When cognitive control harms rather than helps: Individuals with high working memory capacity are less efficient at infrequent contraction of attentional breadth

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Abstract

Different attentional breadths facilitate performance on different types of perceptual tasks. For instance, a narrow attentional breadth improves spatial resolution, whereas a broad attentional breadth enhances face perception. This means that to optimise attention for the dynamic demands of real-world vision, it is necessary to efficiently resize attentional breadth. Previous research has shown that individuals differ considerably in how efficiently they can resize their attentional breadth. Since working memory capacity can be conceptualised as the ability to effectively regulate one's attentional resources, the present study examined whether individual differences in attentional resizing efficiency were related to working memory capacity. Tasks that gauge the efficiency of attentional contraction (resizing from broad to narrow focus) and attentional expansion (resizing from narrow to broad) were used, in addition to standard working memory measures. It was found that individuals high in working memory capacity experienced a greater cost in attentional contraction, that is, they were less efficient in resizing from a broad to a narrow attentional focus. This is likely because the attentional resizing tasks encourage the setting of a particular attentional breadth for the majority of trials in a block, and then gauge efficiency in changing from this breadth on the minority of trials. This means that high-capacity individuals may have more readily adopted the dominant attentional breadth, particularly in the majority-global condition, thereby incurring a greater cost on the infrequent trials requiring resizing to the local level. This shows that at least in some circumstances, greater cognitive control can be a relative disadvantage.

Keywords: visual attention; working memory; attentional breadth; resizing; global; local; cognitive control

Visual scenes typically confront the human brain with volumes of information – more than it is capable of processing to the level of awareness at any given moment. Therefore, visual attention plays a key role, triaging relevant information for preferential processing, while delaying, discarding, or inhibiting less relevant information. Visual attention can be allocated to objects (Chen, 2012), features (Maunsell & Treue, 2006), or locations in space (Carrasco, 2011). Spatial attention can be shifted to new locations (Posner, Snyder, & Davidson, 1980), it can also be split to multiple non-contiguous regions (M. Muller, Malinowski, Gruber, & Hillyard, 2003), reshaped (Jefferies & Di Lollo, 2015), and resized (C. W. Eriksen & St. James, 1986; Lawrence, Edwards, & Goodhew, 2020; N. G. Muller, Bartelt, Donner, Villringer, & Brandt, 2003). In relation to resizing, a unitary focus of attention can be applied over a small region of space, (narrow attentional breadth) or spread over a larger region (broad attentional breadth). A narrow attentional breadth results in a benefit for perceptual tasks that require fine spatial acuity (Balz & Hock, 1997; C. W. Eriksen & St. James, 1986; Lawrence, Edwards, & Goodhew, 2020; Mounts & Edwards, 2017). In contrast, a broad attentional breadth appears to facilitate performance on tasks that require the pooling or integration of signals or monitoring of a large spatial extent, such as perceptual averaging (Chong & Treisman, 2005), change detection (Pringle, Irwin, Kramer, & Atchley, 2001), and face identification (Gao, Flevaris, Robertson, & Bentin, 2011; Gerlach & Starrfelt, 2018; Macrae & Lewis, 2002). Indeed, narrow versus broad attentional breadths lead to different spatial extents (small versus large) and magnitudes (large versus small) of activation in primary visual areas of the brain (N. G. Muller et al., 2003).

The fact that different attentional breadths benefit different perceptual processes implies that real-world visual processing will require the ability to efficiently *resize* attentional breadth. That is, there is not a single attentional breadth that benefits all tasks. While laboratory tasks are typically designed to measure the impact of attentional breadth on a single perceptual

process, different attentional breadths are required to meet the complex and dynamic demands of real-world visual processing that reflect the interplay of multiple perceptual processes at different points in time. For example, imagine a scenario in which a person is searching for a friend in crowded place, and they are attempting to switch between the tasks of reading a text on their phone as well as frequently glancing up and monitoring the crowd for the face of their friend. Reading the text would require a narrow attentional breadth to resolve the fine spatial details, whereas monitoring the crowd would require a much broader focus, especially since face identification is enhanced with a broad relative to a narrow attentional breadth. Therefore, in order to have the optimal attentional breadth for the task at hand in this situation, one would need to rapidly and efficiently resize between a narrow and a broad attentional breadth.

There is not a large literature on the temporal dynamics of attentional breadth resizing, and the evidence that does exist comes from studies that use often starkly different methodologies. For example, attentional resizing has been operationalised as response efficiency to targets following the presentation of shapes of different sizes, under the assumption that attentional breadth changes to match the size of the objects (Benso, Turatto, & Gastone, 1998), and via response efficiency to a configuration of shapes that has expanded or contracted in diameter to reach different eccentricities (Kosslyn, Brown, & Dror, 1999; Najmi, Kuckertz, & Amir, 2012). Moreover, attentional breadth resizing has been gauged through accuracy at different response intervals in the flanker task (Heitz & Engle, 2007), and it has also been gauged based on when lag-1 sparing is observed in dual rapid serial visual presentation (RSVP) streams designed to induce an attentional blink (AB) (see Dux & Marois, 2009 for a review), with the assumption that lag-1 sparing is observed only when the first and second targets fall within the same spatial attentional scope (Jefferies & Di Lollo, 2009; Jefferies, Enns, & Di Lollo, 2017). Despite these differences in methodology, with a few exceptions aside, a general consensus from the field is that attentional resizing is relatively sluggish. That is, whereas a process such as covert shifts

of attention can occur within tens of milliseconds, resizing of attentional breadth generally appears to take upwards of hundreds of milliseconds. In this vein, it is interesting that a number of studies that sought to examine the effect of different-sized attentional breadths on perceptual performance have opted for blocked designs, in which the size of the attended region is kept constant for a whole block of trials rather than changing from trial to trial (C. W. Eriksen & St. James, 1986; Goodhew & Edwards, 2016; Lawrence, Edwards, & Goodhew, 2020; Macrae & Lewis, 2002). This suggests that there is at least an implicit understanding that the resizing of attention is slow, and therefore the best chance of observing effects of this manipulation arise from conditions where a given attentional size applies across a whole block of trials. Where authors have sought to manipulate the size of the attended region on a trial-to-trial basis, at times the manipulation has been unsuccessful (Yeshurun & Carrasco, 2008), or considerable selection history or “carryover” effects have been observed from one trial to the next, even with up to *three seconds* duration between trials (Fuggetta, Lanfranchi, & Campana, 2009; Hubner, 2000; Hübner, 2014; Robertson, 1996; Ward, 1982). These findings suggest that attentional resizing is slow.

While there have been claims that attentional breadth can be exogenously manipulated on shorter timescales (Benso et al., 1998; Mounts & Edwards, 2017), it is unclear how to reconcile these with the findings implying the process is more sluggish. It is likely that the differences in methodology are at least partly responsible for the different time estimates (for a review, see Lawrence, Edwards, Talipski, & Goodhew, 2020). For example, at least some of both Benso et al.’s (1998) and Mounts and Edwards’ (2017) experiments likely compelled *shifts* of attention in addition to changes in the size of the attentional breadth. Moreover, given that in Benso et al. (1998), the primary dependent variable was simple detection response time (RT) to a small disc following either a small or larger “inducer” stimulus, it is possible that their results reflect response priming (small inducer primed response to the small stimulus more

effectively than the large stimulus), rather than true changes in attentional breadth. This means that it is crucial to choose a method that genuinely operationalises attentional breadth, and not inadvertently another attentional processes instead of / as well as attentional breadth.

