension errors would be less likely to occur for images presented with noise.

7.3 Pilot Study

Stimuli preparation

We wanted to establish distinct semantic categories of images so that our arousal manipulation would be isolated to the category presented with the noise. Dunsmoor, Murty, Davachi, and Phelps (2015) demonstrated that participants could be conditioned to associate external arousal with a particular category of images: participants anticipated arousal for images that were semantically related (e.g. from the same image category of "tools") to images that were earlier presented with an arousing stimulus (a shock). We identified 203 images from the International Affective Picture System (IAPS; Lang, Bradley, & Cuthbert, 2008) and the Nencki Affective Picture System (NAPS; Marchewka, Zurawski, Jednoróg, & Grabowska, 2014) with a normed emotional valence rating (4-6 on a scale where 1 = very unpleasant, 9 = very pleasant) across five possible categories—nature, everyday-objects, people, tools, and scenes. Fifty participants from Amazon Mechanical Turk rated how well each image fit into each category e.g., "How well does this image fit into the category: nature?" (1 = a very good example, 7 = a very poor example; adapted from Rosch, 1973). We excluded images rated as highly associated with more than one category (e.g., nature *and* everyday-objects). We found that the nature and everyday-objects categories had the most images with unique category membership. We excluded images that did not contain a central object on a wider background, or that we could not crop without cropping the central object in the image. We chose 12 images per category with the best category membership rating, and cropped each image to 75% of its original size and then resized it, which resulted in two versions of each image (i.e., 75% cropped and 100% uncropped).

7.4 Experiment 5a Method

Participants. No previous boundary error research has directly manipulated arousal. Thus, using G*Power, we calculated the sample size required to detect a small-medium within-subjects effect (*d* = 0.30) of arousal on boundary judgements (planned comparisons of noise vs. no noise on boundary extension errors, and on boundary restriction errors). At alpha = .05, power = .80, the recommended sample size is 90. This sample size is consistent with previous experiments investigating boundary restriction, which have used 80-150 participants (Mathews & Mackintosh 2004; Safer et al., 1998; Takarangi et al., 2015; though we note Anderson, Kelley, & Maxwell, 2017). To enable our counterbalance—which required divisibility by four—we increased the target sample size to 96. We recruited 99 participants with normal or corrected-to-normal colour vision from an undergraduate research participation pool, but excluded three participants due to technical errors. Most (78%) were female, and identified as Caucasian (including “White”; 66.7%). Participants ranged from 18 to 46 years (*M* = 23.08, *SD* = 6.07). Participants received course credit, or payment (AUD$10) for their time.

Design. We used a 2 (noise: noise, no noise) x 2 (encoded image: cropped, uncropped) x 2 (category-noise combination: noise-nature, noise-object) mixed design where noise and encoded image were within-subjects and category-noise combination was between-subjects. Our key dependent variable was the proportion of images on which participants made errors.

Materials

Anxiety measures. We measured participants' trait and state anxiety using the using the 20-item trait and state subscales of the State-Trait Anxiety Inventory *(STAI-T, STAI-S;* Spielberger, Goruchm, & Lushene, 1970). Trait anxiety refers to those prone to anxiety, while state anxiety refers to feelings of unpleasantness, such as stress, fear, and discomfort. For STAI-T, participants read a series of statements about how they feel *generally* (e.g., I am "calm, cool and collected"), and rated how often they felt this way on a 4-point Likert scale. For STAI-S, participants read a series of statements about how they felt *at the present time*, (e.g., "I feel calm") and rated how much they agreed with each statement on a 4-point Likert scale. We measured state anxiety over time, because state anxiety is malleable (unlike trait anxiety). Overall, the STAI has good reliability and validity for both trait and state subscales ($\alpha = 0.88$ (Grös, Antony, Simms, & McCabe, 2007). We also found high internal consistency for STAI-S: $\alpha = 0.91$ at time 1, and 0.93 at time 2.

Manipulation of noise. White noise above 90 decibels (db), in short bursts, briefly increases stress, fear and surprise similar to that of emotional arousal (Rhudy & Meagher, 2001). We administered a 95-98db range of white noise with headphones for the full two seconds that each image was presented. The noise was paired with one category of images (i.e., nature/everyday-objects), which was counterbalanced so that both categories of images were presented with noise in the sample.

Procedure

The Flinders University's Social and Behavioural Research Ethics Committees approved this research. Participants completed the experiment on an Apple iMac desktop computer using Superlab. Participants were situated approximately 40cm from the screen.

Encoding Phase. After informed consent procedures, participants completed demographic questions, the STAI-T[9] and the STAI-S. Participants viewed 23[10] neutral images for two seconds each.[1] Participants viewed one version of each image (e.g., 75% cropped *or* 100% uncropped); half the images were cropped, the other half uncropped. Presentation of cropped and uncropped versions of the images was counterbalanced. The order of image presentation at encoding was randomised. In line with Mathews and Mackintosh (2004), and other boundary restriction studies (Candel et al., 2003, Safer et al., 1998, Takarangi et al., 2015), we did not inform participants that we would test their memory of the images.

Participants viewed one category of images (nature or everyday-objects) with noise and one without. Participants rated the pleasantness (1 = very unpleasant, 7 = very pleasant) and emotional arousal (1 = not at all emotionally arousing, 7 = highly emotionally arousing; from Porter, Brinke, Riley, & Baker, 2014) of each image separately. During a ten-minute delay, participants again completed the STAI-S—measuring their change in arousal after viewing the images—and an unrelated filler task.

Test Phase. Following the delay, participants saw two versions, side-by-side, of each image they originally viewed. One of these images was in the same format as encoding, the other in the other format (i.e., 75% cropped vs. 100% uncropped). This test was similar to previous boundary error studies (Kreindel & Intraub, 2017; Mathews & Mackintosh, 2004; Safer et al., 1998). Participants selected which of the two images they originally viewed. Note

[9] To examine whether noise condition would moderate the relationship between trait anxiety and boundary errors, we ran hierarchical regressions on proportion of boundary restriction errors and boundary extension errors separately. We found no significant effects, *ps* > .05.
[10] Due to a technical issue, participants only viewed 23 images at encoding.

that for each trial, participants could only either make one type of error (boundary restriction *or* extension), or be correct. When participants viewed an uncropped image at encoding (half the time), incorrectly selecting the cropped version at test was a boundary restriction error; when participants viewed a cropped image at encoding, incorrectly selecting the uncropped version at test was a boundary extension error. Participants were then debriefed.

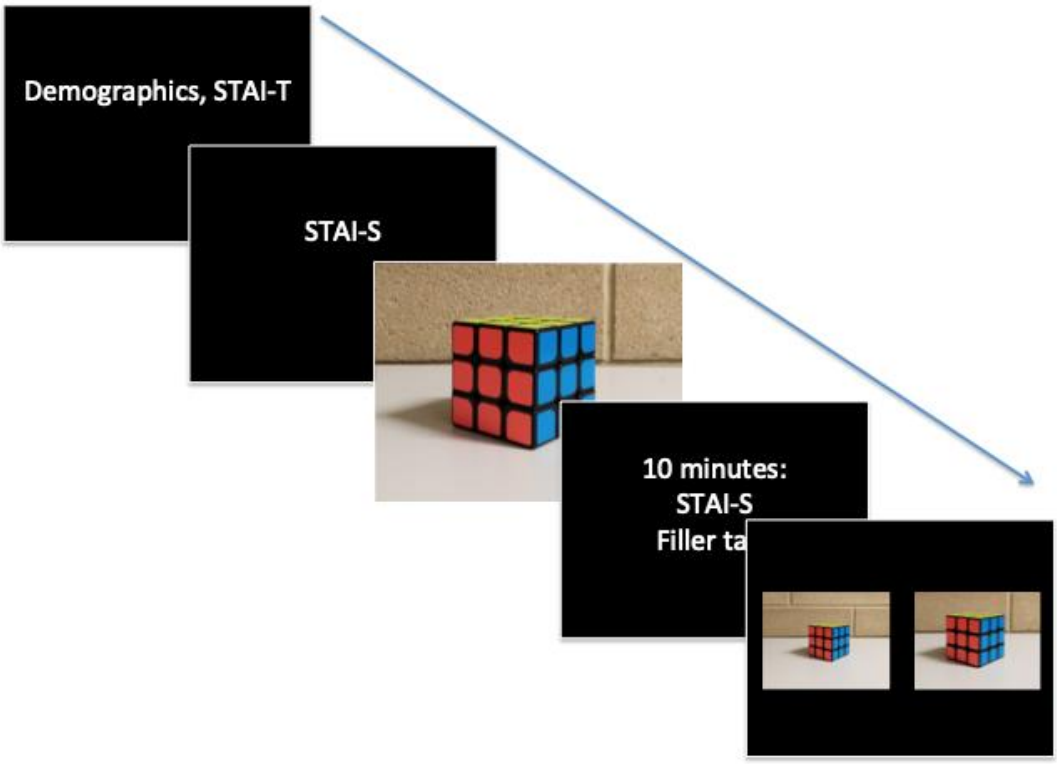

Figure 7.1 Diagram of the procedure

7.5 Experiment 5a Results and Discussion

First, we examined whether noise or image category led participants to rate images as less pleasant and/or more emotionally arousing. Because our design meant that image category and noise exposure were not crossed within-subjects—i.e., each participant only saw one category of images with noise and the other category without noise—we could not run a typical 2 x 2 within-subjects ANOVA. However, we were able to test whether participants' ratings depended on which image category was paired with noise. We ran a 2 (noise condition: noise, no noise) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on pleasantness ratings. As shown in Figure 7.2a, participants rated images as less pleasant when presented with noise, compared to without noise, a main effect for noise condition, $F(1, 94) = 23.77$, $p < .001$, $\eta_{p2} = .20$. There was no significant interaction between noise condition and category-noise combination, indicating the effect of noise on pleasantness did not depend on which image category the noise was paired with, $F(1, 94) = 3.82$, $p = .05$, $\eta_{p2} = .04$; and no main effect of category-noise combination, $F < 1$.

We next ran a 2 (noise condition: noise, no noise) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on arousal ratings. As shown in Figure 7.2b, participants rated images as similarly arousing with and without noise, $F(1, 94) = 2.44$, $p = .12$, $\eta_{p2} = .03$. There was also no main effect of category-noise combination, $F < 1$. However, there was a significant interaction between noise condition and category-noise combination, $F(1, 94) = 18.33$, $p < .001$, $\eta_{p2} = .16$. When we examined the effect of noise within category-noise combination using paired-samples t-tests, we found that nature images without noise were significantly more arousing than everyday-objects with noise, $t(47) = 4.33$, $p < .001$, $d = 0.63$, 95%CI [0.36, 0.99], whereas nature images with noise were not significantly different to everyday-objects without noise, $t(47) = 1.84$, $p = .07$, $d = 0.27$, [0.03-0.66]. This pattern arises because of an underlying difference in arousal ratings where nature images are rated as more arousing than everyday-object images, $t(95) = 4.97$, $p < .001$, $d = 0.51$ [0.34, 0.78] (see Figure 7.2b).

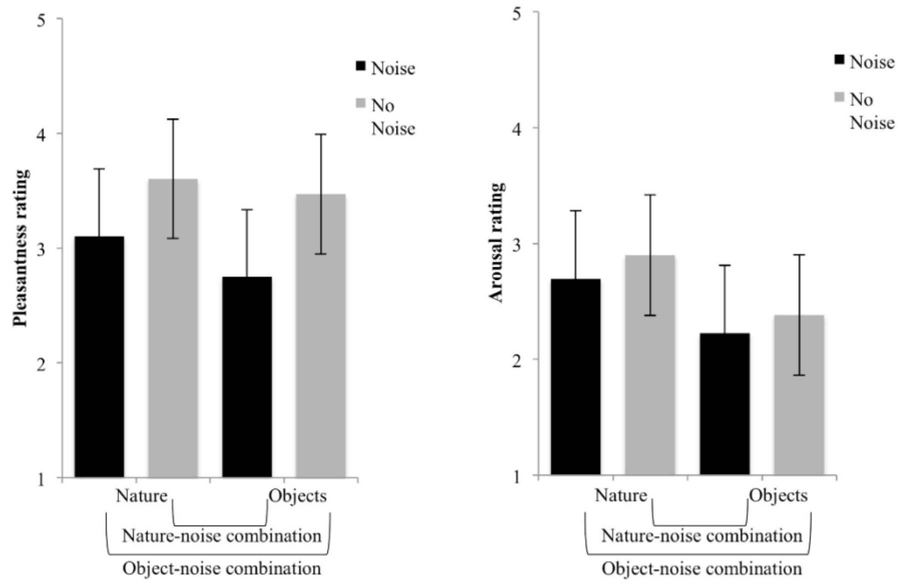

Figure 7.2a (left) and 7.2b (right). Mean pleasantness and arousal ratings for category type by noise condition and image-noise combination for Experiment 5a.

Our data suggest that participants perceived images with noise as more unpleasant but not as more emotionally arousing than images without noise, perhaps because they got used to the noise blasts over time. To test this explanation, we split the data into three time points: the first 8 images (block 1), the second 8 images (block 2) and the third 7 images (block 3) participants viewed. However, arousal ratings did not change significantly over time (time 1 M = 2.43, SD = 1.02; time 2 M = 2.61, SD = 1.08; time 3 M = 2.52, SD = 1.05, $F(2, 190)$ = 2.05, p = .13, η_{p2} = .02), suggesting that participants were *not* habituating to the noise blasts.

However, participants' state anxiety (on the STAI-S) increased from time 1 (M = 44.63, SD = 8.86), to time 2 (M = 50.81, SD = 10.48, $t(95)$ = -8.99, $p < .001$, d = 0.63) supporting the assertion that *overall* arousal increased during the encoding phase. We could assume that the change in state arousal was induced by the noise blasts, but it could be due to other factors (e.g., task demands). Without a change in the mean "emotional arousal" ratings for individual photos, we are unable to determine what drove the change in state arousal. However, recall that we did find differences in valence by noise condition, which demonstrates that the noise was changing participants' perception of the *valence* of images accompanied by noise. The presence of noise may have led to a negatively valenced general state, which persisted across the task and led people to rate the images presented with noise in line with this state (i.e., as unpleasant). Indeed, the effect of white noise on general liking relates to how much the noise itself is liked, rather than the stimulus being rated (e.g., liking of food; Woods et al., 2011). Further, we know that changes in valence persist during a task, while changes in arousal may not (Gomez, Zimmermann, Guttormsen Schar, & Danuser, 2009). It is possible then, that any increased arousal induced by the noise did not last across the task. The change in state arousal does not fit with this idea, however, though this change may have arisen because of other factors. From these data, therefore, we are unable to conclude that the noise changed participants' arousal while viewing the images. Experiment 6 addresses this limitation.

Analysis of boundary errors. Examining the proportion of participants' errors overall, we found that participants were inaccurate—essentially at chance—in identifying the correct image[11] (*M* = 0.46, *SD* = 0.15, Range = 0.13 – 1). Next, we addressed our principal research question: Does arousal increase boundary restriction and attenuate boundary extension? We conducted a 2 (noise: noise, no noise) x 2 (encoded image: cropped, uncropped) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on proportion of errors. There was no difference in the proportion of boundary extension vs.

[11] We asked participants to rate their certainty of their answers and removed responses they reported as guesses. Removing guesses reduced the error rate: (*M* = .38, *SD* = .15). But an ANOVA with guesses removed showed the same pattern of results.

boundary restriction errors: i.e., no main effect of encoded image, $F(1, 94) = 2.64$, $p = .11$, $\eta_{p}^{2} = .03$ (see Figure 7.3).

