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Scene-object semantic incongruity across stages of processing: From detection to identification and episodic encoding

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Visual processes are assumed to be affected by scene-object semantics throughout the stream of processing, from the earliest processes of conscious object detection to the later stages of object identification and memory encoding. However, very few studies have jointly explored these processes in a unified setting. In this study, we build upon a change detection task to assess the influence of semantic congruity between scenes and objects across three processing stages, as indexed through measures of conscious detection, object identification, and delayed recognition. Across four experiments, we show that semantically incongruent targets are easier to detect than their congruent counterparts, but that the latter are better identified and recognized in a surprise memory test. In addition, we used eye-tracking measures, in conjunction with these three behavioral indexes, to further understand the locus of the advantage observed in each case. The results indicate that (i) competition with other congruent objects modulates the effects of congruity on target detection, but it does not affect identification nor recognition memory, (ii) the *detection cost* of scene-congruent targets is mediated by earlier fixations on incongruent targets, (iii) neither fixation times, dwell times, nor pupil dilatation are related to the effects obtained in identification and recognition; and (iv) even though congruent targets are both better identified and remembered, the *recognition benefit* does not depend on the identification demands. The transversal approach taken in this study represents a challenging but exciting perspective that holds the potential to build bridges over the seemingly different but related fields of conscious detection, semantic identification, and episodic memory.

KEYWORDS

semantic congruity, detection, identification, episodic encoding, recognition memory, stream of processing

Introduction

The amount of information with which our cognitive system is continuously faced is overwhelming. Of all the information that gets through our senses, only a small portion reaches a state in which we actually become aware of it. In turn, an even smaller fraction of that information is stored into memory and can eventually be remembered. Understanding what sorts of transformations that information undergoes across the stream of processing is thus a very important, but often neglected, aspect of the study of human cognition. Analyzing the course of the same information across different processing stages can provide new insights into the underlying mechanisms and processes at play throughout this course.

One of the key modulators at several stages of that multiple-filter operation is semantic information. For instance, previous knowledge about the world may bias the information that gets access to our conscious awareness, by anticipating the most likely stimuli given a set of priors (Rao and Ballard, 1999; Summerfield et al., 2006). Similarly, the semantic features of a scene can also determine which objects will actually be attended, even beyond the biases imposed by other lower-level perceptual features (Peelen and Kastner, 2014; Santangelo et al., 2015; Henderson and Hayes, 2017). Moreover, previous knowledge can help us to interpret and give meaning to seemingly meaningless stimuli (Mooney, 1957; Gorlin et al., 2012) and it can even adjust which information gets stored into memory and which does not (Henson and Gagnepain, 2010; Van Kesteren et al., 2012). In this study, we will use prior semantic knowledge of real-world visual scenes to jointly characterize three key stages in the processing of information: detection, identification, and episodic encoding.

Object detection

The unspecific report of the detection of a visual stimulus can be studied by means of many different paradigms. Most of them require participants to press a given key in response to the detection of a target stimulus independently of features such as its location, color, or identity. These seemingly unimportant features are often used as independent variables that either speed up or slow down detection times and can even facilitate or impair detection accuracy, leading to positive and negative effects like priming (Kroll and Potter, 1984), change blindness (Simons and Rensink, 2005) or inhibition of return (Posner et al., 1985), which are often interpreted as the result of a *detection cost* (Lupiáñez et al., 2013).

The semantic features of an image are also thought to bias detection responses during scene processing. Hollingworth and Henderson (2000) showed that the detection of a changing target improves when the to-be-detected object is embedded in a semantically incongruent context (Hollingworth and Henderson, 2000). Moreover, LaPointe and Milliken (2016) showed that incongruent objects had shorter first *fixation latencies.* This variable represents the lag of time from the moment the trial starts until the object is fixated for the first time and it has been often used as a measure of pre-attentional processes influencing attentional capture.

Object identification

Even though detection and identification of an object appear to be two seamless stages of perception, LaPointe et al. (2013) showed that semantic information can be used to dissociate both processes, as they were affected in opposite ways by semantic congruity (LaPointe et al., 2013). They used a change detection task in which the identity of the to-be-detected object either matched or mismatched the gist of the surrounding scene, and they asked participants to detect and subsequently identify the changing object. Their results replicated the previously reported congruity *detection cost*, but they showed a simultaneous benefit for congruent targets on the identification task. This congruity *identification benefit* thus refers to facilitated access to the semantic features of a target when it is presented in the context of other semantically related objects. This finding is in line with research on prior knowledge and expectations, which shows that object identification is improved when the visual input matches what the observer is expecting (Eger et al., 2007; Esterman and Yantis, 2010). Importantly, at least one previous study has looked at *on-target dwell time* (i.e., the sum of time spent fixating the target region) as a proxy for total target processing time in the contest of the *identification benefit* (LaPointe and Milliken, 2016). This study found no differences in dwell time between congruent and incongruent objects thus supporting the notion that this benefit does not reflect merely increased processing time.

Long-term storage and retrieval

Both, the *detection cost* and the *identification benefit* are immediate measurable consequences of embedding an object in a semantic context. However, surrounding semantic information can have also long-term consequences by impacting how the object is encoded into memory. As a consequence, the ability to distinguish a previously seen object from one never seen before (i.e., a recognition memory), will be modulated by the semantic context in which the object was presented. For instance, a congruent background can facilitate later access to a given object by easing its integration into existing schemas (Gronau and Shachar, 2015; Kaiser et al., 2015; Ortiz-Tudela et al., 2016; Brod and Shing, 2019; Wynn et al., 2019). Conversely, an incongruent background can also render memorable a given object by signaling it as salient or unexpected (Henson and Gagnepain, 2010; Van Kesteren et al., 2012). This seemingly incompatible finding is currently the focus of active research (Ortiz-Tudela et al., 2018b; Greve et al., 2019; Quent et al., 2021) and the consideration of the role of the adjacent process can provide important insights into the debate.

Previous research using gaze measures to study memory phenomena (Võ et al., 2008; Otero et al., 2011; Kafkas and Montaldi, 2012) has largely relied on *pupil dilation* which is the variation in the diameter of the pupil, and has often been used as a measure of cognitive effort devoted to the task. These studies consistently observe larger pupil dilation at retrieval for successfully remembered items. This effect is generally assumed to be a consequence of either increased mental effort that leads to better memory or of a subjective feeling of familiarity with the correctly identified items; either of these interpretations must be ascribed to processes taking place at the moment of retrieval. In our study, we placed our focus on semantic congruency effects during encoding (i.e., during visual processing of the stimuli) and how this relates to eventual memory performance.

The present study

Because much of the abovementioned research has focused exclusively on one or a subset of these three different stages, it remains largely unknown whether they rely on independent mechanisms. We argue that a simultaneous study of these different phenomena might provide a more realistic picture of the hierarchical nature of this continuous stream that would have the potential to reveal existing interactions and dependencies between them. Thus, in this study, we intend to better explore how the semantic relatedness between an object and its scene context may affect different stages in the perceptual processing of the object, and ultimately determine its encoding in memory. We designed four experiments with a change detection task in which we manipulated the semantic congruity of the targets with the gist of the scenes in which they were embedded and assessed which of these changing targets were more efficiently detected, identified, and recognized. In Experiments 1A and 1B we compared two presentation procedures and two types of scenes differing in the number of objects presented on the scenes by assessing the indices of detection, identification, and recognition. In Experiment 2, we removed the identification task and replicated the setup for detection and recognition, to assess whether the effects obtained in recognition were independent of explicit identification demands. Finally, Experiment 3 typified the gaze patterns associated with each of these three processes, analyzing separately the amount of time elapsed from the start of the trial to the first fixation on the target, the amount of time spent fixating the target region, and the average pupil dilatation measured on each trial. Because each of these measures has been taken to reflect different cognitive functions such as attentional capture (first fixation), total processing time (dwell time) or cognitive effort (pupil dilatation), we surmise that this study might reveal important information on the impact of semantic relatedness at each of these three processing stages and illustrates a potentially useful approach to the study of how semantic congruity may affect the full stream of processing.

Experiment 1

Whether the semantic effects described in the introduction (i.e., *detection cost*, *identification benefit*, and *recognition benefit*) are a consequence of priming or of object competition mechanisms is still unsolved. Stein and Peelen (2015) recreated a situation in which detection took place with no competition from other objects (i.e., the target was presented alone in the context of visual noise). Their study included a cue which could either match or mismatch the category of an object suppressed under CFS conditions (Tsuchiya and Koch, 2005). With this paradigm, participants benefited from congruent cues. In these conditions, and in the absence of potential competitors, mechanisms such as priming (Kroll and Potter, 1984) or top-down inferences over ambiguous stimuli (Bar, 2003; Gorlin et al., 2012) are most likely responsible for guiding behavior. In contrast, in the conditions imposed by change detection paradigms, can be considered as the opposite situation: responding to cluttered images heavily relies on object competition since the participants' goal is to selectively detect a changing target among many distracters. Under this conditions, the presence of many different but semantically related objects hinders the detection of the specific (changing) target (Hollingworth and Henderson, 2000; LaPointe et al., 2013; LaPointe and Milliken, 2016; Ortiz-Tudela et al., 2016, 2018a). In Experiment 1 of the present study, we attempted at recreating an intermediate situation, using LaPointe et al.'s task, but reducing the presence of distracters, to prevent competition. We presented participants with two types of natural scenes: cluttered scenes, in which the images included many non-target objects together with the target one, and sparse scenes, in which only the target object was presented against a background image.

If semantic effects take place as a consequence of priming-like or top-down inferential mechanisms, they ought to be present in both types of scenes, since the propagation of semantic properties from the scenes to the individual objects can equally occur in both conditions. Conversely, if the aforementioned effects arise as a consequence of stimulus competition, they should appear selectively in cluttered trials, where there are many objects that compete with each other. More specifically: we hypothesized that, in the present experiment, the *detection cost* ought to be present only for cluttered trials. In opposition, the *identification benefit*, which arguably relies on spreading activation from the context image to the object (Palmer, 1975; Davenport and Potter, 2004; Eger et al., 2007), ought to be present in both cluttered and sparse trial types. Lastly, given that the *recognition benefit* has been previously hypothesized to be driven by schema-integration processes (Ortiz-Tudela et al., 2016), and those rely solely on the availability of contextual schema and not on the presence of other objects, we hypothesized that the *recognition benefit* should also be observed for both stimulus types.

Finally, because including qualitatively different sets of images in a task might entail not only the differential processing of those images but an overall change in participants' task set and strategies, we conducted two separate but complementary experiments. In Experiment 1A, the order of presentation of the two stimulus types was randomized so that it was impossible to anticipate the nature of the upcoming trial and to be specifically prepared for it in advance. In Experiment 1B, stimuli from the same set of images (i.e., cluttered vs. sparse) were grouped into blocks, so that all the trials from one group were presented together; this blocked setup allows participants to adjust their strategy to the corresponding block so that the optimal task set can be prepared before the onset of every trial.

Material and methods

Participants

Twenty students (18 female; mean age: 21.84; SD: 6.30) from the Universidad de Granada participated in Experiment 1A; another 20 students (18 female; mean age: 20.45; SD: 5.65), extracted from the same pool, participated in Experiment 1B. All of them volunteered in exchange for course credit and signed an informed consent approved by the local ethics committee. The sample size was determined based on previous studies using a similar paradigm (LaPointe et al., 2013; Ortiz-Tudela et al., 2016, 2018a) and sensitivity analysis was conducted to estimate the smallest detectable effect size. This analysis revealed that, with the available sample size, we would be able to detect effect sizes of at least $d = 0.58$, with 80% power and an alpha level of 0.05 (one-tailed matched samples t-test). All experiments in this paper, which are part of a larger research project approved by the Universidad de Granada Ethical Committee (175/CEIH/2017), were conducted according to the ethical standards of the 1964 Declaration of Helsinki (last update: Seoul, 2008).

