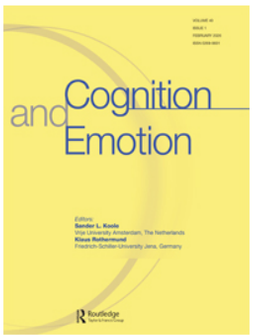

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REGISTERED REPORT

A registered conceptual replication and extension of Gable and Harmon-Jones (2008): does motivational intensity, valence, or perceptual focality drive attentional breadth?

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ABSTRACT

How do emotion and motivation affect the breadth of attention? Competing theoretical accounts propose that valence (pleasantness) or motivational intensity (strength of the urge to approach/avoid) drive changes in attentional breadth. Seminal work by Gable and Harmon-Jones (Gable, P. A., & Harmon-Jones, E. (2008). Approach-motivated positive affect reduces breadth of attention. *Psychological Science, 19*(5), 476–482, Study 2) found that pictures of desserts narrowed attentional breadth relative to pictures of rocks. This was interpreted as higher motivational intensity narrowing attentional breadth, but the desserts were rated as higher in positive valence than the rocks and may have differed in perceptual focality – confound(s) commonly present in the broader literature. Further, recent work questions whether emotion/motivation has reliable effects on attentional breadth. Here, therefore, we conducted a registered conceptual replication where we assessed whether the narrowing of attentional breadth following dessert (vs rocks) pictures replicated with a new picture set. We also assessed any unique contribution of motivational intensity, valence, and perceptual focality to driving changes in attentional breadth. Participants rated the desserts as more positive in valence and higher in motivational intensity than the rocks. However, attentional breadth was invariant across image type and perceptual focality, and across participant ratings of valence and motivational intensity. These findings challenge the robustness with which emotion and motivation influence attentional breadth.

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Attentional breadth

There is typically far too much information in visual scenes for our brains to process at once. Visual attention plays a key role in selecting certain aspects of the visual scene for privileged processing, at the expense of others (e.g. Fiebelkorn & Kastner, 2020). Humans can regulate their visual attention in various ways, such as shifting the central focus of attention to different regions (i.e. shifts of attention) and regulating the spatial extent of the attended region (i.e. changing attentional breadth). Spatial attention has been likened to a spotlight, such that attended stimuli are illuminated within the spotlight's beam while the unattended parts of the scene are in darkness (Posner et al., 1980). Attentional breadth refers to the spatial extent of the attended region and can be thought of as the width of the spotlight's beam (see Figure 1). Thus attentional breadth can be broad, encompassing a whole visual scene, or narrow, honing in intensely on a single object (for a review, see Goodhew, 2020). Critically, attentional breadth affects the perceptual fidelity with which objects are perceived, and

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Figure 1. An illustration of attentional breadth.

Note. The small yellow circle illustrates a narrow attentional breadth in which the fine spatial details of the individual petals can be resolved, versus a broad attentional breadth in which a larger chunk of the scene (the "bigger picture") is attended, but at lower resolution.

whether they are perceived at all (e.g. Eriksen & St. James, 1986; Goodhew, 2020; Jefferies et al., 2015; Lawrence et al., 2020; Müller et al., 2003).

Driver(s) of attentional breadth

A variety of factors appear to influence attentional breadth, such as a person's cognitive load (Ahmed & de Fockert, 2012), their personality (Wilson et al., 2016), athletic expertise (Hüttermann et al., 2014), and their age (Lawrence et al., 2018). But one aspect that has perhaps evoked the greatest interest is the influence of emotional states on attentional breadth. According to one influential model, the *valence* (i.e. pleasantness) of a person's current emotional state influences attentional breadth, such that positive valence emotions (e.g. happiness, contentment) broaden attentional breadth while negative emotions (e.g. sadness, anxiety) narrow it (e.g. Basso et al., 1996; Fenske & Eastwood, 2003; Fredrickson, 2003; Fredrickson & Branigan, 2005; Hüttermann & Memmert, 2015; Rowe et al., 2007; Wadlinger & Isaacowitz, 2006). In contrast, the motivational intensity model proposes that it is instead the *motivational intensity* (i.e. strength of the urge to approach or avoid) of an emotional state that determines attentional breadth, irrespective of valence. Emotional states high in motivational intensity (e.g. desire, disgust) are proposed to narrow attentional breadth, while emotional states low in motivational intensity (e.g. contentment, sadness) to broaden it (e.g. Domachowska et al., 2016; Gable & Harmon-Jones, 2008, 2010a). The motivational intensity model was put forward in response to the valence-based models. That is, the proponents of the model argued that the previous evidence that appeared to support the valence-based model was a spurious product of experimental designs that confounded valence with motivational intensity. More specifically, the proponents suggested that studies testing the valence-based account employed low motivational intensity positive emotions and high motivational intensity negative emotions, and therefore the observed pattern of results could actually be explained by the motivational intensity account (Harmon-Jones et al., 2012). The motivational intensity model has sparked widespread interest as a potential determinant of attentional breadth, as well as other aspects of attention, cognition, and physiology (e.g. Chan & Saunders, 2023; Domachowska et al., 2016; Gable et al., 2015; Gable & Harmon-Jones,

2010b; Gable & Poole, 2012; Gable, Neal, et al., 2016; Kaczmarek et al., 2019; Liu & Wang, 2014; Price & Harmon-Jones, 2011; Roberts et al., 2017; Threadgill & Gable, 2019b).[1] Here, however, our focus is specifically on the driver(s) of *attentional breadth*.

Gable and Harmon-Jones (2008) is a seminal study which claims that motivational intensity drives attentional breadth

A seminal paper in the motivational intensity literature is that of Gable and Harmon-Jones (2008). In this paper, across four studies, the authors claimed to demonstrate that motivational intensity drives attentional breadth, and this claim has shaped subsequent thinking and investigation. However, as we articulate below, there are some reasons to question this conclusion. Here we focussed on their key study, namely Study 2, in which they presented participants with pictures of desserts (intended to be higher in motivational intensity) and rocks (intended to be lower in motivational intensity). We chose to focus on this Study for two key reasons. First, dessert stimuli have frequently featured in subsequent research on the relationship between motivational intensity and both attentional breadth and other aspects of cognition (e.g. Domachowska et al., 2016; Gable & Harmon-Jones, 2010b; Gable & Poole, 2012; Harmon-Jones & Gable, 2009; Kotynski & Demaree, 2017; Liu & Wang, 2014; Roberts et al., 2017; Threadgill & Gable, 2019a). Second, their Study 2 uses a measure of attentional breadth that has been well-validated – responses to Navon stimuli. While many paradigms exist that claim to operationalise attentional breadth, Navon stimuli stand out as having clear validating evidence that they operationalise attentional breadth as intended (Goodhew, 2020; Sasaki et al., 2001). Therefore, Study 2 by Gable and Harmon-Jones (2008) represents an important study in the literature that is potentially highly informative regarding the determinant/s of attentional breadth. The results of this original study indicated that attentional breadth was narrower following exposure to pictures of desserts than of rocks, and the authors interpreted this as higher motivational intensity narrowing attentional breadth. However, there are alternative explanations for this observed result, as outlined below. Therefore, we conducted a registered conceptual replication and extension of this seminal work, where we tested the robustness of the effect with a different stimulus set, and sought to disentangle competing alternative explanations for the results. We explain the rationale for this below.

Rationale for a conceptual replication of Gable and Harmon-Jones (2008) Study 2

More recent work has called into question the replicability of the effects of emotional and motivational states on attentional breadth. That is, multiple studies have observed no reliable influence of affective states on attentional breadth (e.g. Bruyneel et al., 2013; Chan & Saunders, 2023; Kolnes et al., 2022). It is therefore important to establish the replicability of the narrowing of attentional breadth in the seminal work driving this programme of research, such as Gable and Harmon-Jones' (2008) Study 2 using desserts and rocks. We could use the identical images as Gable and Harmon-Jones in an *exact* or *direct* replication. However, *conceptual* replications, which are those that are designed to test the same theoretical processes as the original study without the identical stimuli, can provide stronger evidentiary value for theories than direct replications (Crandall & Sherman, 2016). Here, therefore, we conducted a conceptual replication with a different picture set. Further, conducting a conceptual replication with a different picture set allowed us to address multiple potential confounds, and hence test alternative explanations for the results, as articulated below.

The experimental designs used in Gable and Harmon-Jones' (2008) four studies, and much of the literature since, have confounded differences in *motivational intensity* with differences in *valence* when examining the drivers of attentional breadth. Therefore, it is at least equally likely that the observed effects in these studies reflect valence, rather than motivational intensity influencing attentional breadth. This means that it is still an open question which of these dimensions drives attentional breadth. Further, while not all studies in the literature have used dessert versus rock images like Gable and Harmon-Jones (2008, Study 2), many have, and such stimulus categories likely differ in their perceptual focality (i.e. the extent to which the stimulus depicts one single versus a broader constellation of objects). This means that the intended affective differences between dessert and rocks are confounded with low-level perceptual differences, which could be responsible for the observed differences in attentional breadth between dessert and rocks conditions, without any change in affective state. Alternatively,

these perceptual differences could undermine manipulations of affective state, if the effects of perceptual focality and affective state having countervailing effects on attentional breadth (i.e. one narrows while the other broadens). Therefore, in addition to testing the conceptual replicability of dessert pictures narrowing attentional breadth relative to rock pictures, we also sought to make an important novel contribution: assessing the unique contribution of valence, motivational intensity, and perceptual focality to determine which, if any, drives attentional breadth. In doing so, we acknowledge that more than one could be drivers (e.g. both valence and perceptual focality could be independent drivers). Below we explain in more detail each of these confounds and articulate how the present study addressed them.

The valence confound when comparing the effect of desserts versus rocks to operationalise the influence of motivational intensity

Although motivational intensity was proposed to supersede valence as the determinant of attentional breadth (Harmon-Jones et al., 2012), empirical testing of the motivational intensity model has often failed to adequately control for differences in the degree of valence between conditions. This means that observed differences in attentional breadth between conditions could be the product of valence, rather than motivational intensity. Indeed, in the original Gable and Harmon-Jones (2008) Study 2, participants rated the stimuli designed to be higher in motivational intensity (i.e. desserts) as more pleasant (i.e. higher in *valence*) than the stimuli designed to be lower in motivational intensity (i.e. rocks). Participant ratings of motivational intensity were not collected. As a result, there is direct evidence that the dessert and rocks experimental conditions differed with respect to degree of valence, but not motivational intensity – yet the observed behavioural differences between the conditions were attributed to motivational intensity rather than valence.

Where there is theoretical debate regarding whether dimension *x* or *y* is the driver of a given psychological process, we think that it is problematic to establish that conditions differ along dimension *x* and yet interpret the results in favour of dimension *y*. However, this practice is common in the literature more broadly, with multiple studies attributing differences in behaviour (i.e. attentional breadth) between experimental conditions to motivational intensity despite only establishing that the experimental conditions differ with respect to ratings along the dimension of valence (e.g. Gable & Harmon-Jones, 2010a, 2011; Threadgill & Gable, 2019b). Similarly, others have demonstrated that the experimental conditions differ in the degree to which they elicit ratings of positive valence emotion states or categories (e.g. happy) and have used this to infer that the conditions differed with respect to motivational intensity, which was not directly measured (see Gable & Harmon-Jones, 2008 Study 4, expectancy-to-consume desserts vs neutral).[2] However, this approach is problematic, because increased happiness is consistent with increased positive valence, indicating that these conditions differed along the dimension of valence. This means that valence, rather than motivational intensity, could be the driver of these changes in attentional breadth.