Another potential source of variance in estimates of the time taken to resize attention is individual differences in attentional resizing efficiency. Improving our understanding of how individuals differ with respect to their capacity to efficiently resize their attentional breadth can provide important insight to the mechanisms underlying this process (Goodhew & Edwards, 2019). To date, the research on individual differences in attentional resizing flexibility has been focussed on establishing differences in flexibility between different groups of individuals, such as children with versus without dyslexia (Facoetti et al., 2003), individuals with versus without schizophrenia (Elahipanah, Christensen, & Reingold, 2011), and younger versus older adults (Jefferies et al., 2015; Kosslyn et al., 1999; Pesce, Guidetti, Baldari, Tessitore, & Capranica, 2005). The goal of those studies was to provide insight into the individual difference variable of interest (e.g., a given clinical condition). This tells us relatively little about the mechanisms of attentional resizing per se. For the present study, the focus on individual differences in attentional resizing was instead driven by the goal of providing insight into the mechanisms of attentional resizing itself. One recent study indicated that there are reliable, quantifiable individual differences in attentional resizing efficiency, even within a non-clinical sample of (presumably) healthy young adults (Goodhew & Plummer, 2019). The purpose of the present study was to determine the extent to which it is possible to predict an individuals' attentional resizing efficiency from other, theoretically-relevant variables.

To date, there has been relatively little theoretical development regarding attentional resizing. That is, what cognitive and neural mechanisms underlie this important process? Here, individual differences were used to provide insight into this question. Specifically, it was tested whether attentional resizing efficiency is associated with working memory capacity. Working

memory reflects the ability to regulate attentional resources, to manipulate information held in memory, particularly when this goal-directed behaviour requires inhibiting a habitual, salient, or prepotent response (Kane & Engle, 2003; Kane, Poole, Tuholski, & Engle, 2006; Redick & Engle, 2006). For context, working memory capacity is very highly correlated with the construct of *executive function* which emerges from the neuropsychology tradition, to the extent that it has been proposed that they reflect the same construct (McCabe, Roediger, McDaniel, Balota, & Hambrick, 2010; Redick & Engle, 2006; but see Rey-Mermet, Gade, Souza, von Bastian, & Oberauer, 2019). Executive function governs goal-directed behaviour and is clearly attributable to frontal lobe function (McCabe et al., 2010). Here it was reasoned that the demands of attentional resizing flexibility could be subserved by working memory. This is because attentional resizing requires actively disengaging from a current attentional breadth, which would likely be a prepotent or dominant psychological state, in service of a goal (such as identifying a stimulus that compels a different attentional breadth in order to be optimally processed).

Working memory capacity is best measured using complex span tasks, in which a span (e.g., for remembering letters) is measured while participants simultaneously engage in another task (e.g., arithmetic) (Redick et al., 2012; Turner & Engle, 1989). Individuals differ considerably in their working memory capacity, and an individuals' capacity predicts a host of outcomes, such as the capacity to multitask (Engle, 2018), the ability to deploy attention in the shape of an annulus in the face of task demands (Bleckley, Durso, Crutchfield, Engle, & Khanna, 2003), and aptitude in regulating spatial attention so as to avoid distraction (Lavie & De Fockert, 2005). Furthermore, the construct has discriminant validity, in that individual differences in working memory capacity do *not* predict performance in basic attentional processes that do not demand executive attentional control, such as attentional alerting or orienting (Redick & Engle, 2006), and visual search performance (Kane et al., 2006). In the

present study, the between-participant variation in working memory capacity was utilised to see whether it predicted attentional resizing efficiency. If working memory capacity and attentional resizing efficiency are associated, then it suggests they are related cognitive constructs. If they are not, then (provided that it can be established that each measure had sufficient reliability to detect an association), it suggests that attentional resizing efficiency is not related to working memory or executive function.

Assessing individual differences in attentional resizing efficiency calls for a task that can validly measure this process. As discussed above, attentional resizing efficiency has been operationalised in a number of quite distinct ways in the literature. However, there are a number of limitations with some existing methods. Attentional resizing efficiency as a concept encapsulates both the ability to expand and contract attention, but some methodologies have only examined one of these processes in isolation (Benso et al., 1998; Heitz & Engle, 2007; Jefferies & Di Lollo, 2009). Some paradigms have measured perceptual processing of stimuli at physically different eccentricities to gauge contraction and expansion efficiency (Kosslyn et al., 1999; Pesce et al., 2005). Perceptual acuity changes as a function of eccentricity, and therefore comparing contraction versus expansion measures across stimuli that occupy different eccentricities confounds intrinsic changes in perceptual-acuity with attentional resizing direction. To circumvent such issues, a common (and ideal) approach to operationalising many attentional processes is to have physically identical stimuli in all conditions, and merely change the attentional requirements of the task. This ensures that the measure truly captures attentional processes, uncontaminated by any changes in low-level perceptual acuity. For example, in the attentional blink (AB) paradigm, physically identical stimuli are used in single versus dual-target identification conditions to manipulate temporal attentional demands (Dell'Acqua et al., 2015; Dux & Marois, 2009; Raymond, Shapiro, & Arnell, 1992). Similarly, in the spatial attentional domain, to gauge whether spatial attention

has been shifted, responses on valid versus invalid trials are compared. Valid and invalid trials are physically identical with respect to the cues and targets used, all that differs is whether the cue appears in the location of the target or not (Posner, 1980, 2014). Here, therefore, a measure of attentional resizing efficiency was sought that provided a measure of both attentional *expansion* (increasing attentional breadth) and *contraction* (decreasing attentional breadth), and employs physically identical stimuli for in all conditions, and just the attentional requirements of the task change, thereby isolating the contribution of the attentional resizing process uncontaminated by any changes in any low-level visual processes. To meet these criteria, a modified version of the Navon task was used, which is described below. Moreover, a final requirement, given that this study adopted an individual-differences approach, is that the task produces a large range of performance across individuals. That is, some tasks can produce highly reliable group-level effects, meaning that they are well-suited for assessing the effect of experimental manipulations, but may produce little between-participant variation, rendering them unsuitable for the goals of individual-differences research (Goodhew & Edwards, 2019; Hedge, Powell, & Sumner, 2018). The modified Navon task adopted here has been found to produce substantial and reliable between-participant variance (Goodhew & Plummer, 2019).

Navon stimuli consist of a global shape (e.g., the letter ‘H’) made up of multiple instances of a stimulus at the local level (e.g., multiple letter ‘F’s constituting the global shape H) (see Figure 1). Attending to the global versus local levels of such stimuli broadens versus narrows attentional breadth, as revealed by fMRI evidence of early visual cortical areas (Sasaki et al., 2001). Historically, participants were instructed to perform *directed* tasks with these stimuli. That is, participants were explicitly instructed to identify the letter at a particular level (global or local), and the magnitude of interference from incongruent stimuli at the other level was gauged (Caparos, Linnell, Bremner, de Fockert, & Davidoff, 2013; Navon, 1977). More

recently, Navon stimuli have also been successfully used in *undirected* tasks, in which participants are instead instructed to identify one of a prescribed set of targets (e.g., identify whether the letter ‘T’ or ‘H’ is present in a stimulus), irrespective of what level (global, local) it appears at. Response efficiency to targets at the global versus local level can then be compared. A response advantage at the global level indicates a broader attentional breadth, whereas a response advantage at the local level indicates a narrow attentional breadth (Gable & Harmon-Jones, 2010; McKone et al., 2010; Sasaki et al., 2001).