Stimuli

All of the stimuli included in this and subsequent experiments in this study were either borrowed from previous publications (LaPointe et al., 2013; LaPointe and Milliken, 2016; Ortiz-Tudela et al., 2016, 2018a) or specifically built to match the needs of our experiment (see also below). All the stimuli consisted of scene-object combinations and both, scenes and objects, depicted real-world content (e.g., the image of a forest with a deer as an object). All the scene images were 850 × 565 pixels and the original object images were 500 × 500 pixels in size. All the objects were digitally resized and embedded in the scenes using Adobe Photoshop CS6. Each object was paired with two images, one congruent and one incongruent (Supplementary Table S1). Although the size of the objects was adjusted for each individual scene, an attempt was made to keep the size relatively similar across the two versions. We provide probability maps of the area covered by the objects in both congruency conditions as well as a statistical analysis of the differences in size between conditions and a correlation of each object's size across conditions (Supplementary Figure S1). The analysis confirmed the lack of differences in object size between conditions (BF01 = 4.327) and a strong within-object correlation of the small differences (Pearson's $R = 0.846$, $p < 0.001$). In addition, we also computed pixel-wise saliency (Supplementary Figure S2) and luminance (Supplementary Figure S3) metrics and run a Bayesian t-test between congruency conditions. The results also supported the lack of differences in either of the measures (BF01 = 5.968 and BF01 = 7.951, respectively).

Procedure

Each participant completed three sequential phases: the first one consisted of a change detection task. This phase was followed by 10 min of mathematical operations that served as a distracter task. Finally, memory of the target objects from the change detection task was assessed *via* a surprise recognition test. The duration of the entire session was ∼45 min.

The overall structure of the session was identical for Experiment 1A and 1B with the sole exception of the order of presentation of the cluttered vs. sparse trial types of the change detection task (i.e., randomized for Experiment 1A and blocked for Experiment 1B). In Experiment 1B randomization was applied within each block so that the sequence of trials within that block was different for each participant; the order of the blocks was counterbalanced across participants.

Change detection task

Each trial consisted of a rapid alternation of two versions of the same image, each displayed for 250 ms. The two versions represented scenes which were identical to each other except for the presence or absence of a key object. Participants were required to press the space bar on a QWERTY keyboard as soon as they noticed any detail that was different between the two versions of the scene. To prevent the changing object from popping out, an intervening blank screen was displayed for 250 ms between the two presentations. This intervening screen rendered the standard flickering appearance of the paradigm (Rensink et al., 1997). Critically, we manipulated the congruity between the to-be-detected object and the background scene. On half of the trials, the target identity matched the gist of the scene (i.e., congruent trials) and on the other half, it corresponded to an object that was not expected or frequent in that context (i.e., incongruent trials). After the detection response, or after a maximum of nine alternation cycles, the sequence stopped and a new screen prompted participants to identify the changing object with a few words (e.g., black dog) or by locating it on the screen (e.g., bottom-left) if identification was not possible (Figure 1). To assure participants' engagement in the task, 10% of no-change trials were included (i.e., catch trials). Participants were not informed of the presence of these no-change trials since previous studies have shown that being aware of the presence of those trials can change participants' response bias (Ortiz-Tudela et al., 2016). A total of 90 object-image combinations were used.

More importantly for our purposes, we included two sets of trials. The cluttered set was built so that the target object (i.e., the changing one) was one among many other presented objects. Conversely, in the sparse set scenes, the target object was presented in isolation against an open background image (Figure 2). For the cluttered set complex natural scenes were selected such as a busy city street, a park with children and trees or a big city skyline; for the sparse set, rather *empty* scenes were selected such as a wide prairie, a desert, or an open sky. Cluttered and sparse set scene trials were intermixed within the same block of trials in Experiment 1A and in different blocks of trials in Experiment 1B.

Distracter task

Participants completed paper and pencil math operations for a maximum time of 10 min. None of the participants completed the entire set of proposed operations. The exact operations used are available at https://github.com/ortiztud/three_indices.

Recognition memory test

All the target objects from the change detection task, together with 90 new objects, were used in the memory test. Each object was presented alone (i.e., stripped from any scene context) at the center of the screen and covering ∼10° of visual angle. Participants performed an old vs. new judgment without any time restriction. Correct responses to old objects were coded as hits and incorrect responses to old objects were coded as False Alarms (FAs).

Results

Experiment 1A

Participants ($N = 5$) who reported a change in more than 40% of catch trials were excluded from the analyses. The three dependent variables of interest were analyzed separately using

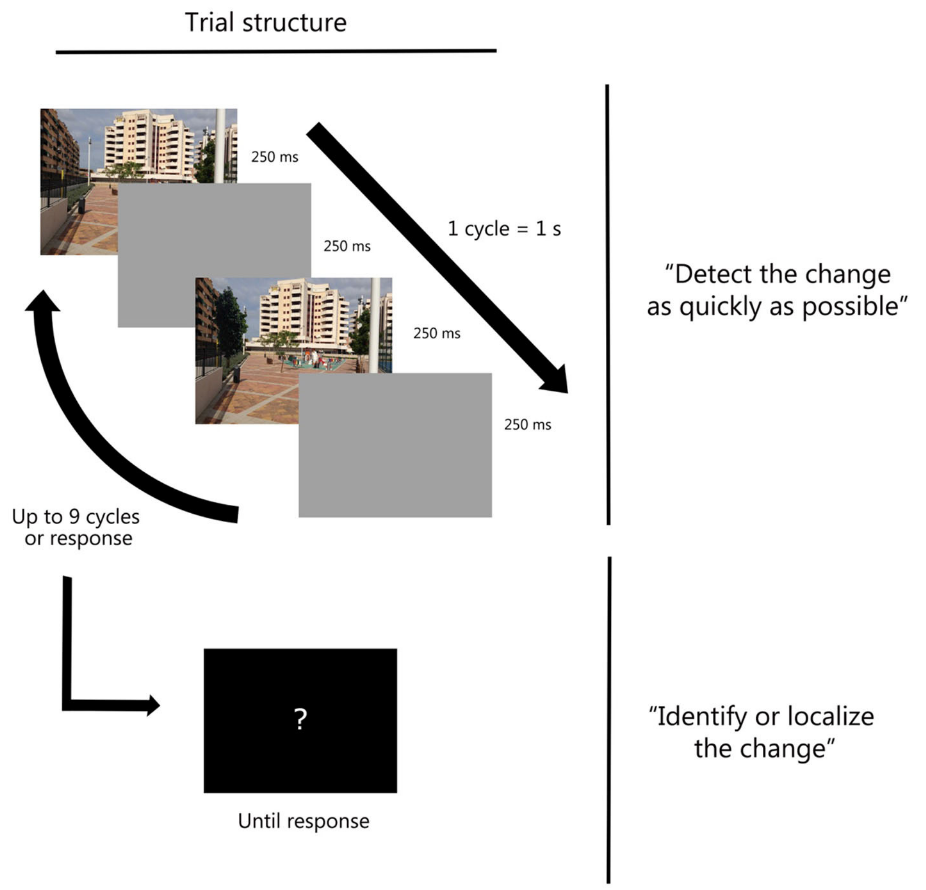

FIGURE 1
Trial structure for the change detection task in Experiments 1A, 1B, and 3. Participants sequentially performed a detection task followed by an identification task (see Ortiz-Tudela et al., 2016, for a simiar procedure).

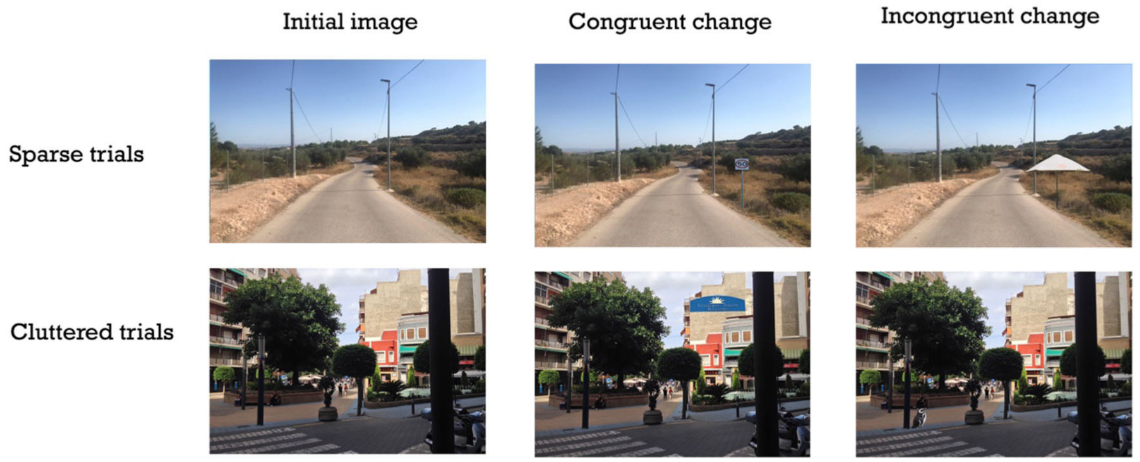

FIGURE 2
Example of stimuli used in Experiment 1A and 1B. Scenes in the cluttered set were taken from Ortiz-Tudela et al. (2016); for the sparse set, scenes with none or just a few non-target objects were selected.

2 × 2 repeated measures ANOVAs with scene-object congruity (congruent vs. incongruent) and trial type (cluttered vs. sparse) as within-subjects factors.

Detection

Performance on the detection task was evaluated by combining detection times with the proportion of correct responses in an overall detection index (proportion of correct responses/detection times; Ortiz-Tudela et al., 2018b). The analysis of the detection index revealed a significant trial type by congruity interaction, $F_{(1,14)} = 5.954$, $p = 0.029$, $\eta p^2 = 0.40$, showing that on the cluttered set responding to congruent targets was less efficient than responding to incongruent targets, $F_{(1,14)} = -3.41$, $p = 0.004$, $\eta p^2 = 0.43$, but there were no differences in the sparse set, $F_{(1,14)} = -1.32$, $p = 0.208$, $\eta p^2 = 0.01$.

Identification

Only correctly detected objects for each participant were included in the following analyses. The results of the analysis of the proportion of correctly identified objects appropriately replicated previous findings of higher identification scores for congruent objects, $F_{(1,14)} = 10.981$, $p = 0.005$, $\eta p^2 = 0.47$. Importantly, the trial type by scene-object congruity interaction was not significant in this measure, $F < 1$, suggesting that the *identification benefit* was present in both trial types, $F_{(1,14)} = 2.49$, $p = 0.026$, $\eta p^2 = 0.36$ and $F_{(1,14)} = 3.24$, $p = 0.006$, $\eta p^2 = 0.43$ for cluttered and sparse respectively.

Recognition

Trials that were correctly detected and correctly identified were passed along to the recognition analyses. Overall *d'* was 1.27 and beta 1.84. Since it was not possible to assess independent FA rates for congruent and incongruent trials, overall hit rates were used as a measure of memory performance. The analysis did not show a significant effect of trial type, $F_{(1,14)} = 3.082$, $p = 0.101$, $\eta p^2 = 0.15$, even though we measured numerically higher recognition scores for objects in the sparse set (0.78) compared to those in the cluttered set (0.75). The numerical pattern also showed higher memory rates for congruent than for incongruent objects, at least for the cluttered scenes (see Table 1), but neither this difference nor the two-way congruity x trial type interaction were close to statistical significance, *F*s < 1.

Experiment 1B

The same approach as in Experiment 1A was adopted for the analyses of Experiment 1B. Data from three participants were excluded from the analysis for poor performance in the detection task.

