STAND AT ATTENTION!
EXAMINING POSTURAL EFFECTS ON ATTENTION

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Abstract

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Examining Postural Effects on Attention

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Office workers consistently report greater productivity in the workplace when standing compared to sitting (Chambers et al., 2019; MacEwan et al., 2015; Mantzari et al., 2018). In contrast, laboratory studies report inconsistent evidence that posture (sitting vs. standing) affects cognitive performance, usually operationalized as selective attention (Caron et al., 2020; 2022; Rosenbaum et al., 2017; Smith et al., 2019). The present work assessed whether the discrepancy between workplace and laboratory findings is because workplace tasks are more difficult than the tasks used in laboratory research. Three visual search experiments are reported. Search difficulty was increased in Experiments 1 and 2 and posture difficulty was increased in Experiment 3. There was no evidence that posture affected attention in any of the experiments suggesting that the failure to find an effect of posture on attention in previous work was not due to the task difficulty.

Keywords: Selective Attention, Posture, Cognition, Cognitive Resources, Embodied Cognition, Dual Task.

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Introduction

According to theories of embodied cognition, cognitive processes are distributed across the brain, the body and the environment (e.g., Clark, 1999; Heyes & Catmur, 2022; Wilson, 2002). For instance, embodied theories of language argue that language comprehension is tied to how the body interacts with the world (Barsalou, 1999; Rizzolatti & Arbib, 1998; Zwann & Taylor, 2006). Similar arguments have been made about memory (Andersson et al., 2002; Bayot et al., 2018; Kerr et al., 1985) and even attention (Huxhold et al., 2006; Morioka, et al., 2005; Woollacott & Shumway-Cook, 2002).

One implication of embodied theories of cognition is that they suggest the body plays a role in how people think. Consistent with this possibility, people report greater productivity in the workplace when standing compared to sitting (Chambers et al., 2019; Gao et al., 2016; Garland et al., 2018; MacEwan et al., 2015; Mantzari et al., 2018). This suggests that posture (the body) affects cognitive processes. Inspired by this observation, several studies have examined how selective attention is affected by posture (Bayot et al., 2018; Caron et al., 2020; 2022; Rosenbaum et al., 2017; 2018; Smith et al., 2019; Straub et al., 2022; Woollacott & Shumway-Cook, 2002). Initial research suggested that attention is affected by posture (Bayot et al., 2018; Rosenbaum et al., 2017; 2018; Smith et al., 2019; Woollacott & Shumway-Cook, 2002), however, more recent studies have failed to replicate these initial findings (Caron et al., 2020; 2022; Straub et al., 2022). The present thesis will examine whether the failure to find reliable evidence that selective attention is affected by posture is due to the difficulty of the attention and posture manipulations in previous work.

Postural Embodiment

Recent work examining the relationship between posture (operationalized here as sitting versus standing) and cognition was inspired by the recent popularity of standing desks in workplace and educational settings due to the beliefs about increased productivity (Chambers et al., 2019; Garland et al., 2018; MacEwan et al., 2015; Mantzari et al., 2018). Researchers have found that workers self-report increased productivity when they are standing. A scoping review by Chambers et al., (2019) found that 19 out of 21 studies reported increased productivity levels when participants were standing compared to when they were sitting. For example, after a six-month intervention with university faculty, Gao et al. (2016) found that participants given standing desks reported higher work ability (e.g., a greater sense of competence) and productivity (e.g., completing more tasks and feeling more accomplished) compared to a control group. Overall, Gao et al. (2016) argue that adjustable desks in office settings are effective for improving health and work-related outcomes.

Two different types of productivity assessments are used when considering productivity in a workplace setting (Chau et al., 2015). One type of productivity assessment is referred to as *subjective productivity*. This type of productivity assessment is based on quantitative and qualitative examinations of employees' work-related feelings (e.g., happiness), energy (e.g., fatigue), and perceptions (e.g., perceived work quality) in addition to their reports of how productive they feel at work (Chau et al., 2015; Karakolis & Callaghan, 2014). For example, Chau et al. (2015) assessed subjective productivity by having participants respond to statements such as *"I am able to sustain the level of energy I need throughout the work day"* and *"I feel positive at work most of the time"* using a 5-point Likert scale. These types of subjective productivity assessments have been used in many studies to understand individuals' thoughts and feelings of productivity (Alkhajah et al., 2012; Chau et al., 2015; Gao et al., 2016; Gibbs et al., 2017; Karakolis & Callaghan, 2014; Nevala & Choi, 2013; Thorp et al., 2014; Van Steenbergen et al., 2024).

The other type of productivity assessment used in workplace settings is referred to as *objective productivity*. This type of productivity assessment is based on the work completed to meet organizational goals and objectives (Chau et al., 2015). It is often measured using the quantity and/or quality of work produced by employees such as keystrokes per minute or error rates while typing (Karakolis & Callaghan, 2014). For example, Chau et al. (2015) also measured the objective productivity of the call center employees using measures such as: call handing time, time spent talking, time on hold, and employee attendance records. The objective reports of productivity are often related to economic outcomes of the workplace. This approach is also quite common (Beers et al., 2008; Britten et al., 2016; Chau et al., 2015; Hedge et al., 2005; Kar & Hedge, 2016; Robertson et al., 2013; Van Steenbergen et al., 2024).

Mood and Subjective Productivity

Upon examination of the literature, it appears as though there are two broad theoretical accounts of the relationship between sitting and standing posture and productivity in the workplace and that they are tied to the type of productivity assessment. Increased subjective reports of productivity are often explained by arguing that posture affects mood, which in turn affects feelings of productivity (Awad et al., 2021; Nair et al., 2015; Riskind, 1984). Increased objective reports of productivity are often explained by arguing that posture affects attention, which in turn affects objective productivity (Lacour et al., 2008; Rosenbaum et al., 2017; 2018; Woollacott & Shumway-Cook, 2002).

According to the mood account of subjective productivity, there is a reciprocal relationship between posture and mood that translates to subjective productivity (Awad et al., 2021; Nair et al., 2015; Riskind, 1984). This account has three premises; 1) mood affects posture, 2) posture affects mood, and 3) mood affects productivity. Consistent with the premise that *mood affects posture*, Silva et al. (2023) reported that participants posture was affected by their mood. Participants completed the Brunel Mood Scale (BRUMS), and then underwent a posture assessment while standing in a comfortable position. Participants who rated normal on the BRUMS for tension, fatigue, and mental confusion had a neutral head position with the head/neck being in line with the rest of the body/spine. Whereas participants who rated high in the negative emotions of tension, fatigue and mental confusion had their head tilted downwards resembling a stooped posture. Participants who rated high in vigor had a neutral head position. Silva and colleagues (2023) argue that their findings support the claim that negative emotions are related to stooped postures and positive emotions correspond with more straight and upright postures.

Consistent with the premise that *posture affects mood*, Veenstra et al. (2016) conducted a study that observed how body posture (sitting stooped, sitting upright or sitting neutral) influences mood regulation processes. They reported that a stooped sitting posture affected mood recovery after a negative mood induction procedure (imagine your best friend just died) whereas upright sitting postures did not affect mood recovery. A stooped posture also induced more negative thoughts and negatively affected emotional regulation recovery. In contrast, the upright posture and neutral posture groups, induced more positive thoughts as time passed in the experiment and they showed increased mood recovery. Veenstra et al. (2016) therefore argued that body posture contributes to mood, emotions and mood regulation.

The effects of mood, posture and productivity are generally explained as follows. Research has shown that happy moods can lead to increased productivity by increasing motivation, creativity, focus, and better overall work performance (Gadigi & Veerabhadrappa, 2023). In contrast, negative moods can lead to decreased productivity by reducing energy levels and increasing distraction rates which decreases performance (Gadigi & Veerabhadrappa, 2023). In terms of posture, when a worker is sitting their posture is considered slumped whereas when standing it is upright. Evidence suggests that slumped postures instill negative moods whereas straight, upright postures instill happy, positive moods (Awad et al., 2021; Nair et al., 2015; Riskind, 1984; Silva et al., 2023; Veenstra et al., 2016). Therefore, standing leads to increases in the positive reports of productivity in the workplace, as people feel happier and enjoy their environment more, so they rate higher on their work related thoughts, feelings and perceptions (Alkhajah et al., 2012; Chau et al., 2015; Gao et al., 2016; Gibbs et al., 2017; Karakolis & Callaghan, 2014; Nevala & Choi, 2013; Thorp et al., 2014; Van Steenbergen et al., 2024). Sitting has the opposite effect as it leads to negativity and therefore decreased ratings of productivity. Therefore, mood appears to be an explanation of the effects of posture on subjective reports of productivity in the workplace.

Attention and Objective Productivity

Whereas changes in mood are argued to explain increased subjective productivity when standing, attention often is argued to explain increased objective productivity when standing. The attention account of productivity assumes that posture and attention use a shared attentional resource (Lacour et al., 2008). According to this account, most cognitive tasks do not require all available attentional resources, which then leaves people open to distraction by irrelevant stimuli in their environment (see Lavie et al., 2004). Maintaining a standing posture uses up some of the still available shared resource, reducing vulnerability to distraction. Therefore, increased productivity in the workplace when standing arises because standing at a desk uses the same attentional resources that are needed to perform the office task, thereby reducing the likelihood that irrelevant stimuli distractions in the environment will affect performance (Rosenbaum et al., 2017; 2018).

Critical to the attention account of increased objective productivity when standing is the claim that postural control requires processing resources. According to this account, postural control is a complex process that requires the integration of vestibular, visual, proprioceptive, tactile information and a representation of the body's orientation in space to hold a body's posture (Lacour et al., 2008; Samuel et al., 2015; Shumway-Cook & Woollacott, 2000). Further, this internal representation of posture must be continuously updated with feedback from these sensory systems (Samuel et al., 2015). This continuously updated representation is then used to elicit motor commands to maintain the body's position in space, while also considering the environment and possible hazards or constraints (Lacour et al., 2008; Woollacott & Shumway-Cook, 2002). Although postural control is a complex and dynamic process, it largely experienced as unconscious and automatic and it is regulated by subcortical nervous structures and spinal-motoneuronal pools (Lacour et al., 2008). Despite this, it argued to require attentional resources. The resource requirements are seen as small for young adults, but are believed to increase with age and under conditions where postural control conditions become more challenging (i.e., balancing on one foot) an increased contribution of cortical structured is required (Lacour et al., 2008; Woollacott & Shumway-Cook, 2002).

It is argued that the operation of the postural maintenance feedback loop can be seen during quiet standing (Lacour et al., 2008; Morioka et al., 2000; 2005; Samuel et al., 2015;

Shumway-Cook & Woollacott, 2000). Quiet standing refers to a situation in which a person is standing still with an upright posture. Under these conditions, the body makes very minor pendulum-like movements from the ankle and above to maintain the upright posture (Morioka et al., 2000; 2005). It is argued that these small movements are the evidence of the motor commands being elicited due to the feedback loop with the central nervous system (Shumway-Cook & Woollacott, 2000). Consistent with postural control requiring cognitive resources during quiet standing Andersson et al. (1998) found that participants made more pendulum-like movements when they had to complete a visuospatial mental task at the same time as standing upright compared to less pendulum-like movements when they stood upright but completed no additional cognitive task. They argue that postural control was reduced when posture and cognition were both using shared resources.

Claims that postural control uses cognitive resources has led to studies examining which cognitive resources are involved in the maintenance of a posture (Bayot et al., 2018; Caron et al., 2020; 2022; Rosenbaum et al., 2017; 2018; Smith et al., 2019; Straub et al., 2022; Woollacott & Shumway-Cook, 2002). If maintaining a posture requires cognitive resources, then depleting these resources should affect both posture and other cognitive tasks that use the same resource. Therefore, there are two predictions that follow from this attention account of postural effects on workplace productivity: 1) posture should be affected by the resource demands of cognitive tasks that use the shared resource, and / or 2) performance in cognitive tasks should be affected by changes in posture that increase demands for this resource.