Valence and motivational intensity typically covary to a considerable extent, such that people want to approach pleasant stimuli and avoid unpleasant ones, with some specific exceptions (Campbell et al., 2021, 2023; Gable & Dreisbach, 2021; Kaczmarek et al., 2021). This means that it is likely that these experimental conditions that were demonstrated to differ with respect to valence also differed with respect to motivational intensity as well. But it means that these two dimensions are confounded, and so the observed behavioural results (i.e. effects on attentional breadth) cannot be attributed selectively to either dimension. See Figure 2 for an illustration of this confound.

Other approaches to operationalising motivational intensity in the literature have, in our view, also not satisfactorily disentangled motivational intensity and valence. Some studies have not reported rating data on either dimension (e.g. Gable, Mechin, et al., 2016; Harmon-Jones & Gable, 2009), which means it is possible that neither dimension was varied as intended, and even if motivational intensity was manipulated, given the high correlation between these dimensions, it is likely that valence and motivational intensity were confounded. Others have assumed that a particular discrete emotion induces a specific motivational direction or intensity, even when their participants' ratings of motivational intensity do not support these assumptions (Gable et al., 2015).[3]

Further, some work assumes that where any two conditions belong to the same valence polarity category (e.g. positive), this is sufficient for them to be

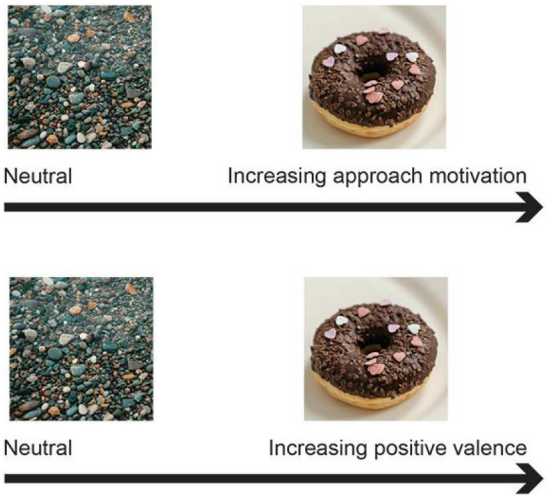

Figure 2. An illustration of how a given stimulus comparison (e.g. desserts versus rocks) can differ along the dimensions of both valence and motivational intensity, creating a confound.

Note. Both valence and motivational intensity are dimensions, as indicated by the arrows. Desserts can be higher in positive valence as well as higher in motivational intensity, creating a confound between these two dimensions when these two conditions are compared on outcomes such as attentional breadth. For simplicity, we have omitted avoidance motivation and negative valence, but the same is true there: an affective state like sadness can differ from neutral along the dimensions of both valence and motivational intensity. Further, two negative affect states, such as disgust and sadness, can differ along the dimensions of both valence and motivational intensity – they are not matched for valence simply by virtue of belonging to the same valence category (e.g. negative valence) because valence is a continuous dimension (i.e. they could fall at different points along this dimension, such that one could has stronger negative valence than the other).

equated with respect to valence (e.g. Gable & Harmon-Jones, 2008).[4] This assumption is incorrect. Valence has long been conceptualised and measured as a dimension (e.g. Lane et al., 1997; Lang et al., 1993; Lang et al., 1998) and can produce differential and even non-monotonic changes in physiological outcomes across that dimension (e.g. Lang et al., 1993). Therefore, we believe that it is important to both conceptualise and measure valence as a dimension. Crucially, this means that two affective states that belong to the same broad valence polarity (e.g. desire and contentment) can still differ along the dimension of valence (i.e. in their degree of positive valence), making valence a viable explanation for observed differences in attentional breadth between such conditions.

Liu et al. (2014) laudably collected systematic ratings of both valence and motivation and were able to assess the effect of motivational intensity on attentional breadth while holding valence constant. That is, they examined the influence of positive states high versus low in motivational intensity, and these conditions did not significantly differ in their ratings along the dimension of valence at the group level; and they examined the influence of negative states high versus low in motivational intensity, and these conditions did not significantly differ in their ratings along the dimension of valence at the group level. As indexed by Navon task performance, they found that states higher in motivational intensity narrowed attentional breadth (relative to both states lower in motivational intensity, and a neutral condition), and this was true for both positive and negative conditions. Liu et al.'s (2014) approach was rigorous in controlling for the effect of valence when examining motivational intensity. However, our approach advances on theirs in two key ways: (1) we treat both valence and motivational intensity as dimensions, rather than discrete categories, which can be more sensitive; and (2) we used individuals' own ratings of valence and motivational intensity, rather than group-level averages, which accounts for individual differences in affective responses to the stimuli. This personalisation can be important, as discussed below.

To summarise, despite this literature being active for almost two decades since the seminal Gable and Harmon-Jones (2008), most studies have failed to systematically distinguish the dimensions valence and motivational intensity and assesses the selective contribution of each to driving attentional breadth. This what we sought to do here in our conceptual replication.

In discussing the confound between valence and motivational intensity present in much of the previous literature, it is important to acknowledge that the Broaden and Build model (Fredrickson & Branigan, 2005) espouses that heightened positive valence broadens attentional breadth. Therefore, the narrowing of attentional breadth observed in Gable and Harmon-Jones (2008) Study 2 following desserts that were more strongly positive valence compared with rocks is *not* consistent with the predictions from this valence-based model. However, valence could still be the determinant of attentional breadth, but in a different way to that predicted by Broaden and Build. For example, it could be more extreme valences (i.e. intense positive or intense negative) narrow attentional breadth relative to less extreme valence (this possibility was suggested in Campbell et al., 2021). This would explain why the more positive valence dessert stimuli narrowed attentional breadth. Therefore, valence remains a viable

candidate driver of attentional breadth in addition to motivational intensity.

We also wish to highlight that we did *not* seek to favour any particular theoretical model here. Instead, we remain genuinely curious about the potential drivers of attentional breadth. Therefore, our goal was to carefully operationalise both valence and motivational intensity concurrently, and to observe which, if either, drives attentional breadth.

Addressing the valence confound in the present study

It is paramount to experimental science that (a) when an independent variable is manipulated, other variables are held constant or controlled for, and (b) the dependent variable validly operationalises the intended construct. We cannot envisage how it is possible to satisfy criterion (a) when assessing the drivers of attentional breadth unless valence and motivational intensity are each independently operationalised. This means that ratings of one of these variables cannot be used to operationalise the other. Neither can it simply be assumed that one of these variables is selectively experimentally manipulated in the absence of demonstrable evidence that this is the case (e.g. assuming that anger = higher approach motivation). Instead, in our view, they must each have their own unique and valid operationalisation.

As reviewed above, many common experimental manipulations have failed to adequately distinguish between valence and motivational intensity. Indeed, our review of the previous literature has identified only one published study (Liu et al., 2014) that meets both criteria (a) and (b) for assessing the impact of valence and motivational intensity on attentional breadth. Commendably, some other studies have independently operationalised valence and motivational intensity and assessed the unique contribution of each dimension, but they were investigating outcomes other than attentional breadth (Kaczmarek et al., 2019; Kaczmarek et al., 2021), and therefore cannot adjudicate regarding the drivers of attentional breadth (i.e. they do not satisfy criterion (b), which is understandable given they were not designed to gauge attentional breadth). The validity of different attentional breadth operationalisations is discussed at length in Goodhew (2020), but the crux is that the there is good reason to think that the Navon paradigm used in Gable and Harmon-Jones (2008) Study 2 satisfies criterion (b). Therefore, independently measuring and then disentangling the effect of valence versus motivational intensity on this metric is a substantive contribution that can help to clarify the driver(s) of attentional breadth. While Liu et al. (2014) also did this, we sought achieve this in a way that respects the individual variation in ratings, which can be important, as discussed below.

One way of de-confounding valence and motivational intensity would be to vary one while holding the other constant (indeed, this is the approach taken by Liu et al., 2014). However, recent research has shown dramatic individual differences in both the direction and intensity of motivational ratings that a given context elicits, in contrast to valence ratings that are more similar across people (Campbell et al., 2023). For example, a sad image of a child crying elicits a negative valence emotion in most or all participants, while this same image elicits a wide variety of motivational directions (approach versus avoidance) and intensities of these motivations across participants (Campbell et al., 2023). Therefore, it is difficult to create experimental conditions that cleanly and consistently dissociate valence and motivational intensity for most or all participants. (We note that while Liu et al. (2014) observed average differences in ratings between their conditions, it is interesting that they had to drop multiple pictures from the analysis because of inconsistency in ratings, suggestive of individual variation). Instead, one practical solution is to use image categories that invoke a range of emotions (here, desserts versus rocks), collect individual participant ratings to measure the valence and motivational intensity elicited, and use these to statistically assess the unique contribution of valence and motivational intensity to explaining variance in attentional breadth. This is what we intended to do here.

The rationale for using ratings to disentangle valence and motivational intensity is strong. Previous evidence indicates that participants can provide fine-grained information about their emotion and motivation via explicit ratings (e.g. Campbell et al., 2021, 2023). Ratings on the valence dimension correlate with a range of objective physiological measures such as skin conductance, facial electromyography, and brain activation in visual cortex (Lane et al., 1997; Lang et al., 1993, 1998), while explicit ratings on the dimension of motivation correlate with an implicit behavioural measure of motivation (Campbell et al., 2021). This approach – of collecting ratings on multiple dimensions and assessing the contribution of each – has been successfully used to assess the

impact of different emotional dimensions (i.e. valence and arousal) on other perceptual-attentional outcomes such as *emotion-induced blindness* (Onie & Most, 2021; Singh & Sunny, 2017), as well as the impact of valence versus motivational intensity on physiological responses (Kaczmarek et al., 2019). It is also recommended as contemporary best practice (Campbell et al., 2024). Here, we applied this approach to distinguish between the impact of valence versus motivational intensity resulting from dessert versus rocks images on attentional breadth.

Potential perceptual confound when comparing the effect of desserts versus rocks on attentional breadth

The use of desserts versus rocks may have introduced a perceptual confound that can explain the observed results, independent of emotion, in the studies that used such stimuli. Typical dessert pictures more often consist of a single, salient central object, whereas rocks more commonly consist of a more diffuse texture array of rocks without a single, salient object (see Figure 3).