While the directed version of the Navon task best captures the capacity to *set* a particular attentional breadth, an adaption of the undirected version has proved useful for measuring attentional *resizing efficiency*. In particular, modifying the proportion of trials at which the target appears at the global or local level compels participants to broaden or narrow their attentional breadth. For example, when the majority of trials in a block have the target at the global level, participants broaden their attention, and then have to contract their attention to identify the target on the minority local trials (Calcott & Berkman, 2014; Goodhew & Plummer, 2019). Similarly, when the majority of trials in a block are local, participants narrow their attention, and then have to expand their attention to identify the target on the minority global trials (Calcott & Berkman, 2014; Goodhew & Plummer, 2019). Therefore, the cost associated with resizing on the minority trials compared with the majority trials provides an (inverse) measure of attentional resizing efficiency, such that the greater the costs, the less the resizing efficiency. This modification of the undirected Navon task as a method for measuring attentional resizing efficiency has a number of key advantages, including being able to gauge both contraction and expansion efficiency, as well as employing physically identical stimuli in all conditions. Instead, all that changes between conditions is the attentional demands of the task, meaning that differences in performance can be clearly attributed to attentional processes, rather than low-level perceptual differences between conditions. Finally, previous research has

revealed considerable inter-individual variability in resizing measures gauged from this task (Goodhew & Plummer, 2019), making it ideally suited to the individual-differences approach adopted here.

Figure 1. An example of a Navon stimulus.

In addition to measuring attentional resizing efficiency, we also sought to measure participants' 'default' attentional tendency. Default attentional breadth is conceptualised as the tendency to adopt a particular attentional breadth when task instructions or design do not compel a particular attentional breadth. Default attentional breadth is influenced by both state variables, such as mood (Gable & Harmon-Jones, 2010; Rowe, Hirsh, & Anderson, 2007), and actions performed with the hands (Goodhew & Clarke, 2016), as well as more stable individual traits such as age (Lawrence, Edwards, & Goodhew, 2018), and clinical status (Koldewyn, Jiang, Weigelt, & Kanwisher, 2013). Broadened attentional breadth has also been linked to variables such as athletic expertise (Huttermann, Memmert, & Simons, 2014) and working memory capacity (Kreitz, Furley, Memmert, & Simons, 2015), and even driving crash risk (Ball, Owsley, Sloane, Roenker, & Bruni, 1993; Wood, Chaparro, Lacherez, & Hickson, 2012),

although the tasks that yielded these results do likely compel participants to broaden their attention.

We measured default attentional breadth to assess whether this constrains attentional resizing flexibility. That is, one theoretical possibility is that participants will have asymmetric attentional resizing efficiency, biased in favour of their default attentional breadth. For instance, an individual with a broad default attentional tendency may have greater efficiency in expanding attention compared with contracting their attention. Conversely, since broadened attentional breadth has been linked to increased working memory capacity (Kreitz et al., 2015), and also expertise in domains which likely demand attentional resizing efficiency – such skill and expertise in sports (Huttermann et al., 2014), another theoretical possibility is that increased attentional resizing efficiency – irrespective of direction – may be predicted by a broader default attentional tendency. In order to measure default attentional tendency, two tasks were used: the flanker task, and fixed target-level Navon blocks, each of which is described below.

The flanker task is a paradigm whereby participants respond to a centrally-presented target, and non-task relevant flankers are presented at various spatial separations, which can either be congruent or incongruent with respect to the target (e.g., A A A for congruent versus B A B for incongruent). Flanker interference is gauged as the difference in response efficiency between the incongruent and congruent trials (B. A. Eriksen & Eriksen, 1974). The extent to which flanker interference occurs at a given spatial separation provides insight into attentional breadth, with the underlying assumption that the flankers falling within one's current attentional breadth is a necessary condition for flanker interference effects to be observed (Rowe et al., 2007). One critique that could be levelled at the flanker effect is that rather than reflecting default attentional tendency, it could instead reflect *minimum* attentional breadth, as participants might adopt a strategy of narrowing their attentional breadth in an attempt to

exclude the flankers (Heitz & Engle, 2007). If this is so, then it means that the task demands are stable – it is always optimal to contract rather than expansion attention. Therefore, irrespective of which of these interpretations is true, the magnitude of flanker interference can be thought of as a more static measure of the breadth of attention, where task demands do not demand *dynamic* resizing. Here, therefore, the flanker task was included to permit assessment of whether such static setting of an attentional breadth was related to the dynamic resizing of attentional breadth.

Similarly, fixed target-level Navon tasks were also used as a measure of default attentional preference. These were blocks where the target was always at the global or local level for all trials. Therefore, comparing RTs for the all-global versus all-local blocks provides insight into participants' tendency to adopt a global (broad) versus a local (narrow) attentional breadth.

Present Study. The purpose of the present study was to assess whether attentional resizing efficiency is associated with working memory capacity. To test this, the relationship between individuals' working memory capacity and their efficiency in attentional resizing (both contraction and expansion) was measured. Recent recommendations are that working memory capacity in university samples should be operationalised via multiple complex span tasks (Draheim, Harrison, Embretson, & Engle, 2018), and therefore here both the automated operation span (AOSPAN) (Unsworth, Heitz, Schrock, & Engle, 2005), and the automated symmetry span (ASymmSpan) (Redick et al., 2012) tasks were used. Attentional resizing efficiency was gauged from the costs associated with contracting and expanding attention from dominant attentional breadths established via task demands with Navon stimuli. In addition, baseline attentional breadth was measured via the flanker task and fixed target-level Navon blocks. Participants ethnicity was also recorded, since individual differences in attentional breadth have been documented as a function of participants' country of birth (McKone et al.,

2010, but see Lawrence, Edwards, Chan, Cox, & Goodhew, 2019). Given age-related individual differences in attentional breadth (Lawrence et al., 2018), participants were required to be between 18 and 40 years of age.

If attentional resizing is related to working memory capacity, then there should be a reliable inverse correlation between performance on the AOSPAN and/or the ASymmSpan on the one hand and attentional resizing cost scores from the Navon task on the other. It is possible that default attentional breadth constrains attentional resizing flexibility – for example, individuals who have reduced flanker interference scores at smaller separations between the target and flanker may have an advantage at contracting rather than expanding their attention. Conversely, since working memory capacity has been linked to broader attentional breadth (Kreitz et al., 2015), it is possible that those with a larger flanker interference scores at larger spatial separations between the targets will have enhanced attentional resizing efficiency regardless of whether this is in the direction of expansion versus contraction. Either way, this will provide insight into any relationship between default attentional tendency and attentional resizing efficiency.

Method

Participants

The sample consisted of 121 participants (120 were sought, and so as not to turn prospective participants away who responded to the ads, 121 were run). This sample size was chosen *a priori* based on a power analysis conducted in G*Power (Faul, Erdfelder, Lang, & Buchner, 2007), where the method of analysis selected was a linear multiple regression, fixed model, R-squared deviation from zero, such that there was 95% power to detect an effect size of .15 with 3 predictors and 119 participants. This was rounded up to 120 participants. Although a variety of analyses will need to be conducted on the data, three predictors was the maximum number that would be included in any intended analysis (allowing for 2 measures of

working memory, a control variable such as the other direction of attentional resizing cost (e.g., expansion) in predicting an attentional resizing measure (e.g., contraction). This sample size would therefore provide more than sufficient power for all other analyses (e.g., simply determining whether there is a correlation between two variables).

Demographic data were unavailable for one participant. For the remaining 120 participants, mean age was 21.97 years ($SD = 3.56$), 42 were male and 78 were female. Of these 120, 101 reported being right-handed, 16 left-handed, and 2 reported being ambidextrous. In terms of country of birth, 31 participants reported an East Asian country (e.g., China, North Korea, South Korea, Japan, Vietnam), 23 a South East Asian country (Indonesia, Malaysia, Philippines, Singapore), and 6 reported a South Asian country (e.g., India, Pakistan, Sri Lanka). A total of 51 reported being born in Australia, while 4 participants reported a European country of birth (e.g., England, Greece), 2 USA, 2 Russia, and 1 Oman. (see supplementary material for analyses based on country of birth). All participants in this study provided written informed consent prior to participation. All aspects of the experiment were approved by the Australian National University's Human Research Ethics Committee (protocol 2017.565).