Some of the initial research examining the relationship between posture and cognition was conducted by kinesiologists (Bayot et al., 2018; Huxhold et al., 2006; Morioka et al., 2005; Lacour et al., 2008; Woollacott & Shumway-Cook, 2002). This research was primarily focused on testing the prediction that posture should be affected by cognitive processes. In contrast, research by cognitive psychologists has examined the prediction that cognitive processes should be affected by posture, and, in particular, the relationship between selective attention and posture (Caron et al., 2020; 2022; Rosenbaum et al., 2017; 2018; Smith et al., 2019; Straub et al., 2022). To begin this systematic review, I will focus on the latter and return to the kinesiology research in the general discussion.

Does Posture Affect Selective Attention?

Psychology research examining the relationship between posture and attention have all used a straightforward manipulation of posture (sitting vs. standing) and well-established cognitive tasks believed to index attentional processes (e.g., The Stroop task, Visual Search, and Task Switching). In what follows, I will provide a systematic review of the research that has reported evidence that posture affects attention. I will then provide a systematic review of the failures to replicate these initial findings.

Evidence that Posture Affects Selective Attention

Initial evidence in psychology research found that posture affects selective attention. The first six experiments that examined postural effects in the Stroop task, the visual search task and task switching reported consistent support that posture affects selective attention (Rosenbaum et al., 2017; 2018; Smith et al., 2019). These findings will now be discussed.

The Stroop Task. The Stroop task is seen by many as to be the ‘gold standard’ of attentional measures (MacLeod, 1992; Stroop, 1935). In the Stroop task, participants are instructed to name the ink-colour of an object (MacLeod, 1991; Stroop, 1935). Typically, the coloured object is a word that is either congruent or incongruent with the ink-colour (Dalrymple-Alford & Budayr, 1966). For example, a congruent trial would be the word ‘blue’ presented in *blue* ink, while an incongruent trial would be the word 'red' presented in *blue* ink. The standard finding, known as the Stroop Effect, is that incongruent trials have consistently longer response times and higher error rates, compared to congruent trials. The Stroop Effect occurs due to a failure to selectively attend to only the ink-colour (see MacLeod, 1991 for a detailed discussion).

Using the Stroop task as their measure of selective attention, Rosenbaum et al. (2017; 2018) provided initial evidence that standing can improve selective attention. They conducted 3 experiments where participants completed versions of the Stroop task while both sitting and while standing. In Experiment 1 ($N = 17$), participants named the ink-colour of colour words. Four colours were used and there was an equal number of congruent and incongruent trials. In Experiment 2 ($N = 16$) a variation of the Stroop task was used where arrows were presented to participants, and they had to press corresponding keys on a keyboard to indicate the direction the arrows pointed (upwards or downwards). The arrows were presented either above or below a fixation point but the spatial position of the arrows was to be ignored. A congruent trial was when the arrow pointed in the direction corresponding to the spatial position of the arrow, for example an arrow pointing upwards while presented above the fixation point. An incongruent trial was when the arrow pointed the opposite direction of the spatial position, for example an upwards pointing arrow located in the spatial position below the fixation point. Experiment 3 ($N = 50$), was a replication of Experiment 1, but participants were unaware of what the study hypothesis consisted of and other demand characteristics. All three experiments showed a smaller Stroop Effect when participants were standing compared to sitting. In Experiment 3, specifically 35 out of 50 of the participants were consistent with a smaller Stroop Effect when standing.

An additional demonstration that posture affects selective attention in the Stroop task was reported by Smith et al. (2019). In their Experiment 1 (*N* = 14), they attempted to replicate Rosenbaum et al.'s (2017; 2018) findings. However, in Smith et al.'s (2019) methods, participants made button press responses (instead of vocal) and only two colours were used (red or green). Consistent with Rosenbaum's findings, Smith et al. found a smaller Stroop Effect when participants were standing compared to sitting, suggesting improved selective attention when standing.

Task Switching. In task-switching experiments, participants are required to alternate between two simple tasks (e.g., shape discrimination and colour discrimination), often without needing to process the meaning of the stimuli (Allport et al., 1994; Rogers & Monsell, 1995; Monsell, 2003). A switch trial is one where the task on trial n is different from trial n-1. For example, identifying a shape on trial 2 when the colour was identified on trial 1. A non-switch trial is one where the task on trial n (identify shape) is the same as the task on trial n-1 (identifying shape). The standard finding is that response times are longer and error rates are higher on switch trials than on non-switch trials (a switch cost) (Allport et al., 1994; Rogers & Monsell, 1995; Monsell, 2003). Switch trials require executive control to retrieve the new task instructions and responses and to shift attention from the past relevant features of the stimuli (i.e. colour) to the new relevant features (i.e. shape; see Allport et al., 1994; Rogers & Monsell, 1995). In cases where the stimuli are bivalent, that is they afford both tasks (e.g., a coloured square), the switch costs are in part due to failures to selective attend to the relevant dimension of the target (Smith et al., 2019).

Smith et al.'s (2019), Experiment 2 (*N* = 30), examined whether posture affected task switching performance. Participants switched between identifying the colour (yellow or blue) or the shape (triangle or square) of a visual stimulus. The stimuli were bivalent so that there was a coloured shape presented on each trial. The task that was to be performed on a given trial was indicated by the border type (solid or dashed line) of a square cue presented before each trial. Participants responded using button presses and type of trials (switch versus no-switch) were randomized. Smith et al. (2019) found a reduction in switch costs when participants were standing compared to sitting (see Table 1). They argue that this provides additional support for improved selective attention when standing.

Visual Search. Visual search is a task that involves searching a display for a target among distractors (Schneider & Shiffrin, 1977; Shiffrin, 1970; Shiffrin & Schneider, 1977; Smilek et al., 2007; Treisman & Gelade, 1980; Wolfe, 1998a; 1998b). Visual search experiments measure the efficiency of selective attention using search rates, sometimes referred to a search slopes, which is the time needed to search the items in the display to find the target as a function of the number of distractors (e.g., Wolfe, 1998a; 1998b).

Visual search has been widely used to gain insight into selective attention. There are two broad types of visual search task; one where participants must decide if a target is present or absent and one where participants must decide which target is in the display. The present/absent variation of visual search has traditionally been used to assess whether the search is self-terminating or exhaustive (Sternberg, 1969a; Wolfe, 1998a). In contrast, the target discrimination variation is often used to examine search efficiency (Wolfe, 1998a). This latter variation of the task has been used in studies examining the relationship between posture and attention.

The standard finding in visual search experiments is that the time to find the target increases linearly with the number of distractors (e.g., Sternberg; 1969b; 1975; Treisman & Gelade, 1980; Wolfe, 1998a; 1998b). It is widely accepted that the linear relationship is due, in part, to participants serially searching the display until a match to the target is made. Once a match is made, response selection and response execution can occur, and the search is terminated. Therefore, more distractors present increases the number of items that need to be serially searched, resulting in a linearly increasing function of mean reaction time (Smilek et al., 2007). This explains visual search reaction times being reported as linear search slopes.

In visual search a larger search slope is interpreted as a less efficient search. Differences in search slope can be due to how quickly attention is drawn to a target (selective attention), how difficult the distractor / target discrimination is once a target is selected, and the probability of missing the target and having to re-search the display (Schneider & Shiffrin, 1977; Shiffrin, 1970; Shiffrin & Schneider, 1977; Treisman & Gelade, 1980; Wolfe, 1998a; 1998b). When the target and distractor are easily discriminated, search slope differences are typically seen as arising from differences in selective attention.

Smith et al. (2019), Experiment 3, examined whether posture affects selective attention using a visual search task. Participants had to indicate which target of either *H* or *S* was present among distractors of *E*s or *U*s (see Figure 1, a). The set sizes were 4 and 8. Participants responded via button press. The target and distractors were randomly selected on each trial. Smith et al. (2019) found that standing increased the search rates and error rates compared to sitting. Therefore, their third experiment shows that selective attention was hindered when standing. They argued that participants took less time to inspect each item in the display when sitting. Smith and colleagues explain standing may have impaired search because of the resources needed to maintain standing, leaving not enough resources to process unattended items the search display.

Summary. Rosenbaum et al. (2017; 2018) and Smith et al. (2019) found postural effects on selective attention in all 4 Stroop, 1 task-switching and 1 visual search experiment. They argue that their findings support other research that has shown the selectivity of attention has improved when under stress and high perceptual load because of the limited resources available to process distractors (Chajut & Algom, 2003; Lavie, 1995; Lavie et al., 2004). Rosenbaum et al. (2017; 2018) and Smith et al. (2019) claim standing invokes the stress and high perceptual load that improves selective attention whereas sitting is an automatic process that does not involve these conditions.

Evidence that Posture Does Not Affect Selective Attention

Despite the support that posture affects selective attention from the first 6 studies to examine the issue (Rosenbaum et al., 2017; 2018; Smith et al., 2019), there have been several compelling failures to replicate the initial findings. These studies have failed to find evidence that posture affects performance in the Stroop task, the visual search task and task switching.

Stroop Task. Caron et al. (2020), report five replication attempts of the standard Stroop experiments (Experiments 1 and 3) in Rosenbaum et al. (2017; 2018). The experiments were conducted at two different institutions and used larger sample sizes for increased power. Experiments 1 to 4 were conceptual replications. Experiment 5 was designed in consultation with the original researchers to be as close as possible to the original studies to ensure accurate replication. The first two experiments used vocal responding in the Stroop task. These studies were initially designed to test a prediction of the theoretical account proposed by Rosenbaum et al. (2017; 2018). Caron et al.'s Experiment 1 (*N* = 122) included a neutral condition that was not present in the original experiments. The neutral stimuli were strings of Xs (e.g. XXXX) that were coloured. In Experiment 2 (*N* = 122), the neutral stimuli were removed, number of trials in each condition was increased; 36 trials (Exp.1) to 60, and practice trials were added for each posture condition (sitting vs. standing), instead of at the start of the experiment as in Experiment 1. Experiment 2 also gave stricter posture instructions to participants. In Experiment 1, participants were just instructed to sit or stand depending on the condition, whereas in Experiment 2, participants were instructed to keep their feet flat on the floor approximately hip-width apart, to avoid putting their hands on the desk, and to avoid leaning on the desk in both the sitting and standing conditions. Sample sizes for Experiments 1 and 2 were predetermined to be double the sample size of Rosenbaum et al's (2017; 2018) Experiment 3 ($N = 50$), however, in anticipation of some loss of participants due to missing data such as failing to detect a vocal response, 122 participants were tested for both Experiments 1 and 2 instead of just 100 participants.

Caron et al.'s (2020) Experiments 3 (N = 99) and 4 (N = 80) were also conceptual replications that did not include neutral trials. Unlike Rosenbaum's original experiments, they used manual instead of vocal responding (manual responses were also used by Smith et al., 2019). These experiments were run independently at a different institution. Experiment 4 was identical to Experiment 3 except the difficulty of the standing manipulation was increased by having participants stand on one foot in the standing condition. An *a priori* minimum sample size of 80 was set for each of these experiments. However, data collection continued until the end of the university term when course credit participation was shut down. All four experiments failed to find any postural effects on attention regardless of the increased sample sizes and power. Stroop Effects were consistently found but there were no differences in the size of the Stroop Effects across posture conditions.

Given that Caron et al.'s Experiments 1 – 4 were conceptual replications of the initial studies reported by Rosenbaum et al. (2017; 2018) Experiment 5 ($N = 61$) was designed in consultation with the original researchers to be as close as possible to Rosenbaum et al.'s (2017; 2018) Experiments 1 and 3 to ensure accurate replication. Unlike the previous experiments, this experiment was pre-registered, and the sample size was determined using the Bayesian sequential analysis procedure (Wetherill, 1961). This entailed collecting data until a Bayes Factor of 5 was obtained in favour of either the null or alternative hypothesis for the interaction between posture and congruency (Wetherill, 1961). Experiment 5 failed to find any effects of posture on attention regardless of the increased sample sizes and power. There was evidence of Stroop effects but there were no differences in the size of the Stroop effects across posture conditions.