This is problematic because one of the most robust ways to experimentally manipulate attentional breadth is to have participants attend to small versus large stimuli (Goodhew & Edwards, 2016; Lawrence et al., 2020) or a single object versus a collection of objects (Belopolsky et al., 2007; Chong & Treisman, 2005). This means that it is possible that perceptual differences between the conditions alone could have elicited the changes in attentional breadth. That is, more focal dessert pictures could have narrowed attentional breadth relative to the more diffusely spread rock pictures, due to their differences in focality, not due to differences in the emotion or motivation that they induce.[5]

That said, in many studies where attentional breadth is experimentally manipulated, the inducing stimuli (e.g. the single object versus collection of objects) are explicitly task-relevant (e.g. participants are instructed to identify the orientation of a small versus large object). In contrast, in the designs typically used to assess the influence of emotion or motivation on attentional breadth, including the design in Gable and Harmon-Jones (2008) Study 2, the pictures are not task-relevant, and instead participants are merely viewing them. It is unclear whether viewing images in the absence of specific task requirements is sufficient to induce changes in attentional breadth. Here, therefore, we directly tested the impact of perceptual focality (i.e. from focal to diffuse) of pictures that are merely viewed on attentional breadth.

It should be noted that Gable and Harmon-Jones (2008) reported that their Study 2 pictures "were matched for colour, brightness, and object size". However, no information was given about how this was done, and their pictures were not made publicly available, making it difficult to independently assess whether this matching was successful in achieving

Figure 3. Example dessert and rocks pictures.

Note. Dessert pictures commonly consist of a single, focal object, whereas pictures of rocks often consist of a more diffuse collection of objects. This can create a perceptual confound when dessert and rock pictures are used to manipulate motivational intensity.

its aim. Here, our goal was to assess the potential influence of perceptual focality and to provide detailed information about how we did so (see Supplementary Material), including making all the specific images publicly available (see Appendix).

It is also important to qualify that there are studies in the literature that have sought to manipulate motivational intensity in a way that likely eliminates the perceptual focality confound, such as the use of similar-size geometric shapes that signal the availability of reward (pre-goal) versus receipt of reward (post-goal) (Gable & Harmon-Jones, 2011). Therefore, the focality confound issue only applies to studies that have used certain stimulus types, such as dessert versus rock images. However, while the results of Gable and Harmon-Jones (2011) *cannot* be explained the focality confound issue, they *are* susceptible to the valence alternative explanation described in the previous section. This is because participants provided significantly higher valence ratings in the pre-goal condition relative to the post-goal condition. This means that the results do *not* support the conclusion that motivational intensity, rather than valence, drives attentional breadth. In our review of the literature, while we have identified studies that eliminate the perceptual focality confound when attempting to manipulate motivational intensity, these studies still had the valence confound. That is, with one possible exception, we have not identified any studies that are immune to *both* the valence and perceptual focality alternative explanations when assessing the drivers of attentional breadth. This one possible exception is Liu et al. (2014) – they used a range of stimulus categories rather than desserts versus rocks, and thus may not be affected by the perceptual focality issue, and these authors *did* control for valence when assessing motivational intensity. But as articulated above, they did not assess the relative contribution of both valence and motivational intensity when these are treated as dimensions, and they assessed the effect of group-average-level rather than individual level ratings. Given the marked individual variation in motivational intensity in response to identical stimuli that has recently been found (Campbell et al., 2023), we believe that there is utility to assessing individual level ratings as predictors, and this is what we did here. Liu et al. (2014) also did not explicitly test the role of perceptual focality, and our goal here was to do this too. Below we explain how.

Addressing the perceptual focality confound in the present study

To assess the impact of perceptual focality on attentional breadth, we experimentally varied perceptual focality in a way that is fully crossed with image type (dessert versus rocks). Perceptual focality is a relatively low-level perceptual dimension that is less subject to individual differences than a dimension like motivational intensity. Therefore, it is possible to create experimental conditions that differ in their perceptual focality in consistent ways across participants. We conducted a pilot rating study (see Supplementary Material) to identify dessert and rock images that are low versus high in perceptual focality. This allowed us to assess the effect of an experimental manipulation of focality (crossed with image type) on attentional breadth.

Domachowska et al. (2016) conceptual replication still leaves questions unanswered

Domachowska et al. (2016) conducted a replication of Gable and Harmon-Jones (2008) Study 2, which included a direct replication (using the same desserts versus rock pictures as Gable and Harmon-Jones (2008) Study 2 to operationalise high versus low motivational intensity respectively) and a conceptual replication (using food versus animals/flowers to operationalise high versus low motivational intensity respectively). Domachowska et al. (2016) observed the narrowing of attentional breadth following desserts or food (relative to rocks or animals/flowers), consistent with Gable and Harmon-Jones (2008) Study 2.

Still, Domachowska and colleagues' work does not adequately disentangle valence and motivational intensity as potential drivers of attentional breadth. Domachowska and colleagues collected participants' ratings of the valence, arousal, and *desire* in response to their stimuli. Previous studies also collected desire ratings (e.g. Gable & Harmon-Jones, 2008, Study 2). One argument is that *desire* could be said to approximate *motivational intensity*; in which case such studies could be said to differentiate valence and motivational intensity. However, there are several issues with this argument. First, desire is not an ideal measure of motivational intensity, because either a strong avoidance motivation or low motivational intensity could produce similar low desire ratings, despite differing in their motivational intensity. Second, as the ratings Domachowska and colleagues

collected in both their direct and conceptual replication demonstrate, the experimental conditions with significantly higher desire ratings also had significantly higher valence ratings. This means that even if we accept the claim that desire indexes motivational intensity, then this still leaves valence and motivational intensity confounded when comparing high versus low desire conditions. If this claim is unfounded, then desire ratings merely (redundantly with the valence ratings) confirm that these conditions differ only with respect to valence. Either way, the selective contributions of valence and motivational intensity to attentional breadth were not assessed. This means that these studies cannot identify whether valence or motivational intensity was the driver of the differences in attentional breadth observed between their experimental conditions. There is thus a need for further work that uses ratings on dimensions which cleanly separate valence and motivational intensity, and then the unique effects of both dimensions to predicting observed attentional breadth behaviour are assessed. This is the approach we took here. More broadly, our novel conceptual replication assesses the unique contributions of valence, motivational intensity, and focality to attentional breadth to make a valuable contribution that goes beyond the existing literature, including Domashowska and colleagues' replication.

Present study

To summarise, in their Study 2, Gable and Harmon-Jones (2008) found that pictures of desserts narrowed attentional breadth relative to pictures of rocks. The authors interpreted this as evidence that *motivational intensity* narrows attentional breadth, and this interpretation has influenced scientific thinking for many years. However, rating data demonstrate that the conditions differed with respect to where they fell along the dimension of *valence*. While the tight covariance between valence and motivational intensity means that the conditions likely also differed with respect to motivational intensity, it leaves both dimensions as viable candidate drivers for the observed differences in attentional breadth. This means that their study results *cannot* adjudicate whether valence or motivational intensity is the driver of attentional breadth. Further, it is unclear to us whether the likely confounds in perceptual focality that arise when identifying images of desserts versus rocks was adequately addressed. In addition, other work has questioned the replicability of any affective influence of attentional breadth. Here, therefore, we addressed two key research questions: (1) Do pictures of desserts (versus rocks) narrow attentional breadth when using a different picture set to Gable and Harmon-Jones (2008) Study 2? (2) If so, do differences in valence, motivational intensity, and/or perceptual focality between desserts and rocks drive this effect?

To test this, we compared participants' attentional breadth between four different conditions: after viewing dessert or rock pictures that were either perceptually focal or diffuse. Participants rated the valence and motivational intensity of the affective states that the pictures induced. Based on previous findings, we anticipated that participants would on average experience both more positive valence and higher motivational intensity following dessert relative to rock pictures. If motivational intensity selectively drives attentional breadth, then attentional breadth should be *narrower* following dessert pictures (irrespective of focality), and participants' ratings of motivational intensity should explain significantly more variance in attentional breadth than their valence ratings. If valence selectively drives attentional breadth, then attentional breadth should be significantly *different* following dessert versus rock pictures (irrespective of focality), and participants' ratings of valence should explain significantly more variance in attentional breadth than their motivational intensity ratings. While the broaden-and-build model specifically predicts broader attentional breadth following desserts relative to rocks, we note other valence-based models (e.g. extremities of valence idea) predict the reverse; that is, the pattern that the motivational intensity model also predicts. This is why the predictions for valence are in relation to *differences* between conditions without specifying a direction. Therefore, the key to distinguishing between the motivational intensity and valence-based models is which type of ratings more strongly predict observed attentional breadth.

If the perceptual focality of a task-irrelevant image can drive attentional breadth, then attentional breadth should be narrower following focal pictures (regardless of whether they are dessert or rock pictures) compared with following diffuse pictures (regardless of whether they are dessert or rock pictures). Perceptual focality could work in addition to, instead of, or in contrast to any effect of valence or motivational intensity.

Method

Participants

The design was a 2 (Perceptual Focality: Focal versus Diffuse) × 2 (Picture Type: Desserts versus Rocks) × 2 (Navon Target Level: Global versus Local) mixed design. Perceptual focality was manipulated between-subjects, whereas Picture Type and Target Level were manipulated within-subjects. Therefore, we conducted two power analyses to ensure that the study was sufficiently powered. One was to consider what sample size we needed *within* each level of Perceptual Focality to replicate Gable and Harmon-Jones' (2008) Study 2 effect when comparing desserts versus rocks, and the other was to assess how many participants total required to have sufficient power to compare the two between-subjects levels of Perceptual Focality.

In Study 2 of Gable and Harmon-Jones (2008), $N = 32$ introductory psychology students completed for course credit. We conducted a power analysis in the tool provided and described by Anderson et al. (2017a, 2017b). Specifically, using the *F*-value for the interaction between picture type and Navon level that Gable and Harmon-Jones (2008) obtained ($F = 35.61$ from $N = 32$ participants), assuming an alpha level of .05, assurance of .95 and power of .95, $N = 31$ participants are required. Therefore, we needed at least 31 participants in each level of Perceptual Focality.

If Perceptual Focality was the true driver of observed differences between desserts versus rocks in previous research, then we needed to have sufficient power to detect similar magnitude effect that arises from comparing desserts versus rocks in within-subjects designs, but power to detect this when comparing between-groups (i.e. each level of Perceptual Focality collapsed across picture type).[6] Gable and Harmon-Jones (2008) reported a very large effect size for the Study 2 interaction between Picture Type and Navon Level (partial-eta-squared = .53). To be conservative in ensuring we had sufficient power, we chose to power for a medium effect instead, which means we should have ample power to detect the effect sizes observed in these previous studies. A power analysis in G*Power's *t*-test function for comparison of two independent groups indicates that $N = 210$ (105 per group) is required for 95% power to detect a medium effect size in an independent-groups *t*-test. This satisfies (indeed considerably exceeds) the minimum *N* of 31 within each condition identified above. We added 10% to the required *N* to ensure sufficient power even after potential exclusions (e.g. due to non-compliance with task instructions). Therefore, we recruited $N = 232$ (116 per group).