Recall we predicted that when images were accompanied by noise, participants who saw an uncropped image at encoding would be *more* likely to make boundary restriction errors and participants who saw a cropped image at encoding would be *less* likely to make boundary extension errors. Our analysis supported this prediction (see Figure 7.3): there was a significant interaction between noise and encoded image, $F(1, 94) = 33.52$, $p < .001$, $\eta_{p}^{2} = .26$. Planned comparisons showed participants made more boundary restriction errors when cropped images were paired with noise, compared to when they appeared without noise, $t(95) = 4.27$, $p < .001$, $d = 0.26$, 95% CI [0.14, 0.39]. Participants made fewer boundary extension errors when cropped images were paired with noise compared to without noise, $t(95) = 4.28$, $p < .001$, $d = 0.53$ [0.28, 0.80]. We ran paired-samples Bayesian t-tests with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) on these variables and found $BF_{10} = 383.30$ for boundary restriction errors, and $BF_{10} = 377.50$ for boundary extension errors. According to the statistical interpretation that Wetzels et al. (2011) suggest, these Bayes factors indicate decisive evidence that these differences are present. In other words, participants were more likely to incorrectly recognise uncropped images as having *narrower* boundaries when images were presented with noise, and more likely to recognise cropped images as having more *extensive* boundaries when images were presented without noise.

We next examined whether these effects depended on category-noise combination. The three-way interaction between category-noise combination and our two primary IVs—encoded image and noise, was not significant, $F(1, 94) = 2.74$, $p = .10$, $\eta_{p}^{2} = .03$, meaning that the critical interaction between encoded image and noise did not depend on which category the noise was paired with. There were no other significant effects involving category-noise combination, *Fs* < 1.

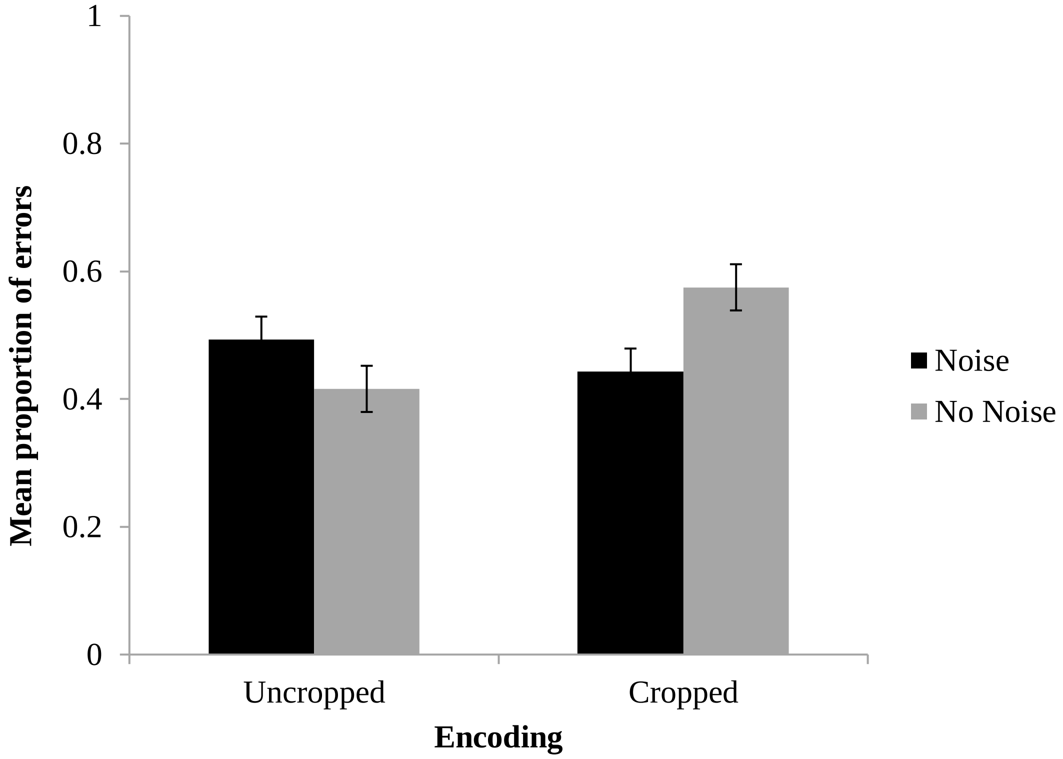

Figure 7.3. Mean proportion of boundary errors by encoded image and noise collapsed across noise-category combination. Error bars represent 95% confidence intervals.

In summary, seeing an image with noise affected how participants remembered the boundaries of that image. One explanation for this pattern is that noise increased arousal, which led to a narrowing of attention to the central details of the image. However, this interpretation depends on how arousal is quantified. Because noise did not increase how participants rated the emotional arousal of the images, we cannot conclude that the underlying mechanism is arousal. We did find that noise increased ratings of image *unpleasantness*. Thus, it is possible that unpleasantness also leads to a narrowing of attention. Indeed, previous research has found that negative images, compared to neutral images, increase focus on central details vs peripheral details (e.g., Berntsen, 2002; Fawcett et al., 2013; Mathews & Mackintosh, 2004; Safer et al., 1998; Takarangi et al., 2015). It is also possible that the underlying mechanism *is* arousal, but our measure did not capture changes in participants' experienced arousal.

Another limitation of this experiment was our use of images from existing databases (IAPS and NAPS). We used these databases because of their available arousal and valence norms. However, there were few images with a central object and expansive background, as required for the present study. Research has found that boundary extension only occurs for images where people could potentially extrapolate beyond the image boundaries (McDunn et al., 2014). Additionally, in selecting the images that best fit our image composition and category match criteria—our nature images (*M* = 5.40, *SD* = 0.40, range 4.70-5.96) were more pleasant than everyday-object images (*M* = 4.90, *SD* = 0.30, range 4.43-5.42) and more arousing (*M* = 4.18, *SD* = 0.95, range 2.82-5.90) than the everyday-object images (*M* = 2.66, *SD* = 0.52, range 1.76-3.44; based on the normed IAPS data on pleasantness and arousal). To address these issues, we sourced new images that gave us the ability to crop the background without any extra details in the periphery, and match across category on valence and arousal.

7.6 Experiment 5b Method

Participants. We recruited 102 participants from the Flinders University research participation pool and from the public. Our analyses focused on 96 participants; we excluded participants who did not follow instructions (*n* = 2) or experienced technical problems (e.g., sound inadvertently turned off; *n* = 4). The majority of participants (76%) were female, and Caucasian (including "White" 69.8%). The participants ranged in age from 18 to 53 years (*M* = 23.13, *SD* = 6.89). Participants received either course credit, or payment (AUD$10) for their time.

Design. We used the same design as Experiment 5a.

Materials. We used the same anxiety measures[12] and noise manipulation as Experiment 5a, but our stimuli were a combination of images from the IAPS, NAPS, and images taken specifically for our study. We selected or edited (using Photoshop) 128 images—in various categories—to have one central object, on an expansive background. We asked 180 pilot participants to rate our new images on valence (1 = very pleasant to 5 = very unpleasant) and arousal (1 = relaxed/calm, 5 = aroused/excited). We asked a second sample to categorise the images into distinct categories and rate their category membership. We determined that 48 images fit into the same two distinct categories as we used previously: nature and everyday-objects. We selected 12 nature images and 12 everyday-object images based on the best category-fit ratings, and neutral valence and arousal range. Our new image sets were matched on valence (nature: $M = 2.81$, $SD = 0.41$, range 2.00-3.30; objects: $M = 2.91$, SD = 0.11, range 2.67-3.04) and arousal (nature: $M = 3.23$, $SD = 0.61$, range 1.62-4.25; objects: $M = 3.24$, $SD = 0.13$, range 3.07-3.60).

Procedure

We used the same procedure as Experiment 5a.

7.7 Experiment 5b Results and Discussion

We again ran a 2 (noise condition: noise, no noise) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on pleasantness ratings. Like Experiment 5a, participants rated images as less pleasant when presented with noise, compared to without

[12] We again examined whether noise condition would moderate the relationship between trait anxiety and boundary errors. We ran hierarchical regressions on the proportion of boundary restriction errors and boundary extension errors separately. We found no significant effects, $ps > .05$.

noise, a main effect for noise condition, $F(1, 94) = 62.62$, $p < .001$, $\eta_{p}^{2} = .40$ (see Figure 7.4a). There was no main effect of category-noise combination, $F < 1$. However, contrary to Experiment 5a, there was a significant interaction between noise condition and category-noise combination, indicating the that effect of noise on pleasantness *did* depend on which image category the noise was paired with, $F(1, 94) = 29.14$, $p < .001$, $\eta_{p}^{2} = .24$. When we examined the effect of noise within category-noise combination, we found that nature images without noise were significantly more pleasant than everyday-objects with noise, $t(47) = 12.33$, $p < .001$, $d = 1.78$, 95%CI [1.58, 2.19], whereas nature images with noise and everyday-object images without noise were not significantly different, $t(47) = 1.49$, $p = .14$, $d = 0.22$ [0.12, 0.84]. As shown in Figure 7.4a, this pattern arises because of an underlying difference in pleasantness ratings where nature images were rated in the experiment as more pleasant than everyday-object images, $t(95) = 4.20$, $p < .001$, $d = 0.43$ [0.40, 1.13].

We again ran a 2 (noise condition: noise, no noise) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on arousal ratings. Consistent with Experiment 5a, participants rated images on emotional arousal similarly when presented with noise compared to without noise, $F < 1$, with no main effect of category-noise combination, $F(1, 94) = 1.60$, $p < .21$, $\eta_{p}^{2} = .02$. There was a significant interaction between noise condition and category-noise combination, $F(1, 94) = 51.38$, $p < .001$, $\eta_{p}^{2} = .35$. Specifically, nature images with noise were rated as higher in arousal than object images without noise, $t(47) = 5.18$, $p < .001$, $d = 0.75$, 95% CI [0.63, 1.43], whereas object images with noise were rated as lower in arousal than nature images without noise, $t(47) = 4.96$, $p < .001$, $d = 0.72$, 95% CI [0.61, 1.45]. As shown in Figure 7.4b, this pattern is similar to Experiment 5a: again, participants rated nature images as more arousing than everyday-object images, $t(95) = 7.21$, $p < .001$, $d = 0.74$ [0.75, 1.31]. This difference was somewhat surprising here, given that we matched this new set of images on arousal ratings.

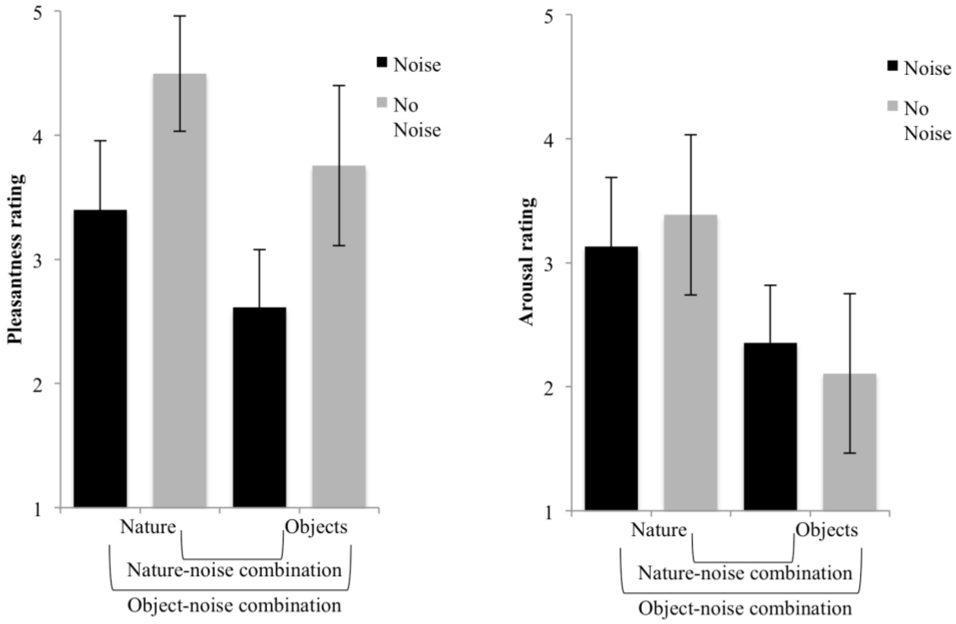

Figure 7.4a (left) and 7.4b (right). Mean pleasantness and arousal ratings for category type by noise condition and image-noise combination for Experiment 5b.

Again, participants' state anxiety (on the STAI-S) increased from time 1 (*M* = 35.95, *SD* = 9.52), to time 2 (*M* = 41.91, *SD* = 7.78, *t*(95) = -4.79, *p* < .001, *d* = 0.49, supporting the assertion that *overall* arousal was increased during the encoding phase.

Analysis of boundary errors: Examining the proportion of errors overall, we found that participants were often inaccurate in identifying the correct image[13] (*M* = .45, *SD* = .09, Range = 0.17 – 0.67). We again conducted a 2 (noise: noise, no noise) x 2 (encoded image: cropped, uncropped) x 2 (category-noise combination: noise-nature, noise-object) mixed

[13] We asked participants to rate their certainty of their answers and removed responses they reported as guesses. As with Experiment 5a, removing guesses reduced the error rate: (*M* = .39, *SD* = .12). But an ANOVA with guesses removed showed the same pattern of results.

ANOVA on proportion of errors. Contrary to Experiment 5a, participants made significantly more boundary extension errors than boundary restriction errors, $F(1, 94) = 439.55$, $p < .001$, $\eta_p^2 = .82$, which is consistent with previous research on boundary restriction (Mathews & Mackintosh, 2004; Safer et al., 1998; Candel et al., 2003; Takarangi et al., 2015).

Figure 7.5 shows boundary errors split by noise and encoded image. We expected that when images were accompanied by noise, participants who saw an uncropped image at encoding would be *more* likely to make an error at test (boundary restriction) and participants who saw a cropped image at encoding would be *less* likely to make an error at test (boundary extension). Indeed, like Experiment 5a, there was a significant interaction between noise x encoded image, $F(1, 94) = 5.10$, $p = .03$ $\eta_p^2 = .051$. As shown in Figure 7.5, and consistent with Experiment 5a, planned comparisons showed that participants made significantly more errors for cropped images (boundary restriction errors) presented with noise compared to without noise, $t(95) = 2.39$, $p = .02$, $d = 0.24$, $BF_{10} = 1.07$; contrary to Experiment 5a however, participants made a similar number of errors for uncropped images (boundary extension errors) for images that were presented with and without noise, $t(95) = 1.04$, $p = .30$, $d = 0.10$, $BF_{10} = .19$.

There was no main effect of category-noise combination, $F < 1$. There was also no three-way interaction between category-noise combination and encoded image and noise, $F(1, 94) = 2.96$, $p = .09$, $\eta_p^2 = .03$. All two-way interactions with category-noise combination were non-significant ($Fs < 1$).