Detection

The analysis of detection efficiency replicated those of Experiment 1A. The trial type by congruity interaction was close to significance for the detection index, $F_{(1,16)} = 3.977$, $p = 0.063$, $\eta p^2 = 0.20$. In other words, again more efficient responses were made on incongruent than on congruent trials on cluttered trials, $F_{(1,16)} = -3.89$, $p = 0.001$, $\eta p^2 = 0.43$, but no differences between congruent and incongruent target objects were obtained on sparse trials, both $F_{(1,16)} = -1.56$, $p = 0.139$, $\eta p^2 = 0.13$.

Identification

The pattern of the identification scores in Experiment 1B mimicked that of Experiment 1A. Consistent with an *identification benefit* effect, congruent target objects were better identified than incongruent objects, $F_{(1,16)} = 4.746$, $p = 0.045$, $\eta p^2 = 0.21$. There was no indication of an effect of trial type, or of interaction between stimulus type and congruity, $F < 1$.

Recognition

The memory pattern in Experiment 1B also resembles that of Experiment 1A. Overall *d'* was 1.35 and overall beta was 2.05. The main effect of trial type was close to significance, $F_{(1,16)} = 4.92$, $p = 0.05$, $\eta p^2 = 0.23$, with better memory for objects in the sparse trials (0.75) than in the cluttered ones (0.66). No significant effect of congruity nor an interaction between trial type and congruity were observed, both *F*s < 1.

Discussion

The aim of Experiments 1A and 1B was to test whether the semantic congruity effects reported in the literature on the detection, identification and delayed recognition of objects could rely on different combinations of semantic facilitation and object competition. To that end, we used a change detection paradigm, that reliably produces the expected indexes [i.e., a *detection cost*, *identification benefit*, and *recognition benefit*; (LaPointe et al., 2013; Ortiz-Tudela et al., 2016, 2018a)], and we compared two stimulus sets which either included the target among many distracter objects or presented the target embedded in a sparse background. Because we reasoned that participants' responses can be affected by the adoption of a specific mindset evoked by surrounding trials, Experiment 1A and 1B also explored the potential effect induced by presenting these two types of contexts either in a random order (Experiment 1A) or grouped into blocks (Experiment 1B).

The results of the two experiments showed that while the *identification benefit* is present when using both cluttered and sparse stimuli, the *detection cost* is only found in the presence of stimulus competition. This result suggests that the *detection cost* arises only when there is a number of coactive stimuli competing for attentional resources, whereas the benefits found for identification seem to depend on semantic facilitation which might arise either from the activation of a group of semantically related objects or from the overall meaning of the background scene (Eger et al., 2007; Esterman and Yantis, 2010). The absence of differences in detecting congruent and incongruent trials in the sparse set is consistent with the idea that sparse scene contexts represent an intermediate situation between Stein and Peelen's minimalistic setup (in which better detection followed a category-matching cue) and the cluttered arrangement of LaPointe et al.'s (2013) paradigm (in which a *detection cost* was obtained).

Lastly, and surprisingly, we were not able to measure a statistically significant *recognition benefit* in spite of having arranged conditions very similar to those presented in Ortiz-Tudela et al. (2016). This unexpected result can be due to the inclusion of the sparse trials within the list of items to be retrieved at the memory test. Indeed, performance in any memory test is highly dependent not only on the processes taking place at encoding but also on those taking place during consolidation and retrieval and those can be affected by the amount and

TABLE 1 Mean RT and percentage of accurate detection responses (in parenthesis) for object detection, and percentage of accurate responses for object identification and delayed recognition, for each of the four experiments.

Experiment	Object detection				Object identification				Object recognition			
	Cluttered		Sparse		Cluttered		Sparse		Cluttered		Sparse	
	C	I	C	I	C	I	C	I	C	I	C	I
1A	2,784 (0.7)	2,757 (0.84)	2,049 (0.94)	2,072 (0.95)	0.87	0.72	0.83	0.73	0.70	0.65	0.74	0.74
1B	2,423 (0.75)	2,539 (0.87)	1,891 (0.97)	1,783 (0.96)	0.83	0.76	0.84	0.78	0.67	0.65	0.74	0.76
2	2,393 (0.83)	1,883 (0.94)	–	–	–	–	–	–	0.41	0.35	–	–
3	2,351 (0.87)	1,882 (0.96)	–	–	0.84	0.73	–	–	0.69	0.64	–	–

C, congruent; I, incongruent.

nature of the elements to be held in memory. Thus, before jumping to speculative conclusions about the *recognition benefit*, we decided to further explore and characterize the processes in another experiment.

The purpose of Experiment 2 was, therefore, two-fold. First, replicating the *recognition benefit* by attempting to measure it only with the standard cluttered scenes (as used in previous studies). Second, to further characterize this memory process by dissociating the *recognition benefit* from the identification task.

Experiment 2

LaPointe et al. (2013) used the *detection cost* and the *identification benefit* to claim that a clear dissociation could be behaviorally established between the detection and identification processes. Ortiz-Tudela et al.'s (2018a) later report of the *recognition benefit* followed the same direction as the *identification benefit*. However, the dual-task conditions arranged in this latter study, in which participants were required to detect and then identify the changing object, made it impossible to separate the influence of each of these two tasks in the memory results. Thus, it is possible that the *recognition benefit* arises as a consequence of the offline elaboration required to respond to the identification question and not to the mechanisms at play while the processing of the scene was carried out.

Therefore, in Experiment 2 we eliminated the identification question altogether to avoid any effects of this post-response task on later recognition. In addition, in order to ensure the *detection cost* and to improve the chances of measuring the *recognition benefit* effect, we used only cluttered scenes as in previous reports (LaPointe et al., 2013; Ortiz-Tudela et al., 2016, 2018a; Spaak et al., 2020).

Material and methods

Participants

To guarantee enough power to replicate previous results, we increased the sample size to 40 participants (37 female; mean age: 20.7; SD: 1.6). Participants were recruited from the Universidad de Granada in exchange for course credit. All of them signed informed consents approved by the local ethics committee. Four of them were unable to complete the entire experimental session and therefore were eliminated from the final sample.

Procedure

The overall procedure was the same as that described for Experiment 1 except for the following: to eliminate any potential interference from the sparse set on memory, we only used stimuli from the cluttered set. In addition, the identification question was removed, so that participants only had to perform the detection task that required them to respond as soon as they noticed any change during the flickering period. Following their response, the alternation of images stopped, and it was replaced by a fixation point, which indicated the beginning of the next trial after 1,000 ms. Proper task performance was assessed from accuracy in responding to both change and no-change trials. Each session had an approximate duration of 30 min.

Results

All participants reached the required threshold of 80% detection accuracy. We did not conduct a specific comparison between experiments concerning this result, but the absence of any participant below the threshold suggests that the inclusion of trials in which change detection was easier (i.e., the sparse trials) may have biased participants in Experiment 1 against reporting more subtle changes in cluttered trials.

Detection

Analysis of detection responses replicated the previous *detection cost*: more efficient responses for incongruent than congruent trials, $t_{(35)} = 8.05$, $p < 0.001$, Cohen's $d = 1.34$.

Recognition

Overall d' and beta were 0.80 and 2.35, respectively. Contrary to Experiments 1A and 1B, but replicating previously published results, analysis of hit rates showed the expected *recognition benefit*: targets from congruent scenes were remembered better than those from incongruent ones, $t_{(35)} = 2.21$, $p = 0.034$, Cohen's $d = 0.34$.

Discussion

Experiment 2 aimed at replicating the previously reported *recognition benefit*, which was surprisingly absent in Experiments 1A and 1B, and at testing whether *recognition benefit* would appear in conditions in which identification was not required. According to our predictions, participants in this experiment showed the standard *recognition benefit*, even though overall recognition scores were considerably lower in this case (38%) as compared to that found in previous studies when an identification task was included after detection (e.g., 62%, in Ortiz-Tudela et al., 2016). This lower recognition rate is most likely due to the shallower processing of the stimulus (Craik and Lockhart, 1972) and can be taken as indirect proof of the reduced level of identification achieved during the change detection task. Although it is not possible to claim that identification processes were completely absent when the task was removed (since these are most likely automatic and dynamically engaged when any stimulus is processed), it is fair to assume that they were at least minimized in this experiment. More importantly, the fact that we observed the *recognition benefit* under these conditions rules out the possibility that this benefit is due to post-detection processes engaged during the identification task itself.

It is worth noting that, since participants did not carry out the identification task, it is not possible to further correct the detection responses to discard incorrectly detected trials (i.e., trials in which the participant reported a change but were not able to locate it). However, given that these are rare and that participants had high accuracy in discarding the no-change trials, we argue that they are unlikely to have a meaningful impact on the results. Nevertheless, future studies with a location report task (e.g., *via* mouse click) will be needed to further clarify this issue.

The results of Experiment 2 show that the *recognition benefit* arises independently of the identification task and that it is more clearly observed when object competition is present in the scenes (i.e., when targets are presented in a cluttered context). However, the nature of this memory effect is still puzzling. Indeed, congruent trials are generally displayed for longer periods of time (Hollingworth and Henderson, 2000; LaPointe et al., 2013; Ortiz-Tudela et al., 2016), but incongruent objects tend to attract eye gaze and to be looked at longer than congruent ones (Henderson et al., 1999; LaPointe and Milliken, 2016). The literature on mere exposure effect shows that extended exposure strengthens memory simply by virtue of longer processing time. What happens then under the circumstances in which the *recognition benefit* is produced? Are incongruent targets looked at longer than the congruent ones, but still they get more poorly remembered? Or might it be the case that, under these particular conditions, congruent targets produced longer dwelling times, and *recognition benefit* arises as a by-product of this extended exposure? Some *post-hoc* analyses have been conducted before as tentative attempts at controlling these and related issues (Krebs et al., 2015; Rosner et al., 2015; Ortiz-Tudela et al., 2016), but no study to date aimed at ruling out this possibility by directly measuring gaze patterns.

On Experiment 3 we used a high temporal resolution eye tracking system to record eye movements while the scenes were being processed to gain more insights into the mechanisms underlying each one of these processes.

Experiment 3

Building upon previous studies recording eye movements in change detection tasks (Henderson et al., 1999; Hollingworth et al., 2001; LaPointe and Milliken, 2016), in Experiment 3 we used the full paradigm (i.e., the three sequential tasks) to be able to characterize the gaze patterns associated respectively with the *detection cost*, the *identification benefit*, and the *recognition benefit*. This characterization provides an indirect measure of potential underlying mechanisms such as attentional capture, processing time or cognitive effort (see Methods section for more on this).

Material and methods

Participants

As this experiment made use of the standard procedure for obtaining the three semantic indexes, the minimum sample size usually required to measure them (LaPointe et al., 2013; Ortiz-Tudela et al., 2016, 2018a; see Experiment 1 for analysis of power). Twenty students (15 female; mean age: 20.65; SD: 3.8) from the Universidad de Granada volunteered to take part in the study. All of them signed informed consent according to the local ethics committee.

Design

The overall structure of the experiment resembles that of the previous studies in the present paper: a change detection + identification task was followed by a distracter task, and then by a surprising recognition test. As in Experiment 2, we only used cluttered scenes as stimuli. The duration of the session was 1 h approximately.