Experimental Tasks.

Attentional Resizing Efficiency. To measure attentional resizing efficiency, the cost in response time (RT) to contract or expand attention from a dominant broad or narrow attentional breadth was measured, such that a greater cost indicates lower efficiency. To this end, participants completed two blocks of Navon tasks. For both, participants' task was to identify which of the two target letters (T / H) was present in the stimulus on each trial as quickly and accurately as possible. Response keys were the 'T' and 'H' keys on a standard keyboard. One and only one of the target letters was present in each stimulus. Specifically, using the nomenclature of GLOBALLETTER-LocalLetter, the eight possible stimuli were: He, Hf, Te, Tf, Eh, Eh, Et, Fh, and Ft. The global letter subtended $3^\circ \times 3^\circ$ of visual angle, with each

local letter subtending $0.6^\circ \times 0.6^\circ$ ¹. The fixation dot subtended $0.1^\circ \times 0.1^\circ$. The fixation dot and the Navon letters were black, presented centrally on a white background (i.e., 100% contrast). Stimuli were presented on a 21.5" iMac computer monitor with a refresh rate of 60Hz.

On any experimental trial employing Navon stimuli (including the fixed target level blocks described in the following section), a fixation dot was displayed for 500ms, followed by the Navon stimulus for 150ms, and then a blank screen was shown until a response was made. A 1,000ms inter-trial interval occurred between response and the beginning of the next trial. RT was recorded from the onset of the blank screen. Note that RTs were analysed from trials in which participants responded correctly (i.e., correct RT). Accuracy was expected to be at or near ceiling, with the meaningful variation present on RT. Accuracy was analysed to confirm this assumption and participant compliance with task instruction.

In the *Majority Global* block, on average 80% of trials had the target at the global level, and 20% of trials had the target at the local level. Participants were informed of this trial structure, and these trial types were randomly intermixed, with the constraint that two minority (local) trials could not occur consecutively. From this, *Contraction Cost* = RTs on the minority local trials – RTs on the majority global trials, such that a larger number indicates a greater cost (i.e., reduced efficiency) in contracting attention. In the *Majority Local* block, on average 80% of trials had the target at the local level, and 20% of trials had the target at the global level. Participants were informed of this structure, and these trial types were randomly intermixed, with the constraint that two minority (global) trials could not occur consecutively. From this, *Expansion Cost* = RTs on the minority global trials – RTs on the majority local trials, such that a larger number indicates a greater cost (i.e., reduced efficiency) in expanding attention. There were 100 trials in each of the Majority Global and Majority Local blocks. Participant-paced rest breaks were offered after 25, 50, and 75 trials. All stimuli were presented and responses recorded via MATLAB's Psychophysics Toolbox (Brainard, 1997).

Measures of ‘default’ attentional tendency. One measure of default attentional tendency arose from two blocks of fixed target-level trials. That is, participants completed one block of trials where the target always appeared at the global level (*All Global* block), and one block of trials where the target always appeared at the local level (*All Local* block). In both instances, participants were informed about the target level. Participants’ task was to identify whether the letter ‘T’ or ‘H’ appeared at the specified level as quickly and accurately as possible. There were 30 trials in each of the All Global and All Local blocks (participant-based rest breaks offered after 15 trials). If participants have a greater tendency to adopt a global attentional breadth, then their RTs should be facilitated in the All Global block relative to the All Local block, and vice versa if they tend to adopt a local attentional breadth. Specifically, $\text{Global Preference} = \text{Local RTs} - \text{Global RTs}$ from these two blocks.

Some previous experiments have employed randomly intermixed global and local trials with a 50% probability of each in order to measure attentional tendency (Gable & Harmon-Jones, 2010; McKone et al., 2010), however, recent work has suggested that this may gauge the dynamics of attentional resizing at least to an extent (Goodhew & Plummer, 2019). Here, therefore, the fixed target level blocks were used instead as measures of default attentional tendency, independent of attentional resizing which was measured via the Majority Global and Majority Local blocks.

The other measure of default attentional tendency was operationalised from participants’ performance in the flanker task. Here, one target letter (N or M) was presented in the centre of the screen in size 18 Courier New font. On either side of the target letter was two flankers, the same size and font, which were either congruent (i.e., the same identity as the target) or incongruent (i.e., different identity) with respect to the central target. The two flanker letters were always the same identity as one another on a given trial, were either the letter N or M, and were presented an equal distance to the left and right of the central target. The distance

separating the target from the flankers was one of: 0.25°, 0.5°, 0.75°, or 1°. Each of the eight possible combinations of congruency and target-flanker separation was represented equally amongst the 160 trials of the flanker block, where trial type was randomly intermixed. A participant-based rest break was offered after 40, 80, and 120 trials. All stimuli were black presented on a white background.

Each trial began with the fixation dot presented for 500ms, followed by the flanker stimuli for 150ms. The screen was then blank until the participant responded. Participants' task was to identify the central letter (as N or M) as quickly and accurately as possible. Response keys were the 'N' and 'M' keys on a standard keyboard. After response, the screen remained blank for 1000ms prior to the start of the next trial. Flanker interference scores (i.e., Flanker Effect scores) were calculated as the difference in RT between the Congruent and Incongruent trials, such that a larger positive value indicates slower responses on the Incongruent trials as expected.

Working memory tasks. Participants' working memory capacity was measured via two complex span tasks: the Automated Operation Span Task (AOSPAN) (Unsworth et al., 2005) and the Automated Symmetry Span Task (ASymmSpan) (Unsworth, Redick, Heitz, Broadway, & Engle, 2009). The AOSPAN and the ASymmSpan were administered using Inquisit 4 (2015) using the standardised code that is freely provided to researchers by the Engle lab.

In the AOSPAN, participants' task was to recall a series of sequentially-presented letters. Interleaved between the presentation of each letter was a math equation (e.g., $(8/2) + 9$), followed by a single number. Participants' task was to determine if the number was the correct answer to the equation, by clicking either 'true' or 'false' on on-screen buttons. Following a series of math equation and letter presentations, they were required to recall the series of letters. Participants made their response by selecting letters from a 3 x 4 onscreen

matrix of letters. The AOSPAN provides a variety of measures, here the partial storage score was used as recommended (Redick et al., 2012), which is the total number of letters recalled in the correct serial position, regardless of whether the entire trial was correctly recalled. Therefore, higher scores indicate greater recall performance. The AOSPAN has high internal consistency (Cronbach's $\alpha = .84-.86$), test-retest reliability (.83) and strong convergent and divergent validity. Normative data indicates an average partial score of 57.36 (SD = 13.65) (Engle, Tuholski, Laughlin, & Conway, 1999; Kane et al., 2004; Redick et al., 2012).

In the ASymmSpan, participants' task was to recall a sequence of red squares presented in a 4 x 4 matrix. Interleaved between each presentation of a square, a shape was presented that consisted of some black and some white squares in an 8 x 8 matrix. Here, participants' task was to determine whether the stimulus was symmetrical about its central vertical axis. Following the series of square and symmetry-stimuli presentations, participants' task was to recall the locations of squares in the order that they were presented. As per the AOSPAN, the metric assessed was participants' total corrected squares recalled in the correct serial position. Again, therefore, higher scores once again indicating greater recall performance. The ASymmSpan has high internal consistency (Cronbach's $\alpha = .76-.81$), test-retest reliability (.77), and strong convergent and divergent validity. Normative data indicates that the mean score is 27.87 (SD = 8.26). The correlation between the AOSPAN and the ASymmSpan has been found to range between $r = 0.36-0.55$ (Engle et al., 1999; Kane et al., 2004; Redick et al., 2012).