An additional failure to find evidence that the Stroop Effect is affected by posture was reported by Caron et al.'s (2022), pre-registered Experiment 1. In Experiment 1, they report a replication attempt of Smith et al.'s (2019) Experiment 1. The only notable difference Caron et al. (2022) included was an increased sample size from 14 (Smith et al., 2019) to 50 to increase the statistical power. Sample size was determined *a priori* by calculating the statistical power for repeated trials (Goulet & Cousineau, 2019) using SuperPower (Lakens & Caldwell, 2021). The power calculations were based on the reported effect sizes in Smith et al. (2019), to determine the minimum effect sizes of interest. Based on a minimum effect size of $\eta_p^2 = 0.05$ it was determined that there was a 99.50% chance of obtaining the minimum effect size of interest and a 99.99% chance of obtaining the effect sizes reported in Smith et al. (2019) when a sample size of 50 was used (Caron et al., 2022). As a consequence of this power analysis, sample sizes were larger than Smith et al. (2019). Caron et al.'s (2022) Experiment 1, found no effects of posture on attention along with a large Bayesian factor in support of the null hypothesis (BF = 16).

Following the failed replications, Straub et al. (2022) conducted a meta-analysis of effect sizes for 10 posture experiments that use the Stroop task. Straub found that the effect size for all experiments was a very small effect size (Cohen's d = 0.06). The effect size confidence intervals also included the null (*CI* = [-0.04, 0.15]).

Task Switching. Caron et al. (2022) also attempted to replicate the finding that posture affects task switching performance. They conducted a pre-registered replication of Smith et al.'s (2019) task switching experiment. Caron et al.'s (2022) used the same method as discussed prior (Exp. 1) to calculate their sample size (*N* = 51). The increase in sample size was the only difference in their task switching replication of Smith et al.'s (2019) Experiment 2. Caron et al.'s (2022) Experiment 2, found no effects of posture on attention along with a large Bayesian factor in support of the null hypothesis (BF = 7).

Visual Search. Caron et al. (2022) attempted to replicate the effects of posture on visual search. They conducted a pre-registered replication of Smith et al.'s (2019) Experiment 3. The only difference between Smith et al.'s (2019) and Caron et al.'s (2022) work was that Caron et al. (2022) again used a larger sample size (*N* = 50), based on their power calculations. Caron et al.'s (2022) Experiment 3, found no effects of posture on attention along with a large Bayesian factor in support of the null hypothesis (BF = 5).

Summary. Caron et al. (2020; 2022) conducted eight replication attempts of Rosenbaum et al.'s (2017; 2018) and Smith et al.'s (2019) work. None of the experiments conducted by Caron et al. (2020; 2022) found postural effects on attention when using the Stroop task, task switching and the visual search task. All experiments found evidence that attention was manipulated (Stroop Effects, switch costs and set size influences on search rates were reported) however, none were influenced by posture and, were therefore, inconsistent with postural effects on attention. In fact, Straub et al. (2022) found with a meta-analysis that only 1 out of 16 studies on postural effects on attention has been able to report a main effect of posture. The failures to replicate initial findings that posture affects selective attention leads me to believe that the effects originally found do not appear to be as robust as once reported (see Table 1 for comparison of past research effects).

Table 1.

Comparison of Past Research Effects

Study	Experiment	Sample Size	Interaction of Interest	Interaction
Rosenbaum et al. (2017; 2018)	1	17	Posture X Congruency	$F(1, 16) = 5.701, p = .03, \eta_p^2 = .263$
	2	16	Posture X Congruency	$F(1, 15) = 4.062, p = .062, \eta_p^2 = .213$
	3	50	Posture X Congruency	$F(1, 49) = 8.964, p = .004, \eta_p^2 = .155$
Smith et al. (2019)	1	14	Posture X Congruency	$F(2, 26) = 4.73, p = .018, \eta_p^2 = .27$
	2	30	Posture X Switch	$F(1, 29) = 5.54, p = .026, \eta_p^2 = .16$
	3	12	Posture X Set Size	$F(1, 11) = 5.9, p = .033, \eta_p^2 = .35$
Caron et al. (2020)	1	122	Posture X Congruency	$F(1, 106) = 0.92, MSE = 838, p = .339, \eta_p^2 = .009$
	2	122	Posture X Congruency	$F(1, 106) = 0.35, MSE = 507, p = .557, \eta_p^2 = .003$
	3	99	Posture X Congruency	$F(1, 96) < 0.01, MSE = 4{,}769, p = .974, \eta_p^2 < .001$
	4	80	Posture X Congruency	$F(1, 76) < 0.001, MSE = 3{,}897, p = .993, \eta_p^2 < .001$
	5	61	Posture X Congruency	$F(1, 49) = 0.13, MSE = 675, p = .720, \eta_p^2 = .003$
Caron et al. (2022)	1	50	Posture X Congruency	$F(2, 98) = 0.08, MSE = 521, p = .922, \eta_p^2 < .002$
	2	51	Posture X Switch	$F(1, 50) < .001, MSE = 1{,}519, p = .95, \eta_p^2 < .001$
	3	50	Posture X Set Size	$F(1, 49) = 0.03, MSE = 728, p = .861, \eta_p^2 < .001$

Note. This table compares all the posture X attention interaction from past psychology research examining postural effects on attention. All interactions are comparing reaction times. The general guidelines for interpreting Cohen's d effect size are; small .20, medium .50, and large .80 effect size (Cohen, 1988).

Present Study

The repeated failures to find an effect of posture on selective attention (objective productivity) and subsequent conclusion that there is no effect of posture on selective attention (Caron et al., 2022; Straub et al., 2022) are surprising given the consistent subjective and objective reports of improved workplace performance when using standing desks (Chambers et al., 2019; Garland et al., 2018; MacEwan et al., 2015; Mantzari et al., 2018). One possible explanation for the discrepancy between workplace reports and laboratory findings could be that posture affects a different attentional system such as sustained attention ((Mackworth, 1948; Robertson et al., 1997) or central attention (Pashler, 1994; Tamber-Rosenau & Marois, 2016; Wickens, 1984). I return this possibility in the General Discussion. The failure to find laboratory support could also be due to posture affecting a cognitive process other than attention (e.g., working memory, executive function, or visual processing). However, there are other options that must be ruled out before the selective attention account is abandoned. Therefore, we will return to the alternative accounts of the workplace improvements and to the discrepancies between the workplace (real-life settings) and the lab-based tasks.

The previous research examining postural effects used standard cognitive tasks that are known to measure attention (e.g. Stroop, visual search, task switching). However, relative to what is observed in the real world (e.g. creating/typing documents, replying to emails, answering phones at a call centre, etc.) the laboratory tasks are quite simple; often being completed in under one second and with near perfect accuracy. For example, when considering visual search, in the laboratory task used by Smith et al. (2019; Caron et al., 2022), the targets (H *or* S) and distractors (U *or* E) were easily distinguished from distractors by one or more line features, and the search displays contained very few distractors (3 vs. 7). This is much simpler and quicker to complete than tasks in an office setting such as searching for a name on an email list with hundreds of similar stimuli or a piece of paper on a desk (Sellen & Harper, 2003). It is therefore possible that the failure to find consistent evidence that posture affects selective attention is because the selective attention tasks were too easy. If the posture and attention task share resources, but do not use enough of the shared capacity because of their simplicity, then there will still be sufficient resources for distractors to be processed. Therefore, having cognitive and posture tasks that are too easy could be the cause of null effects. It is considered if the simplistic approach to the tasks used prior is the reason for the lack of effects.

With this simplistic framing of the capacity problem in mind, it is important to consider what constitutes an easy visual search task. Wolfe (1998a; 1998b) describes a search task with a slope of 20 ms/item as an easy visual search task for target present/absent searches. The distribution for his entire sample of one million search trials resulted in a mean target present slope of 14.6 ms/item and a target absent slope of 33.0 ms/item. The target absent is higher as participants must search every single item to ensure it is not the target. Whereas in target present they only must search until they find the target, which will occur sometimes as the first stimuli examined and sometimes as the last (2:1 ratio with target absent). When considering the search slopes from the past research examining posture, they are all in this easy typical range. Smith et al. (2019) reported search slopes of 11.20 ms/item when sitting and 17.20 ms/item when standing compared to Caron et al. (2022) who reported 20.97 ms/item when sitting and 21.31 ms/item when standing. Following Wolfe's (1998a; 1998b) analysis, these past works used easy search conditions, which may not compare to the real-world settings. Perhaps all is needed is increased difficulty of the lab-based tasks used to compare more to the real world, where more of the attention capacity could be being used up, and leaving less capacity for distractor processing.

In order to provide the field a more exhaustive test of the claim that posture affects cognition, we conducted a series of three visual search experiments that address the concerns about the difficulty of the attention and posture manipulations when compared to Smith et al.'s (2019; Caron et al., 2022) study. In Experiment 1, we increased the difficulty of the search task by using larger set sizes (5, 9, 13 and 17) and we increased the difficulty of the search process by using heterogenous distractors (searching for an *S* or *H* amongst Es *and* Us). In Experiment 2, the search task difficulty was increased again by combining conjunction search (Treisman & Gelade, 1980) and variable mapping techniques (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). Experiment 3 used the same search displays as Experiment 2, however, the manipulation of posture was made more difficult by using a slant board in the standing condition.

It is predicted that if there are effects of posture on attention as suggested by Rosenbaum, Smith and colleagues, then increasing the difficulty of the visual search task and the posture manipulation should increase the chance of finding an interaction between set size and posture (a difference in search efficiency). Following past work, we expect worse selective attention in the visual search task when performed in the standing condition, relative to the sitting condition. This will take the form of steeper search slopes (less efficient search) in the standing condition. However, given the current discrepancies, a failure to observe an effect of posture on search efficiency will be interpreted based on support for the null as indicated by the Bayes Factor (Rouder et al., 2012).

All experiments were conducted at Trent University and approved by the Trent University Research Ethics Board. Participants volunteered using the Trent University SONA research portal and received partial course credit toward an eligible course. All participants provided written informed consent before beginning the experiment and they were given written and verbal debriefings at the end of the study. Following Caron et al. (2022), sample sizes of 50 participants were used for each experiment. Caron et al. (2022) completed power calculations using SuperPower (Goulet & Cousineau, 2019; Lakens & Caldwell, 2021; Supplemental Materials on https://osf.io/kwrjd). They used Smith et al.'s (2019) reported effect sizes to determine the minimum effect sizes of interest for their replication. Based on a minimum effect size of 0.05 η_p^2 it was determined that there was a 99.50% chance of obtaining the minimum effect size of interest and a 99.99% chance of obtaining the effect sizes reported in Smith et al. (2019) with a sample size of 50. Due to the similarity between this current work, Smith et al.'s (2019) work and Caron et al.'s (2022) work, the same sample sizes were chosen to allow for high statistical power.