Eye movement recording

A high sampling frequency (250 Hz) SMI [SensoMotoric Instruments (SMI), 1991] system was used to record participants' eye movements during the change detection task. A maximum of 1 degree of tracking error was accepted for every participant during a calibration phase at the beginning of the experiment. Participants' heads were placed on a chinrest 60 cm away from the monitor to avoid unwanted movements and to allow for a comfortable posture. Regions of interest were defined for each scene as a rectangular area encapsulating the object. This area extended vertically from the highest to the lowest pixel in the object image and horizontally from the left-most to the right-most pixel. All of the eye-tracking measures reported here were pulled from the built-in SMI's software [BeGaze; SensoMotoric Instruments (SMI), 1991]. BeGaze's default method of event detection for high-speed eye tracking data uses saccades (computed with a velocity-based algorithm) as primary event; fixations and blinks are, in turn, derived from saccades. Blinks are defined as saccades with a pupil diameter of 0 mm [see SensoMotoric Instruments (SMI),

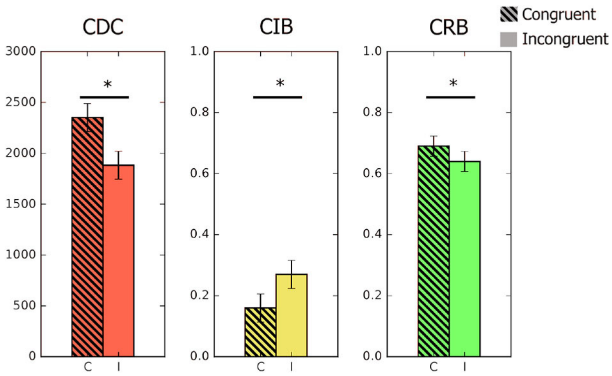

FIGURE 3
Graphic representation of the three semantic indexes in Experiment 3. From left to right: CDC, congruity *detection cost* (efficiency index); CIB, congruity *identification benefit* (errors); CRB, congruity *recognition benefit* (hits). Asterisks denote significant differences between congruent and incongruent trials ($p < 0.05$).

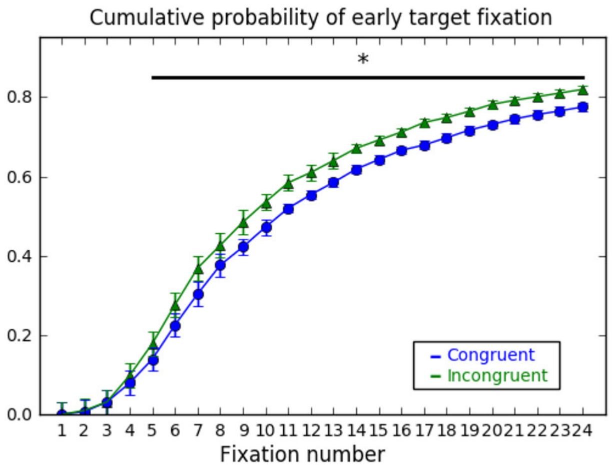

FIGURE 4
Cumulative probability of early target fixation for congruent and incongruent trials. Probability of having fixated the target as a function of ordinal fixation number. $^*p < 0.05$.

1991 for more details on how these measures are computed]. Post-processing of saccades, fixations and pupil diameter was performed with custom MATLAB scripts.

Results

Two participants were removed from the analyses due to an eye-tracking malfunction which caused the loss of all the session data.

The change detection analyses adequately reproduced the expected results. Namely, we obtained the usual *detection cost* of more efficient detections on incongruent trials than on congruent ones, $t_{(17)} = 9.34$, $p < 0.001$, Cohen's $d = 2.20$. We were also able to measure the *identification benefit*, $t_{(17)} = 4.82$, $p < 0.001$, Cohen's $d = 1.14$, and the *recognition benefit*, $t_{(17)} = 3.06$, $p = 0.007$, Cohen's $d = 0.72$ (see Table 1 and Figure 3 for a graphic representation of the three indexes).

Eye-movement measures

We selected three key variables of interest obtained from the raw pattern of gaze data. Namely, *target first fixation latency, on-target dwell time, and pupil dilation*. To analyse these three variables of interest during the study phase we took a step-by-step approach. First, we analyzed the three measures of interest (i.e., target first fixation latency, target dwell time, and pupil dilation) for all correctly detected trials, separately for congruent and incongruent targets. Then, we performed the same analyses for the subset of these correctly detected trials that corresponded to correctly identified targets. Finally, we examined the same variables of interest for the subset of these correctly identified trials that were also correctly remembered. Together with these three variables of interest, we also report here other secondary variables often used by researchers in similar areas for the sake of convergence of results. It is important to note that for all these three analyses, including those involving the memory results, the eye movements of interest were those recorded at the time of encoding. During the analysis stage, the eye-tracking measures were retroactively coded as a function of memory performance. This procedure is usually referred to as the "subsequent memory approach" and it is very common in the neuroimaging literature aiming at exploring encoding processes that have either a successful (i.e., eventually remembered) or unsuccessful (i.e., eventually forgotten) outcome (Brewer et al., 1998; Paller and Wagner, 2002).

Detection

Target first fixation latency

We analyzed target first fixation latency for successfully detected congruent and incongruent objects. Mimicking RT, fixation latency was shorter for incongruent objects (1,419 ms) than for congruent ones (1,755 ms), $t_{(17)} = 5.13$, $p < 0.001$, Cohen's $d = 1.21$.

To further explore this result and its relation with the *detection cost*, we run a Pearson correlation between the congruity effect on response times and on-target first fixation latency and found a high correlation of $r = 0.69$, $t_{(16)} = 3.76$, $p = 0.002$, Cohen's $d = 0.89$., suggesting that shorter first fixation latencies are at the basis of the *detection cost*. Besides, we also assessed the probability of fixating the target as a function of the number of fixations, which has also been used as a measure of early attention attraction (LaPointe and Milliken, 2016). The cumulative probability of fixating the target object within the first four fixations on the scene reached 10% and was equally probable for congruent and incongruent targets, $t < 1$. However, from the 5th fixation onwards the probability of fixating the target object started growing significantly faster for incongruent than for congruent targets (see Figure 4).

Target dwell time (ms)

Target dwell time was assessed by adding the total amount of fixation and saccades time (i.e., the total time spent exploring in the target area) for congruent and incongruent trials. No differences

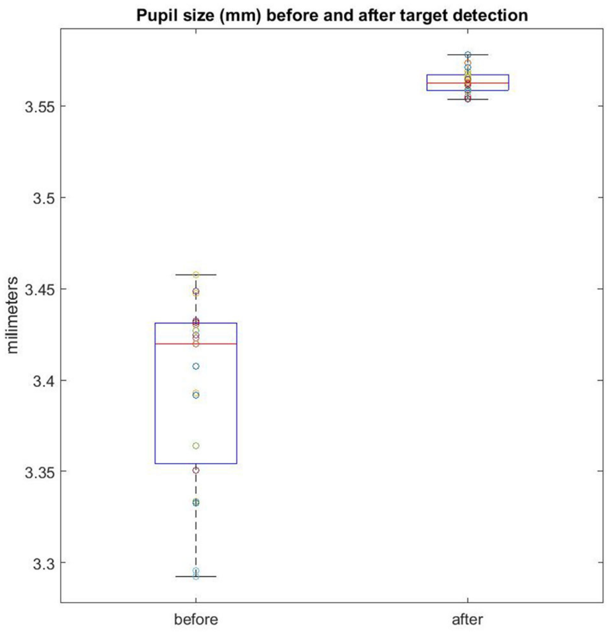

FIGURE 5
Pupil size (mm) before and after conscious reports of target detection. Each colored dot represents an individual participant.

were found in the time spent looking at the target for congruent (1,899 ms) or incongruent objects (1,867 ms), $t < 1$.

Pupil dilation

Pupil size has been used as a proxy for cognitive effort (Kahneman and Beatty, 1966; Peavler, 1974) with larger pupil diameter for effortful responses. Our results revealed larger average pupil dilation for those trials in which the target was detected (3.47 mm) compared to trials in which participants were unable to detect any change (2.48 mm), $t_{(17)} = 6.13$, $p < 0.001$, Cohen's $d = 1.44$. However, in order to ascertain whether increased pupil dilation is either cause or consequence of detecting the change, we compared mean pupil dilation before and after the target was found. The comparison revealed that pupil dilation following a properly detected target was smaller before (3.39 mm) than after (3.56) target detection, $t_{(19)} = 10.83$, $p < 0.001$, Cohen's $d = 3.04$ (Figure 5). This result suggests that increased pupil dilation can be a direct consequence of consciously detecting the target object (Critchley et al., 2005; Braem et al., 2015; Wessel and Aron, 2017). In any case, among the detected trials, no differences were found between congruent and incongruent objects, $t < 1$.

Identification

Target first fixation latency

The latencies of the first fixations for those targets that were correctly identified and those that were not did not differ from one another, $t < 1$. However, among the identified ones we still measured shorter latencies for incongruent objects (1,406 ms) than for congruent objects (1,748 ms), $t_{(17)} = 4.66$, $p < 0.001$, Cohen's $d = 1.10$.

Target dwell time (ms)

Total target dwell time for identified (1,927 ms) and unidentified (1,869 ms), did not differ significantly, $t_{(17)} = 1.01$, $p = 0.324$, Cohen's $d = 0.24$. When we considered only correctly identified objects, target dwell time did not differ between congruent (1,929 ms) and incongruent objects (1,802 ms), $t < 1$.

Pupil dilation

No differences in average pupil size were found either between identified and unidentified, $t < 1$, or between congruent and incongruent objects when these were identified, $t_{(17)} = 1.20$, $p = 0.24$, Cohen's $d = 0.28$.

Recognition

Target first fixation latency

No differences in target first fixation latencies were found between remembered and forgotten items, $t < 1$, among those that were correctly detected and identified. As in the previous analyses, when we took into consideration exclusively correctly remembered items, we also found the same pattern of shorter latencies for incongruent targets (1,441 ms) than for congruent ones (1,750 ms), $t_{(17)} = 4.78$, $p < 0.001$, Cohen's $d = 1.13$.

Target dwell time (ms)

No differences in dwell time were found either between remembered (1,886 ms) and forgotten (1,874 ms) items, $t_{(17)} = 1.17$, $p = 0.258$, Cohen's $d = 0.28$, or between congruent (1,880 ms) and incongruent (1,822 ms) remembered items, $t_{(17)} = 1.26$, $p = 0.22$, Cohen's $d = 0.30$.

Pupil dilation

Average pupil size in the change detection phase did not vary between later remembered (3.36 mm) and later forgotten trials (3.36 mm), $t < 1$. Finally, no differences were found between congruent and incongruent later remembered objects, $t_{(17)} = 1.19$, $p = 0.25$, Cohen's $d = 0.28$.

Discussion

Experiment 3 aimed at characterizing the gaze patterns associated with the three effects that semantic congruity causes on detection, identification, and recognition, namely the *detection cost*, *identification benefit*, and *recognition benefit*. We showed that the first fixations on the target region were shorter for incongruent than for congruent trials and that this difference was highly correlated with the strength of the *detection cost*. Moreover, we also showed that the cumulative probability of fixating the target over the first n-fixations grew faster for incongruent than for congruent targets, which is also an indicator of early gaze (and arguably attention)-attraction toward the target region (LaPointe and Milliken, 2016). This result suggests that the shorter time required to fixate an incongruent target, due to the attraction provoked by semantic mismatch, is responsible for the *detection cost*. Thus, considering that it has been previously shown that scene viewing is guided by meaning maps even more strongly than by saliency maps (Henderson and Hayes, 2017), we argue that in our paradigm, attention is, by default, endogenously guided by meaning in search for a change; however, whenever a mismatch between the meaning map and the bottom-up semantic information of the incongruent object occurs attention is quickly attracted toward this object.

In addition, we showed that no differences in dwell time were found for congruent and incongruent targets, which reflects that longer processing time is not responsible for the *identification benefit*. Rather, eased access to the objects' semantic content is most likely due to pre-activation by semantically related information either from context or from neighboring objects (Davenport and Potter, 2004; Eger et al., 2007; Henderson and Hayes, 2017). In a similar fashion, none of the other explored gaze measures during the processing of the scene were related to the *recognition benefit* and therefore we argue that this asymmetry between the remembering of congruent and incongruent trials is most likely due to the integration of the object identity into a coherent schema established by the scene and not to longer processing time. Accordingly, it is often found in the schema integration literature that information that matches pre-existing structures is more easily retrieved than one that is hard to reconcile with prior information (Van Kesteren et al., 2012; Brod et al., 2013; Gronau and Shachar, 2015).