Procedure

When running a study focussed on individual differences, it is desirable *not* to counterbalance tasks, as this introduces another source of variability (i.e., task order) in addition to the individual (which is a unit of analysis) (Dale & Arnell, 2013; Goodhew & Edwards, 2019). Therefore, a prescribed running order was used for all participants. This

means that all participants completed the flanker task first. Prior to the main experimental block, participants completed a practice flanker which consisted of eight trials with onscreen visual feedback about the correctness of the response. Participants were required to score a minimum of seven correct before progressing to the experimenter proper (practice repeated as required). Following this, participants completed the Navon conditions. This began with a practice block which contained 50% Global and 50% Local trials, randomly intermixed. Again, this consisted of eight trials with onscreen feedback, and participants were required to score a minimum of seven correct to progress. Participants then completed the blocks in the following order: Navon All Global, Navon All Local, Navon Majority Global, Navon Majority Local. Next, participants completed two self-report questionnaires via Qualtrics (see supplementary material for information about these questionnaires and analyses with these measures, which did not predict attentional resizing efficiency). Finally, participants completed the ASymmSpan, followed by the AOSPAN. Participants were tested individually.

Results

Preliminary data assessment. For one participant, the working memory task components crashed during running and so data were unavailable, and two participants did not complete testing. This means that there were 118 remaining datasets used in the analysis. Two participants' data was not available for the All Local condition only, and similarly, one participant had available data for AOSPAN but not ASymmSpan, whereas two participants had available data for ASymmSpan but not AOSPAN. However, for these cases, since there was available data for key measures of attentional resizing efficiency and at least some for WM capacity, their available data was included in the analysis. Initially all analyses were conducted on all available data following the above exclusions, the results of these analyses are provided in the supplementary material, and then the data analysis was repeated with outlier exclusions applied to determine whether a robust pattern of results emerged from both approaches. All

correlations that are significant here are also significant on the unconditional analysis, unless otherwise specified. (Raw data are available here: <https://osf.io/fyk7u/>).

Invalid Proportion Excluded. For the flanker task and Navon tasks, individual responses were screened for invalid responses. Invalid responses were defined as responses other than one of the designated response keys for that task, responses quicker than 100ms (given that these most likely reflect anticipatory rather than genuine responses), and responses slower than 2.5 SDs above the participant's mean RT (given that these are outliers and are unlikely to reflect the process of interest, but instead may reflect artefacts such as the participant sneezing, and are likely to disproportionately impact the mean RT in a given condition). For a discussion of the utility of an absolute lower RT exclusion criterion and a relative higher RT cut-off, see Goodhew, Dawel, and Edwards (in press). For all tasks, the mean invalid proportion excluded was low (Means from 1.9%-3.4%). However, the proportions were relatively high (up to 29% in flanker) for isolated cases. The key correlation was not materially impacted by the inclusion or exclusion of these cases, and therefore they were retained.

Accuracy checks. While RT was the primary dependent variable, it is also important to assess accuracy as a check for compliance with task instructions. For all of the Navon and flanker conditions, mean accuracy exceeded 90%, indicative of compliance with task instructions for the group. Two individual participant's mean accuracy was approaching chance (less than 60% where chance is 50%) in a condition, and therefore the key analyses below were performed with and without these cases (i.e., imposing a 60% accuracy minimum). The inclusion or exclusion of this case had no substantive bearing on the results, and therefore they were retained. Minimum individual accuracy scores for all other conditions were acceptable (67%-90%).

Reliability of Experimental Tasks. It is important to establish the reliability of experimental measures, because the maximum observed correlation is a product of the reliability of measures contributing to the correlation (Spearman, 1910). While this is typically well understood for questionnaires, it is generally less well appreciated for experimental measures (for a discussion and practical recommendations, see Goodhew & Edwards, 2019). Here, correlations between the first and half of experimental blocks (i.e., split-half reliability) was used to estimate reliability of the Contraction Cost RT, Expansion Cost RT, Global Preference RT, and the Flanker Effect at each of the four target-flanker spatial separations measures. That is, the higher the correlation, the more reliable the measure. The difference score values were used, rather than the raw variable RTs, because raw variable RTs can artificially inflate reliability due to individual differences in RT unrelated to the process of interest (see Goodhew & Edwards, 2019). The split-half variables (i.e., the difference score for the first and second half of trials) were screened for univariate outliers (defined as z-score absolute value exceeding 3.29) and these cases were removed only for calculating the reliability for each measure. For instance, outliers identified on the Contraction Cost measure were removed for the purposes of calculating the reliability for this measure, but not necessarily removed from the calculation for Expansion Cost, unless they were also identified as outliers on that measure as well (please see supplementary material for analysis on all data with no outlier exclusions). For the Flanker Effect, each separation was considered separately for the purposes of outliers. Non-parametric analyses were used.

For Contraction Cost, reliability was reasonably high, as indicated by the Spearman's rho correlation coefficient between Contraction Cost RT calculated from the first half of trials versus the second, $r_s(116) = .56, p < .001$. Similarly, for Expansion Cost, $r_s(116) = .52, p < .001$. For Global Preference score, the correlation was somewhat lower, although still highly reliable, $r_s(111) = .31, p = .001$. For Flanker Separation 1, the two halves of trials were clearly

related, $r_s(116) = .64, p < .001$, as they were for Flanker Separation 2, $r_s(116) = .58, p < .001$, Flanker Separation 3, $r_s(117) = .64, p < .001$, and Flanker Separation 4, $r_s(117) = .72, p < .001$.

Validity. Next, the goal was to assess whether particular measures were associated in the way in which would be expected if they were valid measures. Outliers ($|z| > 3.29$) were removed for each measure entered into the given correlation. Contraction Cost and Expansion Cost were correlated with one another, $r_s(116) = .45, p < .001$, consistent with previous findings (Goodhew & Plummer, 2019). Similarly, AOSPAN ($M = 61.8, SD = 9.9$) and ASymmSpan were correlated ($M = 32.9, SD = 4.9$), $r_s(110) = .39, p < .001$. This is also consistent with previous literature (Engle et al., 1999; Kane et al., 2004; Redick et al., 2012).

Furthermore, the group-level effects were assessed with the same outlier exclusions as above. In particular, consistent with Goodhew and Plummer (2019) Experiment 2, individuals showed a greater Contraction Cost (154ms) than Expansion Cost (121ms), as revealed by a Wilcoxon Signed Ranks Test (non-parametric equivalent of repeated-measures t -test), $Z = 2.20, p = .028$. However, this should be interpreted with due caution given the fixed running order that was employed here.

The next step was to determine whether the flanker task produced patterns of performance consistent with previous research. That is, the standard flanker pattern is that whereby the magnitude of the flanker effect reduces as target-flanker separation increases (B. A. Eriksen & Eriksen, 1974). This was checked using Wilcoxon Signed Ranks Tests, which compared the mean RTs for the congruent versus incongruent condition, at each separation, where outliers on the difference score Flanker Effect at any separation were removed (115 cases remained). This indicated that the group-level Flanker Effect was significant and largest at Separation 1 (18.4ms, $p < .001$), significant at Separation 2 (5.8ms, $p = .003$), significant at Separation 3 (7.1ms, $p = .015$), but not at Separation 4 (3.2ms, $p = .152$). This is consistent with the standard flanker pattern. Note that these scores arise from an intermixed block and

therefore are balanced for order effects. Altogether, there is clear evidence for the reliability and validity of the measures used in this experiment. The next step was to assess the experimental hypotheses.