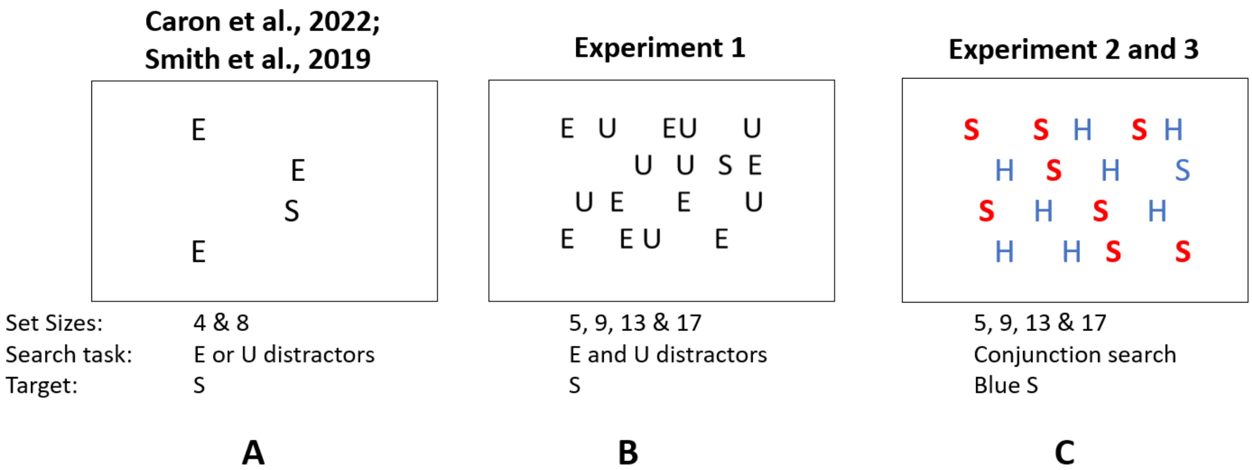

Figure 1. Comparison of visual search displays from past and present experiments. To account for black and white versions of this figure, Image C has red stimuli bolded and blue stimuli normal. In the experiment displays, the red items were not bolded.

Experiment 1

In Experiment 1, the difficulty of the visual search task was increased relative to past work in two ways. First, additional larger set sizes were included compared to past work (Caron et al., 2022; Smith et al., 2019). Larger set sizes are a simple way of increasing the difficulty of the search task without changing other elements of search process such as the target-distractor and distractor-distractor similarity (Duncan, 1980). Original set sizes consisted of 4 and 8 (Caron et al., 2022; Smith et al., 2019). The set sizes in the present studies consisted of 5, 9, 13 and 17. Secondly, the difficulty of the search was increased by using heterogenous distractors instead of homogenous distractors (Duncan & Humphreys, 1989). By using heterogenous distractors of *Es and Us* it made the target harder to distinguish compared to past work's homogenous *Es or Us* (Duncan & Humphreys, 1989). The odd number set sizes allowed an equal number of *Es and Us* to be presented as distractors on each trial. If the search slopes increase from past work's ~ 20 ms/item slopes (Caron et al., 2022; Smith et al., 2019) then it will be assumed that the difficulty of the search task was successfully increased (Wolfe, 1998a; 1998b).

Methods

Participants

Fifty Trent University undergraduate students participated for partial course credit. All participants reported normal or corrected to normal visual acuity and normal colour vision. Participants provided written informed consent before the experiment, and they received written and verbal debriefings at the end of the experiment.

Stimuli

Search displays were constructed to match the descriptions provided by Smith et al. (2019) and the replications reported by Caron et al. (2022). Search displays consisted of black block letters on a white background. The letters presented were E, U, S, and H and they were all approximately 2.26° high × 1.13° wide. Each display consisted of one target (either S or H) alongside 4, 8, 12, or 16 distractors. Unlike Smith et al. (2019), who only used a single distractor type per display (E or U), here both distractors were displayed (E and U) in equal numbers (see Figure 1, b) on each display. The letters were randomly assigned a location on a 5 x 5 grid with each letter at least 0.57° apart. The grid was presented with an extended visual angle of 10° horizontally and 8° vertically on either side of fixation.

Apparatus

The experiment was programmed in E-Prime 3.0. The experiment was conducted on a DELL XPS 8930 computer using Windows 10 Pro and an NVIDIA Geforce GTX 1050TI video card. The monitor used was a DELL 24-inch Gaming Monitor (Model S2421HGF) with a native resolution of 1920 × 1,080 (running at 120 Hz). The monitor sat on an Ergotron® WorkFit™-TX standing desk converter that was placed on an Ikea Jerker desk. This allowed stimuli to be presented at eye level for both the sitting and standing conditions. Responses were collected using USB buttons that were 3 cm in diameter and programmed/labelled as S or H for the two target options (usbbutton.com). An adjustable chinrest was used to hold participants' viewing distance at a constant of 90 cm.

Procedure

Prior to the experiment beginning, participants were informed they would be completing a visual search task under different postures. They were instructed to indicate whether the target in the search array was an S or an H by pressing the corresponding button as quickly and accurately as possible. Buttons were held in participants' left (S) and right (H) hands in a relaxed position, without resting their arms on the desk. Participants were told to keep their chin in the chinrest as best as possible during the whole experiment to limit movement. Participants stood in a normal standing position for the standing condition and sat in an ergonomic office chair during the sitting condition. The height of the chin rest and the height of the desk/monitor were adjusted at the start of each block to ensure the center of the search display was presented at eye level.

The experiment consisted of a single practice block and two experimental blocks. The practice block contained 8 trials and the experimental blocks contained 128 trials each, taking approximately 15 minutes to complete each experimental block. There was an equal number of trials per block for each set size (5, 9, 13, 17) and for each target. The display of the target letter and the set size were factorially combined and randomly selected without replacement to ensure each display was shown the same number of times during each experimental block. Following Smith et al.'s (2019) and Caron et al.'s (2022) work, the practice block was completed while sitting for all participants. Each participant also completed one experimental block sitting and the other standing, with the order of posture condition counterbalanced. Counterbalancing was assigned pseudo-randomly based on the order participants attended the lab.

Following methods from Smith et al. (2019) and Caron et al. (2022), all trials began with the fixation cross (+) at the center of the computer screen. After 1,000 ms the fixation cross disappeared, and a search array appeared. The search array remained on the screen until the participants responded. Following the participant's response, a feedback screen was presented for up to 2,000 ms under the following conditions. If participants responded within 100 ms they were given a feedback screen of "Too fast!". If participants took longer than 2,500 ms to respond they were given visual feedback of "Too slow!" (Note; relative to previous work, this was increased by 1,000 ms to account for the harder search task). If participants responded incorrectly to the target, they were given the visual feedback of "Wrong key pressed!". If participants responded correctly within the appropriate timeframe, they did not receive a feedback screen but rather were presented with a blank white screen for 2,000 ms to account for an inter-trial interval. At the end of each block, participants were given the option to take a quick break before continuing with the experiment.

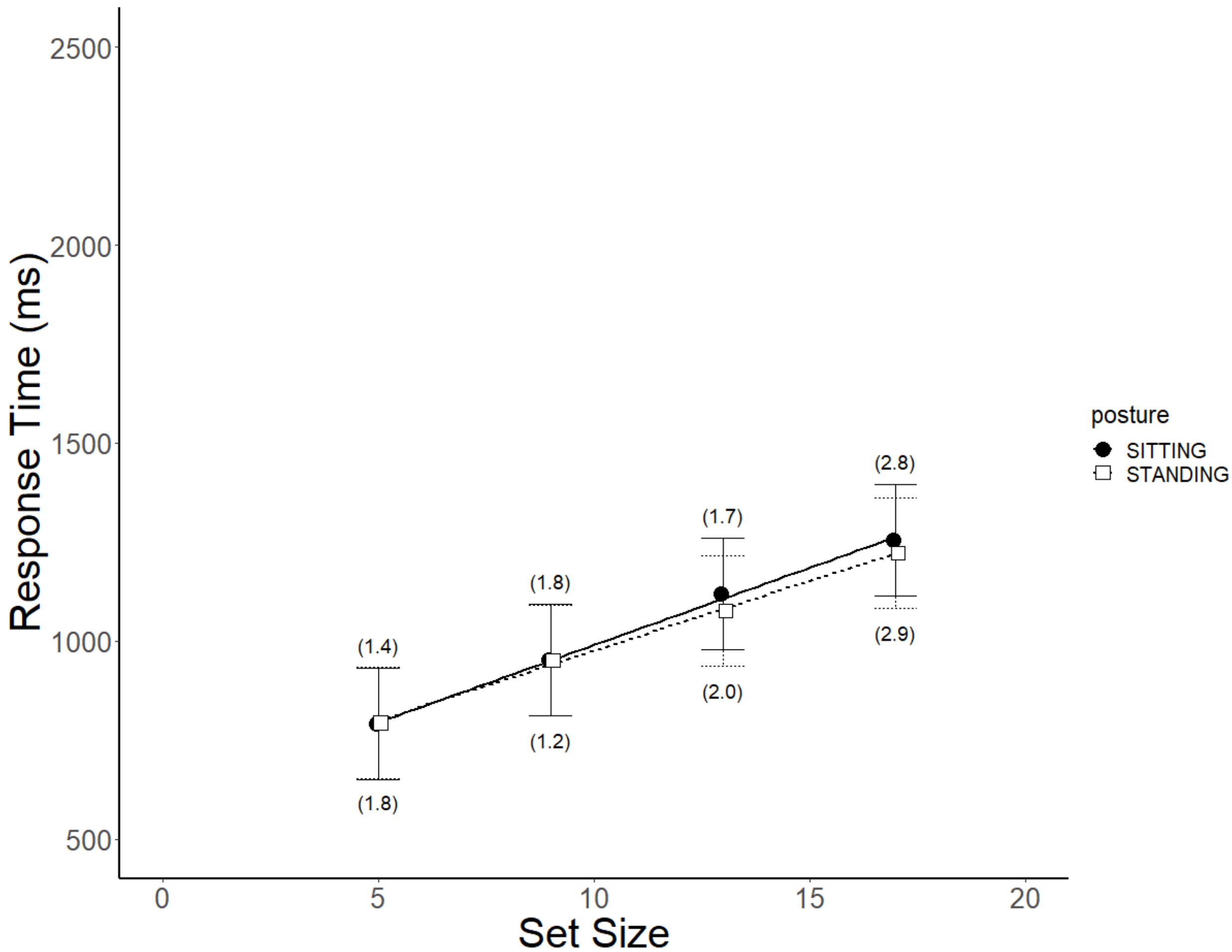

Figure 2. Mean response times and percentage error (presented in parentheses) as a function of posture from Experiment 1. Error bars represent the 95% confidence intervals.

Results

All experiment data were analyzed following the same procedures as in Caron et al. (2020; 2022) using R statistical software program (R Core Team, 2022). Separate repeated-measures analyses of variance for response time and percentage error using the EZ package were conducted (Lawrence, 2016). The anovaBF function from the BayesFactor package was used to calculate Bayes factors (BF) for each experiment (Morey et al., 2021). See Figure 2 for a visualization of mean response time (RT) and percentage error (PE) data that was averaged across participants for each condition.

Before analysis began, participants with more than 20% of their data lost due to errors, missing data and/or mistrials were excluded (Caron et al., 2022). This resulted in 2 participants being excluded due to high error rates. Therefore, the final *N* was 48 participants. Prior to analyzing the RT data, trials on which there was an error were removed (1.95%). The remaining RT data was submitted to the Van Selst and Jolicoeur (1994) recursive data trimming procedure as used in Caron et al. (2020; 2022). This procedure sets the outlier criteria separately for each cell for every participant based on the number of observations in that cell (Van Selst & Jolicoeur, 1994). This procedure removed 2.50% of the remaining RT data.

Response Time

The analysis revealed a main effect of set size, $F(3, 141) = 186.1$, $MSE = 18980$, $p < .001$, $\eta_p^2 = .798$, where RT increased as set size increased. There was no main effect of posture, $F(1, 47) = .992$, $MSE = 32270$, $p = .324$, $\eta_p^2 = .021$, inconsistent with standing improving task performance. The interaction of posture and set size was not significant, $F(3, 141) = 1.506$, $MSE = 8398$, $p = .216$, $\eta_p^2 = .031$, inconsistent with posture affecting attention. The BF results favoured the null hypothesis approximately 15.55 times more than the alternative. The search rate while standing (35 ms/item) did not differ from the search slope while sitting (39 ms/item; $t(47) = .243$, $p = .840$, *95% CI* [-34.991, 44.617]. A t-test was used to assess whether the Experiment 1 search slope (37 ms/item) was significantly larger than the average search slope of 14 ms/item reported by Smith et al.'s (2019; http://rabrams.ddns.net/wordpress/resources/). Consistent with expectations the search slopes in the present study were significantly larger, $t(47) = 10.003$, $p < .001$, *95% CI* [32.478, 41.781].