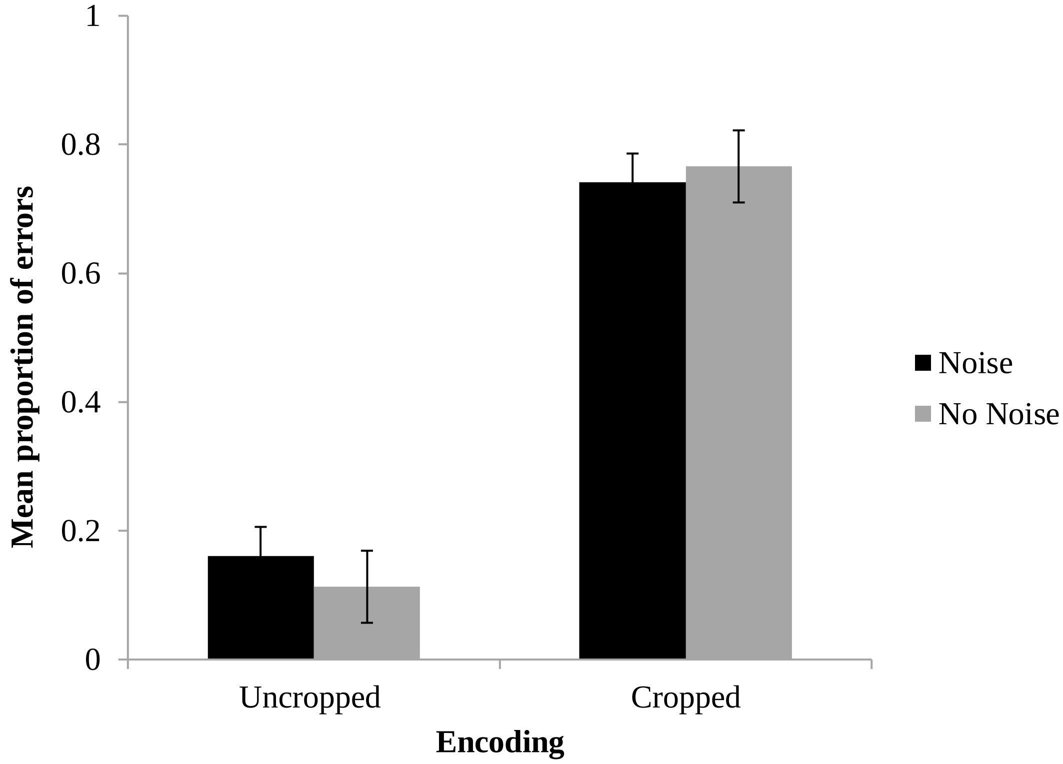

Figure 7.5. Mean proportion of boundary errors by crop type and noise collapsed across category. Error bars represent 95% confidence intervals.

Consistent with Experiment 5a, our data show that images presented with noise resulted in more boundary restriction errors, and fewer boundary extension errors compared to those without the noise, but here the difference in boundary extension errors was not significant.

As with Experiment 5a, our arousal question asked participants to rate how emotionally arousing the *image* was. Thus, participants may not have been introspecting about their own state of arousal, but instead judging the neutral image to be un-arousing. Additionally, it should be noted that, like other studies (e.g., Candel et al., 2003; Safer et al., 1998), our participants could only make either a correct response, or a boundary restriction or extension error, depending on whether they viewed the cropped or uncropped image at encoding.

Therefore, the type of error they could make was confounded with the view at encoding. This procedure assumed participants' memory matched one of the two views presented at test, which may not necessarily be true. Further, although we found that noise decreased the number of boundary extension errors and noise increased the number of boundary restriction errors, these were only in comparison to "correct" responses. Experiments that investigate boundary extension often use a camera distance paradigm, ostensibly to avoid confounds such as the one from our two experiments (e.g., Candel et al., 2003; Daniels & Intraub, 2006; Intraub, 2004; Intraub et al., 2006; Takarangi et al., 2015). With a camera distance paradigm, participants can make both types of errors for each image. To address these issues, in Experiment 6 we changed the question at encoding to reflect how participants *felt* while viewing the images, and changed the test to a camera distance judgement.

7.8 Experiment 6 Method

Participants. We recruited 96 participants from the Flinders University research participation pool and from the public. The majority of participants (65.6%) were female, and Caucasian (including "White" 69.8%). The participants ranged in age from 18 to 63 (M = 22.27, SD = 6.91). Upon completion of the study, participants were reimbursed (AUD$10) for their time.

Design. We used the same design as Experiments 5a and 5b with a change in the wording of the arousal measure, and a distance judgement at test.

Materials. We used the same materials as Experiment 5b.[14]

[14] We again examined whether noise condition would moderate the relationship between trait anxiety and boundary errors. We ran hierarchical regressions on the proportion of boundary restriction errors and boundary extension errors separately. We found no significant effects, $ps > .05$

Procedure

We pre-registered this study on the Open Science Framework (https://osf.io/cd6wt/). Our procedure was identical to Experiment 5a and 5b, with two exceptions. During encoding, we asked participants how they *felt* while viewing the images (arousal; How did you feel while viewing that image? 1 = not at all emotionally aroused, 7 = highly emotionally aroused; Betella & Verschure, 2016), and at test we introduced a camera-distance paradigm. Participants viewed the same images at encoding and at test, and were told that some of the images had been altered (Takarangi et al., 2015). Participants then rated the images as: -2 much closer than the original (the participant remembered the original image as having wider boundaries: boundary extension error), -1 slightly closer than the original (boundary extension error), 0 no change (no error), 1 slightly farther than the original (boundary restriction error), or 2 much farther than the original (the participant remembered the original image as having more restricted boundaries: boundary restriction error).

7.9 Experiment 6 Results and Discussion

Again[15], we examined whether exposure to noise or a particular category-noise combination led participants to rate images as less pleasant, and feel more emotionally aroused while viewing images with noise. We ran a 2 (noise condition: noise, no noise) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on pleasantness ratings. Like Experiments 5a and 5b, participants rated images as less pleasant when presented with noise, compared to without noise, a main effect for noise condition, $F(1, 94) = 48.52$, $p < .001$, $\eta_p^2 = .34$ (see Figure 7.6a). Consistent with Experiment 5a and contrary to

[15] Note that we did not pre-register analyses that were not testing our main hypothesis directly, but these analyses are in line with Experiments 5a and 5b.

Experiment 5b, there was no significant interaction between noise condition and category-noise combination, indicating that the effect of noise on pleasantness *did not* depend on which image category the noise was paired with. There was no main effect of category-noise combination, $F < 1$.

We next ran a 2 (noise condition: noise, no noise) x 2 (category-noise combination: noise-nature, noise-object) mixed ANOVA on arousal ratings. Contrary to Experiment 5a and 5b, participants reported feeling higher emotional arousal when viewing the images with noise, compared to without noise, $F(1, 94) = 136.76$, $p < .001$, $\eta_p^2 = .59$ (see Figure 7.6b). There was also no significant interaction between noise condition and category-noise combination, indicating that the effect of noise on reported arousal did not depend on category-noise combination, and no main effect of category-noise combination, Fs< 1.

Importantly then, when participants rated their emotional arousal after each image—rather than arousal specific to the images themselves—we found what we had originally predicted: that participants experienced more emotional arousal when viewing images with noise than images without noise.

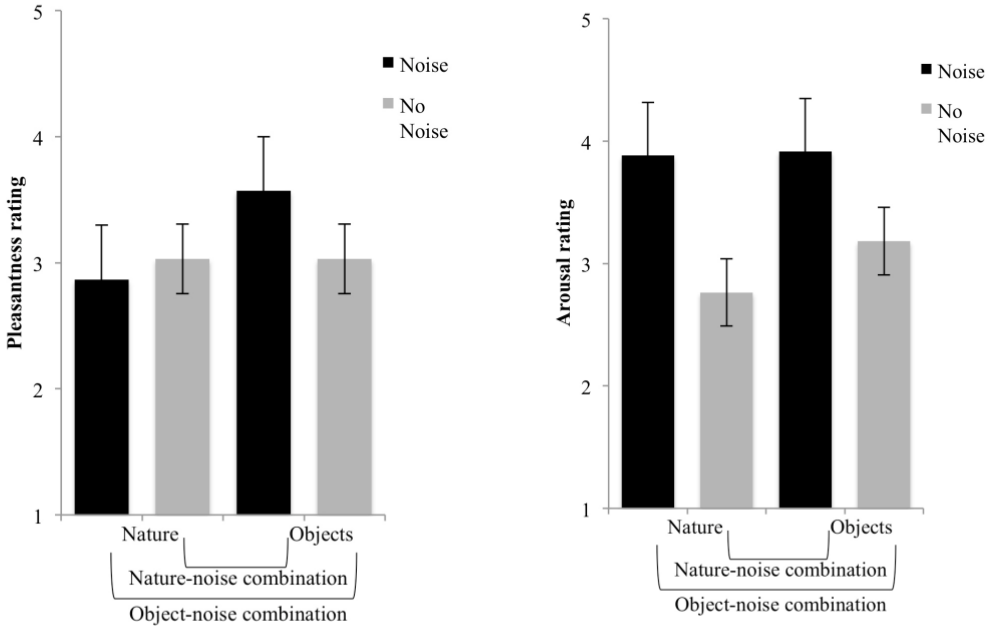

Figure 7.6a (left) and 7.6b (right). Mean pleasantness and arousal ratings for category type by image-noise combination for Experiment 6. Error bars represent 95% confidence intervals.

Additionally, consistent with Experiments 5a and 5b, participants' state anxiety (on the STAI-S) increased from time 1 (*M* = 37.29, *SD* = 9.32), to time 2 (*M* = 42.26, *SD* = 11.02, $t(95) = -5.90$, $p < .001$, $d = 0.49$, showing that *overall* arousal was again increased during the encoding phase.

Analysis of boundary errors. We first examined how often—on average—participants judged the images as much closer (3.95%), slightly closer (29.30%), no change (58.94%), slightly farther (5.95%) and much farther (1.87%). Participants were correct more than half the time in recognising no change to the images[16]. We then calculated the mean reported camera distance for each participant on the −2 to +2 scale across *all* images (M = −0.09 SD = 0.18), and conducted an exploratory one-samples t-test on this variable (Beighley et al., 2018; Intraub et al., 1996; Takarangi et al., 2015). We found that the mean camera distance was significantly less than zero (where 0 indicates no error), $t(95) = -5.00$, $p < .001$, $d = 0.51$, 95% CI [0.06, 0.13]. Therefore, when participants made errors, those errors tended to be boundary extension.

Next, we classified average camera distance judgements by noise condition and category to address our principal research question: Does arousal increase boundary restriction and attenuate boundary extension? Figure 7.7 displays these data. According to our pre-registered analysis plan, we ran a 2 (noise: noise, no noise) x 2 (category-noise combination: noise-nature images, noise-everyday-object images) mixed ANOVA on mean camera distance judgments, which revealed that when images appeared with noise they were less likely to be judged as closer than the original compared to when images appeared without noise, a significant main effect of noise, $F(1, 94) = 13.58$, $p < .001$, $\eta_{p}^{2} = .13$, BF_{10} = 0.72. There was no main effect of category-noise combination, nor a significant interaction between noise and category-noise combination, Fs < 1.

16 We asked participants to rate their certainty of their answers and removed responses they reported as guesses. As with the earlier experiments, removing guesses reduced the error rate: (M = .35, SD = .21). But an ANOVA with guesses removed showed the same pattern of results.

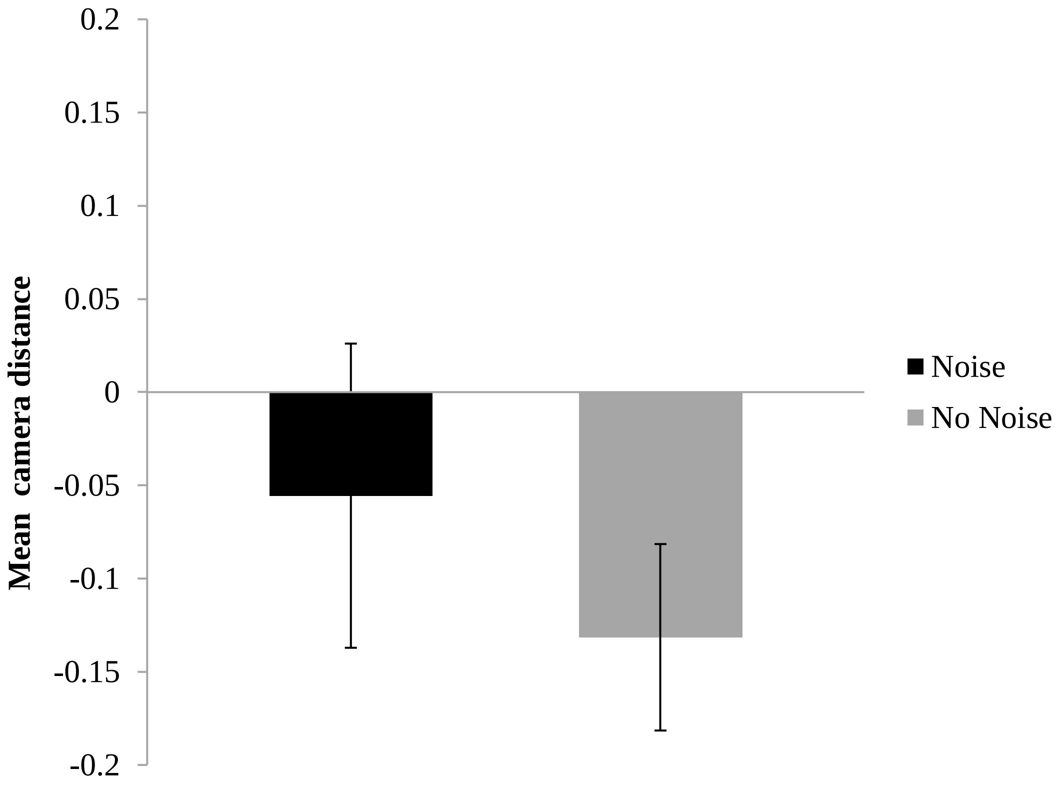

Figure 7.7. Mean camera distance by noise condition.

Overall, these data suggest that noise attenuated the extent of boundary extension. To examine the absolute number of errors that participants made, and—in line with Takarangi et al. (2015) and Beighley et al. (2018)—whether noise affected the rare occasions where participants made boundary restriction errors, we determined the error rate for boundary restriction and boundary extension. We calculated proportions by dividing the number of test images on which participants made a particular type of error (i.e., BE = distance judgements < 0; BR = distance judgements > 0) by the total number of test images. We then ran the pre-registered 2 (noise: noise, no noise) x 2 (noise-category combination: noise with nature, noise with objects) x 2 (error type: BE, BR) mixed ANOVA. This analysis showed that participants were more likely to make boundary extension errors (incorrectly judging the images as closer at test; 17% of trials) than boundary restriction errors (incorrectly judging the images as farther at test; 4% of trials). This effect of error type was the only significant effect in the model, $F(1, 94) = 72.24$, $p < .001$, $\eta_p^2 = .44$; there was no main effect of noise condition, $F(1, 94) = 1.43$, $p = .24$, $\eta_p^2 = .02$; and no significant interaction between noise and category-noise combination, $F(1, 94) = 1.64$, $p = .20$, $\eta_p^2 = .02$; *F*s < 1 for all other effects. Thus, the presence of noise did not affect the frequency of boundary extension or boundary restriction errors, when operationalised as the proportion of total test items. Indeed, boundary restriction errors in particular were very low. This result is inconsistent with the pattern of data revealed by the mean distance judgements. However, we propose that the mean distance judgement is a more sensitive measure, because it takes into account the rate at which participants make *both* types of errors, and also the extent of those errors (e.g., “slightly closer” vs. “much closer”).

In short, the data from Experiment 6 suggest that noise attenuates boundary extension, based on participants’ average distance judgments. However, noise does not affect the absolute number of distance errors—whether restriction or extension—that participants make. Based on participants reporting higher arousal in response to images presented with noise, we interpret these data as noise eliciting high state arousal.