Relationship between working memory and attentional resizing efficiency. Next, the goal was to assess the relationship between working memory and attentional resizing efficiency. To this end, the AOSPAN, ASymmSpan, Contraction Cost, and Expansion Cost measures were screened for outliers and these cases were removed. Not all of the data was normally distributed, and so non-parametric analyses were employed. There was a significant association between attentional resizing efficiency and working memory capacity, as revealed by a Spearman's rho correlation coefficient between AOSPAN and Contraction Cost, $r_s(110) = .32$, $p = .001$ (see Figure 2). Notably, this was a *positive* correlation between resizing cost measure and individuals' working memory capacity. In other words, working memory capacity, as measured by AOSPAN, was related to a *greater* cost in attentional contraction efficiency.

Figure 2. A scatterplot of the positive association between AOSPAN and Contraction Cost RT (s). Note that this correlation was also assessed for multivariate outliers using Mahalanobis distance, and none exceeded the critical value.

The correlation between AOSPAN and Expansion Cost was not reliable, $r_s(115) = .17$, $p = .075$. There were no reliable correlations between ASymmSpan and either cost measure ($r_s(110) \leq .05$, $p_s \geq .614$).

Next, several additional exclusion criteria were applied to determine whether the results were robust. The positive relationship between AOSPAN and Contraction Cost was still present when an additional inclusion criterion of minimum accuracy score on the Navon-based measures of 60% was applied, $r_s(108) = .30$, $p = .002$. This did not alter the non-significance of the other correlations ($p_s \geq .105$). Similarly, when individuals whose accuracy on the AOSPAN fell below 90% were excluded in addition, the correlation was equivalent to its original value, $r(87) = .32$, $p = .002$. This also did not alter the non-significance of the other correlations ($p_s \geq .462$). Furthermore, additionally excluding cases with >10% invalid responses also did not alter the correlation between AOSPAN and Contraction Cost, $r_s(82) = .31$, $p = .005$, and did not alter the non-significance of the other correlations ($p_s \geq .464$). This indicates that the pattern of results is robust.

While the correlation for Contraction Cost with AOSPAN was reliable whereas the one for Expansion Cost was not, the next analysis sought to assess whether it was Contraction Cost that was uniquely predicting variance in AOSPAN. That is, a regression was performed with AOSPAN as the dependent variable and Contraction Cost and Expansion Cost as predictors. The overall model was significant, $F(2, 111) = 5.50$, $p = .005$. Furthermore, Contraction Cost predicted unique variance in AOSPAN, $\beta = .27$, $p = .010$, whereas Expansion Cost did not, $\beta = .07$, $p = .526$. (Note that the standardized residuals had skew and kurtosis z-scores $< |3.29|$, indicative of normally distributed residuals. Additionally, the above results also replicate when

the above accuracy-based exclusions were applied, see supplementary material). Finally, if the ASymmSpan was also added as a predictor, then the results were unchanged, and it was not a significant predictor ($p = .356$).

Why was it uniquely Contraction Cost that was related to working memory? One potential explanation for this is measurement sensitivity. That is, there may also be a relationship between working memory and attentional expansion, but it was not observed here. For instance, since a fixed running order was used in the present study, as per recommendations for individual-difference designs (Goodhew & Edwards, 2019), it cannot be ruled out that Contraction Cost was more sensitive stemming from the fact that participants completed the block from which it was calculated prior to the block from which Expansion Cost was calculated. However, it appears unlikely that differential measurement sensitivity is the cause of the differential pattern of results for Contraction Cost versus Expansion Cost. This is because whereas in previous work Contraction Cost has been more reliable than Expansion Cost measures (Goodhew & Plummer, 2019), here the reliabilities for the two measures were very similar. Furthermore, since the reliability of each measure imposes a ceiling on any correlation between two variables, there was clearly sufficiently reliability for both Expansion Cost and Contraction Cost to robustly correlate with one another. This suggests that the reason that contraction but not expansion was related to working memory capacity likely reflects the true state of affairs. A fuller theoretical discussion of this is conducted in the Discussion section.

Altogether, all of the analyses converge on the conclusion that Contraction Cost was uniquely related to AOSPAN. In other words, as individuals' working memory capacity (as measured by AOSPAN) increased, so did the extent to which they showed a cost to their attentional contraction efficiency. This relationship was not present for attentional expansion efficiency.

Relationship between default measures of attentional breadth and attentional resizing and working memory. Following this, the relationship between working memory and default measures of attentional breadth was assessed. Cases that were outliers on any of the default measures (Flanker or Navon Global Preference), the resizing measures (Contraction Cost or Expansion Cost), or the working memory measures (AOSPAN and ASymmSpan) were excluded for the purposes of the following analyses. This left 106 cases.

Global preference did not correlate with the Flanker Effect at any of the four separations ($ps \geq .135$). Global preference correlated with neither Contraction Cost, $r_s(106) = -.12$, $p = .206$, nor Expansion Cost, $r_s(106) = -.12$, $p = .231$. Contraction Cost was modestly correlated with one of the Flanker Effects, at Separation 3, $r_s(106) = -.19$, $p = .048$. However, this correlation was not reliable in the raw data (see supplementary material), and since it only just met the criteria for statistical significance, and was the only flanker separation to do so (all others $ps \geq .338$), it seems unlikely to reflect a genuine relationship between these default measures of attentional breadth. Expansion Cost was similarly unrelated to any of the Flanker Effects ($ps \geq .201$).

Global preference was not reliably related to either working memory measure ($ps \geq .064$). Similarly, none of the Flanker Effects were reliably related to either working memory measure ($ps \geq .191$). In sum, the most parsimonious description of the results is that the default measures of attentional breadth were unrelated to either the measures of dynamic attentional resizing or the measures of working memory.

Time-course analysis. The positive correlation between Contraction Cost and AOSPAN was not predicted. However, as addressed in the discussion, this likely reflects the high-capacity individuals adapting most strongly to the dominant level of the block. If this is true, then the relationship between Contraction Cost and AOSPAN should be stronger in the second half of the Contraction Cost block than the first. This was the case. Using all available

data, for the first half of trials in the Mostly Global block, the correlation between AOSPAN and Contraction Cost was smaller in the first half, $r_s(116) = .23$, $p = .015$, than it was in the second, $r_s(116) = .30$, $p = .001$. Similarly, a regression with AOSPAN as the dependent variable and first versus second half of trials-based Contraction Cost measures as predictors, the overall model was significant ($p = .033$), and the second half Contraction Cost was a reliable predictor ($\beta = .29$, $p = .020$), whereas first-half Contraction Cost was not ($\beta = -.08$, $p = .519$). This indicates that the relationship between these variables emerged more strongly in the second half of trials, consistent with this reflecting the high-capacity individuals over-adapting to the dominant majority (and doing this more strongly with a greater experience with the biased target levels).

Summary. AOSPAN performance was positively related to Contraction Cost RT, which implies that individuals high in working memory capacity were more impaired in resizing their attentional breadth from a broad to a narrow focus. The measures of default attentional preference were largely unrelated to attentional resizing efficiency.

Discussion

The ability to rapidly resize attentional breadth is a critically important psychological function. Despite this, current estimates from the literature suggest that this process may be particularly sluggish. The primary aim of the present study was to assess whether this process is related to individuals' working memory capacity. Working memory capacity was measured using two well-validated tests with excellent psychometric properties, the AOSPAN and the ASymmSpan (Redick et al., 2012). Attentional resizing efficiency was measured via modified Navon tasks, that is, by comparing response efficiency on trials where the target was at the attentional breadth that it was for the majority of trials in that block, versus trials where the target was at the opposing attentional breadth. Comparing majority-global and minority-local trials yielded a measure of contraction cost, whereas comparing majority-local and minority-global

trials provided a measure of expansion cost. These measures have previously been found to be reliable, and that individuals differ quite dramatically in the extent to which they experience these costs, with some individuals being able to do this virtually instantaneously, and others taking up to half a second (Goodhew & Plummer, 2019).