We planned to then use Bayesian analysis to assess the level of evidence for the effect of picture type on attentional breadth. Specifically, since in Study 2 Gable and Harmon-Jones (2008) found a significant difference between global and local response times (RTs) following rocks pictures, we planned to conduct a Bayesian *t*-test comparing RTs on global versus local trials following rocks pictures for each level of focality (i.e. focal and diffuse). For these analyses, the alternative hypothesis tested was that the RTs differ (no direction assumed), whereas the null hypothesis was that they do not differ. Default JASP priors were used. We planned that if the BF_{10} was in the indeterminant zone (i.e. <3 and >.33) for either level of focality, then we would collect more data (in increments of $N = 10$ participants) until the BF_{10} indicated at least moderate support for either the alternative ($BF_{10} > 3$) or the null hypothesis ($BF_{10} < .33$), or a maximum sample size of $N = 462$ was achieved (i.e. about 2 x that indicated by the power analysis).

Participants were recruited from the Testable Minds recruitment platform (https://www.testable.org/minds). Participants were required to be verified minds in the platform, which means that the platform has taken multiple steps to ensure that they are humans (not bots), including face recognition login. To maximise similarity to the sample collected in Gable and Harmon-Jones (2008) Study 2, participants were required to be residing in the United States and aged between 18 and 40 years. To exclude non-compliant responders, they were required to have 90% or higher approval rating from researchers in the platform. We decided to use a paid recruitment platform rather than undergraduate students participating in exchange for course credit, given that in our experience compliance with instructions is higher in paid platforms. This study required extensive ratings, and we thought it more likely that we would obtain higher rates of good-faith completions from the paid platform. All participants provided informed consent prior to participation.

Stimuli

A total of 64 pictures, comprising 16 pictures per category (i.e. focal desserts, diffuse desserts, focal rocks,

diffuse rocks) were selected from the rating data collected in the pilot study. More specifically, in the pilot study, 33 participants rated 200 candidate pictures along the dimension of perceptual focality. That is, for each image, they were asked, *To what extent does this picture focus on a single object?* and responded on a 9-point scale: 1 (singe object focus) to 9 (not single object focus). The mean ratings across these raters for each picture were used to select the 16 pictures per category (i.e. pictures that had mean ratings closest to 1 were chosen for the focal conditions, and pictures with the mean ratings closest to 9 were chosen for the diffuse conditions). For the selected category sets, the ratings showed that perceptual focality was successfully manipulated. That is, the mean perceptual focality ratings for the selected focal dessert stimuli ($M = 1.55$, $SD = 1.09$) were significantly more focal than the diffuse dessert stimuli ($M = 8.50$, $SD = 1.07$), and the mean perceptual focality rating for the selected focal rock stimuli were significantly more focal ($M = 1.17$, $SD = .56$) than the diffuse rock stimuli ($M = 8.79$, $SD = .57$) (full details of the selection of images for the pilot study, and the method and results from the pilot study, are provided in the Supplementary Material).

As neither Gable and Harmon-Jones (2008) nor Domachowska et al. (2016) reported the picture size, we resized and cropped the images to be 400×400 pixels to match the Navon stimuli (described below). This corresponds to about 8 cm × 8 cm on a 27-inch iMac screen following Testable's calibration process. Smaller versions of all the images are provided in Appendix A, B, C, and D. All the pictures were presented in colour (unless the original picture was in greyscale in which case it appeared in greyscale). Please consult the online version of the Appendix for colour images.

Consistent with Gable and Harmon-Jones (2008) Study 2, we used Navon stimuli (Navon, 1977) to operationalise attentional breadth. These are hierarchical stimuli in which a global letter is made up of multiple instances of smaller local letters (see Figure 4). There is compelling neuroimaging evidence that when people attend to the global level, they have a larger region of activation in primary visual cortex, and when they attend to the local level, they have a smaller and more intense region of activation in the primary visual cortex (Sasaki et al., 2001). While there are many behavioural tasks that have been claimed to measure attentional breadth in the literature, Navon stimuli stand out as having this compelling validating evidence that they operationalise attentional breadth as intended.

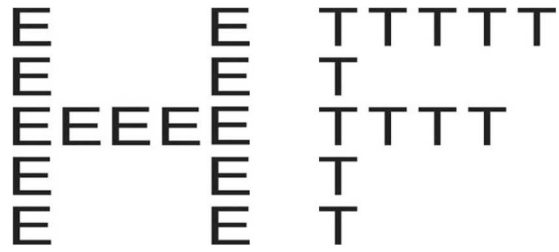

Figure 4. Example Navon stimuli.

Note. Where the task is to identify which target letter (T or H) is present in the stimulus, the Navon stimulus on the left has the target H at the global level, while the Navon stimulus on the right has the target T at the local level.

Navon stimuli are typically used in one of two major ways, either the Directed Navon or Undirected Navon task (Goodhew, 2020). The Directed Navon task measures participants' capacity to adopt and maintain a particular attentional breadth, whereas the Undirected Navon measures preferred or adopted attentional breadth. Given that the latter is our aim here, as per Gable and Harmon-Jones (2008), we used the Undirected Navon task.

We used the same Navon stimuli presented at the same size as they have been in previous research, where they proved sensitive to the effect of different experimental conditions (Goodhew & Edwards, 2022a), as well as able to induce demonstrably distinct attentional breadths (Goodhew & Edwards, 2024a). Here, participants were instructed to identify whether the target letter "T" or "H" was present in the stimulus by pressing the corresponding key on the keyboard. There were eight different Navon stimuli, where target letters were T and H, and non-target letters were E and F. Of the eight stimuli, four had the target present at the global level (two had global Ts and two global Hs, and for each of these two global letters, one had local Es and one had local Fs), and four had the target present at the local level (two had local Ts and two local Hs, and for each of these two local letter stimuli, one had global E and one had global F). Example stimuli are shown in Figure 4. They were each 400×400 pixels. All stimuli were presented centred on the centre of the screen.

Apparatus

The Experiment was run online remotely. The Experiment was coded in Testable (www.testable.org),

which interfaces seamlessly with the Testable Minds recruitment platform. Testable has inter-trial timing variability in the range of 3.2–8.4 ms (Bridges et al., 2020) and we have found it to be sensitive to effects of experimental manipulations on attentional breadth and the effect of attentional breadth on other processes (e.g. Goodhew & Edwards, 2022a, 2024a).

The visual angle of a stimulus (i.e. perceptual size on the retina) is a product of both stimulus size on screen and viewing distance from the screen. We used Testable Minds' capacity to prevent participation on phones or tablets, restricting participants to using computers to complete the study. We also used Testable's inbuilt calibration function to promote consistent stimulus sizing on each participant's screen. Although viewing distances tend to be reasonably similar across people (e.g. it would be unusual to find someone seated more than 1 m from a computer screen), in an online format, we cannot tightly control viewing distance in the way that is possible in the lab. However, our experiment was a within-subjects design, meaning that we are quantifying attentional breadth in one condition (desserts) *relative* to another (rocks). Therefore, like all other sources of potential variation at the individual levels (e.g. individual differences in absolute motor response time), they are eliminated as potential sources of between-condition variance by virtue of the within-subjects design, where the same people complete both conditions on the same computer at their individually-set distance from the screen. We note that Gable and Harmon-Jones (2008) reported neither stimulus size nor viewing distance, and thus as explained in the Stimuli section, we have chosen stimulus sizes for the Navon stimuli that have demonstrated sensitivity to experimental manipulations when used in previous online studies with naturalistic variation in viewing distance.

Procedure

Participants first completed calibration, and then they were presented with the Information Sheet. They only continued to the Experiment if they clicked "AGREE" following the Information Sheet. This means that they provided informed consent. The ethical aspects of this project were approved by the ANU Delegated Ethical Review Committee (2024/930). Then they were presented with onscreen instructions:

> Here on each trial, you will see a picture appear for a few seconds and then a stimulus that contains multiple small (local) letters that make up a large (global) letter. Your task is to identify whether the letter T or H is present in that stimulus. The T or H may appear at either the local or global level. If you see the letter 'T' press the 'T' key, if you see the letter H, press the 'H' key. Please respond as quickly and accurately as possible. Note that once you respond, the screen will be blank for a long time (about 20 seconds) between trials. Please be patient, the next trial will begin automatically. We'll start with some practice trials.

Participants then completed eight practice trials, where onscreen feedback about whether each response was correct was presented. Dessert and rock pictures that were not used in the main study were selected for the practice block. They were then reminded of the task instructions onscreen again before progressing to the main experiment. Feedback was not provided in the main experiment.

In total, the stimulus set consisted of 64 images (16 for each of the four fully crossed combinations of dessert versus rocks and focal versus diffuse). However, participants were randomly assigned to either the focal or diffuse condition and so each participant viewed 32 images (that correspond to the level of perceptual focality to which they were assigned, e.g. 16 focal desserts and 16 focal rocks). They were then presented with each of these 32 images twice, once with a local Navon target and once with a global Navon target. This means that in the main experiment, there were 64 trials, consistent with the 64 trials in the original Gable & Harmon-Jones' Study 2.

On each trial, a fixation cross was presented for 500 ms, followed by a picture (dessert or rocks) for six seconds, then the fixation cross again for 500 ms, and then the Navon stimulus until response (see Figure 5). There was an intertrial interval of 18–20 s (randomly determined). These durations match those of Gable and Harmon-Jones (2008) Study 2.

Next, participants completed two blocks of ratings, one for valence, and one for motivational intensity (order randomised). In each block, each of the 32 pictures used in their experimental condition was presented in a randomised order, and they made their response on the relevant rating scale. The images were presented until the participant responded.

For the valence block, for each picture, participants were asked to indicate *How do you feel while viewing the picture?* They responded on a 9-point scale: −4 (unpleasant) to +4 (pleasant) with 0 as the neutral mid-point. For the motivational intensity block, for

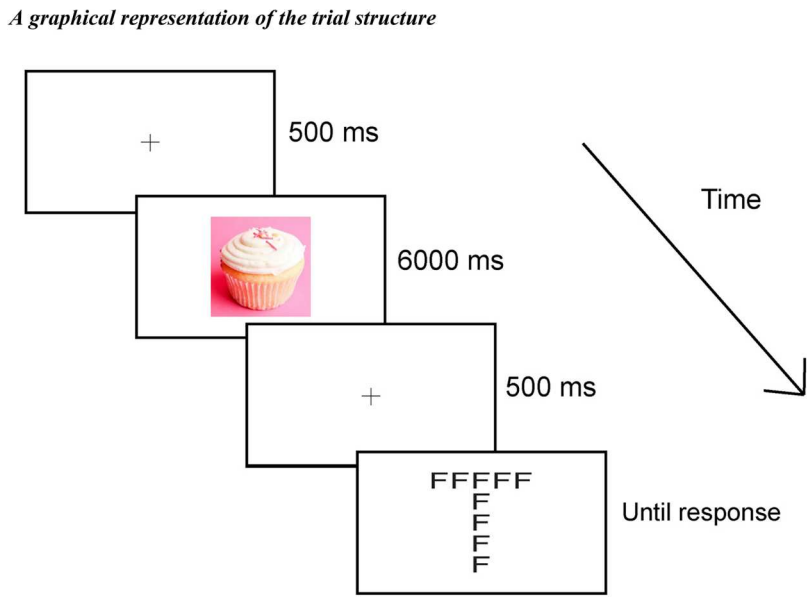

Figure 5. A graphical representation of the trial structure.
Note. Figure for illustration only; pictures are not necessarily to scale.

each picture, participants were asked to indicate *How strong is your urge to avoid or approach while viewing the picture?* They responded on a 9-point scale: −4 (avoid) to +4 (approach) with 0 as the neutral midpoint. While we were interested in motivational *intensity*, this can be derived from taking the absolute value of the rating on this scale, and including direction in the scale allowed us to understand whether any strong motivational intensity was to approach or avoid.