7.10 General Discussion

To summarise, over three experiments we found that images presented with noise were less pleasant compared to images presented without noise. Further, exposure to noise increased state anxiety in all three experiments; in Experiment 6 participants’ emotional arousal was higher when viewing images with noise compared to without noise. In Experiment 5a, images presented with noise led to more boundary restriction errors and fewer boundary extension errors (compared to images without noise). In Experiment 5b, noise led to more boundary restriction errors, and in Experiment 6, we found that noise attenuated boundary extension. Taken together, our data suggest that noise can lead to both a reduction in boundary extension, and an increase in boundary restriction. Our findings are consistent with the idea that heightened arousal, which may be accompanied by lowered pleasantness, increases a person's perception of threat (Cole, Balcetis, & Dunning, 2013). Increased focus on threat may result in selective attention to central objects in a scene, in order to locate potential danger. Indeed, previous research shows that threat is associated with an immediate fear response, which focuses attention towards threatening objects (Koster, Crombez, Van Damme, Verschuere, & De Houwer, 2004; Schmidt, Belopolsky, & Theeuwes, 2015; Van Damme, Crombez, & Notebaert, 2008), and produces a feeling of closer proximity to the object (Cole et al., 2013). Here, we found—consistent with Levine and Edelstein, 2009; Mathews and Mackintosh; 2004; and Safer et al., 1998—that arousal may also lead to an increased tendency to remember an image with narrower boundaries. An increased focus on threat (or perceived threat) may also reduce people's opportunity—and available resources—to extrapolate beyond the boundaries of an image. This process, in turn, reduces the likelihood of internally generating additional mental representations, and thus of making a source monitoring error that results in boundary extension.

Importantly, although we aimed to isolate arousal from the other qualities of a scene, our noise manipulation also affected how pleasant participants found the neutral images, in addition to affecting the images' emotional arousal. Thus, the proposed mechanism of increased focus towards central, threatening aspects of a scene could arise from negative valence, increased arousal, or a combination of the two.

Our findings have methodological implications. Previous studies may have failed to find boundary restriction because of how arousal was manipulated, e.g., using negative images (see Mathews & Macintosh, 2004). We instead used an external manipulation of arousal along with neutral images, rather than relying on negative images to elicit arousal. This procedure meant other aspects of the image were consistent (e.g., content, context, or composition), and is thus a more methodologically sound way of manipulating the arousal of an image (Porter et al., 2014; also see Beighley et al., 2018). Indeed, noise consistently elicits an arousal response similar to fear across individuals (Rhudy & Meager, 2001). By contrast, how *unpleasant* a negative image is perceived to be may vary considerably across individuals; for example according to their age (Charles, Reynolds, & Gatz, 2001), gender (Davis et al., 2012; Collignon, et al., 2011; Felmingham, Tran, Fong, & Bryant, 2012; McRae, Ochsner, Mauss, Gabrieli & Gross, 2008), cultural background (Davis et al., 2012); anxiety diagnosis (Cook, Melamed, Cuthbert, McNeil, & Lang, 1988), or personality (Lang, Greenwald, Bradley & Hamm, 1993). Further, because eyewitness testimony, for example, is dependent on visual memory accuracy and is often tied to highly arousing events, we can see potential applications for our findings in that context.

Our study has limitations. First, although we did not control the number of times the noise was presented in a row, we randomised the presentation for every participant to minimise habituation to the noise. Second, the majority of our participants were female. Previous research has shown that arousal can improve the recollection of negative (vs neutral) images among females vs. males (Felmingham et al., 2012). Thus, the difference in reaction to emotional arousal could lead to different patterns of boundary judgement errors. However, we were reluctant to investigate sex differences with our data, due to the low number of males. Further research could address this issue. Third, it may be useful to measure induced arousal objectively, such as with a Galvanic Skin Response measure (Rhudy

& Meagher, 2001), to confirm a temporal rise in emotional arousal without relying on self-report. Fourth, our manipulation only investigated the effect of *negative* arousal on boundary judgement errors. Future research should investigate whether these effects can be distinguished from positive arousal. Last, it is possible that the noise blast—in addition to being an arousal manipulation—also served as a distractor. Thus, the noise blasts may have somewhat counteracted the "attentional focus" that was meant to occur as a result of arousal. Future research could investigate this possibility by replicating these studies using an alternative arousal stimulus.

Finally, we intentionally confounded noise and image category within version to produce a conditioning response (see Dunsmoor et al., 2015), and in some cases, there were differences between the noise-category combinations. Further, we found that participants rated the nature images as more pleasant in Experiment 5b and more arousing in Experiments 5a and 5b. Indeed, the potential composition differences between the image categories could have implications for boundary judgements. For example, some researchers have posited that boundary extension errors are the result of participants imagining and extrapolating beyond the periphery (see Hubbard, Hutchison, & Courtney, 2010 for a review). Participants may be more likely to contextualise something that exists in an unseen wider world (on a table, in a room—as in our object images) —which would potentially lead to more boundary extension. Participants may be less likely to contextualise something that has more "wide world" information (such as a house, in a field, as in our nature images) —which would potentially lead to less boundary extension. However, in a recent experiment, Munger and Multhaup (2016) did not find any differences in image type (i.e., various outdoor scenes, and various object images) and the number and extent of boundary errors that were made. Yet, Munger and Multhaup found that imagining isolated objects *improves* memory, reducing source memory errors. Therefore, it is possible that participants treat object images (where an object is available to be visually represented), and nature images (where there is no specific “object” to have a visual representation of) differently. To examine the effect of arousal arising specifically from composition differences between image categories, future research could compare nature images with object images in the absence of an arousing stimulus. Furthermore, to investigate whether conditioning arousal to one category of images is necessary, future research could instead use only *one* category of neutral stimuli.

To summarise, our results reveal that images presented with noise led to more boundary restriction errors and fewer boundary extension errors than images presented without noise. Although our data fit well within the literature on boundary judgements, our results also indicate that arousal may be an important mechanism in the boundary restriction phenomenon—one that has not been examined in isolation until now.

8 A Methodological Investigation of “The Role of Arousal in Boundary Judgement Errors”

8.1 Abstract

In the previous chapter (Experiments 5a, 5b, and 6; Green et al., 2019), we made two methodological decisions that warrant further investigation. First, we confounded noise and image category—creating two versions of the encoding phase—to produce a conditioned response where participants associated the noise stressor with one category of images and not the other. Second, we isolated arousal from valence—by using neutral images—to test arousal’s influence on boundary judgement errors. To investigate whether conditioning noise to image category was necessary, Experiment 7a used one category of images, with the noise stressor on half of the images. Participants undertook the same procedure as in Experiment 5a, but with 24 single category images (rather than two categories of images), with or without a stress-inducing noise. At test, participants selected the image they had seen during encoding. To examine the effect of arousal arising specifically from composition differences between image categories (rather than the addition of the noise stressor), Experiment 7b compared the same image categories used in Experiment 5a in the absence of the noise stressor. Participants undertook the same procedure as in Experiment 5a. In Experiment 7a, participants made boundary restriction errors and boundary extension errors regardless of noise condition, suggesting that conditioning the noise stimulus to an image category was necessary. In Experiment 7b, participants made boundary restriction errors and boundary extension errors regardless of image category, and overall participants made fewer errors in the absence of the noise stressor, when compared to Experiments 5a, 5b, 6, and 7a. We conclude that in order to induce boundary restriction, the arousal stimulus may need to be aversive (e.g., noise stressor) rather than positive (e.g., nature images), and need to be linked

to a particular image category.

8.2 Introduction

The results from Chapter 7 (Experiments 5a, 5b, and 6) showed that presenting images accompanied by a noise stressor led to more boundary restriction errors and fewer boundary extension errors than presenting images without a noise. However, in these previous experiments, we made two methodological decisions that warrant further investigation.

First, we confounded noise and image category—creating two versions of the encoding phase—to produce a conditioned response where participants associated the noise stressor with one category of images and not the other. The reason for this decision was twofold: one, to have participants associate the noise stressor with images from a specific category, so that the arousal did not bleed over to the other images from the other category, and two, to eliminate participants becoming habituated to the noise stressor. Put differently, if noise was presented on every trial throughout the experiment, its stress-inducing impact would not be tied to one particular category (Dunsmoor Murty, Davachi, & Phelps, 2015).

Second, we separated the arousal arising from the valence of the image—by using neutral images—to test arousal's influence on boundary judgement errors. However, consistently throughout Experiments 5a – 6, we found that participants rated the "nature" images as more arousing than the "object" images. This difference may have arisen due to the complexity of the nature images compared to the object images. Indeed, there is evidence that the more complex an image is, the more arousing it is, especially when compared with prosaic, simple images (such as images of everyday objects; Madan, Bayer, Gamer, Lonsdorf & Sommer, 2018). This pattern raises another question: were the arousal differences between nature and object images enough to drive boundary restriction, in the absence of the noise stressor? It is important to consider that two different types of arousal are possible: aversive (e.g., elicited by our noise stressor) and positive (e.g., elicited by our relatively complex nature images). In Chapter 6, we demonstrated that an attention-grabbing stimulus—which captured and held attention solely due to the differences between it and the background—induced boundary restriction. In Chapter 7, we demonstrated that arousal also induces boundary restriction, presumably by the same process: capturing and holding attention. However, it is possible that in order to capture and hold attention, an arousal stimulus needs to be aversive, because positive arousal may not capture and hold attention in the same way that aversive arousal does (Mather, 2007).

Thus, in the present series of experiments, we aimed to determine whether conditioning the noise stressor to an image category was necessary (Experiment 7a), and whether the differences in perceived (pleasant) arousal between image categories—in the absence of the aversive noise stressor (Experiment 7b)—would induce boundary restriction or attenuate boundary extension.

Our rationale for conditioning the noise stressor to an image category comes from the conditioning literature. Visual stimuli (such as images) can be easily conditioned to other sensory stimuli (such as sounds). For example, we know that visual learning is enhanced by additional non-visual stimuli (see Jacobs & Shams, 2010 for a review), and sound in particular can enhance the learning of visual stimuli. For example, Kim, Seitz, Shams and Herzog (2008) had participants learn to identify the direction of moving stimuli amongst visual noise stimuli (randomly moving dots). Participants either viewed the visual stimuli on their own (uni-sensory condition) or paired with a moving sound stimulus (multi-sensory condition). With training, all participants became faster and more accurate at the task over time. However, participants in the multi-sensory condition became more accurate at a quicker rate. Indeed, once a visual stimulus (e.g., an image) has been associated with a particular secondary non-visual stimulus (e.g., a noise), that association can transfer to other perceptually similar visual stimuli (i.e., perceptually similar images then become associated with the same noise; Davies, Davies, & Bennett, 1982; Stussi, Pourtois, & Sander, 2018). Further, humans are very good at categorising visual images into semantic categories (e.g., identifying animals from among objects), so good that it happens seemingly instantaneously (Grill-Spector & Kanwisher, 2005). Taken together, this evidence suggests that participants would automatically associate the noise stressor with the compositionally similar (i.e., outdoor scenes: nature; rather than indoor scenes: objects) and perceptually similar (i.e., close distances: objects; rather than far distances; nature) images in the same category, such that seeing any image from that category, with or without noise, would elicit the same response. Thus, to condition a noise stressor to a specific image category, and avoid the association of that stimulus to images in a different category—or "bleed over" from one category to another—the image categories in Experiments 5a, 5b, and 6 needed to be compositionally and perceptually different (Dunsmoor et al., 2015; see also Stussi et al., 2018). Hence, we used two different image categories: everyday objects, and nature, and conditioned the noise stressor to one image category for each participant.

However, this conditioning resulted in a confound of noise and image category within participants (note here that we counterbalanced the pairing of noise and category between participants). Thus, we wanted to know whether it was *necessary* for the noise stressor to be conditioned to an image category at all. It is possible that participants would make more boundary restriction errors and fewer boundary extension errors for images presented with the noise stressor, even when the comparison images presented without the noise stressor are from the same the image category. This possibility seems unlikely (e.g., Dunsmoor et al., 2015; Stussi et al., 2018), but is nonetheless important to test empirically, particularly because of the confound we introduced in Chapter 7.

To confirm that the noise stressor/image category conditioning was necessary, we ran a version of Experiment 5a without conditioning the noise stressor to an image category. We used images from only one category (everyday objects) but presented half of them with noise. We expected that with no conditioning, we would see more "bleed over" of the noise stressor to the images presented without noise. In other words, we predicted that participants would associate the noise stressor with *all* of the images from that category rather than just the images that were actually paired with the noise stressor, leading to two outcomes: First, the noise stressor would be associated with all of the images, resulting in no difference in image arousal ratings, and no difference in image pleasantness ratings. Second, participants would make the same type of boundary errors for all of the images regardless of whether it was presented with or without the noise stressor.

8.3 Pilot Study

Stimuli preparation

We used the same pilot study data from Experiment 5a to identify stimuli for the present experiment. We found that the everyday-objects category had the most images with a unique category membership, and so we used this category of images for the present study. We excluded images that did not contain a central object on a wider background, or that we could not crop without cropping the central object in the image. We chose 24 everyday-object images with the best category fit ratings, and neutral valence. We created two groups of 12 object images that were matched on valence and arousal, $t(22) = 0.08$, $p = .94$, $d = 0.02$ (see

Table 8.1 for descriptive statistics).We cropped each image to 75% of its original size and then resized it, which resulted in two versions of each image (i.e., 75% cropped and 100% uncropped).

Table 8.1. Pilot ratings of valence (1 = very unpleasant to 9 = very pleasant) and arousal (1 = least arousing to 9 = most arousing) per image group for Experiment 7a.

	Group 1		Group 2	
	Valence	Arousal	Valence	Arousal
Rating *M*(*SD*)	3.23 (0.20)	3.15 (1.06)	3.17 (0.24)	3.12 (0.66)
Rating range	2.95-3.44	1.72-5.74	2.81-3.44	2.00-4.27

8.4 Experiment 7a Method

Participants. We used the same small-medium effect size we used in Experiments 5a and 5b to calculate the sample size required to detect a within-subjects effect (*d* = 0.30, alpha = .05, power = .80). However, since we removed the between-subjects factor (category-noise combination), we halved the recommended sample size of 90, to 45. To enable our counterbalance—which required divisibility by four—we increased that number to 48. We recruited 48 participants with normal or corrected-to-normal colour vision from an undergraduate research participation pool. Most (66.70%) were female, and Caucasian (including “White”; 64.60%). Participants ranged from 18 to 46 years (*M* = 23.19, *SD* = 6.12) and received course credit, or payment (AUD$10) for their time.

Design. We used a 2 (noise: noise, no noise) x 2 (encoded image: cropped, uncropped) within-subjects design. We created two matched groups of object images and counterbalanced the group that was presented with noise. Our key dependent variable was the proportion of images on which participants made errors (boundary extension errors, boundary restriction errors).

Materials. We used the same anxiety measures (trait and state subscales of the STAI) and noise manipulation (2s of 95-98db white noise) as in Experiments 5a-6 and the 24 images we selected from our piloting procedure.

Procedure

We used the same procedure as Experiments 5a, with only one category of image instead of two.

8.5 Experiment 7a Results and Discussion

First, we examined whether exposure to noise led participants to rate images as less pleasant and/or more emotionally arousing, using a paired samples t-test. Recall we expected that—without conditioning the noise stressor to an image type—participants would rate images presented with and without noise as similarly pleasant and arousing. However, contrary to our prediction, participants rated images as less pleasant when presented with noise (M = 2.97, SD = 0.88) compared to without noise (M = 3.59, SD = 0.97), $t(47) = 4.50$, $p < .001$, $d = .65$, 95% CI [0.34, 0.96].

This result suggests that participants associated the unpleasantness of the noise with only those images accompanied by noise, rather than all images in the same category. However, it is important to note that we did not use a conditioning paradigm where participants first spend time associating the noise with a particular type of image category, prior to rating the images. Instead, participants associated the noise with an image category *during* encoding. Thus, perhaps the noise only affected pleasantness ratings with their paired image for the first few presentations of the noise, with “bleed over” occurring for later images, once the noise had been associated with the category. To examine this possibility, we split the data into three phases (the first, middle, and last 8 trials) and used a 3 x (time: time 1, time 2, time 3) x 2 (noise: noise, no noise) repeated measures ANOVA to investigate potential changes in pleasantness ratings over the three time points. There was no interaction, $F(2, 94) = 2.52$, $p = .09$, $\eta_p^2 = .05$, and no main effect of time, $F < 1$, and there remained a main effect of noise with images without noise being rated as more pleasant than images with noise, a main effect of noise $F(1, 47) = 21.48$, $p < .001$, $\eta_p^2 = .31$. Thus, we can conclude that there was no "bleed over" of the noise stressor's effect on pleasantness ratings.