Percentage Error

There was a main effect of set size, $F(3, 141) = 6.617$, $MSE = 5.586$, $p < .001$, $\eta_p^2 = .123$, where errors increased as set size increased. There was no main effect of posture, $F(1, 47) = .034$, $MSE = 3.011$, $p = .855$, $\eta_p^2 < .001$, inconsistent with standing improving task performance. The interaction of posture and set size was not significant, $F(3, 141) = .848$, $MSE = 5.00$, $p = .470$, $\eta_p^2 = .018$, inconsistent with postural effects on attention. The BF results favoured the null hypothesis 13.92 times more than the alternative. The pattern of errors was consistent with the RT data.

Discussion

Experiment 1 showed that set size impacted participants' mean response time and mean error percentage. As the set size increased between 5, 9, 13 and 17, there was an increase in the time it took participants to respond as well as an increase in the probability of making an error. The search slopes were approximately 40 ms/item, which is a 20 ms/item increase from past work (Caron et al., 2022; Smith et al., 2019). This significant increase in search slopes relative to previous work suggests the difficulty of the search task was successfully increased. However, the set size increases in RT and PE did not differ between posture conditions. There was no effect of posture (sitting vs. standing) on the search slopes of participants inconsistent with postural effects on attention.

Experiment 2

In Experiment 1 there was an increase in the difficulty of the search task, however there were still no effects of posture on attention. Therefore, it was decided to conceptually replicate Experiment 1, but make the search tasks even more difficult. Two new ways were used to increase the difficulty of the search task. First, a conjunction search was used. In the previous visual search experiments (Smith et al., 2019; Caron et al. 2022) the target and distractors could be distinguished by one or more form / line feature differences (Treisman & Gelade, 1980; Wolfe et al., 1989a) of the block text letters (either *S* or *H* amongst *E*s or *U*s). Therefore, the targets were distinguished from distractors by a single feature (Duncan & Humphreys, 1989; Treisman & Gelade, 1980; Wolfe, 1998). Evidence suggests that visual search is much more difficult when the target and distractor are distinguished by the conjunction of 2 features (Duncan & Humphreys, 1989; Treisman & Gelade, 1980; Wolfe, 1998a). For example, searching for a blue circle (target) amongst blue squares and red circles (distractors). In this case, neither the colour of the target (blue) nor the shape (circle) is unique to the target. Experiment 1 consisted of a feature search looking for S or H amongst Es and Us. For experiment 2, we implemented a conjunction search by having the target identified by the unique combination of colour (red and blue) and identity (S and H). Participants had to find the S or H that was coloured differently than the other items of its same identity to be able to find the target. A blue S amongst blue Hs and red Ss (Figure 1, c).

Secondly, the difficulty of the search task was increased by using variable mapping techniques. Schneider and Shiffrin (1977), define variable mapping as a situation where the target on one trial can be a distractor on subsequent trials so the target is never predictable. In the past research, consistent mapping has been used which entails the targets being set and different from the distractors. For example, the targets were always set as S or H and the distractors were always Es or Us (Smith et al., 2019; Caron et al., 2022). Whereas in variable mapping techniques the target and distractors would not be set, but rather S, H, E and U would all be interchangeably targets and distractors in the same experiment (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). In Experiment 2 the target and distractors are always interchangeably S and H and interchangeably red or blue in colour. It was predicted that if the lack of postural effects is due to the difficulty of the tasks, then a complex conjunction search with variable mapping techniques will be able to find effects as it is relatively comparable to tasks completed in the real world where subjective reports of increased productivity are observed.

Methods

Participants

A novel group of 50 Trent University undergraduate students participated for partial course credit. All participants reported normal or corrected to normal visual acuity and normal colour vision. Participants provided written informed consent before the experiment, and they received written and verbal debriefings at the end of the experiment.

Stimuli

Displays used the same S and H block letters from Experiment 1. Targets and distractors were displayed in the same 5 x 5 grid and the set sizes continued to be 5, 9, 13 and 17. Unlike Experiment 1 the target and distractors were S and H. They were presented in red and blue colours. In all trials both S and H distractors were present and all of each letter were either red or blue. The target was the uniquely coloured letter. For example, one trial might contain distractors of 2 blue Hs, alongside 2 red Ss, the target would be a single blue S (see Figure 1, c). To meet the variable mapping procedure a target could be a distractor on the very next trial and vice versa.

Apparatus

Identical to experiment 1.

Procedure

Participants were instructed to identify the unique letter as the target by pressing the corresponding button. The number of practice trials in this experiment was increased to 16 as it allowed all possible combinations to be used as practice (4 set sizes by 4 possible target conditions). The criteria for feedback to slow responses was increased from 2500 ms to 3500 ms to again account for increased difficulty. Participants completed the search task in the sitting and standing posture conditions from Experiment 1. In all other ways the procedure was identical to Experiment 1.

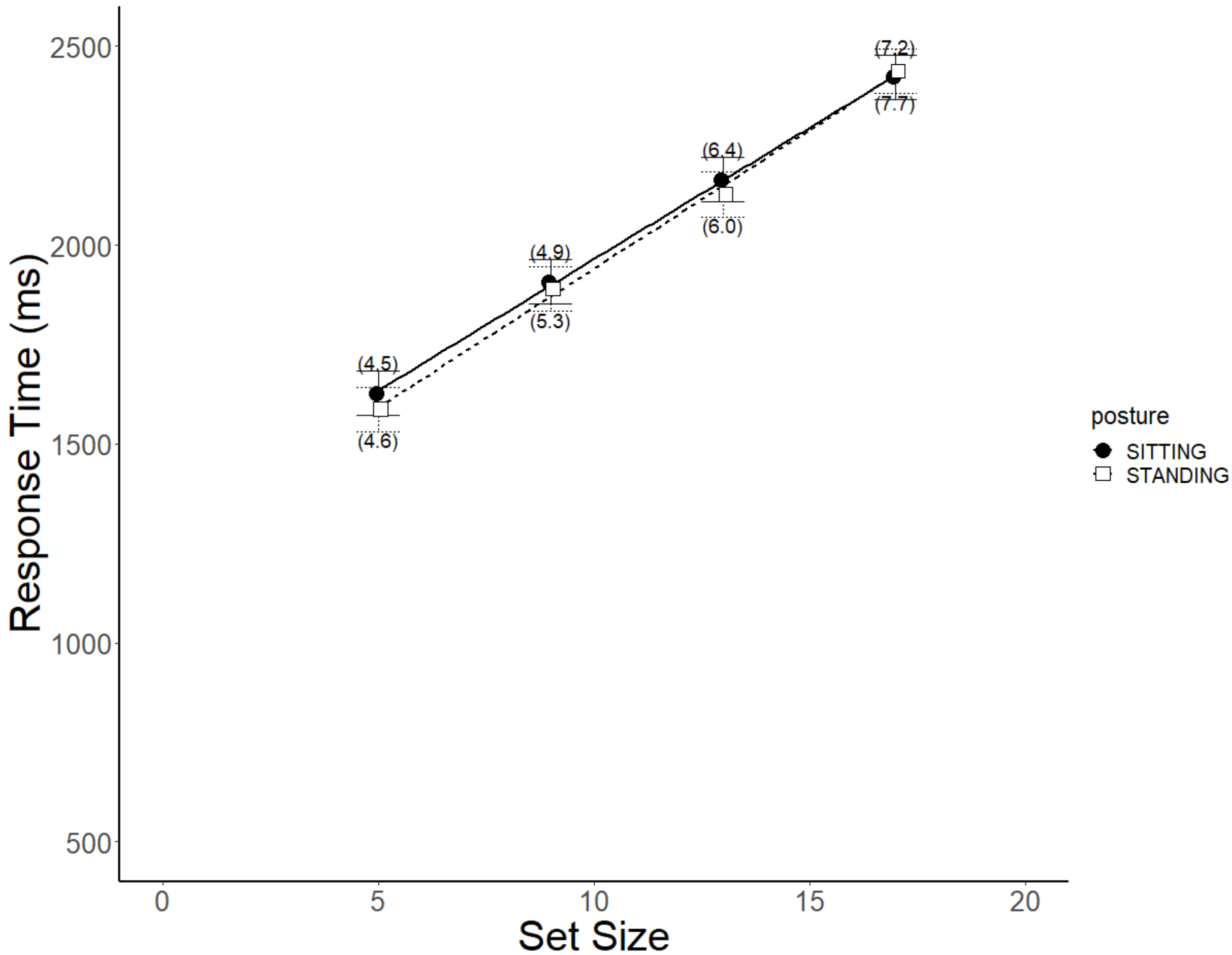

Figure 3. Mean response times and percentage error (presented in parentheses) as a function of posture from Experiment 2. Error bars represent the 95% confidence intervals.

Results

See Figure 3 for response time (RT) and percentage error (PE) data. Data analysis is the same as Experiment 1. Before analysis began, participants with more than 20% of their data lost due to errors, missing data and/or mistrials were excluded (Caron et al., 2022). This resulted in 2 participants being excluded due to high error rates. Therefore, the *N* used for analysis was 48. Prior to RT analysis, 5.84% of error trials were removed. The remaining correct RT data underwent the Van Selst and Jolicoeur (1994) procedure, which trimmed 1.53% of the data.

Response Time

There was a main effect of set size, $F(3, 141) = 229.7$, $MSE = 51360$, $p < .001$, $\eta_p^2 = .830$, where RT increased as set size increased. There was no main effect of posture, $F(1, 47) = 0.238$, $MSE = 167728$, $p = .628$, $\eta_p^2 = .005$, inconsistent with standing improving task performance. The interaction of posture and set size was not significant, $F(3, 141) = 0.731$, $MSE = 20802$, $p = .535$, $\eta_p^2 = .015$, inconsistent with posture affecting attention. The BF favoured the null hypothesis approximately 28.40 times more than the alternative. The search slope while standing was 70 ms/item, and the search rate while sitting was 66 ms/item, [$t(47) = -0.690$, $p = 0.494$, *95% CI* [-88.921, 43.491]. A Welch two sample t-test revealed the search slopes were significantly larger than Experiment 1, $t(77.38) = 6.88$, $p < .001$, *95% CI* [39.613, 21.834].

Percentage Error

There was a main effect of set size, $F(3, 141) = 8.548$, $MSE = 19.156$, $p < .001$, $\eta_p^2 = .154$, where errors increased as set size increased. No main effect of posture was found, $F(1, 47) = .042$, $MSE = 29.712$, $p = .839$, $\eta_p^2 < .001$. The interaction for posture and set size was not significant, $F(3, 141) = 0.332$, $MSE = 12.955$, $p = .802$, $\eta_p^2 = .007$, inconsistent with postural effects on attention. The BF favours the null hypothesis approximately 29.71 times more than the alternative. The pattern of errors was consistent with the RT data.

Discussion

Experiment 2 revealed that as set size increased, RTs and PE increased, however, this did not differ between posture conditions. The search slopes were approximately 72 ms/item for both conditions which was 30 ms/item larger than Experiment 1. This suggests that again the increase in search task difficulty compared to Experiment 1 was successful. And this suggests the search task was quite difficult as it was approximately a 50 ms/item increase from Wolfe’s (1998a; 1998b) suggestion that average easy searches have slopes around 20 ms/item. Therefore, it appears as if the lack of search difficulty is not the reason for the lack of previous evidence that posture affects attention.

Experiment 3

Experiments 1 and 2 were successful in increasing the difficulty of the search tasks however they revealed no effect of posture on attention. This suggests that task difficulty is not the cause of disconnect between real-world subjective reports of productivity and laboratory findings.