Participants were then asked demographic questions, including age, gender, handedness, and country of birth. We originally intended for them to be asked three data-validity check questions with Yes/No answers: (1) I am personal friends with the King of England, (2) I swim across the Pacific Ocean to commute to work each day, (3) I breathe. However, due to an error these questions were not presented to participants. These questions were designed to detect potential bot responses. However, we had two other protections against bots: (1) we only used verified minds in the pool, which means that they have undergone rigorous screening to determine that they are human, including logging in via face recognition; and (2) participants could only progress to the study if they correctly completed a Captcha response. This variation from the original In Principle Acceptance was approved by the Editor after data collection and prior to the submission of Stage 2.

Data analysis

Our approved data analysis plan articulated prior to data collection was as follows. (We have adjusted the writing in this section to be in past tense for consistency with the rest of the Method section (as is typical for a Method section), but the plan was submitted, reviewed, and approved at Stage 1 *a priori*, as was the rest of the Methods. The only deviation from the a priori plan was that noted above). For the Navon data, individual trials were excluded from further analysis if the response was greater than five seconds (consistent with the maximum response window in Gable and Harmon-Jones (2008, Study 2)). Participants' accuracy was then calculated for each condition that they completed (four conditions, each combination of picture type and target level for their assigned level of perceptual focality). Their mean response time (RT) on correct-response trials was then calculated for each condition. Consistent with Gable and Harmon-Jones (2008) Study 2, mean RTs were log transformed for analysis.

Participants were excluded from further analysis if their accuracy in one or more conditions was below 75% accuracy. We chose this cut-off because values

below this are closer to chance (50%) than ceiling (100%), and given that stimuli were shown until response, accuracy should be close to ceiling. Low accuracy is indicative of a participant being either unable or unwilling to complete the task as instructed. Instead, variation is expected on mean RT, and this is the primary dependent variable. Further, low accuracy necessitates that the means for *correct* response trials are computed from a small number of trials, which can make them unstable. Therefore, it is important to exclude participants with low accuracy rates. We also planned for participants to be excluded from further analysis if they fail any of the data-validity check questions.[7]

For the rating data, we planned to conduct a total of three matched-pairs *t*-tests (or non-parametric equivalent where the data were not normally distributed) comparing the Desserts versus Rocks for each rating scale (valence and motivational intensity). For motivational intensity, one *t*-test used the raw scale that included motivational direction (approach versus avoid), and the other used the absolute value which reflects non-directional motivational intensity.

We planned to take two approaches to the data analysis concerning Navon performance, one reflecting the common approach of examining the effect of categorical conditions, as was done in Gable and Harmon-Jones (2008) Study 2, and the second taking a Multi-Level Modelling (MLM) approach in which picture ratings by each individual participant were considered as predictors of attentional breadth.

For the Navon performance data, while the primary dependent variable was RT because accuracy is expected to be close to ceiling when stimuli are presented until response, we also analysed accuracy data because at times it can be informative (see e.g. Goodhew & Edwards, 2022a; Yabuki & Goodhew, 2021), and at a minimum it establishes whether any of the RT results can be explained by a speed/accuracy trade-off. Therefore, a 2 (Perceptual Focality: Focal versus Diffuse) × 2 (Image type: Dessert versus Rocks) × 2 (Target level: Global versus Local) mixed ANOVA was performed, once where the dependent variable was accuracy, and again where it was RT. If there was a significant three-way interaction, then we planned to assess two-way interactions within each level of Perceptual Focality. If significant two-way interactions were found, then consistent with Gable and Harmon-Jones (2008) Study 2, we planned to compare performance (i.e. RT and/or accuracy) for local and global trials following each Image Type, as well as performance for each Image Type for a particular Navon level.

Following this, we took an MLM approach, in which valence and motivational intensity (i.e. absolute value of rating on motivational intensity scale) ratings from each participant were entered as fixed-effect predictors, and participant and picture stimulus as random effects. We also conducted a secondary analysis in which motivational intensity was replaced with motivational direction (i.e. signed value of rating on motivational intensity scale). The difference score between local and global RTs was the outcome variable (which operationalised attentional breadth). This would reveal which dimension, if any, was a significant predictor of attentional breadth. This approach has the advantage of taking into account not only differences across individuals in their responses to pictures but also differences in responses to different specific pictures within a category within an individual.

Results

Data screening and final participant sample

In Principle Acceptance (IPA) for this Registered Report was given on 4 February 2025. All data collection occurred after this, with all 232 participants completing the experiment between 10 February 2025 and 24 March 2025. Non-numeric information in each raw data file was removed via an Excel Macro so that the data could be imported as numeric values into MATLAB, where one code was used to compile the required information for the ANOVA and *t*-tests (i.e. mean ratings for each stimulus type for each dimension for each participant, and mean accuracy and mean correct RTs following exclusions for long RTs for each condition for each participant), and another was used to compile the required information for the MLM (i.e. difference score for each individual image for each participant following exclusions and the rating on each dimension for each individual image from each individual participant). The ANOVAs and t-tests were conducted in JASP (Version 0.14.1), while the MLM was conducted in SPSS (Version 29.0.2.0). The Stage 1 registration is available here: https://osf.io/nf3a4/files/osfstorage while here: https://osf.io/4phxg/files/osfstorage are the codes, stimuli, and data.

$N = 28$ participants' data was excluded for accuracy below 75% in one or more conditions, leaving $N = 204$ files for analysis ($N = 100$ Focal, $N = 104$ Diffuse). This

exceeds the $N = 31$ required for minimum power to detect an effect of image type within each focality and approximates the $N = 205$ indicated to detect the effect of focality. The planned Bayesian analysis (next section) confirmed more than sufficient evidence in the dataset to adjudicate between the null and alternative hypotheses.

Of these $N = 204$, the mean percentage of individual trials excluded for long RTs (>5 s) was 1.58% (SD = 4.64, range = 0–34.38). One participant had accuracy on the Navon task that satisfied the a priori screening criteria, but they failed to provide any rating data. We had not planned for any exclusion based on the rating data. Therefore, the data from this participant was included in the ANOVA assessing the effect of focality and image type from the Navon task (i.e. $N = 204$ for this analysis). However, their data was excluded from the ratings data analyses and the MLM analysis, given that for both, an absence of ratings necessarily precludes their inclusion in the analysis (i.e. the analysis would automatically exclude them anyway with these cells missing) (i.e. $N = 203$ for these analyses).

Bayesian analysis on RT data to test for sufficient observed power

As planned, to confirm that there was adequate power in the sample, we conducted a Bayesian paired-samples *t*-test comparing RTs on global versus local trials following rocks pictures for both the Focal and Diffuse conditions. For the Focal condition, the BF_{10} value = 7171.60 (i.e. 7172 times greater evidence for the alternative hypothesis relative to the null hypothesis), while for the Diffuse condition, the BF_{10} value = 1842.45 (i.e. 1842 times greater evidence for the alternative hypothesis relative to the null hypothesis). Both these values far surpass the requisite metric of 3 times greater evidence for one of the hypotheses, thus confirming more than adequate power in the present study with the present sample size, and therefore further data collection was not required.

Participant demographics

Participants' mean age was 31.38 years (SD = 5.94, range 18–40), and 106 reported their gender identity as a woman, 88 reported their gender identity as man, 5 as non-binary, 1 as agender, 1 as non-binary woman, and 3 preferred not to say. With respect to handedness, 180 were right-handed, 15 left-handed, and 9 ambidextrous. The most commonly reported country of birth was the United States of America (181), followed by Nigeria (6).

Rating data analyses

These analyses were performed on the $N = 203$ participants with available rating data. Some rating data were not normally distributed, and so we used non-parametric *t*-tests throughout (Wilcoxon signed rank). On the dimension of valence, desserts were rated as significantly higher in valence (i.e. more positive) (M = 1.59, SD = 1.20) than rocks (M = .07, SD = 1.06) (W = 18494, $p < .001$). On the dimension of motivational direction, desserts (M = 1.54, SD = 1.32) were rated as significantly higher in the direction of approach motivation than rocks (M = −.03, SD = 1.24) (W = 18142, $p < .001$). On the dimension of motivational intensity (i.e. calculated by taking the absolute value of their motivation rating, non-directional), desserts were rated as significantly higher in motivational intensity (M = 2.27, SD = 0.79) than rocks (M = 1.75, SD = .83) (W = 16026, $p < .001$). To summarise, this means that desserts were on average rated as more pleasant and more approach-motivating (and hence higher in motivational intensity) than rocks.

While we did not explicitly propose this in the Stage 1 proposed data analysis, we thought it would be useful to also perform the above analyses separately for the Focal and Diffuse conditions, and this confirmed that the results were similar for both conditions (see Supplementary Material). To summarise, this means that desserts were on average rated as more pleasant and more approach-motivating (and hence higher in motivational intensity) than rocks, and this was true for both focal and diffuse images.

Performance data: ANOVAs and t-tests

A 2 (Perceptual Focality: Focal versus Diffuse) × 2 (Image type: Dessert versus Rocks) × 2 (Target level: Global versus Local) mixed ANOVA was performed on correct RTs (log transformed). As is typically seen in the Navon paradigm (Navon, 1977; Rezvani et al., 2020), this analysis found a significant main effect of Target Level, $F(1, 202) = 52.15$, $p < .001$, $\eta_p^2 = .205$, such that responses were significantly faster on Global trials relative to Local trials. No other main effects or interactions were significant, as shown in

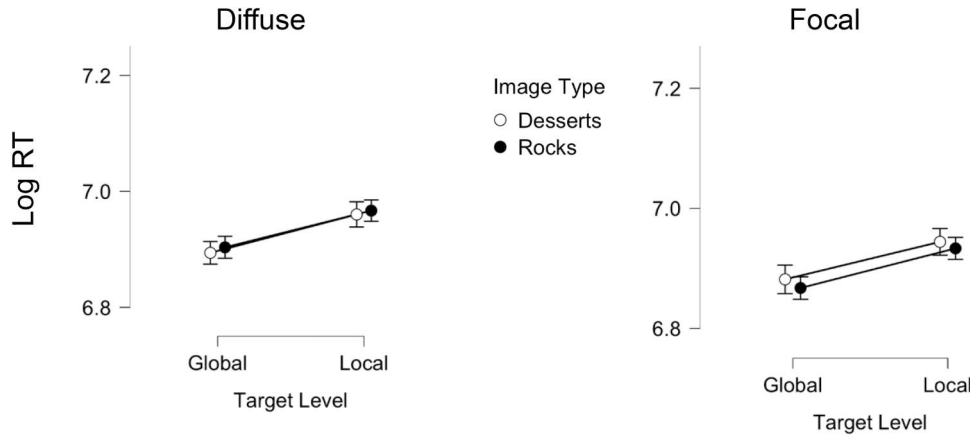

Figure 6. Log transformed correct RTs (y-axis) as a function of focality, image type, and target level.