We next compared the arousal ratings[17] for images presented with noise versus without noise. Participants rated images as similarly arousing when presented with noise ($M = 2.28$, $SD = 1.18$) compared to without noise ($M = 2.07$, $SD = 0.92$), $t(47) = 1.75$, $p = .09$, $d = 0.25$, 95% CI [-0.04, 0.54]. Note that this finding is consistent with our previous findings (Experiments 5a, 5b and 6) and is most likely the result of asking participants to rate the perceived image arousal ("How emotionally arousing was that picture" 1 = not at all emotionally arousing, 7 = highly emotionally arousing), rather than reflect on their own state of emotional arousal.

We also wanted to find out whether—consistent with participants' pleasantness ratings—there was a similar change in arousal ratings *during* the encoding phase. We hypothesised that in the absence of conditioning, we would see a "bleed over" of the effect of noise onto the no noise images, and therefore images presented without noise would become

[17] The current experiments were run before Experiment 6, and therefore before the valuable reviewer recommendation that we change our anxiety measure from asking participants to rate the arousal of the *image* to rating their own emotional arousal at the time of viewing.

more arousing over time. To investigate this possibility, we again split the data into three phases (the first, middle, and last 8 trials) and used a repeated measures ANOVA to investigate changes in arousal ratings over time. We found that over time, arousal increased, a main effect of time, $F(2, 94) = 5.09$, $p = .01$, $\eta_{p2} = .10$. This finding fit with our hypothesis that in the absence of conditioning, the arousal effect of the noise would "bleed over" to the images without noise. However, there was no main effect of noise condition $F(1, 47) = 3.46$, $p = .07$, $\eta_{p2} = .07$ (consistent with our earlier analysis), and no interaction between time and arousal ratings, $F < 1$.

Consistent with Experiments 5a and 5b, our data suggest that participants perceived images with noise as more unpleasant, but not as more emotionally arousing, than images without noise. However, despite no change in image arousal, participants' state anxiety (on the STAI-S) increased from time 1 ($M = 36.09$, $SD = 10.06$), to time 2 ($M = 38.89$, $SD = 8.85$, $t(46) = 2.66$, $p = .01$, $d = 1.89$, 95% CI [12.74, 17.43], supporting the assertion that the noise increased participants' own arousal. This change in state arousal from time 1 to time 2 was consistent with Experiments 5a, 5b, and 6.

Analysis of boundary errors. Examining the proportion of participants' errors overall, we found that participants were inaccurate—essentially at chance—in identifying the correct image ($M = 0.43$, $SD = 0.11$, range = 0.25 - 0.75). Next, we addressed whether the noise stressor increased boundary restriction and attenuated boundary extension, in the absence of image-noise conditioning. Recall we hypothesised that—since we did not condition the noise to one semantic category of images—we would not see an effect of noise on boundary judgement errors. However, note that we did find a difference in valence (pleasantness ratings) for images with and without noise, consistent with Experiments 5a, 5b and 6. Thus we wondered whether this valence difference was enough to affect boundary judgement error rates and types. We conducted a 2 (noise: noise, no noise) x 2 (encoded image: cropped, uncropped) within-subjects ANOVA on proportion of errors. Consistent with boundary extension research, and our previous experiments (excluding Experiment 5a), participants made more boundary extension (*M* = 0.68, *SD* = 0.22) than boundary restriction errors (*M* = 0.18, *SD* = .17), $F(1, 47) = 103.26$, $p < .001$, $\eta_{p}^{2} = .69$. There was no main effect of noise condition, $F(1, 47) = 0.50$, $p = .49$, $\eta_{p}^{2} = .01$, and no significant interaction between noise and encoded image, $F(1, 47) = 0.13$, $p = .72$, $\eta_{p}^{2} = .003$ (see Figure 8.1).

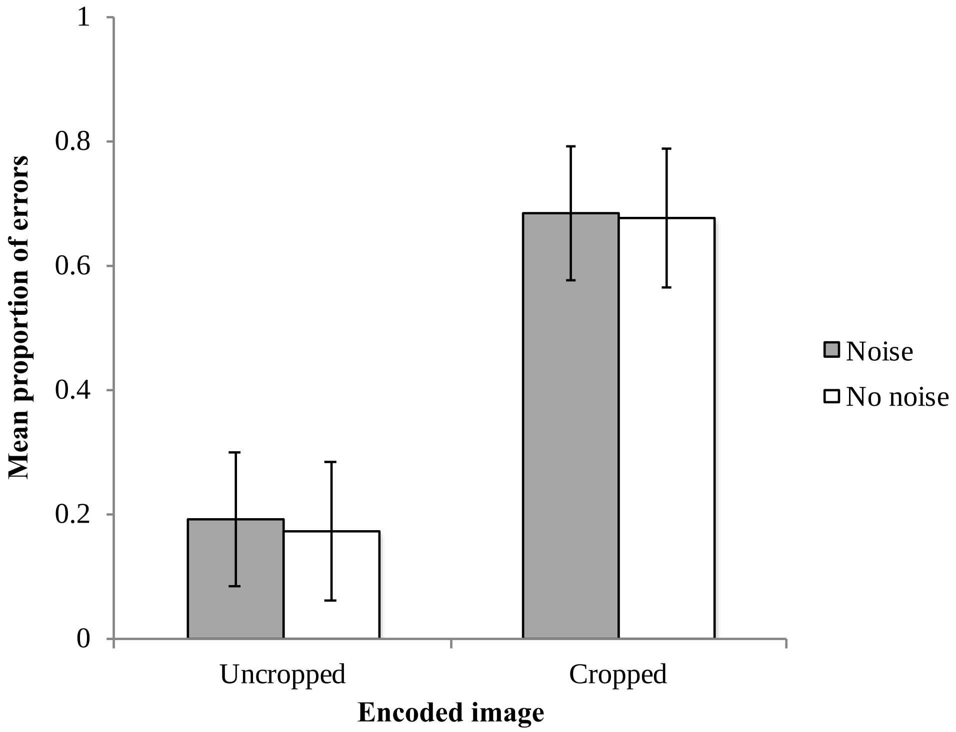

Figure 8.1. Mean proportion of boundary errors by encoded image and noise collapsed across noise-object group combination. Error bars represent 95% confidence intervals.

Recall we were most interested in whether noise induced boundary restriction errors. We next ran a paired-samples Bayesian t-test with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) on these variables and found $BF_{10} = 0.21$. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates no evidence that the difference is significant. In other words, participants were not more likely to make boundary restriction errors for images presented with noise than without noise.

Our primary aim in this experiment was to determine whether the noise stressor needed to be conditioned to an image type, to avoid “bleed over” of arousal and negative valence onto the other images. In previous experiments we predicted that when images were accompanied by the noise stressor, participants who saw an uncropped image at encoding would be *more* likely to make boundary restriction errors and participants who saw a cropped image at encoding would be *less* likely to make boundary extension errors. Consistent with our prediction, we found that noise condition did not affect participants’ error type, thus we can conclude that the effect of the noise stressor may have bled across to all the images in the same category, regardless of whether they were presented with noise.

Together, our results revealed three important findings. First, there was a distinct difference in valence (pleasantness ratings) for images presented with and without the noise stressor, indicating that conditioning the noise stressor to a specific category was not necessary to manipulate the *valence* of a particular subset of images. This difference was consistent with Experiments 5a, 5b, and 6, where the noise was paired with one image category only.

Second, there were no differences in image arousal for images presented with and without the noise stressor, indicating that conditioning may have been necessary to manipulate image arousal. However, participants rated images presented later in the experiment as more arousing, compared to the images presented earlier in the experiment. Thus, it appears that the negative effect of the noise stressor may have bled over to the images presented without noise. However, note again that we did not use the same arousal question that we did in Experiment 6—where we specifically asked participants how aroused they *felt*, rather than how arousing the *image* was. Thus, without this adjusted question, we cannot say definitively that there was no change to the way participants felt. Third—unlike Experiment 5a—there were no differences in boundary judgement errors between images with and without the noise stressor, indicating that the conditioning of the noise stressor to the image category may have been necessary to induce differences in boundary errors. Taken together, the present experiment demonstrates that differences in valence were not enough, in their own right, to drive a boundary restriction effect, and likewise any differences between image categories on perceived image arousal did not translate to differences in boundary restriction errors.

The current study has two limitations that needs to be acknowledged. First, a limitation to the paradigm we used both here and in Chapter 7 is the mismatch between the noise stressor and the images used. In our attempt to separate arousal from negative valence, participants viewed images that were incongruent with the noise stressor. Indeed, participants preferentially associate aversive stimuli with congruent, naturalistic images (such as animals, nature scenes, or faces), rather than the objects on a neutral background used in our study. One study demonstrated this bias by examining the differences in conditioning using neutral (object) images, versus baby faces and angry faces (Stussi et al., 2018). Participants had a learning bias—with conditioning happening more quickly and extinction happening more slowly—for the face stimuli, than for the neutral images. In fact, many studies have found that aversive stimuli itself shows a learning bias (faster conditioning) when conditioned with a congruent image, whether with angry faces (Öhman & Dimberg, 1978), potentially phobic objects (e.g., snakes; Öhman, Fredrikson, Hugdahl, & Rimmö, 1976; Olsson, Ebert, Banaji, & Phelps, 2005), and even comparing angry faces and phobic stimuli (Mallan, Lipp, &

Cochrane, 2013). Thus, although conditioning was likely necessary to observe the effect of the noise stressor on boundary judgement errors, the use of object images meant there may have been a mismatch between the noise stressor and image type.

A second limitation is that we manipulated arousal—via the noise stressor—at encoding only (and not at retrieval). In typical boundary restriction studies, researchers use negative images to elicit arousal. Thus, the same level of arousal/negative valence is elicited at both encoding and retrieval. In attempting to separate out arousal from valence, we have also removed arousal from the retrieval phase of the study. It is possible that Experiments 5a, 5b and 6, where arousal was tied to an image category at encoding, participants felt a similar heightened arousal at retrieval for the images in that category. However, in the present experiment, participants may have associated the noise stressor with all of the images. Since all of the images were semantically related, the participants may have felt heightened arousal for *all* of the images at retrieval—and therefore the effect of arousal was washed out amongst all of the images, rather than only associated with the images that were presented with the noise stressor.

If participants—in the absence of conditioning—associated all the images at test with the noise stressor, then the fact that we saw no difference in boundary judgement errors is understandable. Recall however that for Experiments 5a and 5b—where we asked participants to rate the arousal of the *image* rather than the arousal they *felt*—we found that participants rated the nature images as more arousing than the object images, regardless of noise condition. Therefore, for these images, participants would have felt the same arousal level for the nature images at both encoding and test. Arousal is an emotional construct that can be both positive (pleasant) and negative (aversive; Solomon & Stone, 2002) thus in Chapter 7 there were two possible types of arousal driving the effect. Arousal arising from the noise stressor is an example of aversive arousal, whereas arousal arising from neutral images (e.g., nature images) is an example of pleasant arousal. Perhaps the nature images themselves were eliciting much of the arousal, and when paired with noise, the combination resulted in even higher arousal. This high arousal, rather than the noise alone, may have been what was actually driving the differences in boundary error judgements in Experiments 5a, 5b, and 6. Thus a secondary question arises: was the noise stressor necessary at all? Indeed, it is possible that if the nature images were eliciting arousal, this same arousal would be felt at both encoding and test. If, as expected, we find the same pattern of results in the absence of the noise stressor, then we could conclude that aversive noise is not necessary to induce boundary restriction, and that the (presumably) pleasant arousal of the nature images at both encoding and test was enough to induce the effect.

To investigate whether the noise stressor was necessary, or whether the arousal from the nature images was enough to drive the effect, we replicated Experiment 5a without a noise stressor. This procedure allowed us to investigate two things. First, whether the change in STAI state anxiety was attributable to the noise stressor, and second whether the higher perceived image arousal of the nature images would induce boundary restriction errors, and attenuate boundary extension errors. We predicted that in the absence of noise, participants' STAI ratings would not change from time one to time two, and participants would make the same type of errors for both image types, suggesting that the noise stressor *was* necessary for changing boundary error judgements.

8.6 Experiment 7b Method

Participants. Like Experiment 7a, we recruited 48 participants with normal or corrected-to-normal colour vision from an undergraduate research participation pool. Most

(70.80%) were female, and Caucasian (including “White”; 60.04%). Participants ranged from 18 to 53 years (*M* = 21.73, *SD* = 5.78). Participants received course credit, or payment (AUD$10) for their time.

Design. We used a 2 (encoded image: cropped, uncropped) x 2 (image type: nature, object) within-subjects design.

Materials. We used the same anxiety measure and image stimuli as in Experiment 5a (Chapter 7).

Procedure

We used the same procedure as Experiment 5a, without the noise stressor.

8.7 Experiment 7b Results and Discussion

Like the previous experiments, we first examined participants’ pleasantness and arousal ratings of the nature vs. object images. As expected and consistent with experiments 5a – 7a, participants rated the nature images (*M* = 4.33, *SD* = 0.78) as more pleasant than object images (*M* = 3.27, *SD* = 0.86), $t(47) = 8.80$, $p < .001$ $d = 1.27$, 95% CI [0.89, 1.65]. Participants also rated the nature images as more arousing (*M* = 3.27, *SD* = 1.08) compared to object images (*M* = 2.11, *SD* = 0.89, $t(47) = 9.41$, $p < .001$, $d = 1.36$, 95% CI [0.96, 1.75]. Thus, even in the absence of noise, and despite matching the image categories as closely as possible on valence and arousal (see pilot outline in Chapter 7), participants found the nature images more pleasant—and more arousing—than the object images.

Interestingly, we measured participants’ state anxiety (on the STAI-S) and found that it was the same at time 1—before encoding (*M* = 38.27, *SD* = 10.15), as time 2—after encoding (*M* = 37.35, *SD* = 10.76), $t(47) = 1.61$, $p = .11$, $d = 0.23$, 95% CI [-0.23, 2.06], supporting the assertion that in Experiments 5a, 5b, 6 and 7a, participants’ arousal was increased due to the presence of the noise stressor.

Analysis of boundary errors. Overall, we found that participants were fairly inaccurate in identifying the correct image (*M* = 0.39, *SD* = 0.11, Range = .17-.63). Next, we addressed our principal research question: is noise necessary for inducing boundary restriction errors, or attenuating boundary extension errors? We conducted a 2 (encoded image: cropped, uncropped) x 2 (image category: nature, object) repeated measures ANOVA on proportion of errors. Consistent with our earlier experiments, and with previous boundary restriction research, participants made more boundary extension (*M* = 0.71, *SD* = 0.22) than boundary restriction errors (*M* = 0.07, *SD* = .08), $F(1, 47) = 297.39$, $p < .001$, $\eta_{p2} = .86$. There was also no main effect of image type, $F(1, 47) = .03$, $p = .82$, $\eta_{p2} < .001$, and in line with our predictions, there was no significant interaction between image type and encoded image, $F(1, 47) = .007$, $p = .93$, $\eta_{p2} < .001$.

Recall we were most interested in whether the arousal from the nature images induced boundary restriction errors. We next ran a paired-samples Bayesian t-test with default Cauchy prior (Rouder, Speckman, Sun, Morey, & Iverson, 2009) on these variables and found $BF_{10} = 0.16$. According to the statistical interpretation that Wetzels et al. (2011) suggest, this Bayes factor indicates no evidence that the difference is significant. In other words, participants were not more likely to make boundary restriction errors for nature images than for objects images.