In office settings, postures of sitting and standing may be held for long durations such as a 7-hour workday and be difficult to sustain (Mantzari et al., 2018). However, in past laboratory studies the posture component has involved sitting or standing for brief periods of time, as the experiment took roughly 30 minutes total (Caron et al., 2022; Smith et al., 2019). In the studies examining the relationship between posture and attention there has only been one attempt to increase the difficulty of the standing posture beyond quiet standing. In Experiment 4 of Caron et al. (2020) participants were required to stand on one foot during the standing condition. However, this manipulation also failed to find any effects of posture on attention. Therefore, for Experiment 3 a more traditional manipulation of standing difficult was used. Participants in the standing condition were required to stand on a slant board (e.g., Kluzik et al., 2005). In all other ways the experiment was identical to Experiment 2.

Methods

Participants

A novel group of 50 Trent University undergraduate students participated for partial course credit. All participants reported normal or corrected to normal visual acuity and normal colour vision. Participants provided written informed consent before the experiment, and they received written and verbal debriefings at the end of the experiment.

Stimulus

Displays were identical to Experiment 2 (Figure 1, c).

Apparatus

The chinrest was disregarded in this experiment. Instead, for the sitting condition a stationary office chair was placed 90 cm away from the monitor which held participants viewing angle at a constant. For the standing condition, participants stood on a slant board instead of a normal floor. The slant board was positioned 90 cm away from the monitor to hold the visual angle constant. The slant board had incremental slanted angles between 15° to 35°. Participants chose the difficulty of the slant board angle. They were informed to use an angle that was difficult, but not impossible, to stand on without raising concerns of physical harm. All participants chose either the 20° or 25° level. The conjunction search with variable mapping techniques from Experiment 2 was used. All other apparatuses remained the same as in Experiments 1 and 2.

Procedure

Participants were given an optional break after trial 64 in both experimental blocks. A 7-point Likert scale was developed for posture difficulty quantification. At the end of the experiment, participants were asked two questions about the difficult of the standing condition.

One question was “Compared to sitting, how difficult was standing on the slant board?”. The other question was “Compared to standing normally, how difficult was standing on the slant board”. The scale went from -3 (much easier) to +3 (much more difficult) with 0 being neutral (no difference). All other elements of the procedure were identical to experiment 2.

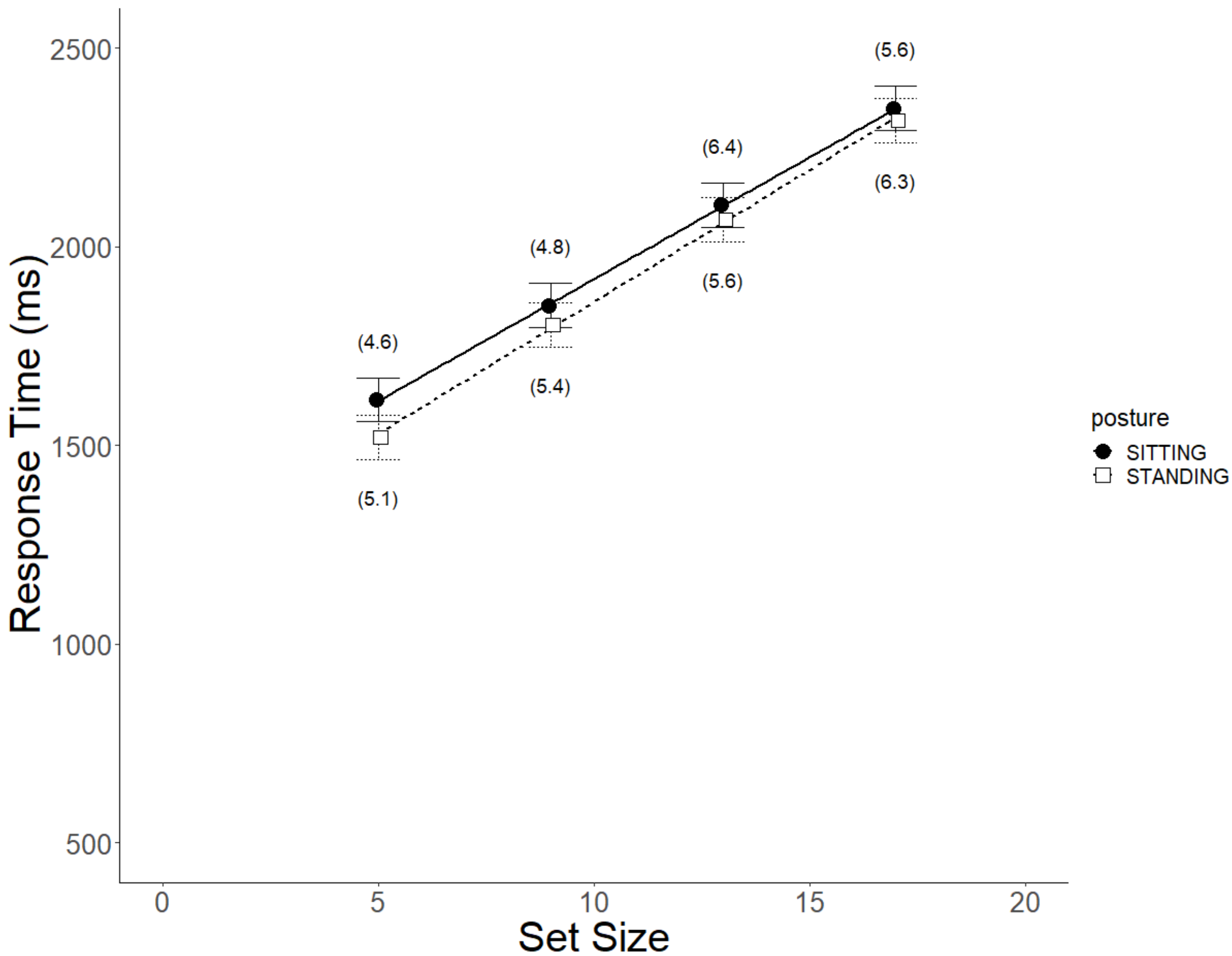

Figure 4. Mean response times and percentage error (presented in parentheses) as a function of posture from Experiment 3. Error bars represent the 95% confidence intervals.

Results

See Figure 4 for a visualization of mean response time (RT) and percentage error (PE) data that was averaged across participants and conditions. No participants were excluded due to high error rates above 20%. Prior to RT analysis, 5.453% of error trials were removed. The remaining RT data underwent the Van Selst and Jolicoeur (1994) trimming technique from Experiments 1 and 2 and 1.273% of RT data was trimmed.

Posture Difficulty

Participants rated standing on the slant board harder than sitting (1.020) [$t(99) = 7.975$, $p < .001$, *95% CI* [0.766, 1.274] and harder than standing normally (1.520) [$t(99) = 15.359$, $p < .001$, *95% CI* [1.324, 1.716] consistent with the slant board increasing the difficulty of the posture manipulation.

Response Time

Mean RT data revealed a main effect of set size, $F(3, 147) = 252.6$, $MSE = 43043$, $p < .001$, $\eta_p^2 = .838$, where RT increased as set size increased. There was no main effect of posture $F(1, 49) = 1.980$, $MSE = 138174$, $p = .166$, $\eta_p^2 = .038$, inconsistent with standing affecting task performance. The interaction between posture and set size was not significant, $F(3, 147) = 0.971$, $MSE = 21261$, $p = .408$, $\eta_p^2 = .019$, inconsistent with posture influencing attention. The BF favoured the null hypothesis approximately 24.53 times more than the alternative. The search slope while standing on the slant board was 66 ms/item compared to 61 ms/item while sitting [$t(49) = -1.427$, $p = .160$, *95% CI* [-12.226, 2.074]. A Welch two sample t-test revealed the search slopes were not significantly different from Experiment 2, $t(92.69) = .801$, $p = .425$, *95% CI* [-5.922, 13.926]. This is expected as it was the same search task used in Experiment 2 and 3.

Percentage Error

Analysis of the PE data revealed there was no main effect of set size, $F(3, 147) = 1.888$, $MSE = 18.359$, $p = .134$, $\eta_p^2 = .037$, where error rates remained the same across set sizes. There was no significant main effect of posture, $F(1, 49) = 0.236$, $MSE = 26.479$, $p = .629$, $\eta_p^2 =.005$. The interaction of posture and set size was not significant, $F(3, 147) = 0.896$, $MSE = 14.320$, $p =$

.445, $\eta_p^2 = .018$, inconsistent with postural effects on attention. The BF favoured the null hypothesis approximately 17.06 times more than the alternative.

Discussion

As set sizes increased, RTs and PE data increased, however, this did not differ between posture conditions. The search slopes were approximately 63 ms/item for both conditions. As expected, the search slopes were similar to Experiment 2. Subjective reports from participants revealed that standing on the slant board was more difficult as a posture manipulation than prior experimental manipulations. However, even with the increased difficulty of posture, there was still no significant effect of posture on attention.

General Discussion

Three visual search experiments were conducted to test whether task difficulty explains the discrepancies between workplace reports of increased objective productivity when standing and the laboratory findings that standing does not affect selective attention. Relative to previous laboratory research, visual search difficulty was increased in Experiments 1 - 3 by using larger set sizes (e.g., Sternberg, 1998). Search difficulty was also increased relative to previous studies in Experiment 1 by using heterogenous distractors (e.g., Duncan & Humphreys, 1989) and in Experiments 2 and 3 by using conjunction search (e.g., Treisman & Gelade, 1980) with variable mapping techniques (e.g., Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). Posture difficulty was increased in Experiment 3 by having participants stand on a slant board for the standing condition (e.g., Kluzik et al., 2005). All three experiments successfully increased the difficulty of the search task relative to previous work. There was evidence that the use of the slant board increased the difficulty of the standing condition in Experiment 3. Despite the increases in visual search and postural difficulty, posture did not affect task performance in any of the three experiments, nor did posture affect attention. Therefore, the outcomes of these experiments are inconsistent with the hypothesis that task difficulty explains the discrepancy between postural effects on performance in workplace and laboratory studies. Instead, they provide additional support for the claim that posture does not affect selective attention.

In what follows I consider the validity of the present findings by examining whether visual search is an appropriate measure of selective attention for testing the hypothesis that posture affects selective attention. Then I will discuss why there has been inconsistencies in the past and present laboratory studies. This is followed by a discussion of the implications of the present findings for the hypothesis that postural control uses cognitive resources. Finally, a new approach is suggested that might provide a better test of the shared resource hypothesis.

Is Visual Search a Good Measure of Selective Attention?

The present studies used a visual search task to examine the effects of posture on selective attention. One concern may be that visual search is not an adequate measure of the processes involved in selective attention. Such a concern would stand in strong juxtaposition with its widespread use by cognitive psychologists to understand the nature of selective attention (Duncan & Humphreys, 1989; Schneider & Shiffrin, 1977; Shiffrin, 1970; Shiffrin & Schneider, 1977; Smilek et al., 2007; Sternberg, 1969a; 1969b; Treisman & Gelade, 1980; Wolfe, 1998a; 1998b). Another concern might be that the search slopes (aka search rates) do not provide a sensitive measure of selective attention. In contrast, inferences about selective attention in visual search are often based on changes in search slopes which are a widely accepted measure of search efficiency (Duncan & Humphreys, 1989; Schneider & Shiffrin, 1977; Shiffrin, 1970; Shiffrin & Schneider, 1977; Smilek et al., 2007; Sternberg, 1969a; 1969b; Treisman & Gelade, 1980; Wolfe, 1998a; 1998b).

Consistent with the claim that search slopes are sensitive measures of selective attention researchers have used them to examine the nature of the selection process. For instance, Treisman & Gelade (1980) examined search slopes to assess which stimulus properties could be distinguished outside of selective / focal attention (features vs. conjunctions). Subsequent researchers have argued that differences in search slopes indicate differences in which selective attention is guided to the target (Deco et al., 2002; Eastwood et al., 2001; Neisser, 1967; Smilek et al., 2007; Sternberg, 1969a; 1969b; Wolfe, 1989; 1994). For instance, Eastwood et al. (2001) argued that selective attention is affected by emotion based on the observation of shallower search slopes for negative facial expressions among neutral distractors than for positive facial expressions.