Combined analysis

Finally, to enable a robust examination of the presence of the three indices reported above, we collapsed all four experiments (for cluttered scenes only) and used generalized or linear mixed-effect models (with lme4 in R; Bates et al., 2015) to account for individual variability in either the participants or the stimuli. For all three indices we modeled participants and stimuli (and their interaction with congruity) as random effects and our manipulated variable (i.e., congruity) as a fixed effect. Model comparison was performed to test for the significance of the random effects included. The winning model was determined following a backwards model selection procedure where a maximal model (i.e., the model including all possible random effects and interactions) is contrasted for explanatory power against a reduced model (i.e., obtained by removing one random effect from the maximal model). The two models are compared using a likelihood ratio test with an alpha level of 0.2 (Matuschek et al., 2017). If a significant decrease in model fit is observed, the removed effect is accepted to significantly contribute to the overall fit of the model and it is kept in for further analysis; if no significant decrease is observed, the removed effect is discarded, and another reduction step is performed. In this new reduction step, the previously reduced model now becomes the complex one and a new reduced model is created by removing one effect from the complex model. This process is repeated until no further random effects can be removed without significantly decreasing model fit. Each reduction step is labeled with a sequential number in the reports (e.g., *reduced 2* is a model obtained in the second reduction step). The order of removal went from interactions to main effects and from slopes to intercepts. In case a given reduction step involved two models with the same number of parameters, two alternative models were created (labeled as "a" and "b" in the reports) and both were separately tested against the complex one. Once the winning model was obtained, its parameters were estimated with Maximum Likelihood and the significance of the fixed effects was tested through analysis of deviance using Wald chi-square. Only the winning models (and their corresponding statistical comparisons) are reported in this section but see online materials at https://github.com/ortiztud/three_indices for the full set of scripts to reproduce the results.

Results

Detection

Trial-level detection accuracy scores were submitted to the modeling procedure with the winning model including random slopes and intercepts for participants and stimuli (AIC complex: 2,408.3; BIC complex: 2,459.5; against reduced 1a, $\chi^2(2) = 127.992$, $p < 0.001$, AIC reduced 1a: 2,532.3, BIC reduced 1a: 2,570.7; against reduced 1b, $\chi^2(2) = 10.050$, $p < 0.001$, AIC reduced 1b: 2,414.3, BIC reduced 2b: 2,452.7). We observed a main effect of congruity, $\chi^2(1) = 15.787$, $p < 0.001$, with higher accuracy for incongruent than congruent trials, $z = 3.973$, $p < 0.001$, $\beta = 3.61$.

In a similar way, trial-level RTs for correct detections were submitted to the modeling procedure with the winning model including random intercepts for participants and random slopes only for stimuli, $\chi^2(2) = 519.420$, $p < 0.001$ (AIC complex: 14,112, reduced 1: 14,625; BIC complex: 14,156, reduced 1: 14,650). As expected, we observed a main effect of congruity, $\chi^2(1) = 45.910$, $p < 0.001$, with faster RTs for incongruent than congruent trials, $t_{(217)} = -6.776$, $p < 0.001$, $\beta =$ -0.561.

Identification

As Experiment 2 did not include the identification task, trial-level identification accuracy scores for Experiments 1a, 1b, and 3 were submitted to the modeling procedure with the winning model including random slopes and intercepts for participants and stimuli (AIC complex: 2,194.7; BIC complex: 2,241.4; against reduced 1a, $\chi^2(2) = 4.515$, $p < 0.001$, AIC reduced 1a: 2,195.2, BIC reduced 1a: 2,230.3; against reduced 1b, $\chi^2(2) = 5.172$, $p < 0.001$, AIC reduced 1b: 2,195.9, BIC reduced 1b: 2,230.9). We observed a main effect of congruity, $\chi^2(1) = 14.762$, $p < 0.001$, with higher accuracy for incongruent than congruent trials, z $= 3.842$, $p < 0.001$, $\beta = 0.737$.

Recognition

Mimicking previous steps, trial-level recognition accuracy scores were submitted to the modeling procedure with the winning model including random intercepts for participants and stimuli (AIC complex: 4,901.1; BIC complex: 4,926.2; against reduced 2a, $\chi^2(2) = 136.391$, $p < 0.001$, AIC reduced 2a: 5,035.5, BIC reduced 2a: 5,054.3; against reduced 2b, $\chi^2(2) = 125.261$, $p < 0.001$, AIC reduced 2b: 5,024.3, BIC reduced 2b: 5,043.2). Exploring the winning model revealed a main effect of congruity, $\chi^2(1) = 10.499$, $p = 0.002$, with higher accuracy for congruent than incongruent trials, $z = -3.137$, $p = 0.002$, $\beta = -0.246$.

Discussion

The cross-experiments analysis provides a richer picture of the described effects. The LMM approach taken here allows the consideration of variability in overall performance across individuals in all three tasks and overall detectability, identifiability and memorability across all items included. Moreover, we were able to also test for the presence of significant variability in our participants and stimuli reactions to the congruity manipulation in all three tasks. Indeed, the model comparison approach revealed that these are important factors to consider as they explained different portions of the variance in all three tasks (see Supplementary material for the full report of the model comparison procedure).

More interestingly, after controlling for the between participants and stimuli variability, our three targeted indices remained significant. Namely, semantically congruent objects were detected worse but identified and recognized better than semantically incongruent ones. These results are, therefore, robust across different participants (i.e., four different samples), experimental setups (i.e., standard behavioral vs. eye tracking) and paradigm idiosyncrasies (i.e., inclusion of different stimulus types—Experiment 1a and 1b and alterations to the main paradigm—Experiment 2).

General discussion

Meaning maps have been shown to guide attention in scene viewing beyond the influence of perceptual saliency maps (Henderson and Hayes, 2017), thus acting as a tool for selecting which information to prioritize. Nevertheless, we still do not have a clear understanding of which processes are affected by semantic information and when and how that influence is exerted. Is semantic information only capable of influencing behavior after conscious access has taken place? Can semantic properties of objects bias conscious access itself? What is the relationship between these different processes? The lack of integrative theoretical models is most likely responsible for the mixed pattern of results found in previous studies about object detection, identification and remembering (Henson and Gagnepain, 2010; Van Kesteren et al., 2010; LaPointe et al., 2013; Stein and Peelen, 2015; Stein et al., 2016). Although extensive research has been conducted on these three processes, very few studies have attempted at studying them together by pursuing the flow of processing from early perception to remembering. In this study we aimed at jointly studying three key steps across the stream of processing; namely, from the moment in which one becomes aware of the presence of an object (i.e., detection), through that in which we gain access to its identity (i.e., identification) to that in which one is confronted with the need to retrieve it from memory (i.e., delayed recognition).

To approach these three processes, we have made use of three sequential behavioral indices, which result from the semantic relationship between selected objects and the natural scenes in which they are embedded. The first of these three indices, the *detection cost*, is defined as the impaired detection of certain objects when their semantic properties overlap with their surrounding scenes. The second one, the *identification benefit*, refers to an improved identification of certain objects precisely when they are embedded in semantically matching scenes. Finally, the *recognition benefit* reflects a relative advantage in retrieving objects when they had been presented within a congruent background. In order to deepen our knowledge of the processes underlying those effects, we conducted four experiments in which we explored their dependency on the structure of the scenes and the participant's task set (Experiments 1A and 1B), the relationship between the *recognition benefit* and the identification task (Experiment 2), and gaze patterns associated with each index (Experiment 3).

Experiments 1A and 1B showed that using scenes involving multiple objects (i.e., cluttered scenes) was a condition *sine qua non* to measure the *detection cost*. In other words, the *detection cost* only appears when competing objects are present together with the to-be-detected one and, therefore, when object selection is not trivial. The change detection task requires participants to explore different objects in search for a changing one; the *detection cost* effect indicates that such an exploration does not take place randomly, but it tends to prioritize processing of incongruent objects. Even if this exploration is performed on the basis of semantic maps (Henderson and Hayes, 2017) and congruity guides standard search (Peelen and Kastner, 2014), incongruent objects act as a circuit-breaker of the search process and capture attention. In other words, context incongruent objects tend to win the race for attention. Thus, in the same way that an abrupt onset captures spatial attention, incongruent objects seem to automatically segregate themselves from the semantic map and capture attention.

In addition to the implications for the detection process, Experiments 1A and 1B also showed that object competition is not needed to measure the *identification benefit*. Our identification results did not differ when object competition was removed by using sparse scenes. The propagation of activation from the gist of the scene to the identity of the target is most likely underlying the observed benefit in identification obtained in congruent conditions. However, without a neutral condition (i.e., not congruent and not incongruent), it is hard to ascertain whether the *identification benefit* actually reflects an absolute advantage for the processing of congruent targets, or rather an impairment produced in the identification of incongruent targets (see Ortiz-Tudela et al., 2016 for a discussion in favor of the latter account).

Experiment 2 provided a strong dissociation between the *recognition benefit* index and the *identification benefit* one. In this experiment, we eliminated the requirement of performing an identification task after the change detection task. Previous studies that have shown the *recognition benefit* effect could not ascertain whether the effect was due to processes occurring while participants were trying to detect the change, or it rather arose later, as a by-product of the subsequent identification task. By removing the identification task altogether and still measuring the *recognition benefit*, one can safely rule out that such offline re-elaboration was responsible for the better remembering of the congruent objects. Even though we cannot claim that identification processes were completely absent from the task, identifying those targets was not part of the explicit requirements, and thus their importance was reduced with respect to previous experiments (Ortiz-Tudela et al., 2016). Accordingly, compared to previous studies, Experiment 2 produced a much lower overall recognition rate [38% compared to an average of 62% in the experiments reported in Ortiz-Tudela et al. (2016)]. This result attest to the shallower processing (Craik and Lockhart, 1972) promoted by the conditions arranged in Experiment 2, and therefore indicate that the *recognition benefit* does not require the deep semantic elaboration brought about by the explicit requirement to explicitly identify the target.

Experiment 3 characterized the gaze patterns associated with each of the three main effects studied in this article. We found that the *detection cost* is mostly driven by shorter first fixation latencies and fewer saccades required to detect an incongruent target than to do so for a congruent one. Indeed, previous reports of the effect focusing exclusively on response times were unable to clarify whether faster detection of a change in incongruent trials could be due either to earlier attention attraction or to quicker decision process produced after the target was fixated. In other words, faster detection times in response to an incongruent trial may be produced because, once detected, participants responded right away, whereas responding to a congruent target might require them to wait until the next display is shown, in order to ascertain that this is precisely the object that is undergoing the change. However, our data point otherwise. The shorter latencies to fixate incongruent targets, together with the higher cumulative probability of early target fixation for incongruent targets, and the lack of significant differences in dwell time between congruent and incongruent targets, all suggest that incongruent objects act as efficient attractors of attention (Hollingworth and Henderson, 2000; LaPointe and Milliken, 2016).

Interestingly, our findings are in agreement with LaPointe and Milliken (2016) which also found no differences in dwell time between congruent and incongruent items in a change detection paradigm. They aimed at testing two major accounts proposed for explaining differences in object detection times. One of them, the attention attraction hypothesis, states that context-incongruent objects effectively draw attention and therefore speed detection responses; the other one, the attention disengagement hypothesis, proposes that while the scanning of an image would take place randomly, incongruent objects, when found, force attention to linger on them as a mean of extracting more information and when the change occurs attention is already at the appropriate location. Both LaPointe and Milliken (2016)'s results and the present ones seem to be more consistent with the former claim. Future studies restricting eye movements could further extend this finding and reveal whether shorter first fixation latencies are a necessary requirement for the *detection cost* or are just one of many other components.