We predicted that in the absence of noise, participants would make the same type and number of errors for both image types, suggesting that the noise stressor *was* necessary for changing boundary error judgements. Recall also that in the current experiment, participants rated the nature images as more arousing, and more pleasant. Interestingly, our data showed that though participants *rated* nature images as more arousing, this judgement did not translate into the boundary errors that they made (see Figure 8.2).

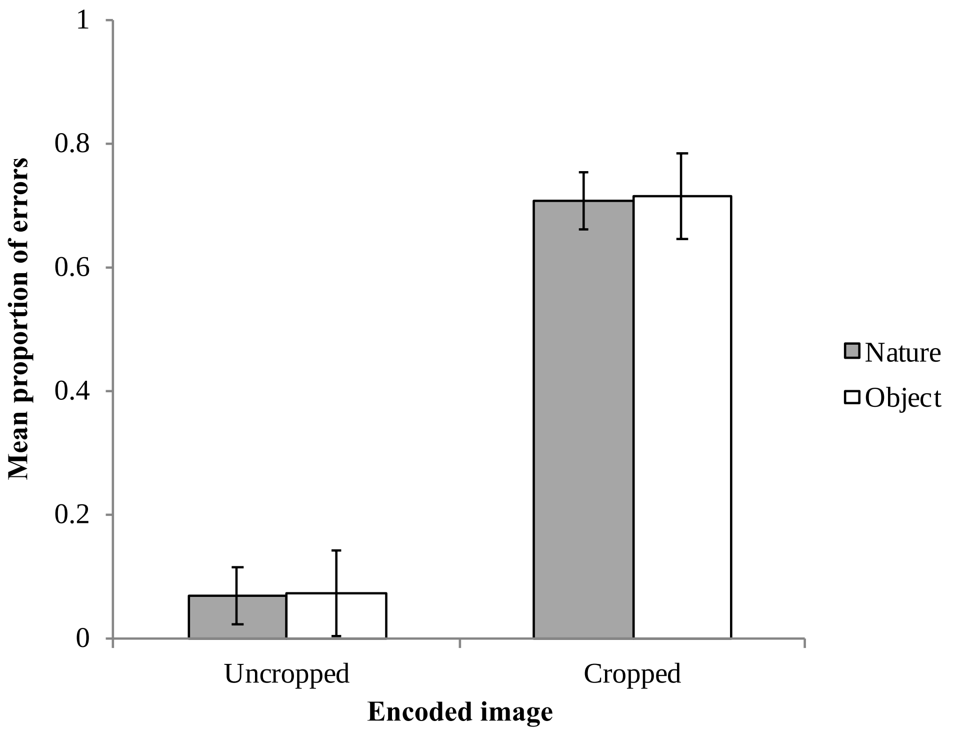

Figure 8.2. *Mean proportion of boundary errors by encoded image. Error bars represent 95% confidence intervals.*

Experiment 7b produced three key findings. First, the absence of noise reduced overall boundary restriction errors, when compared to all of our previous experiments. Second, despite the nature images being rated as more arousing and more pleasant than object images, this difference in perceived image arousal did not affect boundary errors. Third, we found no difference in the STAI between time 1 and time 2 (in the absence of noise), indicating that the noise was likely responsible for this change in Experiments 5a, 5b, 6, and 7a.

These results have several important implications. Taken together with the findings of our previous experiments (Experiments 5a – 7a), they suggest that the combination of a highly arousing aversive stimulus (noise stressor) *and* highly arousing scenes (complex visual stimuli; such as would occur during a stressful event like an armed hold up), are necessary for boundary restriction errors to occur. Indeed, our findings in Chapter 4 (Experiments 2a – 2e)—where we separated valence and measured its effect on boundary restriction in isolation—support this assertion. Even though Experiment 7b demonstrated that nature images were more arousing than object images, the higher level of arousal for nature images was not enough to translate to a change in boundary judgement errors. Additionally, perhaps the reason that participants in the present experiment rated the nature images as more arousing than the object images was because they were making a direct comparison between the two types of images. Indeed, compared to the prosaic object images, the nature images may have seemed more exciting and interesting than the object images, and this comparison was reflected in an elevated arousal rating (Madan et al., 2018). To confirm this finding, future studies could replicate this study with a between-subjects design. Additionally, this arousal type, was presumably *pleasant* (as inferred by the pleasant valence ratings), in contrast to the one employed in 5a-7a, which was *aversive;* this difference may have been the reason that arousal in the present experiment was not enough to induce a boundary restriction effect. Indeed, studies that have found a boundary restriction effect have done so with aversive stimuli (e.g., Mathews & Mackintosh, 2004; Safer Christianson, Autry, & Osterlund, 1998). However, it should be clarified here that the arousal differences in the present study asked how the participant rated the *image* rather than how they rated their own *feelings* of arousal.

8.8 General Discussion

Taken together, the current experiments (7a and 7b) highlight two important methodological issues. First, to induce boundary restriction, it seems necessary to condition a noise stressor to an image category when using neutral images. This conditioning means that arousal is more likely to be felt both at encoding and at retrieval, and that the noise stressor does not bleed over to the other image category. Second, these data provide preliminary evidence for the idea that boundary restriction may be induced by only *aversive* rather than *pleasant* arousal. Previous research is consistent with this conclusion—albeit without testing it directly—since boundary restriction has only been demonstrated with highly *negative* stimuli (e.g., Mathews & Macintosh, 2004; Safer, 1998). Despite our present experiments demonstrating higher arousal ratings for nature images, this high arousal was alongside high pleasantness ratings. In other words, high arousal coupled with high pleasantness (rather than low pleasantness) did not lead to the memory narrowing effect seen in other boundary restriction studies. The findings from the current experiments therefore suggest that boundary restriction may only be induced with aversive arousal, and this aversive arousal may need to be combined with negatively valenced (unpleasant) stimuli in order to replicate what people experience during stressful events.

In summary, though our participants rated the images as different in arousal and pleasantness, this difference did not have an effect on the type of error than participants made. Recall that the aims of the current experiments were to rule out whether it was necessary to condition the noise stressor to an image category (Experiment 7a) and whether the noise stressor was necessary for changes in pleasantness/arousal ratings, or on boundary errors (Experiment 7b). The results from these experiments further demonstrate that in order to induce boundary restriction, there must be a combination of both a high *negative* arousal and negative valence. Indeed, this combination is a more ecologically valid way of measuring what would usually happen during a negative event, and thus may be why boundary restriction is found in naturalistic settings but is difficult to replicate in a laboratory setting.

9 General Discussion

9.1 Summary of Aims and General Findings

My thesis aimed to determine which elements of negative scenes are most likely to result in boundary restriction. I achieved this aim by investigating three elements of negative images that may contribute to boundary restriction:

1) Valence: negative visual imagery (Chapter 3) and isolating valence from negative visual imagery (Chapter 4);
2) Attention: combining negative visual imagery with short presentation times (Chapter 3), and isolating attentional bias from negative visual imagery (Chapters 5 & 6); and
3) Arousal: isolating arousal from negative visual imagery (Chapter 7), and investigating the methodological procedures associated with Chapter 7 (Chapter 8).

Overall, my experiments revealed three important findings. First, valence appears to contribute little—in its own right—to the boundary restriction effect. Participants' boundary judgement errors remained fairly consistent (they made predominantly boundary *extension* errors) throughout a series of experiments, regardless of image valence (Chapters 3 & 4). Second, attention does contribute to boundary restriction. Limited opportunity to attend (i.e., shorter exposure time to images) increased the rate of boundary restriction errors (Chapter 3) and focussed attention induced boundary restriction errors (Chapter 6). However, using an incidental measure of attention —relying on participants' natural tendency to over-attend to the left hemifield—did not yield the same results (Chapter 5). Third, arousal can both induce boundary restriction errors, and decrease boundary extension errors (Chapter 7). However, we only observed this effect for aversive arousal, and not for pleasant arousal (Chapter 8).

Importantly, my experiments can be divided into paradigms that elicited top-down attentional capture (Experiments 1 – 2e) and bottom-up attentional capture (Experiments 3a – 7b). Taken together, my findings demonstrate that boundary restriction—when induced by negative images—may actually be the result of bottom-up attentional capture. The heightened arousal and focussed attention induced by negative visual imagery, rather than the inherent negative valence of negative images, is likely what drives the boundary restriction effect.

9.2 Theoretical Implications

The results of my experiments inform theoretical explanations of boundary extension and boundary restriction, including perceptual schema, attention processes, and arousal biased competition. The results also offer some insight into how people remember negative events more generally: negative stimuli may limit imagination and extrapolation due to a combination of heightened arousal and focussed attention, leading to a reduction in boundary extension, and an increase in boundary restriction.

9.2.1 Perceptual Schema

The current understanding of boundary extension is that we have a perceptual schema—an internal representation of the world—such that when we see something in isolation (e.g., an image), we contextualise it into the wider world, using imagination and extrapolation (Gottesman & Intraub, 1999). This contextualising of a single image to fit it into the wider world leads to an extrapolation of that image, and then confusion between what was actually seen in the image and what was imagined beyond the boundaries of that image (source monitoring error; Johnson, Hashtroudi, & Lindsay, 1993); this confusion is observed as a boundary extension error (Intraub, 2010; Intraub, 2012; McDunn et al., 2014). Thus, in order for boundary restriction to occur, this normal process of perceptual schema activation needs to be curtailed. One way that this process could be curtailed is by viewing negatively valenced stimuli, which in turn creates a focus of attention.

9.2.2 **Tunnel Memory**

Studies that demonstrate memory errors similar to boundary restriction—such as weapon focus, “tunnel vision”, or memory narrowing (e.g., Fawcett, Russell, Peace, & Christie, 2013; Kramer, Buckhout, & Eugenio, 1990; Mathews & Mackintosh, 2004; Pickel, Ross, & Truelove, 2006)—show that it is the presence of negatively valenced stimuli that results in tunnel memory errors. This association is consistent with threat biases (i.e., people tend to pay more attention to threatening than non-threatening objects; Jewell & McCourt, 2000; Simons & Levin, 2000; Park et al., 2007; Wolfe, 2010). My experiments demonstrated that boundary restriction errors are rare, and often overshadowed by boundary extension errors. Yet tunnel memory and weapon focus seem to occur more readily for negatively valenced stimuli. However, though both boundary restriction and tunnel memory are phenomena that result in the loss of peripheral information, they are distinctly different memory errors. Tunnel memory results in the loss of visual details in the periphery, while boundary results in a misremembering of the space around a salient object. Thus, it is possible for loss of detail to occur (tunnel memory), without a misremembering of the space around an object (boundary restriction). Indeed, a possible future direction may be to determine what we can learn about boundary restriction by examining the research on tunnel memory.

In Experiments 2a – 2e, I successfully altered the valence of neutral images. I demonstrated that negative valence alone does not induce boundary restriction (when compared to neutral and positive valence). Likewise, in Experiment 1, I demonstrated that participants' boundary judgement errors did not differ for negative versus neutral images. Furthermore, in Experiment 4, I demonstrated that boundary restriction could be induced in the absence of negative valence (by directing attention), suggesting negative valence is not necessary for inducing boundary restriction. Taken together, my findings indicate that valence alone is not driving the boundary restriction effect.

The implications of this finding for tunnel memory are that boundary restriction is not simply or reliably the result of viewing negatively valenced stimuli. My findings demonstrate that when people experience boundary restriction—whether as a result of emotional personal experiences (Berntsen, 2002; Talarico, Berntsen, & Rubin, 2009); emotional scenes (Christianson & Loftus, 1987; Christianson, Loftus, Hoffman, & Geoffrey, 1991; Ménétrier et al., 2013; Safer, et al., 1998); and negative images (Fawcett, Russell, Peace, & Christie, 2011; Kramer, Buckhout, & Eugenio, 1990; Mathews & Mackintosh, 2004; Pickel, Ross, & Truelove, 2006; Takarangi, Oulton, Green, & Strange 2015; Waring, Payne, Schacter, & Kensinger, 2010)—it may be the result of focussed attention, rather than the presence of negatively valenced content.

9.2.3 **Attentional Processes**

Memory has limited capacity, and thus attention directs us to what needs to be attended to and eventually consolidated into memory. Attention can also be captured in such a way that a person's entire focus is on the salient aspect of the stimulus. Attentional capture may be so complete that a person *only* encodes the attention-grabbing part of the image, interrupting the processes that lead to boundary extension, and instead leading to boundary restriction—or tunnel memory. In Experiment 4 I demonstrated that capturing attention and drawing it to images induced boundary restriction, and that capturing attention and drawing it away from images induced boundary extension.

However, despite my success at inducing boundary restriction by capturing attention, my results from Chapter 5 suggest there is more to the story: specifically, the *way* attention is captured may affect boundary restriction errors. The distinction between two possible brain processes that direct attention—top-down processing, and bottom-up processing—seems pertinent here. Top-down processing involves the prefrontal cortex and is a volitional process by which a person consciously directs their attention according to their own experiences or goals (e.g., finding a friend’s face in a crowd) and/or the salience of the information being attended to. Within this framework, a person makes a judgement about the visual information before deciding whether it is worthy of attention. In this case, because attentional resources are dedicated to encoding the stimulus rather than imagining beyond the scene, boundary extension may not occur, and boundary restriction may become more likely. Although previous studies have successfully demonstrated that negative stimuli induces boundary restriction, my studies suggest the *salience* of the images in those studies directing attention accounts for these results (Power & Dalgleish, 1997).

In Chapter 4, I demonstrated a top-down attentional process. Participants’ attention was directed using top-down processing via priming, leading participants to interpret ambiguous images as emotional, and thus heightening the salience of the image. However, this series of experiments demonstrated that top-down attentional processing did not result in a boundary restriction effect. In Chapter 6, I investigated the alternative process of *bottom-up* attentional processing. Bottom-up attentional processing involves the amygdala. It is a process by which a person’s attention is directed automatically, without conscious effort (e.g., by heightened arousal), and much more readily than top-down processing (Connor, Egeth, & Yantis, 2004). This attentional capture successfully resulted in both reducing boundary extension errors, and inducing boundary restriction errors. Taken together, my work demonstrates that bottom-up attentional processing may be necessary to induce boundary restriction.

However, importantly, I found that not all bottom-up attentional capture results in boundary restriction. Pseudoneglect—the attentional bias towards the left side of space among neurotypical people (e.g., Jewell & McCourt, 2000)—is an example of automatic, bottom-up attentional capture. Dickinson and Intraub (2009) demonstrated that this bias can result in more extensive boundary extension on the right side of images, presumably because of a lack of attention to that side. Yet, in Chapter 5 (Experiments 3a & 3b), my experiments did not replicate this pattern. Key methodological differences between the two sets of studies may account for this discrepancy. Instead of presenting images across the visual field (as in Dickinson & Intraub), I presented images to each visual field separately, either one at a time (3b) or one in each visual field (3a). It is possible that Dickinson and Intraub's findings reflected participants' overestimation of the right side of space, a pattern akin to line bisection, whereby participants estimate the centre point to be slightly to the left of actual centre. Dickinson and Intraub (2009) found that participants paid more attention to the left side of space, usually resulting in participants perceiving *more space* on the left. However, we expected that more attention to the left would result in boundary extension, where people expand scenes from which their attention is diffuse (e.g., Intraub, Daniels, Horowitz, & Wolfe, 2008). These competing phenomena meant that any boundary restriction effects we *might* have seen due to attention to images presented on the left were washed out by an overestimation of the images on the left. Dickinson and Intraub's paradigm in contrast was more consistent with a line bisection task whereby participants may have estimated the "centre" point of an image to be further on the left than it actually was, leading the right side of the image to seem more expansive and hence resulting in more expansive boundary extension errors on the right and thus was not affected by these competing phenomena.