Despite the widespread use of search slopes as a measure of selective attention, some researchers have argued that search slopes can be affected by post-attentive processes such as stimulus comparison mechanisms (Mitroff et al., 2002; Mitroff & Simons, 2002; Wolfe, 1994). According to this account, search slopes are affected by the likelihood that the target will be missed when selected. When the target is missed, the display must be re-searched, effectively increasing the search slope. This account predicts that the more similar the target and distractors, the steeper the search slopes (Mitroff et al., 2002; Mitroff & Simons, 2002; Williams & Simons, 2000; Wolfe, 1994). This account is post attentive because it does not require properties of the display (e.g., target-distractor similarity) to influence the likelihood that the target will be selected. The post-attentive process is thought to occur because the critical feature that defines the target cannot be detected outside of attention. When the target and distractor are difficult to discriminate, search slope differences will arise because the target is likely to be missed even when attended (Williams & Simons, 2000; Wolfe, 1994). If the target is missed, then the display is essentially re-searched. The cost of the re-search of the display increases with set size resulting in steeper search slopes for targets that are more likely to be missed (Wolfe, 1994).

Two instances where the post attentive account has been invoked to explain differences in search slopes concern emotional facial expressions (Reynolds et al., 2009) and change detection (Reynolds & Withers, 2015). Reynolds and colleagues tested these accounts by monitoring eye-movements during visual search. In both cases, the search slope differences were entirely predicted by how long it took participants to first fixate the target (guidance to the target). The search slopes were not predicted by the time after the first target fixation (miss and re-search). This suggests that search slopes were entirely determined by the efficiency of selective attention and not the time to re-search the display, inconsistent with the post-attentive account. These findings are consistent with early work by Sternberg (1969a; 1969b) who demonstrated that post attentive processes such as response selection affected the intercept of the search slopes, and not the slopes themselves. Overall, these findings suggest that search slopes are a valid measure of selective attention processes. Therefore, I do not believe that the use of a visual search task and search slopes as the measure of selective attention are problematic for the claim that posture does not affect selective attention.

A second criticism that could be raised about the use of visual search to examine the claim that posture affects selective attention is that the components of selective attention that are indexed by search slopes in visual search are insensitive to embodied factors such as posture. According to this account, selective attention has multiple component processes and that the processes indexed by search slopes are not sensitive to embodied processes. Concerns about the sensitivity of search slopes in visual search to embodied processes is addressed by research examining near hand effects (Abrams et al., 2008; Bröhl et al., 2017; Perry et al., 2016). The near hand effect refers to the observation that a target placed in very close proximity to one's hand (in the hand's pericutaneous space) is processed quicker and more reliably than visual targets presented outside the pericutaneous space of the hands (far away) (Brown et al., 2008; Graziano & Cooke, 2006; Langerak et al., 2013). The standard explanation is that humans have bimodal neurons with overlapping visual and tactile receptive fields on the hand, meaning these bimodal neurons represent the pericutaneous space near the hands more densely than space far from the hands and other parts of the body (Brown et al., 2008; Graziano & Cooke, 2006).

Abrams et al. (2008) examined whether visual search performance was affected by the proximity of hands to the search display. The search task was similar the one used to examine the role of posture by Smith et al. (2019). Targets were either *S* or *H* and distractors were *E*s and *U*s. Set sizes were 4 or 8 items. Participants performed the visual search task with their hands near the search display (i.e., on sides of the computer screen) and far from the search display (i.e., on participant's lap). Critically, search slopes were steeper when the hands were near the display than when they were far from the display, consistent with less efficient search when the hands were near. This outcome is consistent with search slopes being sensitive to manipulations of the body that affect selective attention.

Interestingly, there were concerns that body posture played a role in the initial observation of a near hand effect in visual search (Weidler & Abrams, 2013). Subsequent research has shown that the near hand effects persist when postural elements have been controlled. For instance, Weidler and Abrams (2013) replicated Abrams et al. (2008) with the arms extended towards the display in both the near and far conditions. Bröhl et al. (2017) replicated Abrams et al. (2008) in an eye-tracking study with younger (age = 20 – 39) and older (age = 40 – 60) adults. They found that hand position only affected the search efficiency of the younger adults despite both groups showing similar overall performance.

The repeated demonstrations that search slopes are affected by the proximity of the hand suggests that search slopes should be sensitive to bodily processes such as posture – if posture affects selective attention. Therefore, evidence that search slopes are affected by proximity of the hands to the display is inconsistent with the hypothesis that search slopes are not sensitive to embodiment factors. Since selective attention is shown to be affected by embodiment factors, there is still concern why the present studies resulted in a lack of postural effects on selective attention despite increased task difficulty.

Methodological Issues

There are now 17 experiments that have examined whether posture affects selective attention. The first six studies all reported evidence that posture affects selective attention. In contrast, none of the 11 subsequent studies have reported evidence that posture affects attention. The likelihood that this is due to chance is incredibly small, suggesting that there is some systematic difference that has occurred over time. Here I consider whether the outcomes can be explained by two methodological issues, 1) participant sampling, and 2) counterbalancing methods. These will now be discussed in-depth.

The participant samples differ in at least two ways across the 17 experiments. One way that the studies differ is sample size. The studies that have failed to find effects of posture on selective attention (Caron et al., 2020; 2022; present studies) used larger sample sizes (50 – 122, see Table 2), which increases the power of the study and makes the effects more stable (Button et al., 2013; Gelman & Carlin, 2014; Schönbrodt & Perugini, 2013). The initial studies that found effects of posture on attention (Rosenbaum et al., 2017; 2018; Smith et al., 2019) used smaller sample sizes (12 – 50, see Table 2), which decreases power, makes the effects less stable and is associated with the reporting of inflated effect sizes (Button et al., 2013; Gelman & Carlin, 2014; Schönbrodt & Perugini, 2013). This is due to an increased risk of Type I error. In small samples the estimates of a population tend to be more variable, thereby increasing the chance that extreme values, that deviate significantly from the true population parameter, will be produced (Serdar et al., 2021). These extreme values can lead to the mistaken conclusion that an effect exists, when in reality if does not (Serdar et al., 2021).

A second way in which sampling differs across the studies is the stopping criteria for data collection. Caron et al. (2020) reported using *a priori* criteria for stopping in all experiments (see introduction for detailed explanation of stopping criteria for each experiment). In contrast, the initial studies that have found postural effects on attention did not report *a priori* stopping criteria, nor did they report a justification for the variability in sample sizes.

The use of *a priori* stopping criteria is recommended as means for reducing susceptibility to "p-hacking", which refers to a class of behaviours that bias data analysis so that a significant result occurs. For instance, repeatedly conducting analyses during data collection to decide whether data collection should continue or stop based on the outcome of the analysis; usually when the analysis yields a significant *p*-value (Crane, 2018). Design elements like *a priori* stopping criteria are important because researchers can be unaware that their research methods and approach to data analysis constitute p-hacking; their approach may be based on traditional ways of analyzing data in their field or they may be unaware that specific approaches affect the validity of their data analysis (Veldkamp, 2017). Since the initial studies examining posture and selective attention were published (2017 and 2019) there has been increased awareness of methodological issues, including sampling techniques, that affect the validity of reported findings within the field of Psychology (e.g., p-hacking; Edlund et al., 2022; Stroebe, 2019). This awareness has led to changes in how research is conducted and reported (e.g., using larger sample sizes, preregistration). The first preregistered study in psychology was registered in 2013 (Wiseman et al., 2019). Preregistration involves documenting the plan for an experiment that includes the intended sample size and data analysis methods prior to any data collection occurring. Preregistration is another important process that can potentially stop p-hacking from occurring because the sample size and analysis are already documented (Simmons et al., 2021).

Table 2.
Comparison of Past Research Sample Sizes

Study	Experiment	Sample Size
Rosenbaum et al. (2017; 2018)	1	17
	2	16
	3	50
Smith et al. (2019)	1	14
	2	30
	3	12
Caron et al. (2020)	1	108 (122)
	2	108 (122)
	3	98 (99)
	4	78 (80)
	5	51 (61)
Caron et al. (2022)	1	50
	2	51 (57)
	3	50
Present Study	1	48 (50)
	2	48 (50)
	3	50

Note. This table compares sample sizes of the experiments examining postural effects on attention. Sample size is the total number of participants retained for analysis, value in parenthesis indicates total participants recruited.

A second methodological issue that I consider is counterbalancing. Counterbalancing is a part of experimental design that entails ordering variable conditions differently across participants to minimize carryover effects (Brooks, 2012; Poulton & Freeman, 1966). An example of a counterbalance would be to have two different orders of conditions e.g., Group 1 receives condition A followed by condition B; whereas Group 2 receives condition B followed by condition A (Poulton & Freeman, 1966). Carryover effects refer to practice and/or asymmetrical transfer effects and can result from participants learning how to do the task over time (Brooks, 2012; Poulton & Freeman, 1966). For example, Macleod (1998) reported that in the Stroop task participants tend to have a reduction in their Stroop Effect after the first block because they learn how to complete the task at hand and therefore get better at it, reducing errors and reaction times. If practice and/or other carryover effects are confounded with condition, then this renders interpretation of the experimental outcome difficult because it is unclear whether the observed effects are due to the intended manipulation or carryover effects like practice. Counterbalancing minimizes symmetrical transfer effects. In the case of practice effects, they are absorbed into each condition equally (Poulton & Freeman, 1966).

Studies that examine the role of posture have used blocked designs where the task is completed sitting in one block and standing in a separate block. It is therefore possible that performance across blocks is affected by carryover effects such as practice. Consistent with this possibility in the Stroop Experiments, Caron et al. (2020) reported a significant three-way interaction in Experiment 1 and 2 (vocal responding) between posture, Stroop congruency, and order (counterbalance)[1] but the remaining experiments did not. The interaction arose because the

[1] The statistics statements for the interactions between posture, Stroop congruency, and order (counterbalance) in Caron et al.'s (2020) Experiment 1, [$F(1, 106) = 15.14$, $MSE = 838$, $p < .001$, $\eta_p^2 = .125$] and Experiment 2[$F(1, 106) = 7.62$, $MSE = 507$, $p = .007$, $\eta_p^2 = .067$].

Stroop Effect got smaller across blocks in the experiment. The posture condition that occurred in block 2 consistency showed a smaller Stroop Effect. Therefore, if a study had improper counterbalancing such that the standing condition was more likely to occur in block 1 than in block 2, it would result in the incorrect conclusion that standing yielded a larger Stroop Effect than sitting.

To test whether asymmetric transfer from improper counterbalancing could explain Smith et al.'s (2019) finding that posture affects visual search efficiency, I reanalyzed the reaction time data from the present visual search experiments with counterbalance as a factor. In Experiment 1, there was a significant interaction between counterbalance, posture, and set size interaction, $F(3, 138) = 3.125$, $MSE = 8034$, $p = .028$, $\eta_p^2 = .064$. The interaction arose because the search slopes got smaller across blocks in the experiment. This is similar to Caron et al. (2020), as performance was better in the 2nd block of the experiment. This suggests that asymmetric transfer could explain the findings from Smith et al. (2019) if the counterbalances were unequal. However, a follow up analysis suggests that this is unlikely. When only the data from our counterbalance 2 was used, where participants completed the visual search task under the standing condition first ($N = 24$). There was a main effect of posture (actually a main effect of block), $F(1, 23) = 14.715$, $MSE = 9083$, $p < .001$, $\eta_p^2 = .390$. The interaction of posture (block) and set size was not significant $F(3, 69) = .253$, $MSE = 8804$, $p = .859$, $\eta_p^2 = .011$, despite the change in search slopes going in the correct direction (standing = 44 ms/item, sitting = 37 ms/item), [$t(23) = -0.873$, $p = 0.392$, *95% CI* [-96.236, 39.130]. This suggests that proper counterbalancing is important when examining the effects of posture on selective attention when using visual search, but that the effects are minimal and that they would not explain the pattern reported by Smith et al. (2019).