Note. Responses were faster for Global relative to Local trials, irrespective of Perceptual Focality or Image Type. Note. Error bars depict 95% confidence intervals.

Figure 6. Specifically, the main effect of Image Type was not significant, $F(1, 202) = .12$, $p = .726$, $\eta_p^2 < .001$, and the main effect of Focality was not significant, $F(1, 202) = .21$, $p = .650$, $\eta_p^2 = .001$. The two-way interaction between Image Type and Focality was not significant, $F(1, 202) = 2.43$, $p = .120$, $\eta_p^2 = .012$, the two-way interaction between Target Level and Focality was not significant, $F(1, 202) < .01$, $p = .980$, $\eta_p^2 < .001$, and the three-way interaction among Image Type, Target Level and Focality was not significant, $F(1, 202) = .08$, $p = .781$, $\eta_p^2 < .001$. These results demonstrate that a typical global precedence effect was present (e.g. Navon, 1977; Rezvani et al., 2020), such that participants were faster to identify global than local targets, but the type of image – whether it was dessert or rock, and whether it was focal or diffuse – had no measurable impact on RT or this global advantage.

With respect to accuracy, a 2 (Perceptual Focality: Focal versus Diffuse) × 2 (Image type: Dessert versus Rocks) × 2 (Target level: Global versus Local) mixed ANOVA was also performed. This showed no significant main effects or interactions. That is, there was no main effect of Image Type, $F(1, 202) = .04$, $p = .836$, $\eta_p^2 < .001$, Target Level, $F(1, 202) = .02$, $p = .886$, $\eta_p^2 < .001$, or Focality, $F(1, 202) = .06$, $p = .815$, $\eta_p^2 < .001$. There was no two-way interaction between Image Type and Focality, $F(1, 202) < .01$, $p = .973$, $\eta_p^2 < .001$, between Target Level and Focality, $F(1, 202) = .24$, $p = .626$, $\eta_p^2 = .001$, or between Target Level and Image Type, $F(1, 202) = .18$, $p = .670$, $\eta_p^2 < .001$, and no three-way interaction, $F(1, 202) = 3.10$, $p = .080$, $\eta_p^2 = .015$.

Exploratory Bayesian analysis

While not part of our original analysis plan, to assess the extent to which there was evidence in favour of the null hypothesis for the effect Image Type (versus just a failure to reject it), we conducted two Bayesian non-parametric repeated-measures *t*-tests, one comparing the Dessert versus Rock Global log RTs, and one comparing the Dessert versus Rock Local log RTs. The BF_{10} factors were .085 and .079 respectively, indicating appropriately 12 times more evidence in favour of the null hypothesis compared with the alternative hypothesis. This is strong evidence (van Doorn et al., 2021). This means that there was evidence in favour of no effect of Image Type of performance.

Performance data: multi-level modelling

The outcome variable for the MLM was the difference score between log transformed local and global trials for each picture for each participant, such that larger values are indicative of faster responses on global trials and hence broader attentional breadth. Consistent with the ANOVA analysis, only correct-response trials where the raw RT was less than 5 s were included in the analysis. If either of the trials that would be used to calculate the difference score for that image for that participant was excluded due to incorrect or long response, then the difference score was not calculated (and thus excluded). Valence, motivational intensity, and motivational direction ratings were transformed to z-scores so that they were on the same scale to improve the interpretability of predictor coefficients.

In the primary planned analysis, Valence and Motivational Intensity were included as fixed-effect predictors, while Participant and Picture Stimulus were modelled as random effects. However, the final hessian matrix was not positive definite. This issue was resolved when Picture Stimulus was removed as a random effect, and therefore the results of this model with only Participant as a random effect are interpreted.[8] The overall corrected model was not significant, $F(2, 6199) = 2.17$, $p = .115$. The fixed effect of Valence was not significant (coefficient = .010, SE = .006, $p = .109$, [95% CI = −.002, .021]), and the fixed effect of Motivational Intensity was not significant (coefficient = .005, SE = .006, $p = .402$, [−.007, .017]). The fixed effect intercept was significant (coefficient = .059, SE = .009, $p < .001$, [.042, .076]), as was the random effect intercept (estimate = .009, SE = .002, $p < .001$, [.006, .012]) and the residual effect (estimate = .190, SE = .003, $p < .001$, [.183, .197]). This means that neither Valence nor Motivational Intensity were reliable predictors of attentional breadth.

In the secondary planned analysis, Valence and Motivational Direction were included as fixed-effect predictors, while Participant was modelled as a random effect (the same issue as above arose when Picture Stimulus was included, and therefore it was excluded). The overall corrected model was not significant, $F(2, 6199) = 1.99$, $p = .137$. Neither the fixed effect of Valence (coefficient = .007, SE = .009, $p = .440$, [−.011, .025]) nor the fixed effect of Motivational Direction (coefficient = .005, SE = .009, $p = .554$, [−.012, .023]) was significant. The fixed effect intercept was significant (coefficient = .059, SE = .009, $p < .001$, [.042, .076]), as was the residual effect (estimate = .190, SE = .003, $p < .001$, [.183, .197]), and the random intercept (estimate = .009, SE = .002, $p < .001$, [.006, .012]). This means that neither Valence nor Motivational Direction were reliable predictors of attentional breadth.

Discussion

Here our goal was to assess the impact of valence, motivational intensity, and perceptual focality on attentional breadth. We conducted a large, well-powered registered report study, and the design was sufficiently sensitive to detect a well-established effect in the Navon paradigm (i.e. the global advantage). Further, the ratings indicated that participants on average found the desserts more positive in valence and higher in approach motivation (and therefore motivational intensity) than the rocks. However, despite this evidence of experimental validity, attentional breadth was invariant to either image type (i.e. dessert versus rocks and focal versus diffuse). This invariance means that we did not replicate the findings of Gable and Harmon-Jones (2008, Study 2) where desserts were found to narrow attentional breadth relative to rocks. In addition, the invariance is inconsistent with novel possibility that we raised – that image focality of task-irrelevant images may drive attentional breadth. Further, none of the ratings dimensions (valence, motivational direction, or motivational intensity) were able to reliably predict observed attentional breadth. Thus our results converge with other recent investigations that have failed to find any influence of emotion or motivation on attentional breadth (e.g. Chan & Saunders, 2023; Kolnes et al., 2022), and thereby add to the growing body of work challenging the robustness with which valence or motivational intensity drive attentional breadth. The broader implications of these results are discussed below.

Valence versus motivational intensity and attentional breadth

Contrary to Gable and Harmon-Jones (2008, Study 2), our results indicated no effect of valence or motivational intensity on attentional breadth. This means that the results that Gable and Harmon-Jones (2008) observed in their Study 2 did not generalise to the new stimulus set used here. Of course, there is the risk that our result is a Type 2 statistical error (i.e. false negative). However, there are several pieces of evidence that argue against this possibility. One, our study was strongly powered. Two, our study was sufficiently sensitive and powerful to provide *resounding* evidence for a well-established effect, namely the global advantage for Navon stimuli. This means that it is difficult to explain away our null results for emotion and motivation due to lack of sensitivity in the experimental design. Three, our results converge with a growing body of evidence that has found no effect of emotion or motivation on attentional breadth. Therefore, even if one wanted to dismiss our findings due to any feature specific to our study, they would have to also account for the findings of those other studies as well that used a range of designs (Bruyneel et al., 2013; Chan & Saunders, 2023; Kolnes et al., 2022).

Why then did Gable and Harmon-Jones (2008, Study 2) and also Domachowska et al. (2016) find evidence that desserts narrowed attentional breadth relative to rocks? A key difference between those studies and the present study was that we used a different set of images. If so, then this is potentially informative, because effects that are heavily stimulus dependent are less likely to reflect the effect of emotion or motivation. For comparison, it was initially thought that faces showing different emotional expressions could influence visual search by virtue of their *emotional salience* (e.g. Fox et al., 2000; Hansen & Hansen, 1988; Williams et al., 2005). However, this thinking has been revised in light of the demonstration that the effects of such stimuli on visual search are dependent on the image set from which the faces are drawn, suggesting that they are the result of perceptual artefacts of the stimuli rather than emotion (Savage et al., 2013). This illustrates how effects that were initially thought to reflect affect driving attentional processes have been attributed to image-specific artefacts following more systematic testing with a broader range of stimuli. In contrast, there is strong evidence that *emotion-induced blindness* (i.e. temporary reduction in perception following an emotionally-salient stimulus) (Most et al., 2005) is due to the emotional salience of the stimuli, because this effect generalises across very different types of images, as well as different sets of the same type of image (for a review, see Goodhew & Edwards, 2022b). This demonstrates how assessing the extent to which effects generalise across different image sets can elucidate the true mechanism underlying observed effects. Therefore, the present study makes a meaningful contribution to the literature on attentional breadth in demonstrating that with a new set of images, desserts did not narrow attention relative to rocks.

Operationalising motivational intensity

Seminal work designed to test the influence of motivational intensity on attentional breadth did not collect participants' reports on a dimension of motivation, despite collecting ratings on other dimensions, such as valence (Gable & Harmon-Jones, 2008, 2010a). This omission may have occurred because it has sometimes been thought that participants do not have sufficient insight into their motivational intensity to report it. Consistent with this, in work examining the drivers of another process (time perception), it has been explicitly stated that people have difficulty self-reporting approach motivation in negative states (Gable, Neal, et al., 2016). This statement was associated with dismissal of participants' ratings, such as when they appeared to rate anger as more associated with *avoidance* motivation than approach motivation, but researchers instead interpreted their behaviour as reflecting the consequences of *approach* motivation (Gable, Neal, et al., 2016). Here we challenge this assumption and instead propose that people can meaningfully self-report the extent to which they are experiencing a high versus low motivational intensity, and the direction of that motivation (approach/avoid).

Participants are able to provide reports about other aspects of their affective experiences, such as its valence, and the extent to which it corresponds to a given category of emotion, and these reports align with objectively-measured physiological indices (e.g. Grootswagers et al., 2020). It is unclear to us why motivation would be considered different to these other cognate psychological processes. Participants' self-reports of motivation appear to be fine-grained (Campbell et al., 2021, 2023; Kaczmarek et al., 2021), and align with implicit behavioural indices (Campbell et al., 2021). This suggests that participants can provide meaningful self-reports into the direction and intensity of their motivation. This means that we do not think that our results can be discounted simply because we have operationalised motivational intensity via ratings.

Further, going forward, if we accept that participants do have at least some explicit access to their motivation, then the evidence indicates that both the motivational direction and motivational intensity induced by the identical image varies considerably across individuals, more so than the valence does (Campbell et al., 2023). This means that it is important that any attempt to experimentally vary motivational intensity or direction is supported by data collected from the specific participants in the study, rather than normative or group-level data. Further, the evidence suggests that valence and motivational intensity can considerably covary (Campbell et al., 2021, 2023; Kaczmarek et al., 2021). Given this, we suggest that where the goal is to understand potential drivers of processes such as attentional breadth, it is essential to quantify both dimensions, so that their unique effects can be disentangled. We recommend the ratings-based approach that we have taken here for this purpose.