9.2.4 Arousal Biased Competition

Arousal can also change how a person's attention is directed (arousal-biased competition; Mather, 2007). Although I demonstrated that arousal contributes to boundary restriction (in Experiment 5a – 6), I also demonstrated that this arousal may need to be *aversive* (in Experiment 7b). Arousal can be both pleasant (positive) or aversive (negative), and both types may not capture attention equally (Anderson & Yantis, 2012). Aversive and pleasant arousal are similar in some ways: stress hormones are released for both types of arousal (e.g., Merali, McIntosh, Kent, Michaud, & Anisman, 1998), and arousal from *both* pleasant and aversive stimuli have been shown to improve memory accuracy (Bradley, Greenwald, Petry, & Lang, 1992; Cahill & McGaugh, 1998), perhaps because they have similarities which may lead to focussed attention (e.g., they both activate the amygdala). Thus, we might expect them to result in the same sort of boundary restriction effect.

However, aversive arousal also differs from pleasant arousal in significant ways. While both types of arousal involve bottom-up activation of attentional capture wherein the arousal comes first, and the attention comes second (Sarter, Givens, & Bruno, 2001), there are differences in the way that attention is then held. The automatic capture means that stimuli can raise our arousal and capture our attention very quickly (if aversive) or can be dismissed as non-threatening (if pleasant) with equal speed. We preferentially attend to aversive stimuli (such as a potential threat) over pleasant stimuli (which may briefly capture, but will not hold our attention; Eysenck, 1988; see Yiend, 2010 for a review). Given these differences, we would expect aversive arousal, through its capacity to capture and hold attention, to result in boundary restriction while pleasant arousal would not lead to boundary restriction, because attention can to be shifted away from such stimuli once dismissed as non-threatening. In Chapters 7 and 8, I directed participants' attention by heightening aversive arousal. I found that aversive arousal induced boundary restriction and attenuated boundary extension (Experiment 5a – 7a). Conversely, in Experiment 7b, I demonstrated that pleasant arousal (incidentally induced by nature images) did not lead to boundary restriction. It is therefore possible that *aversive* arousal (e.g., from an aversive noise stressor) rather than *pleasant* arousal (e.g., from pleasant nature images) is necessary to induce the boundary restriction effect; a function of the *differences* between pleasant and aversive arousal.

Note, however, that participants' level of pleasant arousal (induced by the nature images) was relatively *low* (3.27, on a scale of 1 to 7), which may explain why it did not affect boundary judgement errors. It may be the case that high *levels* of arousal (such as those induced by the noise blasts) lead to boundary restriction, whereas lower levels of arousal (such as those induced by the pleasant images) are not intense enough to induce boundary restriction. In this context, the *type* of arousal may not matter. Perhaps a high level of arousal—regardless of whether it is aversive or pleasant—may always lead to focussed attention and thus induce boundary restriction.

This leaves us with a question: is it the pleasant/aversive nature of the arousal, or the overall *intensity* of arousal that is responsible for boundary restriction? The results of my thesis experiments demonstrate that aversive arousal induces boundary restriction. To tease apart whether the *aversiveness* or *intensity* of arousal is more important—or whether they are equally important—future research should compare similar levels of aversive and pleasant arousal.

Beyond the aversiveness or pleasantness of the arousal, I also captured two types of arousal: arousal pertaining to the image itself ("How emotionally arousing was that image?"; Experiments 5a – 5b & 7a – 7b) and how emotionally aroused the participants *felt* ("How [emotionally aroused] did you feel while viewing that image?"; Experiment 6). In asking the latter question, I demonstrated that the emotional arousal participants *felt* (as a result of the noise stressor) rather than how emotionally arousing they found the *image* (a dimension of the image itself) affected boundary judgement errors. Thus, it seems that the more important arousal type is how the participant *feels* (i.e., their own affect) rather than the type of arousing stimulus used. Future studies should focus on investigating raising participants' arousal—and reliably testing arousal affect by perhaps using physiological measures—to induce the boundary restriction effect, rather than on the nature of the stimuli (as highly negative, or highly arousing) as has been the case in most previous boundary restriction studies.

Nevertheless, my assertion that aversive arousal is necessary to induce the boundary restriction effect fits with our understanding of the relationship between arousal and attention. Heightened arousal may depend on the imminence and magnitude of the threat, which in turn may correspond to the level of *state* arousal in the person subjected to such stimulus. Thus, there are three possible future directions for research in the role of arousal in boundary restriction: to investigate aversive versus pleasant arousal, high versus low arousal, and arousal affect versus image arousal.

9.2.5 **Summary of Theoretical Implications**

Taken together, my results demonstrate that tunnel memory may be due to bottom-up attentional processing, directed by captured attention – which can occur with or without an arousing stimulus. When a stimulus is salient, aversive, or threatening enough to heighten arousal, attention is captured in such a way that our ability to imagine or extrapolate details to fit with our perceptual schema is curtailed, however this attention capture must be complete enough such that it prevents the natural process of imagination and extrapolation beyond the boundaries of the image.

9.3 Methodological Implications

My thesis experiments inform current methods of investigating boundary judgement errors. First, across four experiments I demonstrated that boundary restriction can be induced using manipulations of attention (Experiment 4) and arousal (Experiments 5a – 6). Further, I demonstrated that we can attenuate boundary extension (Experiments 5a, 5b, 6, & 7), and induce boundary restriction errors (Experiments 5a & 7) without the potential confounding factors that come with using negative images.

9.3.1 Separating Valence, Attention, and Arousal

Throughout my thesis, I separated the complex nexus of elements that often accompany negative images. I investigated the role of valence, attention-grabbing stimuli, and arousal as separate, testable attributes, in an attempt to isolate the mechanism(s) underlying boundary restriction. I successfully demonstrated that boundary restriction can be induced without having to use complex negative images. However, I also demonstrated that we need to mimic some of the complex features of negative images, such as the arousal and/or the attention-grabbing nature (Chapters 6 and 7)—but interestingly, not the emotional valence (Chapter 4)—to observe this effect. Thus, I can conclude that some aspects of negative images (e.g., attention-grabbing nature and arousal), contribute to boundary restriction, while others (e.g., emotional valence) do not.

The nature of negative images is such that it is hard to isolate any one attribute—such as valence, a stimulus's inherent "attractiveness" (i.e., positive valence) or "aversiveness" (i.e., negative valence; see Solomon & Stone, 2002 for a review—from a nexus of several others (e.g., arousal) that may influence attention. One example is that negative scenes are almost invariably more complex, often more distinctive (i.e., containing content participants have never seen before, such as a dead body; Schmidt, 1991), and more semantically related (i.e., often belong to categories that are perceptually similar such as disease, poverty, or crime; Talmi & McGarry, 2012), than neutral images. The content of negative images may also be perceived as more vivid—clearly and accurately—than the content of neutral images (Mather & Sutherland, 2011; Pourtois, Schettino, & Vuilleumier, 2013). Visual complexity is related to arousal; as the complexity of an image increases—regardless of its valence—so does the arousal (Madan, Bayer, Gamer, Lonsdorf & Sommer, 2018). In short, an image's complexity, distinctiveness, vividness can all contribute to how attention-grabbing it is.

In Experiments 2a – 2e (Chapter 4), I attempted to control for these inherent differences in negative vs. neutral images by priming neutral images with emotional statements, and was able to successfully alter neutral image valence using these statements while keeping all other aspects of the physical image (such as the *content*) constant. In Experiments 5a – 7b (Chapters 7 & 8), I attempted to control the arousal of the stimuli, by using an external noise stressor. However, my paradigm revealed the difficulty in equating all aspects of the images; despite initially matching images on valence and arousal, I still found a difference in the level of arousal between the two image categories (in the absence of the noise stressor). This difference in arousal may have arisen because of the comparative complexity of the nature images compared to the prosaic imagery of the object images. Thus, this arousal was pleasant (as indirectly indicated by the pleasantness ratings)—unlike the aversive arousal used in Chapter 7, and not particularly high arousal. This finding lends partial support to the idea that bottom-up, automatic attention focus is what leads to boundary restriction errors. However, future studies could explore the role of positive arousal further by manipulating and directly measuring pleasant versus aversive arousal (for example, using music; Thompson, Schellenberg, & Husain, 2001).

In short, there are multiple aspects of negative images that contribute to arousal and attention, separate to the valence or content of the image, not all of which I directly explored in my studies. However, in isolating how valence, attention, and arousal do affect boundary judgement errors, I have demonstrated the importance of equating all aspects of negative and neutral images—as much as possible—when using them to study memory effects. While I acknowledge that my methodological paradigms used to isolate these factors may be further refined, the experimental paradigms I used here gave us the best possible opportunity to isolate these confounding attributes.

9.3.2 **Laterality**

Last, I demonstrated the importance of counterbalancing Likert scale ratings, to account for leftward attention biases (Experiments 2a – 6). Camera distance ratings are a common measure of boundary judgement errors (e.g., Intraub et al., 1992; 1998). However, the results from my thesis experiments suggest that these types of ratings bring with them laterality biases that may affect participants' ratings. Specifically, participants tend to mark to the left side of the scale, regardless of the nature of the left-side anchor or label. Therefore, other methods of testing for boundary judgement errors, such as drawing (e.g., Intraub & Berkowits, 1996), boundary adjustment tasks (e.g., Daniels & Intraub, 2009), or even forced choice (e.g., Experiments 1, 3a, and 3b; Candel et al., 2003) may eliminate laterality biases when testing for boundary judgement errors.

9.4 Limitations and Future Directions

There are a number of general limitations to my thesis experiments. First, I was looking at a phenomenon that may occur in the real-world following negative events, with implications for eyewitness recall, for example. However, although I used basic experimental procedures that are standard in the boundary judgement literature, these procedures have limited ecological validity. Thus, as with all laboratory studies, applying these results to real world situations is difficult, but nonetheless important. Indeed, my experiments' take-home message is that boundary restriction is likely to occur during events of high arousal and high attention-grabbing stimuli. While my findings suggest that memory for highly traumatic or emotionally arousing events will result in "tunnel memory", further research should examine how well these findings translate into real-world situations.

9.4.1 Limitations of Arousal Manipulation

In my attempt to isolate arousal from valence (Experiment 5a – 7a), I also manipulated participants' pleasantness ratings, which meant that the valence of the images did change. Indeed, it seems that arousal and pleasantness are so intertwined that isolating one from the other is a difficult task. Nevertheless, our aversive arousal manipulation (Chapter 7) induced boundary restriction and attenuated boundary extension. This finding, coupled with the one from Chapter 4—which demonstrated no difference in boundary judgement errors for negative valence—suggests that although we could not completely isolate arousal from valence, the change in the aversive arousal (and not the change in pleasantness) was what was driving the boundary restriction effect. However, a related limitation was that we initially asked how arousing participants perceived the images to be, rather than how arousing the image actually was. This question was a limitation because participants may not know how to judge the arousal of an image, and may have many subjective interpretations of emotional arousal, whereas state affect allows participants to simply state how they feel, a much more intuitive question. To address this limitation in future studies, researchers should be careful to ask the question relating to participants' feelings, or instead use a measure specifically designed to measure participants' feelings. An example is the Subjective Units of Distress Scale (SUDs; Tanner, 2012), which accurately measures participants' emotional and physical discomfort. It is highly correlated with other measures of distress and accurately measures temporal changes in arousal (Tanner, 2012). Another suggestion to more accurately measure participants' arousal is to employ a method of *objectively* measuring arousal levels (e.g., Galvanic Skin Response measure).

A third limitation to our arousal manipulation was the fact that we only directly investigated aversive arousal, induced by an unpleasant noise stressor (though we did observe the effect of pleasant arousal in Experiment 7b). We know that pleasant and aversive arousal have similarities (e.g., they both activate the amygdala; Anderson et al., 2003; and both can improve memory accuracy for images; Bradley et al., 1992). But we do not know whether pleasant arousal leads an attentional focus, similar to what we see for aversive arousal (Mather, 2007). Indeed, to my knowledge there is no boundary judgement research that has demonstrated a boundary restriction effect for pleasant arousal. The question of whether pleasant arousal would induce boundary restriction errors in a similar way to aversive arousal is a question that goes unanswered in my thesis, and is an important one for future studies to explore. One possibility for eliciting pleasant and aversive arousal would be to use music, which has been used in previous studies to successfully alter arousal levels in both directions (e.g., Thompson, et al., 2001).

Last, a limitation of my arousal paradigm used in Experiments 5a – 7a was that the arousal was only present at encoding, and not at test. Recall that previous boundary restriction studies used stimuli that was in and of itself arousing, thus the effect of the arousal was present at both encoding and test (e.g., Mathews & Mackintosh, 2004). One way in which I mitigated this limitation (in Experiments 5a – 6) was to condition the arousal stimulus to the image type, which meant that participants associated the image type with the noise stressor. Thus, some of the arousal likely carried over to the test phase of the experiments. Indeed, this conditioning did seem to have an impact on boundary judgement errors, because in Experiment 7b, I found that the effect of the noise stressor on boundary restriction disappeared. Thus, it is possible that participants need to experience arousal at both encoding and test in order to observe a boundary restriction effect. Future studies could explore this possibility further by applying the same aversive arousal manipulation (e.g., the noise stressor used in Experiments 5a - 7a) at *both* encoding and test.

9.4.2 **Limitations of Valence Manipulation**

In Chapter 4, I manipulated the valence of images using prime statements. Though we managed to change participants' pleasantness ratings of the images in line with the prime statements, we cannot definitively rule out that participants were simply rating the valence of the *prime statements* rather than the *images*. We attempted to ensure that this possibility was unlikely by first ensuring that our rating question specifically asked participants to rate the pleasantness of the image, and second by separating the image and the prime in the last two experiments (Experiments 2d & 2e), to ensure that the focus of the question was indeed on the image and not the prime statement. Despite these measures, we cannot rule out this possibility, and thus we may have been observing the effect of mood on boundary judgement errors, rather than perceived image valence. However, the fact that the participants' affect did not change after viewing the images suggests that we were tapping into image valence rather than mood. Thus, we can conclude that boundary restriction does not occur simply because of the valence of an image. The results of this chapter lend further weight to my assertion that attentional-capture is the underlying mechanism at play.

9.4.3 **Limitations of Encoding and Test Paradigms**

I used different presentation times to manipulate attention, and different test paradigms to measure boundary judgement errors. The different test paradigms throughout my thesis began with forced choice—and then when limitations of that paradigm became evident—I followed up with camera distance paradigms (with Likert and slider scales). Importantly however, I did not systematically investigate each of these paradigm features in order to find out the best method for testing boundary restriction.

In terms of encoding, there is some evidence from my experiments that presentation time can affect boundary restriction (Experiment 1) and that the type of boundary judgement test can also affect the type of boundary judgement errors participants make (e.g., Experiments 3a, 3b, & 4). Since we know that valence can be determined in as little as 100ms (Sabatinelli, Lang, Bradley, Costa, & Keil, 2009), and that short presentation times do seem to increase boundary restriction judgements (Experiment 1), future studies should investigate whether boundary restriction would increase with even shorter presentation times in the presence of negative stimuli, or even using the attention manipulation (Experiment 4) or the arousal manipulation (Experiment 5a – 7a) to investigate whether short presentation times would exacerbate the boundary restriction effects I found.

Testing paradigms throughout my thesis included forced-choice decisions (Experiments 1, 3a, 3b, 5a, 5b, 7a, & 7b), camera distance paradigms (Experiments 2a, 2b, & 6) and last, slider camera distance ratings (Experiments 2c, 2d, 2e, & 4). Although all the paradigms used in my thesis have successfully demonstrated boundary restriction and boundary extension attenuation (e.g., Mathews & Mackintosh, 2004; Ménétrier, Didierjean,

& Vieillard, 2015; Safer et al., 1998; Takarangi et al, 2015), each has its limitations.

Forced choice requires participants to pick which option best matched their memory. But if none of the options given to the participant matches their memory, then they will be unable to make a choice that accurately reflects their memory. This inability to capture all memory errors means that we may miss some subtle memory effects (if for example the participant's memory *is* a boundary judgement error, but the identical option is closer to their memory of the image than the error option), and it also means we cannot assess the *extent* to which participants made a boundary judgement error.