In contrast to what has been observed for the *detection cost*, neither the *identification benefit* nor the *recognition benefit*, seem to be strongly related to any of the eye gaze measures considered in the current study. Exposure and processing time are potential key variables for both processes and therefore key factors to consider when drawing conclusions about identification or memory effects. The fact that the amount of time spent looking at the objects was equivalent for congruent and incongruent objects in spite of the asymmetry found in the variables measured suggests that exposure or processing time has little to no influence in generating said asymmetries.

Finally, it is worth noting that in our stimulus set, somehow reflecting the real world, there is a marked asymmetry between the amount of congruent and incongruent objects. Indeed, in complex ecological scenes, incongruent objects are, by definition, less frequent than congruent ones. One may argue that this unbalanced proportion could be biasing participants' strategies and affecting some of the processes that we are measuring here. However, the early arising of these effects and the fact that none of them is affected by the manipulation of predictability of the nature of the trials in Experiment 1 weakens the argument that conscious volitional strategies might be responsible for the observed effects. In addition, removing this asymmetry to equate the number of congruent and incongruent objects without disrupting the scene information is virtually impossible. Congruent objects are a constituent part of the scene itself and therefore largely increasing the number of incongruent objects would alter the gist of the scene; in other words, it would change the meaning map that guides the search (Henderson and Hayes, 2017). Some intermediate attempts can nonetheless be taken, for instance adding a few incongruent non-target objects, to reduce the efficiency of a *search-for-incongruity* strategy. Future studies including such manipulation would most likely produce very interesting results concerning the weight of the strategic component on these processes.

In the next subsections, we summarize the major implications of our findings for object detection, object identification and delayed object recognition processes.

Object detection

Conscious detection of a stimulus is arguably the first gate into awareness. Although still controversial, evidence in favor of pre-conscious semantic influences on detection has been continuously increasing. Here we have provided new evidence by showing that, in the presence of many non-target objects, some form of semantic analysis is done prior to conscious detection. When there is competition among a set of objects that are potential targets, the selection process is not randomly performed: rather, it is by default guided by the scene's semantic structure (Peelen and Kastner, 2014; Henderson and Hayes, 2017). This standard search is immediately stopped upon encountering scene-mismatching elements that are prioritized in the analysis since they convey more informational value (e.g., incongruent objects; Santangelo et al., 2015). Eye gaze and selective attention get directed toward those more informative items or regions of the space. Whether object selection occurs first and causes attentional allocation, or whether attention gets allocated and then a given object can be selected, is still unsolved. What can be safely assumed so far is that semantic processing (at least at the category level) leads to conscious detection by accordingly biasing attentional allocation and object selection (Mudrik et al., 2011; Stein and Peelen, 2015).

It is worth noting that, although Stein and Peelen's (2015) results also reflected influences of semantic information on the access to awareness, their manipulation seemed to exert its influence in the opposite direction. In their paradigm, an anticipatory cue was given to participants in the context of a bCFS setup; when the category of the object to be found matched the cue, shorter suppression times were measured. In other words, prior semantic knowledge improved object detection. This result seems in principle to be at odds with the *detection cost* reported here. One might argue that the presence vs. absence of object competition could account for the different results obtained in these two paradigms. However, in the sparse condition of Experiment 1, when most competing objects were eliminated, the *detection cost* was obliterated but no benefit from congruity arose either. Hence, another line of argument is needed to fully explain the overall pattern.

Finding a congruity-driven cost on detection in cluttered scenes, no effect of congruity on sparse scenes, and a congruity benefit when the target appears surrounded by random noise, can all be understood as different points in a continuum of contextual complexity, across which different processes probably operate. For instance, in the bCFS paradigm participants need to report the appearance of any object under a gradually fading mask. Under these circumstances, the ability to differentiate a portion of the display as a meaningful object is crucial, and thus semantic cues that help to disambiguate any *objectness* will speed responses. By way of analogy, consider the scenario of looking for shapes in the clouds. If one is told to look for a dog, it is not difficult to understand that one would find the shape of a dog more easily than something else.

On the other side of the continuum, looking for changes in a cluttered scene makes grasping the *objectness* of an item entirely irrelevant for the task at hand, since a lot of different objects are unambiguously presented. Under these conditions, competition among semantically related objects, or prioritization of highly informative targets, can bias the process to produce the *detection cost*. Halfway through this continuum (i.e., from segregating one object from random noise to selecting one among different clear candidates), is our sparse condition. The sparse scene scenario, in which neither visual noise nor distractor objects compete for the detection of the changing object, would constitute an intermediate case in which neither the cost of competition nor the benefit of semantic cueing would be expected (either due to the absence of the two or to both mechanisms canceling each other across different trials).

Object identification

If one needs to produce a fast and unspecific response whenever something changes, prioritizing novelty seems in order; thus, any information that helps to segregate the new element from its old surrounding environment will aid detection. However, if the task requires instead to identify a target, then it seems much more appropriate to integrate all available information, including the background image, and the semantic properties of all neighboring objects, to disambiguate the to-be-identified targets (Biederman, 1972; Eger et al., 2007; Esterman and Yantis, 2010; Gorlin et al., 2012). Early explorations of this effect showed that when an object is surrounded by a congruent context, its identification is better than when less contextual information is available (Biederman, 1972). Thus, the identification process rather than being negatively affected by object competition is instead positively affected by the propagated activation from the background scene. This pattern of identification responses stands in sharp contrast with that observed for detection responses where semantic incongruity improves performance.

Delayed object recognition

Many different factors determine which information is eventually remembered or forgotten. Here we used recognition just as one of the many ways in which it is possible to assess memory performance. Our results indicate (1) that the congruency-driven *recognition benefit* is observed even under shallow encoding circumstances, not requiring the explicit identification of each object (Experiment 2) and (2) that gaze behavior is unrelated to this memory benefit (Experiment 3). Although more research is needed to further explore these memory results, we hypothesize that the *recognition benefit* arises as a consequence of the integration of the detected object into previous internal schemas (Van Kesteren et al., 2010).

Conclusions

Across four experiments using a change detection paradigm, we explored the influence of semantic congruity on three key stages in the stream of object visual processing. This stream ranges from detection to identification and eventual remembering. We used three behavioral effects: a congruency-driven *detection cost*, an *identification benefit*, and a *recognition benefit*. These behavioral effects were taken as empirical proxies for detection, identification and recognition and used to analyse the dependency of these processes with respect to the semantic factor. We combined online and offline behavioral measures together with eye movement recordings to characterize the gaze patterns associated with each of these three processes. Our results provide new evidence on the nature of these components of visual cognition and illustrate the ways in which semantic support and competition can modulate these successive stages in the flow of information processing. The data presented in this study not only provides insights into the mechanisms underlying key cognitive processes but also presents a challenging yet very promising combined approach by studying the same information across different stages rather than by isolating processes that in the real world are very likely to interact with each other.

Data availability statement

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found at: https://github.com/ortiztud/three_indices.

Ethics statement

The studies involving human participants were reviewed and approved by Ethics Committee of the University of Granada. The patients/participants provided their written informed consent to participate in this study.

Author contributions

JO-T, LJ, and JL collaborated in the design of the experiments. JO-T carried out the data collection, analyses of results, and writing of the present manuscript. LJ and JL conceptually supervised the project and reviewed several drafts of the manuscript. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fcogn.2023.1125145/full#supplementary-material

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Statistical context learning in tactile search: Crossmodally redundant, visuo-tactile contexts fail to enhance contextual cueing

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In search tasks, reaction times become faster when the target is repeatedly encountered at a fixed position within a consistent spatial arrangement of distractor items, compared to random arrangements. Such "contextual cueing" is also obtained when the predictive distractor context is provided by a non-target modality. Thus, in tactile search, finding a target defined by a deviant vibro-tactile pattern (delivered to one fingertip) from the patterns at other, distractor (fingertip) locations is facilitated not only when the configuration of tactile distractors is predictive of the target location, but also when a configuration of (collocated) visual distractors is predictive—where intramodal-tactile cueing is mediated by a somatotopic and crossmodal-visuotactile cueing by a spatiotopic reference frame. This raises the question of whether redundant multisensory, tactile-plus-visual contexts would enhance contextual cueing of tactile search over and above the level attained by unisensory contexts alone. To address this, we implemented a tactile search task in which, in 50% of the trials in a "multisensory" phase, the tactile target location was predicted by both the tactile and the visual distractor context; in the other 50%, as well as a "unisensory" phase, the target location was solely predicted by the tactile context. We observed no redundancy gains by multisensory-visuotactile contexts, compared to unisensory-tactile contexts. This argues that the reference frame for contextual learning is determined by the task-critical modality (somatotopic coordinates for tactile search). And whether redundant predictive contexts from another modality (vision) can enhance contextual cueing depends on the availability of the corresponding spatial (spatiotopic-visual to somatotopic-tactile) remapping routines.

KEYWORDS

tactile search, contextual cueing effect, remapping, multisensory learning, crossmodal plasticity

1. Introduction

1.1. Contextual cueing in the individual modalities of vision and touch

Attention is guided by a number of separable mechanisms that can be categorized as bottom-up driven—such as guidance by salient physical properties of the current stimuli—or top-down controlled—such as guidance by observers' "online" knowledge about (search-) critical object properties (Wolfe and Horowitz, 2017). These processes are augmented by the automatic extraction of statistical co-occurrences of objects in the environment, rendering attention-guiding spatial long-term (LT) memories. For instance, repeatedly encountering a searched-for target item at a particular location within a visual scene of consistently arranged distractor items leads to the formation of LT relational distractor-target memories, that, upon being activated by the currently viewed search display, (relatively) efficiently direct attentional scanning toward the target location (Goujon et al., 2015; Sisk et al., 2019). This effect was first described by Chun and Jiang (1998), who, in their seminal study, had participants search for a target letter "T" (left- or right-rotated) among a set of (orthogonally rotated) distractor letters "L". In half of the trials, the spatial arrangements of the distractor and target stimuli were repeated, permitting participants to learn the invariant distractor-target relations to guide their search (repeated/predictive displays). In the other half, the distractors were distributed randomly on each trial, rendering their arrangement non-predictive of the target's position in the search array (non-repeated/non-predictive displays). Chun and Jiang's (1998) critical finding was that the reaction times (RTs) taken to find and respond to the target were faster for repeated vs. non-repeated display arrangements or "contexts". This effect referred to as "contextual cueing", subsequently was confirmed and elaborated in a plethora of studies using behavioral, computational, and neuroscientific measures (Chun and Jiang, 1999; Chun, 2000; Shi et al., 2013; Zinchenko et al., 2020; Chen et al., 2021a). In the first instance, of course, effective contextual cueing requires successful retrieval of the respective (search-guiding) LT-memory representation. Thus, for example, when the time for which the spatial distractor-target layout can be viewed is limited (Zang et al., 2015) or when encoding of the display layout is hampered by competing visual task demands (Manginelli et al., 2013), the retrieval of acquired context memories may be prevented, abolishing contextual facilitation.

Interestingly, contextual cueing is not limited to the visual modality: tactile predictive contexts can facilitate search, too. For instance, Assumpção et al. (2015) showed that people can become better at finding an odd-one-out vibrotactile target within arrays of repeated vs. non-repeated (homogeneous) vibrotactile distractor items delivered to participants' fingertips (where the vibrotactile distractor-target arrangements consisted of two stimulated fingers, excepting the thumb, on each hand). As revealed by postural manipulations of the hands (Assumpção et al., 2018), tactile contextual cueing is rooted in a somatotopic reference frame: spatial target-distractor associations acquired during training transfer to a test phase (with crossed or flipped hands) only if the target and distractors are located at the same fingers, rather than the same external spatial locations. This finding implies that search in repeated vs. non-repeated tactile distractor-target arrangements evokes, in default mode, a somatosensory reference frame, which is different from (default) spatiotopic encoding of distractor-target relations in visual search (Chua and Chun, 2003). However, while the learning of statistical co-occurrences of target and distractor items is bound to the currently task-relevant sensory modality, the brain has the ability to adapt and reorganize connectivity between different sensory modalities in response to consistent changes in input or experience—referred to as "crossmodal plasticity" (Bavelier and Neville, 2002; Nava and Röder, 2011). Thus, an interesting question arises, namely, whether the encoding of statistical regularities in one modality would facilitate search in another modality through the engagement of crossmodal-plasticity mechanisms. For instance, given that optimal task performance may depend on the use of all available sources of information, spatial learning in the tactile modality might be enhanced by congruent, redundant-signal information in the visual modality (Ho et al., 2009), and this may involve changes in the strength (and number) of connections between neurons in the visual and somatosensory regions of the brain. The possibility of such crossmodal spatial regularity/contextual learning is the question at issue in the current study.