Beyond attentional breadth

Both the valence and motivational intensity models typically situate *attentional breadth* as one of multiple processes belonging to a superordinate category such as *cognitive scope* (e.g. Gable & Dreisbach, 2021; Harmon-Jones et al., 2012). We think it is important that any claims about the grouping of tasks or processes into categories is supported by evidence. For example, latent variable modelling has been able to reveal the common processes underlying performance on different executive function tasks (Burgoyne et al., 2023, 2024; Miyake et al., 2000; Miyake & Friedman, 2012). This approach could be applied to test whether there is a meaningful common factor underlying constructs such as *cognitive scope*.

In the absence of such evidence for a unitary underlying construct, we propose that it is important for the evidence for models (e.g. motivational intensity model), be assessed at the level of *processes* (e.g. attentional breadth), or where there are divergent operationalisation of same psychological process that do not appear to converge (e.g. Dale & Arnell, 2013), then even the level of *tasks* may be more appropriate (e.g. Navon task operationalisation of attentional breadth). This means that the results from the present study on *attentional breadth* do not contribute directly to answering the extent to which emotion or motivation influences other processes such as *time perception* or *left frontal cortical activity*. Instead, the approach we have taken here – where valence and motivational intensity are each independently operationalised and considered simultaneously as potential predictors – needs to be applied to each distinct process as the outcome before conclusions about how and whether emotion or motivation affect each of these processes can be drawn. Equally, this also means that the results observed from studies investigating the impact of emotion or motivation on processes other than attentional breadth (e.g. time perception, left frontal cortical activity, etc.) cannot be martialled as evidence counter to the conclusions of the present study.

Perceptual focality and global advantage

A robust finding in the present study was the observation of the classic global advantage, whereby participants are faster to identify the global level of Navon stimuli relative to the local level (Navon, 1977; for a review, see Rezvani et al., 2020). This is consistent with models that emphasize coarse-to-fine visual processing of scenes (e.g. Bar et al., 2006) – the global "big picture" gist is preferentially processed in advance of the finer-grained details and minutia at the local level. We think that it is informative that our study design was sufficiently sensitive to replicate this well-established effect – it refutes some of the more pedestrian explanations for the absence of reliable effects of other variables observed here, such as that the study design introduced too much random noise to be able to detect effects.

Perceptual focality of the stimulus (i.e. focal versus diffuse) had no discernible impact on attentional breadth. Given that perceptual focality or related constructs such as object size have been found to induce different attentional breadths when the stimuli are task-relevant (e.g. Belopolsky et al., 2007; Chong & Treisman, 2005; Goodhew et al., 2016; Lawrence et al., 2020), this suggests that the mere presentation of a stimulus is not sufficient, and instead task-relevance is likely a necessary condition for these effects to occur. Future research could test this definitively by adapting the design of the present study to make the pictures themselves task-relevant (in addition to the Navon stimuli), and then to assess whether the focal versus diffuse images can then reliably induce different attentional breadths. Alternatively, the paradigms in previous attentional breadth research (e.g. using small versus large objects to induce narrow versus broad attentional breadth) could be adapted to remove the task-relevance of the potential inducers to see if they still successfully induce different attentional breadths. If they do not, then this is consistent with the notion that task-relevance of the inducing stimuli is critical to the induction of attentional breadth.

Limitations

One potential limitation of the present research is that it was run online, rather than in lab. However, we do not see this as a problem, for several reasons. Different people will of course have different screens, but as articulated in the Method, Testable offers ways of matching stimulus sizes across different screens. Further, within reason, the *relative* sizes of global versus local stimuli is important for assessing a person's attentional breadth, and since this factor was manipulated purely within-participants, not between-participants, it *cannot* be confounded by different screens. Indeed, if Gable and

Harmon-Jones (2008, Study 2) thought that a specific stimulus size was especially critical to observing their effect, presumably they would have reported the sizes of their stimuli, but they did not. Of course, remote participation means that an experimenter is not present. However, the main impact that this could conceivably have been to reduce participant engagement with the task. Given that the Navon task has an objectively correct and incorrect response for each trial (unlike subjective responses), it means that our accuracy-based screening would have filtered out such participants. Of course, convergent evidence for our conclusions could arise from repeating our experimental design in an in-person format in the laboratory. But in the meantime, we do not think that the online format is a valid reason for dismissing the implications of our results.

Another potential limitation is that desserts are not typically rated as extremely high in valence or motivational intensity compared with other positive stimuli, and it is possible that more potent induction approaches are required to elicit effects on attention (e.g. Most et al., 2007). However, since one of the goals of this study was to test whether desserts influence attentional breadth relative to rocks, the stimuli we used were ideal for this purpose, and it would not have been appropriate to select more extreme non-dessert images for this purpose. However, it means that we cannot rule out that valence and/or motivational intensity would influence attentional breadth with more extreme images, or with other approaches to manipulating and assessing affect that may prove more potent than images, such as using cognitive tasks to induce emotions (Laybourn et al., 2022), using individual differences in emotional reactions to perceptual aspects of otherwise neutral stimuli (Goodhew & Edwards, 2024b), and experience sampling following exposure to stressors in everyday life outside the laboratory (Benson et al., 2023).

Further, in selecting the dessert and rock images, we took care to consider perceptual focality, because it has previously been shown to influence attentional breadth. In contrast, for factors such as colour, brightness, and contrast – while these could conceivably influence the *measurement* of attentional breadth had the Navon stimuli varied on these dimensions (and therefore we ensured that they did not) – to our knowledge, there is no evidence that such stimulus differences in an inducing stimulus can *induce* different attentional breadth. For example, there is no evidence that a blue square presented prior to a Navon stimulus can narrow attentional breadth relative to a green one, if the squares are matched for size. We also noted that attempting to constrain the inducer images (i.e. the desserts and rocks) to all belong to a homogenous colour palette resulted in stimuli that seemed to us more muted in their emotional potency. This could be problematic, since the goal was to induce affective states. Therefore, we chose to allow these physical properties of the inducing stimuli (such as their colour) to naturally vary. We think it unlikely that these factors play a role, but this choice does mean that we cannot rule out the possibility that such factors created random error variance that undermined the observation of an effect of stimulus type in the present study.

Conclusion

The present work sought to provide a conceptual replication of Gable and Harmon-Jones (2008, Study 2), but also to build on it in an important way, namely, to disentangle whether valence, motivational intensity, and/or perceptual focality drives attentional breadth. We conducted a rigorous and well-powered study supported by manipulation checks to address this question. The results showed a resounding global advantage, which is commonly observed with the Navon stimuli used here to operationalise attentional breadth. Perceptual focality of the dessert versus rocks had no discernible impact on attentional breadth. In contrast to Gable and Harmon-Jones (2008, Study 2) results, pictures of desserts did not narrow attentional breadth relative to rocks. Further, individual participants' ratings of the valence and motivational intensity evoked by each specific image also did not predict their observed attentional breadth. These results contribute to the emerging body of work questioning the robustness with which emotion and motivation affect attentional breadth.

Notes

1. It should be noted that for some psychological processes (e.g. time perception), the model tested espouses that motivational *direction* (approach versus avoid) is the driver, rather than motivational *intensity*. This change from focusing on intensity regardless of direction, to direction itself, is made without explanation or comment, so it is unclear to us what rationale is behind the change. Here we focus on motivational

intensity, because that has been proposed to be the determinant of attentional breadth.

2. Gable and Harmon-Jones (2008 Study 4) had 3 between-subjects conditions: desserts with expectancy to consume, desserts without expectancy to consume, and neutral. In the statistical analysis, assigned weights (−3 for the neutral condition, +1 for the dessert condition without expectancy to consume, and +2 for the desserts with expectancy to consume) were applied to the conditions when assessing their effect on attentional breadth, and the contrast with these weights was significant. However, local comparisons were not performed and so we are unsure whether there was a statistically significant difference in attentional breadth between the two dessert conditions (expectancy to consume vs not), in contrast with the clearer difference in attentional breadth between the dessert with expectancy to consume condition and the neutral condition. Participants' ratings of positive affect were significantly higher in the desserts with expectancy to consume condition relative to neutral, which means that this difference in valence could explain the observed differences in attentional breadth between these conditions.
3. In Gable et al. (2015), motivational intensity ratings were not collected for Study 1–3, but they were for Study 4, where participants rated the degree to which they wanted to *move toward* (1) versus *move away* (9) from each picture (i.e. approach versus avoidance motivation). Ratings on this dimension showed that the *anger* and *neutral* condition were equivalent, and where these values fell along the approach-avoid continuum (5.65 and 5.21) was near the mid-point and at least numerically closer to *avoid* (9) than *approach* (1). This is not consistent with anger being approach motivated. But the authors offered the overall interpretation: "Across four studies, results suggested that anger perceptually and conceptually narrows cognitive scope. That is, an approach-motivated negative state narrowed cognitive scope" (p. 171). Therefore, we suggest that at least some attempted manipulations of motivational intensity via discrete emotions have relied on assumptions contrary to available evidence. More recent systematic testing has also suggested that anger produces ratings of avoidance motivation (Kaczmarek et al., 2021). While some claim that motivation is difficult to self-report, there are multiple issues with such a claim (see Campbell et al., 2024). We return to the issue of how to operationalise motivational intensity in the Discussion.
4. These authors explicitly identified valence as a *dimension* in their Introduction, and their rating data revealed significant differences between their experimental conditions along the dimension of valence in multiple studies in this paper. However, in their General Discussion they say: "the present research provides further evidence suggesting that emotions of the same valence can have very different consequences for attention, cognition, and behavior" (Gable & Harmon-Jones, 2008, p. 481). We interpret their wording "same valence" to indicate that those authors, at least when interpreting their results, conceptualised positive valence as a single category.
5. Even if the confound does not apply to all pictures in a category, if it applies to most or even some, this subset could be sufficient to drive the observed effects.
6. Another way to do the power analysis is to power for the interaction itself, rather than the local comparisons. With this approach, $N = 36$ is required for a medium effect size, which we easily surpass.
7. With the variation noted above, this no longer applies.
8. Given that we had to exclude picture stimulus as a random effect, we checked whether this and the next model were significant if we examined the Focal and Diffuse conditions separately. They were not.