Camera distance and slider rating scales overcome these limitations by allowing participants to indicate more subtle boundary judgements, and allow us to look at the extent to which participants make either error type. However, there is evidence that participants do not treat the anchors to the left and the right of slider and camera distance scales with equal attention. One way that we attempted to compensate for this bias was to counterbalance the side on which we put the anchor of the scales. Another limitation of these paradigms is that that the test may not be as intuitive as the forced choice paradigm. Participants are asked to imagine that the "camera distance" has changed, which is a difficult task and quite a different one from judging whether or not they have seen a particular image (at all) before. I attempted to mitigate this limitation by giving participants practice tests, where they could view what a "changed camera distance" may look like, to prepare them for making camera distance judgements. The camera distance task also may be more likely to activate imagination—where participants have to recall the image in detail in order to assess the camera distance. By contrast, in forced choice participants select from one or more options, and may be able to do so on the basis of familiarity and without specific recollection (see Yonelinas, 2002 for a review). Recall that imagining beyond the boundaries at encoding is a key part of our present understanding of why boundary extension occurs. It is possible that imagining beyond the boundaries of the recalled image during *retrieval* also affects how participants judge the boundary of an image. However, the assumption that imagination at retrieval *and* encoding can affect boundary judgement errors in the same way that imagination at encoding does has not yet been tested and is an avenue for future research.

To overcome these limitations, future studies could instead use a boundary adjustment task that could overcome this bias completely. Indeed, the lack of laterality effects on boundary judgement errors in Experiments 3a and 3b may have been due to the fact that we used a forced choice test, rather than a boundary adjustment task to measure boundary judgements. The test paradigm used in my experiments meant that we could only measure errors for the image as a whole, rather than boundary errors participants made for different sides of the images. This testing paradigm coupled with the fact that we presented images to each visual hemifield (rather than a single image over a participant's whole visual field), meant that we may have been unable to capture the overestimation of the right side that was observed in the original study.

9.4.4 **Individual Differences**

My thesis did not address the effect of individual differences on boundary judgement errors. Indeed, we know that people's own spatial ability can affect boundary judgements (Munger & Multhaup, 2016) with spatial imagery ability positively associated with boundary extension errors. People high (vs. low) spatial abilities tend to make more boundary extension errors. This individual difference could be explored further, by investigating whether the differences translate to boundary restriction errors as well as boundary extension errors. Munger & Multhaup's (2016) finding suggest that people with *low* (vs. high) spatial abilities would make more boundary *restriction* errors, making it a worthwhile question to explore.

Additionally, not all negative stimuli are equally attention-grabbing to all people; how salient an image or object is depends on many external factors and internal factors unique to the person viewing the image. For example, prior information (e.g., a known threat like a gun) or personal history (e.g., losing a friend in a car crash), will mean that images containing those cues will be more salient than for people who have not had those experiences. Moreover, trait anxiety may increase salience of highly negative images—presumably because people with high trait anxiety tend to over-attend to the highly negative content (Mathews & Mackintosh, 2004). The salience of particular stimuli is further limited by visual attention limitations (e.g., saccades), goal orientation (i.e., whether the stimulus is relevant to a person's current task), and even some inherent biases, such as visual biases (i.e., people tend to pay more attention to the left side of space). These internal and external factors may explain why the literature around boundary restriction for negative images has been so mixed. Thus, it seems that individual differences can affect not only people's reactions to images, but also their tendency to overestimate the size of space, and thus make boundary extension errors. Future research should specifically explore how these individual differences affect boundary judgement errors, particularly where participants have the opportunity to make boundary restriction errors.

9.4.5 Unexplored Image Constructs

Last, though my thesis did focus on disentangling the nexus of valence, attention-grabbing stimuli, and arousal, one element I did not focus on was the different types of emotion elicited by negative images. Indeed, negative images may elicit *different* responses when they are associated with different underlying emotions—such as anger, fear, disgust—even though they are all considered "negative" in valence (Levine & Piazzo, 2006). Of particular interest, there is emerging evidence that images that elicit disgust can capture and hold attention, and thus affect memory for these images. In one example participants viewed images that elicited disgust, fear, happiness, or neutral emotion, and identified a peripheral object in each image (van Hooff, van Buuringen, de Gier, & van Zalingen, 2014). Participants spent longest looking at the images eliciting disgust, suggesting that for these images, attention was captured and held more completely than for the other types of images. Thus, we might expect that images that capture attention in this way (via disgust) would lead to more consistent boundary restriction than other types of negative stimuli (via negative valence), due to their ability to capture and hold attention.

9.5 Practical Implications

My thesis experiments demonstrate that memory for events can be unpredictable. While we may feel that we "tunnel" into negative events, remembering only the salient parts, this may not be how our visual memory encodes negative information. Memory for negative events may indeed be limited to the attention-grabbing, arousing stimuli present during the event (e.g., a gun), or attention-grabbing stimuli related to a current goal (e.g., making sure a present family member is safe). Indeed, the results demonstrated here lend weight to the idea that eyewitnesses can miss seemingly "obvious" or "remarkable" details of a negative event. Once famous example is that of Boston police officer Kenny Conley, who failed to observe his fellow police officer being beaten up—ostensibly due to the fact that he was focussing on chasing a suspect while he ran right past it (see Chabris, Weinberger, Fontaine, & Simons, 2011). The investigators of the case assumed that because the visual information was there—and Conley had the *opportunity* to view it—then he must have seen it and was lying; he was charged with perjury. Conley's case was eventually overturned, on evidence that people can indeed miss vital information if they are focussed on goals unrelated to the "remarkable" visual information that may appear in front of them. In the real world, this potential to miss vital details due to competing visual information is especially relevant in situations where elements such as the presence of a weapon, or the perpetrator, attract attention, or in cases of intense arousal. In a laboratory setting, we see the following: when participants' attention was directed (by aversive arousal; Experiments 5a-7a; by abrupt visual onset stimulus; Experiment 4), they misremembered images as being less expansive than they actually were. If the periphery contained other useful information (e.g., a get-away car) while an attention-grabbing stimulus was present (e.g., a weapon) the "useful" peripheral information is likely to be missed. Although this sort of memory error is clearly problematic, boundary restriction could also—in some cases—convey a memory *advantage*. Specifically, recall that internally generated information (from imagination and extrapolation) can be confused with visual sensory information, resulting in a source monitoring error, and a "false memory" of extended boundaries (Intraub & Dickinson, 2008). Without imagination and extrapolation—i.e., which does not happen in cases of boundary restriction—"extra" information is not incorporated into the original memory. Thus, despite real-world limitations, my research adds to this body of evidence demonstrating that eyewitness memory for visual details is oftentimes incomplete. My research here demonstrates that memory errors can occur depending on many factors, including where people attention is directed at the time, and thus adds to the growing body of evidence which demonstrates the often difficult job that eyewitness must undertake when recalling details of a crime.

9.6 Conclusion

Visual memory is complex and prone to errors. Boundary restriction—the visual memory error whereby people remember the boundaries of a scene as being much more restricted than they actually were—seems to occur after viewing negative stimuli, or following negative events. This phenomenon is contradictory to the natural memory process of boundary extension—the visual memory error whereby people recall the boundaries of scenes to be much wider than they actually were. Boundary restriction has been difficult to replicate in the lab, with many studies failing to find the effect, and often finding that boundary extension is the more common visual memory error. This difficulty in replicating boundary restriction in the lab may have been due to the fact that boundary restriction occurs not as a result of viewing negative images, but rather due to the incidental attention-capture that occasionally occurs when viewing such images. This thesis focused on isolating the factors that accompany negative images, to investigate which of these factors contribute to the boundary restriction effect. My findings suggest that boundary restriction only occurs when attention is grabbed in such a way that extrapolation, imagination, and contextualising beyond the scene is curtailed—such as when someone is in a state of high arousal. Thus, I conclude that boundary restriction may be a natural visual memory error, but one that occurs only under specific circumstances, and not one that occurs for all negative visual scenes. Boundary extension remains the more common visual memory error, and is only reversed under specific, highly arousing circumstances.

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Appendix A – State-trait anxiety inventory

A number of statements which people have used to describe themselves are given below. Read each statement and then circle the appropriate number to the right of the statement to indicate how you feel *right now*, that is, *at this moment*. There are no right or wrong answers. Do not spend too much time on any one statement but give the answer which seems to describe your present feelings best.

		NOT AT ALL	SOMEWHAT	MODERATELY	VERY MUCH SO
1.	I feel calm…………………………………………………………………………	1	2	3	4
2.	I feel secure……………………………………………………………………….	1	2	3	4
3.	I am tense…………………………………………………………………………	1	2	3	4
4.	I feel strained……………………………………………………………………..	1	2	3	4
5.	I feel at ease………………………………………………………………………	1	2	3	4
6.	I feel upset………………………………………………………………………	1	2	3	4
7.	I am presently worrying over possible misfortunes…………………………..	1	2	3	4
8.	I feel satisfied…………………………………………………………………….	1	2	3	4
9.	I feel frightened…………………………………………………………………..	1	2	3	4
10.	I feel comfortable…………………………………………………………………	1	2	3	4
11.	I feel self-confident……………………………………………………………….	1	2	3	4
12.	I feel nervous……………………………………………………………………..	1	2	3	4
13.	I am jittery…………………………………………………………………………	1	2	3	4
14.	I feel decisive……………………………………………………………………..	1	2	3	4
15.	I am relaxed……………………………………………………………………….	1	2	3	4
16.	I am worried……………………………………………………………………….	1	2	3	4
17.	I feel confused……………………………………………………………………	1	2	3	4
18.	I am worried……………………………………………………………………….	1	2	3	4
19.	I feel steady………………………………………………………………………..	1	2	3	4
20.	I feel pleasant……………………………………………………………………..	1	2	3	4

Appendix B – IAPS images used in chapter 3

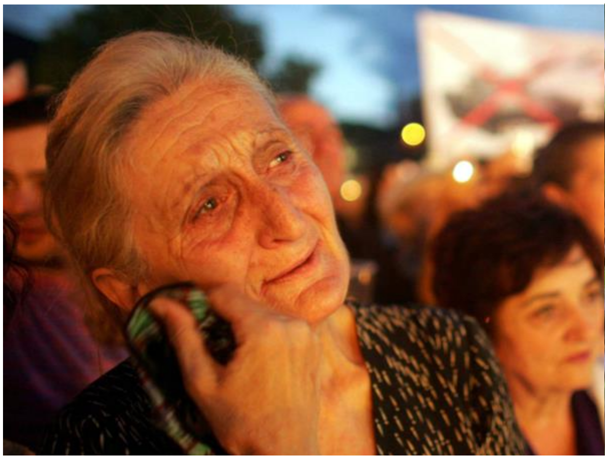
Neutral images	Negative images	Extreme negative subset
1122	3053	3053
1460	9410	9410
1500	9183	9183
1645	3069	3069
2019	3010	3010
2235	3068	3068
2342	3530	3530
2345	3225	3225
2359	3261	3261
2381	9635.1	9635.1
2385	2703	2703
2398	9571	9571
2435	9253	
2442	3230	
2594	9254	
2595	9560	
4574	3150	
5471	9901	
5831	6315	
7497	3061	
7503	2900	
7509	9400	
8040	9561	
9913	8485	

Appendix C – IAPS images used in Experiments 2a – 2d, and Example of images used in Experiment 2e

Porter et al.'s (2014) IAPS images
4598
7620
8117
8190
8300
8400

1. Image taken for our study

2. IAPS image Photoshopped for our study

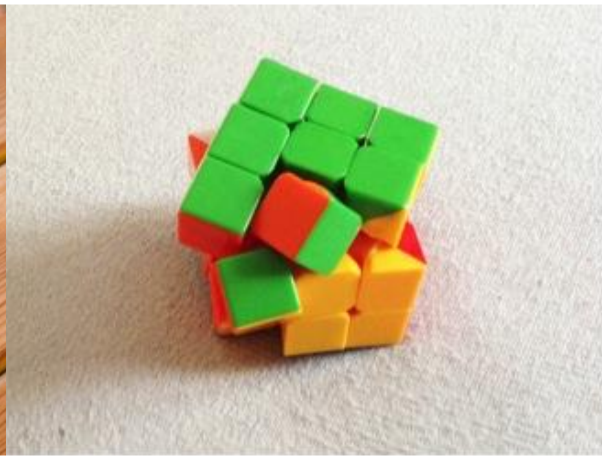


3. Image from Shutterstock

4. Unaltered IAPS image

Appendix D – Example of images used in chapter 5






Appendix E – Flinders handedness survey (FLANDERS)

The ten questions below ask which hand you prefer to use in a number of different situations. Please tick one box for each question, indicating whether you prefer to use the left-hand, either-hand, or the right-hand for that task. Only tick the 'either' box if one hand is truly no better than the other. Please answer all questions, and even if you have had little experience in a particular task, try imagining doing that task and select a response.

		Left	**Either**	**Right**
1	With which hand do you write?			
2	In which hand do you prefer to use a spoon when eating?			
3	In which hand do you prefer to hold a toothbrush when cleaning your teeth?			
4	In which hand do you hold a match when you strike it?			
5	In which hand do you prefer to hold the rubber when erasing a pencil mark?			
6	In which hand do you hold the needle when you are sewing?			
7	When buttering bread, which hand holds the knife?			
8	In which hand do you hold a hammer?			
9	In which hand do you hold the peeler when peeling an apple?			
10	Which hand do you use to draw?			

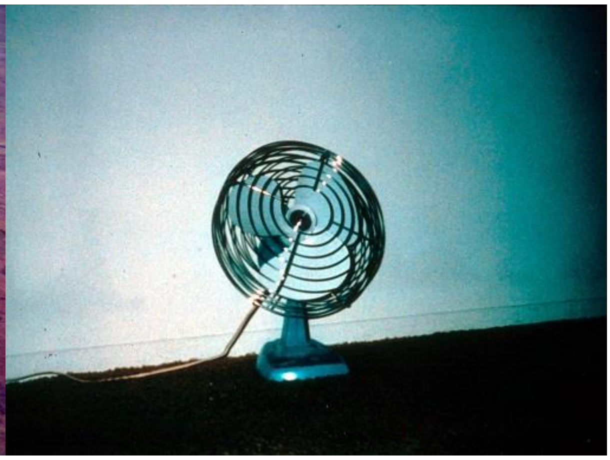
Handedness score (please don't fill this out)	

Appendix F – Example of IAPS, Shutterstock, Photoshopped and in-house developed images used in chapter 6

3. Image taken for our study

4. IAPS image Photoshopped for our study

4. Image from Shutterstock

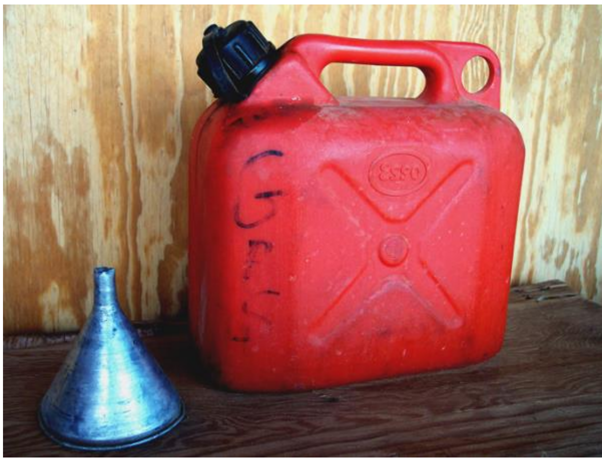


5. Unaltered IAPS image

Appendix G – Example of photoshopped images used in chapters 7 and 8

Original

Photoshopped (nature)



Original

Photoshopped (object)

Original

Photoshopped (nature)