In the present study, there was a relationship between AOSPAN and Contraction Cost, but it was in the opposite direction to that predicted. That is, individuals with higher working memory scores, as indicated by the AOSPAN, were less efficient at resizing their attention on the minority trials, as compared to the majority trials. There was no relationship between AOSPAN and Expansion Cost, and ASymmSpan was unrelated to either measure of attentional resizing. Altogether, this indicates that an individual's working memory capacity does not bestow privileged efficiency in dynamically and rapidly rescaling attentional breadth.

In the following sections, I will discuss why reduced contraction efficiency (but not expansion efficiency) was observed in high working memory capacity individuals, and why working memory appears to confer a disadvantage rather than an advantage in this context. Following this, I will explain why AOSPAN but not ASymmspan might be related to resizing, and discuss the observed absence of a relationship between measures of default attentional preference and resizing. Finally, I will consider the space versus object-based taxonomy in relation to the present study, before drawing final conclusions.

Reduced contraction efficiency in high working capacity individuals

Why were high-capacity individuals selectively impaired in *contraction* resizing efficiency? The measure of contraction cost arises from *global* majority blocks. When attentional breadth is considered outside of a resizing context, it is often noted that humans have a tendency to prioritise or prefer processing the global over the local level (Badcock, Whitworth, Badcock, & Lovegrove, 1990; Baumann & Kuhl, 2005; Hoar & Linnell, 2013; Navon, 1977, 1981). This idea is not uncontested, in that there are stimulus factors, contextual

variables, and individual traits that can all mitigate this tendency for global precedence (Gable & Harmon-Jones, 2008; Kimchi & Palmer, 1982; Kinchla & Wolfe, 1979; Yovel, Reville, & Mineka, 2005). However, more broadly, and beyond Navon stimuli in particular, there is compelling evidence that humans tend to process the broad ‘gist’ of a scene prior to its more elemental details (Bar et al., 2006; Greene & Oliva, 2009), which is consistent with the notion of a general human bias toward the global elements of a stimulus or scene.

In this light, it is noteworthy that attentional contraction entails moving *away* from this preferred or ‘default’ global processing. That is, if we extend the idea of a global precedence into attentional resizing, then it is reasonable to infer that attentional expansion may be smoother, easier, and more efficient, compared with attentional contraction, because expansion entails moving toward the preferred state, and contraction away from it. Indeed, supporting evidence for this comes from the recent finding that when there was an asymmetry in efficiency, it was contraction that was slower than expansion (Goodhew & Plummer, 2019). This same asymmetry was observed here, although given the fixed running schedule (for the purposes of individual-differences, i.e., not introducing an additional source of variance at the individual level) it cannot be ruled out that order effects contributed to this result. In contrast, in Goodhew and Plummer (2019) where experimental factors were examined, counterbalancing was employed and so the increased contraction cost is meaningful and cannot be attributed to order effects.

The notion that attentional contraction is more challenging than expansion highlights the possibility that contraction may be more susceptible to the influence of other variables. In contrast, expansion efficiency may be more asymptotic at maximum efficiency, and thus less sensitive to the influence of other state or trait variables. Consistent with this notion, it has been found that attentional contraction but not expansion costs were susceptible to contextual effects (Goodhew & Plummer, 2019). In particular, the presence or absence of recent experience with

attentional resizing impacted contraction but not expansion efficiency (Goodhew & Plummer, 2019). This increased susceptibility of attentional contraction to the influence of other variables could explain why in the present study, working memory capacity was related to contraction but not expansion efficiency. That is, attentional contraction is the resizing direction that may have sufficient variance to reveal the influence of other variables, such as working memory capacity.

Why was high working memory capacity *inversely* related to attentional contraction efficiency? In other words, why did having a high working memory capacity hurt rather than help when it came to resizing from the global to the local level, such that higher capacity individuals showed a greater contraction cost? At first blush, the inverse relationship between working memory and contraction efficiency may seem counterintuitive. However, on closer examination, it makes logical sense. This is because conceptually, working memory resources facilitate adaptive regulation of attentional resources, and individuals with high capacity have ample resources. These resources may facilitate full and deep adoption of the attentional breadth that is optimal for the majority of trials, and thus these high-capacity individuals would be disproportionately affected (more ‘caught out’) on the minority trials. The reason why this relationship was only revealed for contraction and not expansion may be because contraction is more amenable to the influence of other variables, and/or because the strong implementation of breadth to match the majority of stimuli may be particularly pronounced when this also matches the general human preference for global stimuli (i.e., in the majority-global condition from which the contraction cost measure arises).

More specifically, in the block where the target is present at the global level 80% of the time and contraction cost is measured on the minority 20% of trials, one would maximise their performance on the majority of trials by fully, robustly and rigidly adopting a broad attentional breadth. A consequence of this approach would be to suffer greater cost on the minority of

local trials that do require resizing (i.e., contraction). Compare this to an individual who was less closely tuned to the attentional breadth on the global-majority trials. This individual may therefore have a more intermediate attentional breadth, which would mean less dramatic resizing was required, thereby resulting in a relatively reduced cost on the minority-local trials. Furthermore, the fact that the relationship between working memory and contraction efficiency was strongest in the second half of trials is further evidence in favour of this explanation, because in the second half of trials participants had had greater experience with the biased target-level contingencies, which would have promoted greater adaptation to the majority target level particularly amongst high-capacity individuals. In contrast, in the block where the target is present at the local level 80% of the time, the setting of the majority-level may not have been as pronounced, and expansion to the global level on the 20% minority of trials may have been easier and thus less affected by other variables, including working memory capacity.

This notion that a stronger setting of the global majority could lead to an impairment when flexibility (resizing) was required is consistent with a distinction drawn in the cognitive control literature introduced by Hommel (Hommel, 2015; Hommel & Colzato, 2017). This model espouses that there are two types of cognitive-control orientations, which can be conceptualised along a spectrum. In the extremes, a system can be biased toward *persistence*, and at the other, it can be biased toward *flexibility* (Hommel, 2015; Hommel & Colzato, 2017). A persistence orientation is characterised by the tendency to immerse in and maintain a singular focus, and to suppress irrelevant information. While beneficial in the pursuit of certain outcomes, a persistence orientation can lead to rigidity and inflexibility. In contrast, a flexibility orientation facilitates the switching between alternative possibilities, which is useful, but the downside of a system in this orientation is that it can be too distractible. Unlike existing models which draw a distinction between a control-based system and a more stimulus-driven system, this model proposes that both persistence and flexibility orientations are forms of cognitive

control, just distinct, and invokes the term ‘metacontrol’ to encapsulate gearing towards one versus the other. These persistence versus flexibility orientations have been linked to distinct dopaminergic pathways in the brain, namely mesofrontal for persistence and nigrostriatal for flexibility (Hommel, 2015; Hommel & Colzato, 2017). Different individuals can be inclined toward one style versus the other, that is, individuals can have chronic default values on this spectrum. Moreover, the same individual can be compelled toward persistence or flexibility by state factors too, such as mood (Hommel, 2015; Hommel & Colzato, 2017).

The resizing cost difference scores (contraction in particular) in the present study may gauge this persistence/flexibility cognitive control dimension. That is, a tendency toward persistence would facilitate adopting the global majority, and suffering a greater cost when resizing is infrequently required on the minority trials, especially when these minority trials are local-level targets. A tendency toward flexibility would facilitate rapid switching between the global and local levels, perhaps especially pronounced in the change from a global to a local level which means moving against the grain of the general tendency to prefer the global level. In the context of the present results, it is possible that individuals with high working memory capacity may be more likely to be biased toward persistence rather than flexibility.