The conclusion that Smith et al.'s (2019) findings are unlikely due to improper counterbalancing is reinforced by the subsequent re-analysis of Experiments 2 and 3. Unlike Experiment 1, there was no evidence that counterbalance affected the interaction between posture and set size in Experiments 2 and 3 [respectively; $F(3, 138) = 0.175$, $MSE = 21174$, $p = .913$, $\eta_p^2 = .004$, $F(3, 144) = 1.678$, $MSE = 20970$, $p = .174$, $\eta_p^2 = .034$]. On the one hand the absence of practice effects in Experiments 2 and 3 suggests that although proper counterbalancing is important when examining the effects of posture on selective attention when using visual search, the effects are minimal. On the other hand, the absence of a practice effect on search slopes in Experiments 2 and 3 replicates Shiffrin & Schneider's (1977) original observation of minimal practice effects when variable mapping is used.

Overall, the analyses of counterbalance in the present experiments suggest that the visual search studies reported here are not as influenced by practice effects as the Stroop task (Macleod, 1998). The effects of block were not seen in two of the present studies, suggesting carryover effects are not as impactful on visual search task performance. Therefore, the improper counterbalancing does not appear to be the cause of research discrepancies specifically for the visual search experiments. However, the participant sampling and the counterbalancing concerns for other tasks may still be an issue in these other tasks where failures to replicate the effects of posture on selective attention have been observed.

Implications for the Capacity Sharing Account

On the surface, the failure to find compelling evidence that posture affects selective attention suggests that the theory that postural control uses cognitive resources is wrong. However, the claim that posture affects selective attention is only one specific instance of this more general theory. Researchers have theorized that many different cognitive resources are involved in postural control such as memory, vision, attention, etc. For example, Kerr et al. (1985) argued that posture requires visual spatial processing resources and Dault et al. (2001) argued that postural requires all components of Baddeley's (1992) working memory model: the visuo-spatial component, the articulatory loop and the central executive system.

Further, the repeated failures to find evidence that posture affects selective attention in the Stroop task, task switching, and visual search suggests that if postural control uses cognitive capacity, it is not selective attention. This does not rule out the possibility that postural control shares capacity with *other attention related processes*. Other attentional processes such as sustained attention and central attention have not been examined in regards to posture. Sustained attention is defined as focusing attentional resources on a stimulus for an extended period of time (Mackworth, 1948; Robertson et al., 1997). Central attention is more closely related to executive or cognitive control as it shapes cognitive processes, representations and behaviours to be in accordance with task goals (Pashler, 1994; Tamber-Rosenau & Marois, 2016; Wickens, 1984), such as working to make complex decisions about the action implications of stimuli (Johnston et al., 1995). Beyond attention, the repeated failures to find evidence that posture affects selective attention do not rule out the possibility that postural control shares capacity with other cognitive resources. For instance, Kerr et al. (1985) suggested that visual spatial memory may be involved.

Before the theory that posture requires cognitive resources can be ruled out, researchers need to examine the relationship between posture and other systems such as working memory with the same depth. However, these studies will need to address why only one of 17 experiments that has examined the relationship between posture and attention has found a main effect of posture. If cognitive performance was affected by posture then it should have affected performance in some way (e.g., overall slower responses) in the tasks examined so far – even if these tasks focus on attention. For instance, if posture affected response selection, then minimally an overall slowing in performance would be expected.

Additionally, demonstrations that postural control does not use selective attention for normal adults does not rule out the possibility that selective attention can be used to supplement processes involved in postural control when necessary. For instance, Woollacott & Shumway-Cook (2002) argue that allocating increased attentional resources can supplement reduced sensory inputs and attentional allocation deficits due to aging. These deficits require increased attentional resources to process postural stability because the reduced sensory inputs make the process less automatic and attention has to be consciously allocated to posture for maintenance. Older adults have to pay attention to their posture in order to maintain it. For example, it is known that older adults develop the 'posture first' principle as they age where they focus their attention on their posture rather than other tasks (Lacour et al., 2008; Bayot et al., 2018). This is seen in examples like walking as older adults will stop talking when they walk so their attentional resources are focused on their body moving/walking and not the conversation at hand (Lacour et al., 2008; Bayot et al., 2018). They will continue talking when they have stopped walking (Lacour et al., 2008; Bayot et al., 2018). Therefore, this suggests that attention may be used to supplement or overcome deficits in visual and spatial processing that are required for postural maintenance.

Future Directions

So far, the research examining the effects of posture on cognition, and in particular on attention, have done so by manipulating posture and measuring attention. This method is similar to dual task investigations where cognitive load is manipulated (e.g., visual load, memory load) and the effects on performance are measured in another task (e.g. visual search, reading comprehension). The classic example of this approach is studies that examine the effects of working memory load on reading performance (Baddeley et al., 1981; Baddeley et al., 1985; Bayliss et al., 2005). One limitation to this approach is that performance is often measured in only one of the tasks. For instance, Baddeley et al. (1981) examined articulatory suppression effects on reading judgements, but they only measured the speed and accuracy of reading judgments, there was no equivalent measure of articulatory suppression performance. When performance is measured in only one task, it is possible for effects to go undetected. Participants could sacrifice performance in the unmeasured task to ensure consistent performance in the task being measured (e.g., visual search). The measurement of only one task is a common problem in dual task investigations of posture – above and beyond the studies examining attention. Therefore, before the claim that posture affects selective attention can be ruled out, performance in the posture task also needs to be measured.

Further, if performance is measured in both tasks, there can be trade-offs in performance that can be difficult to interpret. For instance, it is possible for performance to improve on one task and decrease on the other in the dual task condition relative to when they are performed alone. In such a case it is unclear whether the two tasks are interfering with each other, whether they same resource or whether the changes in performance are a consequence of a voluntary strategy for optimizing performance (Meyer & Kieras, 1997). An example of this problem is seen in some studies examining posture and cognition. Kerr et al. (1985) had 24 participants complete either the Brooks spatial memory task or the Brooks nonspatial memory task (Brooks, 1967). The spatial memory task requires remembering number-word pairs by mentally placing the numbers into an imaginary 4x4 matrix. The nonspatial memory task requires remembering number word pairs as paired associates. This task was used as a control. Participants completed the memory tasks in a single task condition while sitting, and in a dual task condition where they stood on a forceplate in a heel-toe (Tandem Romberg) stance. There was also a single task posture condition in which participants stood on the forceplate heel to toe. All trials were 12 seconds in length. Postural sway data was recorded. For the memory task, only accuracy was measured. Here they found that postural sway decreased in the dual task conditions, indicating improved postural control compared to the single task condition, inconsistent with increased resource demands. However, they also found that performance on the spatial memory task (accuracy rates) was worse in the dual task condition compared to the single task condition, consistent with increased resource demands in the dual task condition. There was no significant difference for the nonspatial memory control task. This trade-off in task performance makes it difficult to understand whether there was an overall decrease in performance in the dual task condition.

Dual task researchers argue that the inferences that can be made from dual task scenarios depends on whether the tasks are speeded or unspeeded (Pashler, 1994) and that the use of unspeeded tasks as in Kerr et al. (1985) is not optimal for making inferences about shared processing resources across tasks. One of the more popular methods for examining dual task performance is the Psychological Refractory Period (PRP) paradigm. This paradigm uses two speeded tasks. An advantage of the PRP paradigm is that it has been extensively studied and there are detailed mathematical models of PRP performance that allow for precise predictions. In the PRP paradigm, participants complete two speeded tasks. The overlap between the two tasks is controlled by manipulating the Stimulus Onset Asynchrony (SOA). This means that sometimes the onsets of the stimulus for Task 1 (S1) and onset of the stimulus for Task 2 (S2) are separated by a brief interval (e.g., SOA = 50 ms), so that the two tasks largely overlap and at other times there is a large interval (e.g., SOA = 2500 ms), so that the two tasks are performed separately. Participants are instructed to give priority to Task 1. The standard finding is that as SOA decreases (and task overlap increases) the time to respond to Task 2 increases, whereas the time to respond to Task 1 is largely unaffected (Pashler, 1994; Tombu & Joliecour, 2005). Critically, information about the shared resources can be inferred by manipulating variables in Task 1 and Task 2 and examining how they are affected by SOA. The PRP paradigm allows for clear and accurate predictions of how the processing of Task 1 affects Task 2.

Whether the effect of a variable affects performance before, during or after the shared resource can be determined by examining how the effects of that variable are affected by SOA. First, if a variable is manipulated in Task 1, it can carry forward to Task 2 when the SOA is short. Evidence suggests that a carryover effect from Task 1 to Task 2 can arise due to the effect occurring at a shared limited capacity process. However, early processes in Task 1 that occur before the limited capacity system can also carry forward to Task 2 (see Tombu & Joliecour, 2003; 2005 for mathematical proofs for both sequential and capacity sharing models). Thus, the carryforward of an effect from Task 1 to Task 2 is not sufficient evidence for the identity of the shared process. Further evidence is needed.

If a variable is manipulated in Task 2 of the PRP paradigm, this can be used to determine whether the variable affects performance before or at the shared resource. First, a variable manipulated in Task 2 will have minimal to no effect on performance in Task 1 (Tombu & Jolicoeur, 2003; 2005). This means that if only Task 1 performance is being measured, then the effects of a manipulation that affects the shared resource are unlikely to be observed. This could explain the failure to find evidence of a postural effect in many of the studies examining selective attention; if the posture task is treated as Task 2, an effect of posture would not be observed in Task 1 (e.g., visual search). Further, the effects of the Task 2 variable can either be additive with SOA or decrease as SOA decreases in Task 2. If the variable has additive effects with SOA in Task 2, then this is evidence that the variable is affecting performance at, or after the shared resource. If the variable has underadditve effects with SOA (the effect gets smaller as task overlap increases) in Task 2, then this is evidence that the variable is affecting performance before the shared resource.

The PRP paradigm can therefore be used to assess if a variable is affecting performance before, during or after the shared processing resource. This requires two PRP studies. In one experiment, the Task with the manipulation of interest (e.g., selective attention) is Task 1 and the other task serves as Task 2. If the effect in Task 1 carries forward to Task2 at the short SOA, then this is evidence that the process occurs before or at the shared limited resource. These two alternatives can be distinguished in a second experiment where the Task with the manipulation of interest (e.g., selective attention) is Task 2 and the other task (e.g., posture) serves as Task 1. If the effect of selective attention is additive with SOA, this would indicate that the effect occurs at the level of the shared resource. If the effect of selective attention is underadditive with SOA, this would indicate that the effect of selective attention occurs before the shared resource.

A PRP experiment examining the relationship between cognitive processing and posture could be done with the tasks that have already been used to examine whether posture affects selective attention (Stroop, task switching and visual search). However, a speeded posture task would have to be introduced. It is unclear whether quiet standing, which is a common task in studies of postural control, meets the necessary requirement as a speeded task. Quiet standing does not have a clear onset time and often lasts for periods of 20 seconds or longer. One possibility would be to have participants quickly shift their center of balance in response to an auditory cue (e.g., high vs. low tone). This posture task would work well for future studies and should be incorporated into experiments where posture is Task 1 and selective attention is Task 2 and vice versa to determine the cause of the carryover effects. The difficulty of postural control could be manipulated by having participants stand on a balance board while responding (Gebel et al., 2019). In the easy condition, the balance board will be stable. In the hard condition the balance board will be unstable. This manipulation will attempt to localize the effects of postural control relative to a shared resource and assess whether postural control uses the same limited capacity mechanism as selective attention.

Conclusion

In conclusion, the present studies found no postural effects on selective attention, regardless of the increased difficulty manipulations for both posture and attention. The difficulty manipulations were successful in increasing difficulty compared to past work and gradually in each study. However, we were not able to achieve the same postural effects as subjectively reported in real world settings. These findings suggest that the discrepancies across studies are not due to a lack of task difficulty in the laboratory compared to the real world. These results leave room for further questions and investigations necessary to determine why the discrepancies persist.

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