1.2. Crossmodal contextual cueing across visual and tactile modalities

Initial evidence indicates that the mechanisms underlying contextual cueing may support the functional reorganization of one sensory modality following statistical learning in another modality. For example, Kawahara (2007) presented participants with meaningless speech sounds followed by a visual search display during a training phase. The location of the search was predictable from the preceding auditory stimulus. In the subsequent test phase, this auditory–visual association was either removed for one (inconsistent-transfer) group or maintained for another (consistent-transfer) group. The results revealed the search RTs to be increased for the inconsistent-transfer group but decreased for the consistent group—suggesting that visual attention can be guided implicitly by crossmodal association. In another study, Nabeta et al. (2003) had their participants first search for a T-type target among L-type distractors *visually* in a learning phase, which was followed by a test phase in which they had to search *haptically* for T- vs. L-shaped letters. The haptic search arrays (which were carved on wooden boards and covered by an opaque curtain) were arranged in the same or different configurations compared to the visual displays during initial visual learning. Nabeta et al. (2003) found that target-distractor contexts learned during visual search also facilitated haptic search in the absence of visual guidance. It should be noted, though, that Nabeta et al.'s haptic search involved active exploration, involving serial finger movements to sense the local items. Haptic search may thus have required participants to set up and continually update a visual working-memory representation of the scene layout, and the initially learned contexts may have come to interact with this representation, guiding the haptic exploration toward the target location. However, this would not work with tactile search scenarios involving spatially *parallel*, passive sensing, such as those explored by Assumpção et al. (2015, 2018). Passive tactile sensing and active manual exploration have been shown to involve distinct processes (Lederman and Klatzky, 2009). Accordingly, being based on active exploration, the findings of Nabeta et al. (2003) provide no clear answer as to whether and how target-distractor contexts

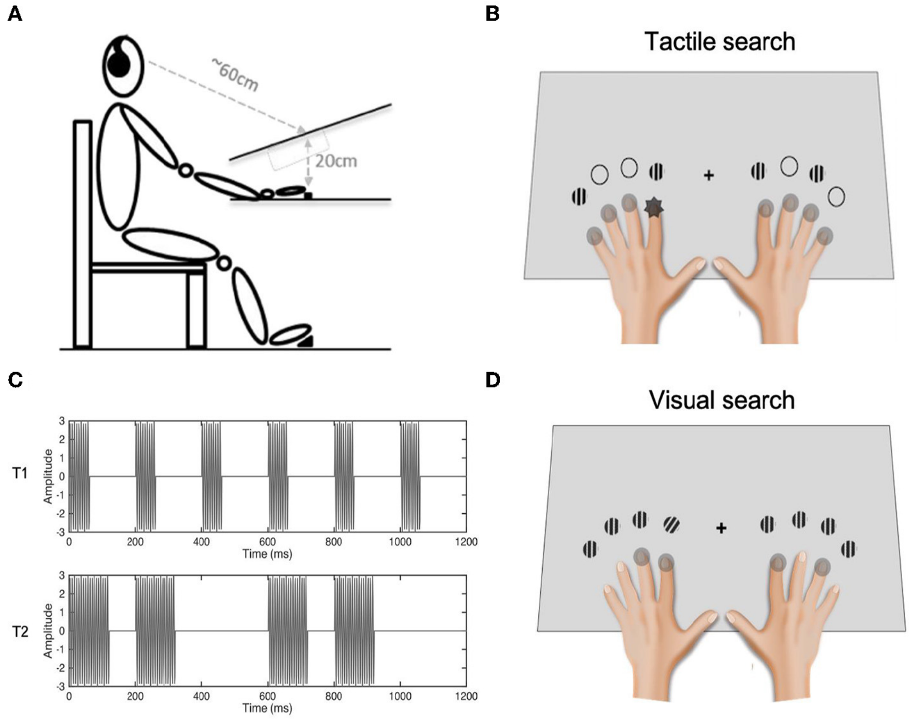

FIGURE 1
Illustration of the experimental set-up. As illustrated in Panel **(A)**, the height difference between the visual and tactile presentation planes was some 20 cm. Visual stimuli were presented on a white canvas surface tilted about 20° toward the observer. The viewing distance was 60 cm. Participants placed their fingers (except the thumbs) on the eight solenoid actuators and responded to the identity of the tactile singleton target *via* a designated foot pedal. Panel **(B)** depicts the visual-tactile stimuli in a tactile search task. The search display consisted of one tactile target (the dark "spark") with seven homogenous distractor vibrations (light gray circles), accompanied by a configuration of four distractor Gabor patches (and four empty circles). The locations of the tactile target for the tactile search and Gabor patches for the visual search varied depending on whether the displays were repeated or not. In the real setting, the hands were placed on the plane below the visual plane, as illustrated in Panel **(A)**. Panel **(C)** depicts the waveforms of the two possible tactile targets in a tactile search task. The upper panel depicts the waveform of target 1 (T1): a 5-Hz square wave with a 30% duty cycle delivered *via* 150-Hz vibrations. The lower panel illustrates the waveform of target 2 (T2): a 5-Hz square wave with an average 60% duty cycle, also composed of 150-Hz vibrations. The distractors were constant vibrations of 150 Hz. Panel **(D)** depicts the visual-tactile stimuli in a visual search task. One visual target was embedded among seven homogenous distractors, with a configuration of four vibrotactile stimulations delivered to two (selected) fingers (gray circles) of each hand.

acquired during visual search would transfer to parallel, passive tactile search.

Recently, Chen et al. (2021b) aimed to directly address this question by adopting a similar tactile-search paradigm to Assumpção et al. (2015, 2018), delivering vibrotactile stimulation to participants' fingertips instead of requiring active manual exploration. In addition, the visual search displays, projected on a white canvas on the top of the tactile array, were collocated with the tactile stimuli (Figures 1A, B). The visuotactile search arrays were constructed in such a way that only the visual configuration was predictive of the tactile target location (Figure 1C). Chen et al. (2021b) found that repeated visual contexts came to facilitate tactile search as the experiment progressed, but only if the tactile items were presented some 250–450 ms prior to the visual elements (Figure 2A). Chen et al. (2021b) attributed this tactile preview time to the need to recode the (somatotopically-sensed) tactile array in a visual reference frame, in order for a search to benefit from the predictive context provided by the visual distractor elements (sensed in spatiotopic format).

Using a similar visual-tactile setup (Figure 1A), Chen et al. (2020) investigated whether a predictive tactile context could facilitate visual search. Participants had to search for a visual odd-one-out target, a Gabor patch differing in orientation (clockwise or counter-clockwise) from seven homogeneous vertical Gabor distractors (see Figure 1D). Critically, unbeknown to participants, visual targets were paired with repeated tactile contexts in half of the trials, and with newly generated tactile contexts in the other half. Again, the visual-tactile display onset asynchrony was varied. Similar to Chen et al. (2021b), the repeated tactile context had to be presented before the visual target in order for crossmodal contextual cueing to manifest—again suggesting that a preview time was required for remapping the somatotopically encoded tactile context into the visual spatiotopic reference frame in which the target is encoded. Of note, in a control experiment, Chen et al. (2020) found that under conditions in which participants flipped their hands, but the visual target and tactile distractors were kept unchanged with respect to somatotopic coordinates, the crossmodal contextual-cueing effect was diminished (Figure 2B). This supports the idea that, with multisensory presentations, the predictive tactile context was remapped into a spatiotopically organized visual—i.e., target-appropriate—format (Kennett et al., 2002; Kitazawa,

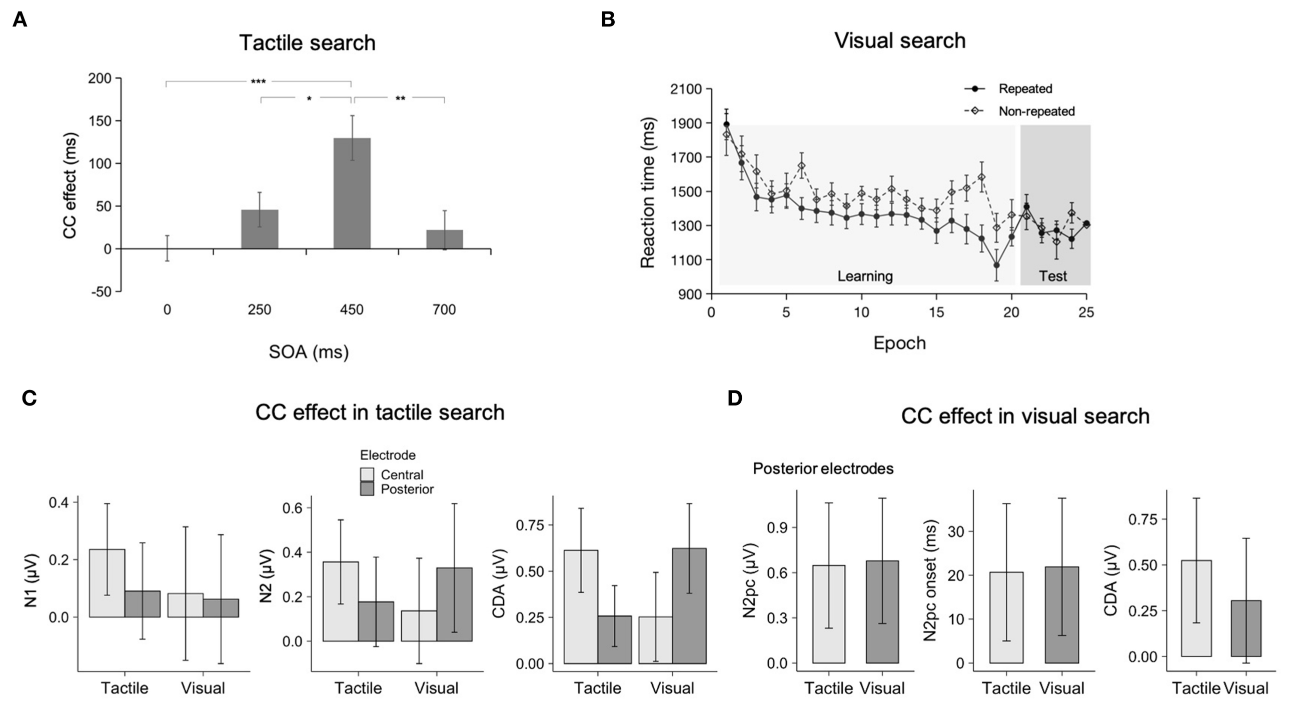

FIGURE 2
(A) Mean contextual-cueing (CC) effects (non-repeated minus repeated trial RTs, collapsed across epochs 1–5) as a function of stimulus-onset asynchrony (SOA) in the crossmodal tactile search task. The error bars show the standard error of the mean. Asterisks represent significance levels of $p < 0.001$ (***), $p < 0.01$ (**), and $p < 0.05$ (*) (see Chen et al., 2021b). **(B)** Mean RTs as a function of Epoch, separately for the repeated and non-repeated displays during learning and testing periods (participants flipped their hands, making the tactile distractors appear at different positions in external space while their somatotopic positions remained unchanged) in the crossmodal visual search (see Chen et al., 2020). **(C)** Mean CC effects on the N1, N2, and CDA amplitudes in the tactile- and visual-predictive context conditions at the central and posterior electrodes. Error bars indicate 95% confidence intervals (see Chen et al., 2022a). **(D)** Mean CC effects in the N2pc amplitudes and onset latencies, and CDA amplitudes in the tactile- and visual-predictive context conditions at the posterior electrodes (see Chen et al., 2022b).