The notion that individuals high in working memory capacity were most responsive-to and persistent-with the attentional breadth that was optimal for the majority of trials in the global-biased block reconciles the present results with previous work. Previous research examining the temporal dynamics of attentional contraction using a variant of the flanker task found that high working memory capacity individuals were more efficient at contracting their attention (Heitz & Engle, 2007). This is the opposite of what was found here. The divergent demands of the different tasks to gauge attentional resizing across the two studies are likely responsible for this difference. In the flanker task, the flankers are never task-relevant, instead it is exclusively the central target that is task-relevant. Therefore, the optimal attentional

allocation for this task is to consistently have as narrow an attentional breadth as possible – filtering out the flankers and just focusing on the target. It makes sense, therefore, why Heitz and Engle (2007) found that individuals high in working memory were able to do this more efficiently. In contrast, when attentional resizing is required only relatively infrequently, as was the case in the present study, then high capacity individuals are more responsive to what is required on the majority of trials which do not demand resizing, and therefore may be less efficient in moving away from this setting.

Altogether, there is a logical explanation for the inverse relationship between working memory capacity and contraction efficiency, such that high capacity individuals more intensely set their attentional breadth to match the global majority trials and therefore incur a greater cost on the infrequent resizing trials. Moreover, the finding that this was limited to contraction and not expansion efficiency likely stems from the general tendency to prefer the global level, which contraction requires moving away from whereas expansion entails moving toward. The greater difficulty of contraction may lend itself to greater relationships with or influence of other variables, such as working memory capacity. It ought to be noted, however, that while the present study was well-designed and appropriately powered, it does remain a single study. Therefore, replication of this result is required in future research to determine its robustness. In doing so, it would be useful to test a range of differently-sized stimuli to determine whether expansion efficiency is also related to working memory capacity under some conditions.

Relationship with Contraction Cost selective to AOSPAN not ASymmSpan

Why were AOSPAN scores but not ASymmSpan scores related to attentional resizing efficiency? There are at least two possible explanations. One relates to measurement – that AOSPAN was more reliable and thus better able to support an observed correlation than ASymmSpan scores. This is possible, since OSPAN measures have been found to have slightly superior psychometric properties relative to the symmetry span measures (Redick et al., 2012).

That said, however, in the present study AOSPAN and ASymmSpan were able to correlate with one another, suggesting that there was not insufficient reliability in the ASymmSpan to detect any relationship. The other explanation is that while AOSPAN and ASymmSpan scores are related, they capture distinct aspects of working memory which are in fact differentially related to attentional resizing efficiency. In support of this notion is the fact that working memory is not a monolithic construct (Baddeley, 2012). The precise alignment between tasks and the theoretical different working memory ‘modules’ is not always clearly articulated in the literature. However, it seems reasonable that AOSPAN reflects something more of a central executive, whereas ASymmSpan may be gauging more visual/spatial sub-components. Thus, the differential correlation may reflect the fact that it is something that AOSPAN preferentially measures relative to ASymmSpan (such as the central executive) that is uniquely related to attentional resizing, or to the cognitive control orientation of persistence more broadly.

No observed relationship between default preference and resizing efficiency

The present study found no evidence of a relationship between default attentional preference and attentional resizing efficiency. The global preference scores had relatively lower reliability, and this may have hampered the ability to observe relationships with other measures. However, the flanker scores had relatively greater reliability, but also did not reliably relate to attentional resizing efficiency. Of course, it is possible that flanker and Navon tasks do not reflect identical processes, especially given the reasons outlined above regarding how a narrow attentional breadth is invariantly the optimal strategy for flanker tasks with fixed-location targets (for a full discussion of different measures of operationalising attentional breadth and the implications of this, see Goodhew, in press). It is possible that if there were more trials in the all global and all local blocks, then global preference scores may have enjoyed greater reliability. All that can be concluded definitively at this point is that there is as yet no

evidence for a relationship between default attentional preference and attentional resizing efficiency.

The fact that default attentional preference scores from the flanker and Navon were not strongly related raises an important issue about the measurement of attentional breadth. Attentional breadth is measured by a whole host of different stimulus and task combinations, including but not limited to relative efficiency to global versus local aspects of hierarchical stimuli (Gable & Harmon-Jones, 2008; McKone et al., 2010), the extent to which flanking stimuli at given spatial separations are processed (Rowe et al., 2007), how inhibition-of-return magnitude manifests at different cue-target spatial separations (Lawrence et al., 2018; Wilson, Lowe, Ruppel, Pratt, & Ferber, 2016), the extent to which individuals can accurately identify shape clusters at different spatial separations (Huttermann & Memmert, 2015; Huttermann et al., 2014), and whether lag-1 sparing is observed in rapid serial visual presentation paradigms conducive to the ‘attentional blink’ (Jefferies & Di Lollo, 2009; Jefferies et al., 2015). Greater psychometric evidence for both the validity of task performance from these paradigms as measures of attentional breadth is required (for a full discussion, see Goodhew, in press). Here, Navon stimuli were chosen because there neuroimaging evidence that when attending to the local components of hierarchical stimuli, there is a smaller and more intense region of facilitation in visual cortical regions relative to when the global components of hierarchal stimuli are attended, consistent with what would be expected for different attentional breadths (Sasaki et al., 2001), and which converges with other measures of attentional breadth (N. G. Muller et al., 2003).

Space versus object-based attention

While attentional breadth resizing is an aspect of spatial attention, it is important to acknowledge that applying attention to objects of different sizes are often an integral means of achieving these changes in attentional breadth (Benso et al., 1998; Goodhew & Edwards, 2017;

Lawrence, Edwards, & Goodhew, 2020; Macrae & Lewis, 2002). One could therefore argue that it is therefore not spatial attention, but object-based attention that is being studied. To be frank, I think that the distinction between space-based and object-based attention is overrated and over relies on superficial characteristics rather than truly divergent mechanisms. However, within the confines of this taxonomy, one could argue that the process measured in this and related studies is spatial attention and not object-based attention, with the key difference being that the change in the size of the attended region continues in the absence of the inducing object. This, therefore, makes it a manipulation of the size of spatial attention (see also validating evidence covered in the previous paragraph).

Conclusion

In conclusion, attentional resizing is an important cognitive-psychological process. Previous research has documented considerable variability across individuals in the extent to which they are relatively efficient versus inefficient at this process. The present study found that individuals high in working memory capacity were relatively inefficient when a task demanded infrequent attentional contraction from a broad to a narrow attentional breadth. This is likely because these individuals were most influenced by the demands of the task and therefore most resoundingly adopted the attentional breadth that facilitated identifying the global-level target on the majority of trials. As a consequence, they experienced a greater cost on the minority of local-level target trials. This shows that high working memory capacity can, under some specific circumstances, be a relative disadvantage. More broadly, this is consistent with a distinction that Hommel and colleagues invoke in the cognitive control literature, between *persistence* versus *flexibility* orientations. The present results suggest that high working-memory capacity individuals may be prone to persistence, which yields a relative disadvantage on infrequent attentional resizing measures.

Notes

Note that the stimuli used here were smaller than those reported in the main manuscript of Goodhew & and Plummer (2019), whereas they were the same size as the experiment reported in the supplementary material.

Compliance with Ethics Standards

The author has no conflicts of interest to declare. The author has full control of all primary data, which is available as an electronic file accompanying this submission. It will be placed in an enduring repository upon manuscript acceptance. The Australian National University's Human Research Ethics Committee approved all aspects of the current research protocol, and all participants provided written informed consent prior to participation. All aspects were in compliance with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. This research was supported by an Australian Research Council (ARC) Future Fellowship (FT170100021) awarded to S.C.G

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