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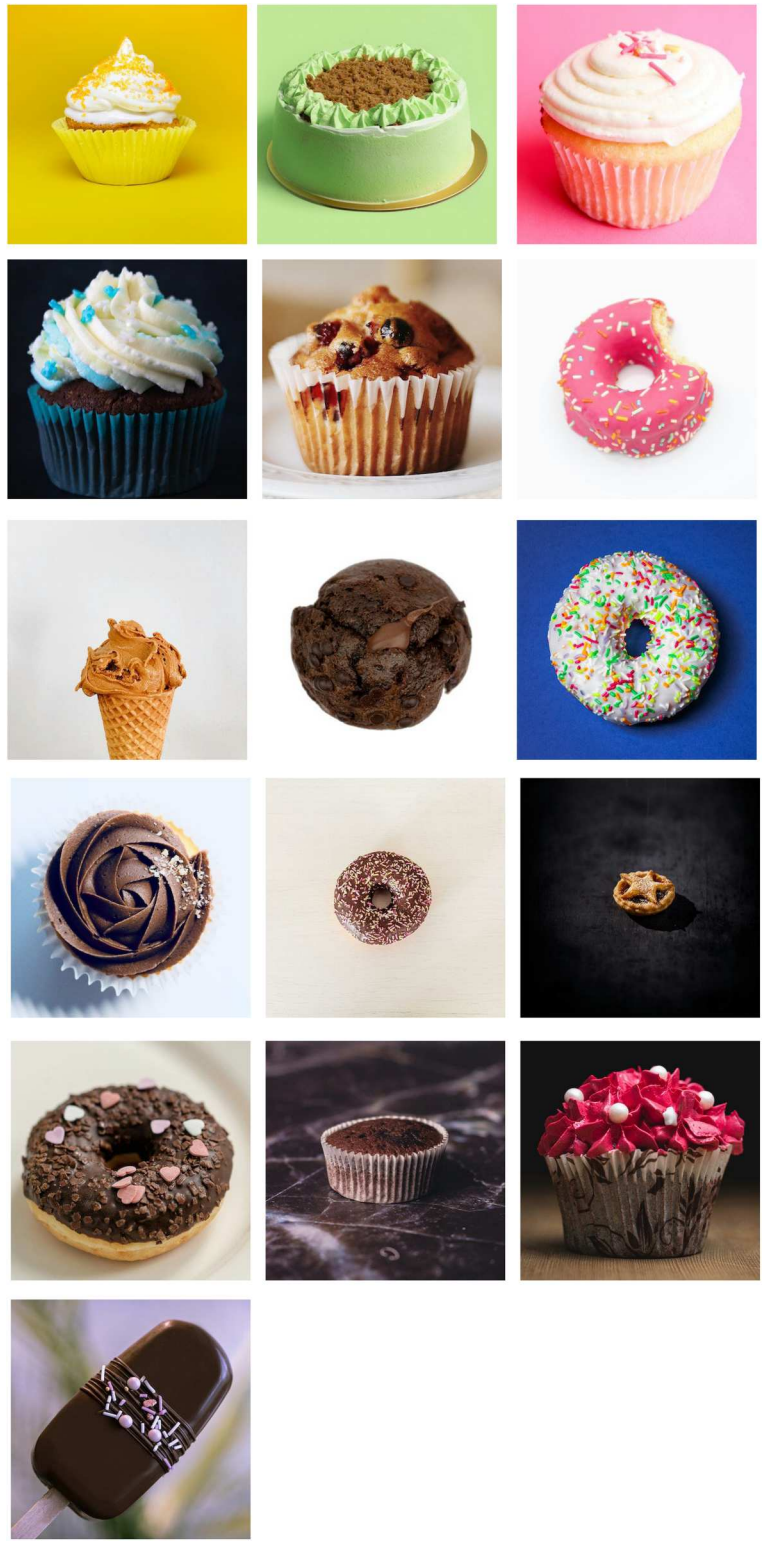
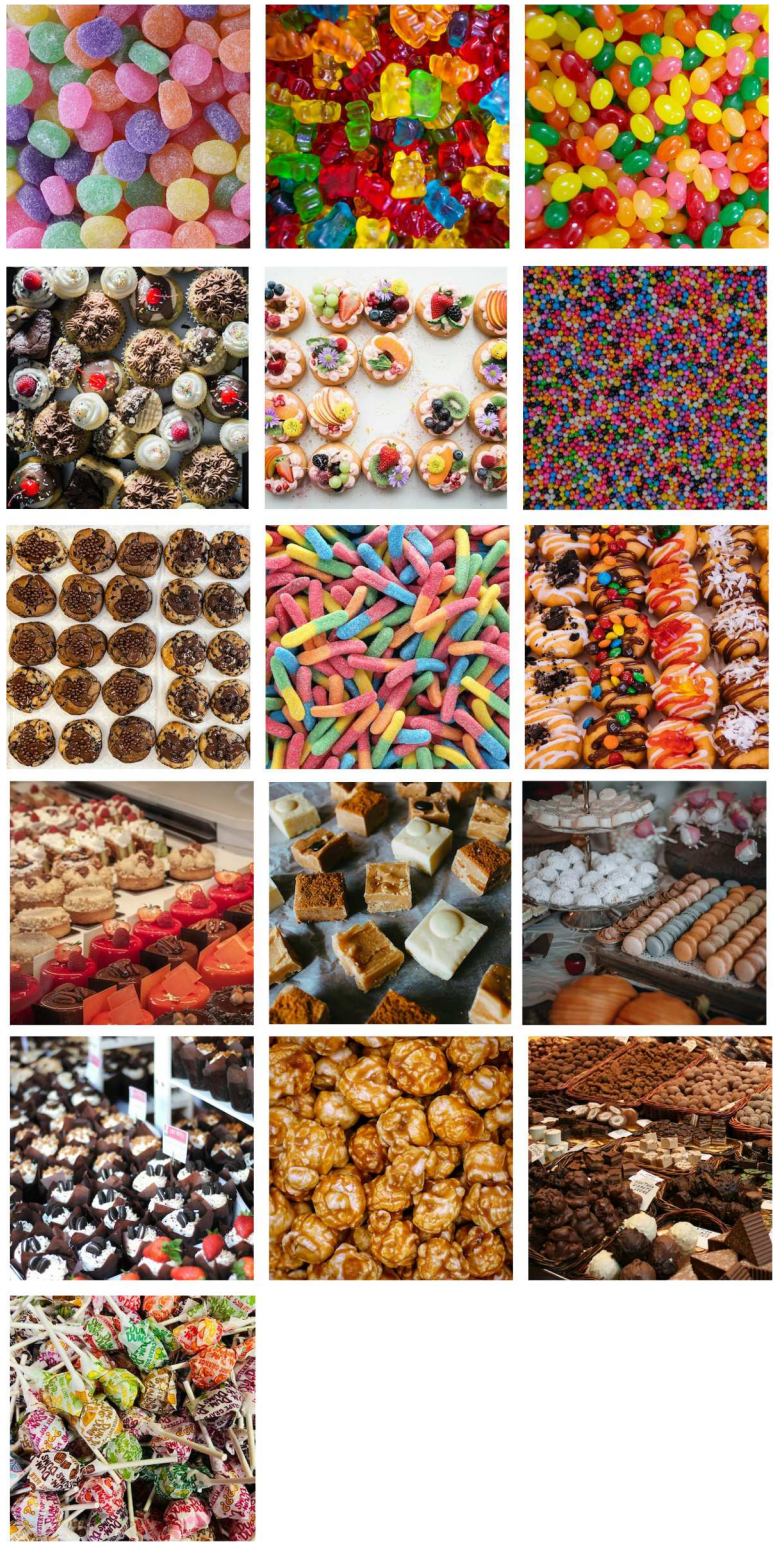
Appendices

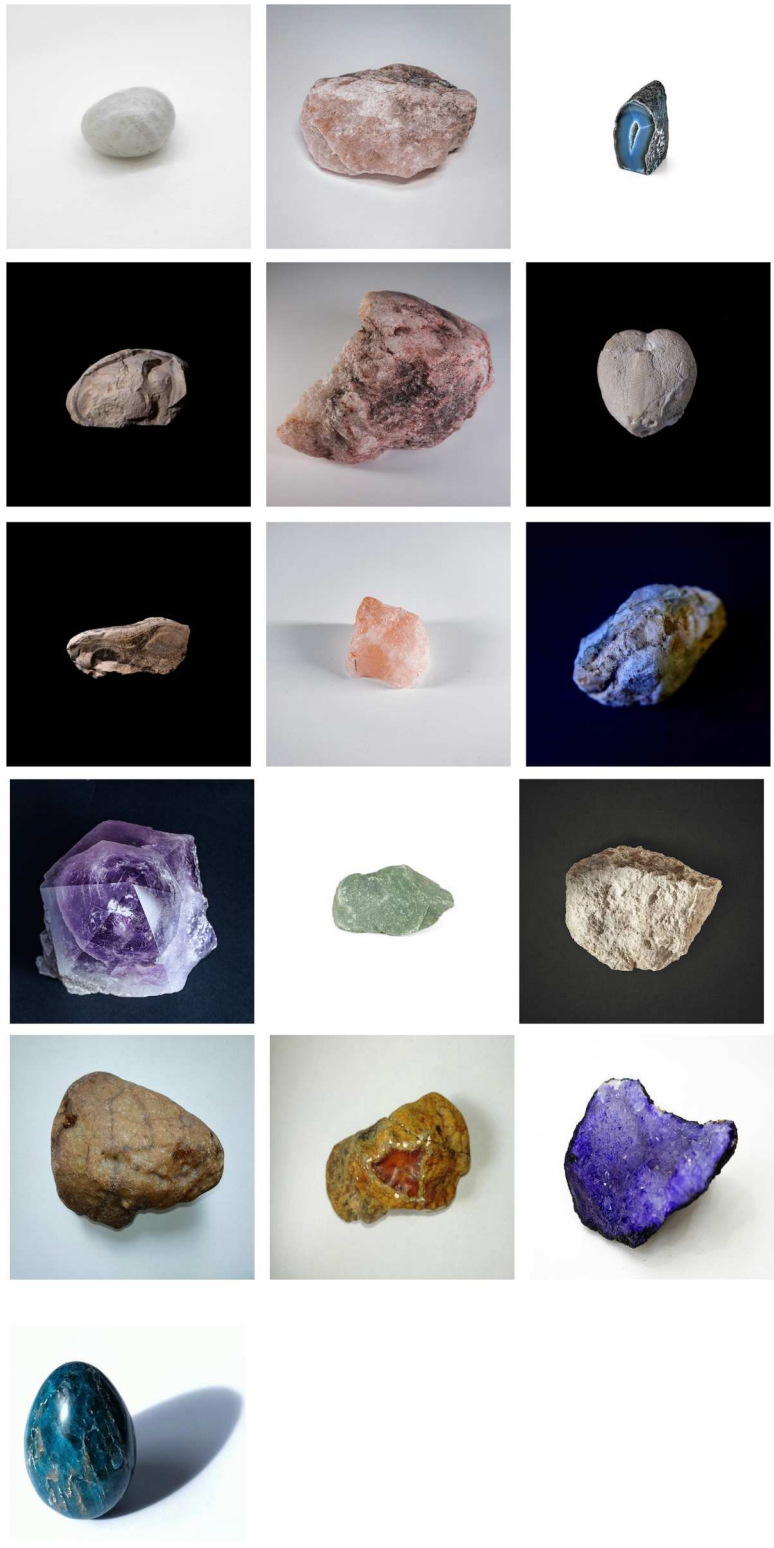
Appendix A. Focal Dessert pictures



Appendix B. Diffuse Dessert pictures



Appendix C. Focal Rocks pictures



Appendix D. Diffuse Rocks pictures