2002; see also Azañón and Soto-Faraco, 2008; Heed et al., 2015).

But is the remapping process still helpful when predictive contexts are concurrently available in two sensory modalities? Recently, Chen et al. (2021a) investigated this issue by presenting redundant visual-tactile contexts intermixed with single visual contexts in a visual search task. Following Chen et al. (2020), the tactile context was presented 450 ms prior to the visual context to promote tactile-to-visual remapping. Interestingly, Chen et al. (2021a) found that contextual facilitation of search was increased with multisensory, i.e., visuotactile, contexts relative to predictive visual contexts alone—suggesting that multisensory experiences facilitate unisensory learning.

Taken together, previous studies (Chen et al., 2020, 2021a,b) investigating visual and tactile search in multisensory arrays consisting of visual and tactile items established that contextual cues available in one—distractor—modality can be utilized in the other—target—modality. Further, redundant contexts consisting of identically positioned visual and tactile elements can enhance visual learning of the relational position of the visual target item over and above that deriving from predictive visual contexts alone.

1.3. ERP evidence on crossmodal contextual cueing

Evidence for crossmodal cueing comes also from recent electrophysiological studies (Chen et al., 2022a,b). For example, when using the crossmodal search paradigm sketched in Figure 1, Chen et al. (2022a) found that in a tactile search task, facilitation of search RTs by repeated visual contexts was also seen in well-established electrophysiological markers of the allocation of visuospatial attention, in particular, the N2pc (Luck et al., 2000) and CDA (Töllner et al., 2013) measured at parietal-posterior ("visual") electrodes; however, the lateralized event-related potentials (ERPs) in the respective time windows were less marked at central ("somatosensory") electrodes (Figure 2C). In contrast, statistical learning of the unimodal (tactile) context led to enhanced attention allocation (indexed by the N1/N2cc/CDA) at central ("somatosensory") electrodes, whereas these effects were less prominent at posterior ("visual") electrodes. These findings indicate that both somatosensory and visual cortical regions contribute to contextual cueing of tactile search, but their involvement is differentially weighted depending on the sensory modality that contains the predictive context information. There is a stronger reliance on or weighting of, either a visual or somatotopic coordinate frame depending on the currently available sensory regularities that support contextual cueing in tactile-visual search environments. Worth mentioning is also the work of Chen et al. (2021b), who observed that crossmodal (tactile) context learning in a visual search resulted in enhanced amplitudes (and reduced latencies) of the lateralized N2pc/CDA waveforms at posterior ("visual") electrodes (see Figure 2D); both components correlated positively with the RT facilitation. These effects were comparable to the unimodal (visual context) cueing conditions. In contrast, motor-related processes indexed by the response-locked LRP at central ("somatosensory") electrodes contributed little to the RT effects. This pattern indicates that the crossmodal-tactile context is encoded in a visual format for guiding visual search.

1.4. Goals of the current study

The studies reviewed thus far show that search is not "a-historic". Rather, LT-memory representations about the searched-for target's relational position within a repeatedly encountered distractor context are accumulated across trials, and then expedite behavioral RTs and enhance lateralized ERP markers—both reflecting the more effective allocation of attention in repeated search displays. Importantly, statistical LT memories can be established in a crossmodal fashion, enabling re-occurring distractor configurations in one sensory modality (e.g., touch) to facilitate search in another (e.g., visual) modality. Theoretically, there are at least two principal accounts for this. One possibility is that crossmodal adaptation processes are set by the sensory modality that is dominant in a given performance function. Accordingly, given that spatial judgments are the province of the visual modality, items from non-visual modalities will be remapped into the coordinate system of this modality in spatial learning tasks (hypothesis 1). An alternative possibility is that functional reorganization of modalities is contingent on the modality that is relevant to the task at hand, i.e., the modality in which the target is defined (hypothesis 2). Critically, these two possibilities would make the opposite predictions regarding measurable indices of crossmodal learning in a tactile search task with redundant—i.e., both tactile and visual—distractor items presented in consistent (and thus learnable) configurations throughout performance of the task (see below for details). Hypothesis 1 would predict the remapping of the tactile items into a visual format and, thus, crossmodal facilitation of unisensory learning. In contrast, hypothesis 2 would predict no or at best a minimal benefit deriving from the presence of additional visual-predictive distractors alongside the tactile predictive distractors in a tactile search task. To decide between these alternatives, the present study implemented a *tactile* search task in which the visual as well as the tactile context were predictive of the target location (on multisensory trials), in order to investigate what context would be learned and in which modality-specific coordinate system the context would be encoded and retrieved to facilitate performance.

In more detail, we conducted two experiments (differing only in the stimulus-onset asynchrony, SOA, between the visual and tactile contexts) to examine the impact of multisensory, visuotactile (relative to unisensory, tactile-only) contexts on contextual facilitation learning in a tactile search task. Adopting a well-established, and demonstrably successful, multisensory learning protocol (Seitz et al., 2006; Kim et al., 2008; Shams et al., 2011; Chen et al., 2021a), observers had to search for and respond to a tactile odd-one-out target item appearing together, in a configuration, with three homogeneous tactile distractor items (see Figure 3). In 50% of the trials, the target-distractor configuration was fixed, i.e., the target appeared at a fixed location relative to the consistent distractor context (there were four such predictive, i.e., learnable contexts); in the other 50%, while the target position was also fixed, the locations of the distractors were randomly generated anew on each trial (there was the same number of such non-predictive contexts). Introducing this basic set-up, we tested contextual cueing in two separate, pure unisensory and mixed, uni- plus multisensory, phases. In the unisensory phase, the search was performed under the pure (unisensory) *tactile* task conditions just described; in contrast, in the mixed, uni- plus multisensory phase, trials with unisensory tactile stimulus arrays were presented randomly intermixed with trials with multisensory visuotactile contexts (the random mixing of trials ensured that participants adopted a consistent set to search for a tactile target). On the latter, visuotactile trials, the visual stimuli consisted of a configuration of three uniform distractor Gabor patches and one odd-one-out target Gabor patch, which were collocated with the positions of the tactile distractor and target stimuli. It is important to note that, in visuotactile studies of contextual cueing, the visual and tactile stimuli need to be collocated—which necessarily limits the number of (collocated) stimuli in the display. Nevertheless, previous work from our group has consistently shown reliable cueing effects using this multi-modal set-up (Chen et al., 2020, 2021b, 2022a,b), as well as with easy, "pop-out" visual search tasks (Geyer et al., 2010; Harris and Remington, 2017). Thus, by comparing contextual facilitation of RTs in tactile search with redundant, i.e., visual and tactile, distractor contexts vs. single, i.e., tactile-only, distractor contexts, we aimed to decide between the two alternative accounts (outlined above) of crossmodal contextual cueing in search tasks.

2. Materials and methods

2.1. Participants

Twenty-eight university students were recruited, and randomly assigned to Experiment 1 (14 participants; six males; $M = 27.4$ years, SD = 5.1 years) and Experiment 2 (14 participants; eight males; $M = 25.8$ years, SD = 3.95 years); they were all right-handed, had normal or corrected-to-normal vision, and reported normal tactile sensation. The sample sizes were determined by a-priori power analysis based on (effect size) $d_z = 0.81$ for a facilitatory effect of multisensory statistical learning in a similar study of multisensory context cueing (Chen et al., 2021a). According to the power estimates computed with G*Power (Erdfelder et al., 1996), a minimum sample size of 13 participants was required (with $\alpha = 0.05$, and power = 0.85). All participants provided written informed consent before the experiment and were paid 9.00 Euro per hour for their services. The study was approved by the Ethics Committee of the LMU Munich Faculty of Psychology and Pedagogics.

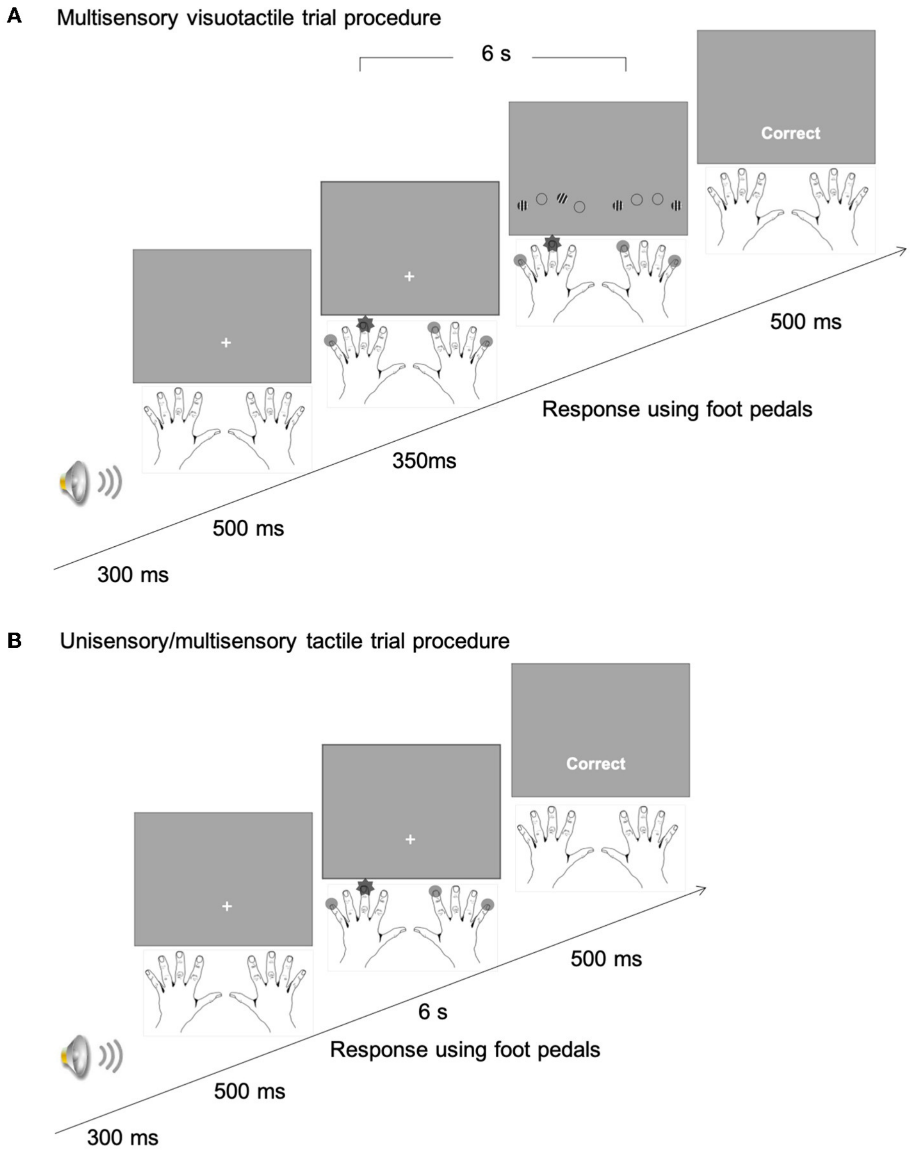

FIGURE 3
(A) An example stimulus sequence of a multisensory-visuotactile trial in the mixed uni- and multisensory phase of Experiment 1. After the initial auditory beep and fixation marker, tactile stimuli were presented for 350 ms prior to the onset of the visual items. In Experiment 2, the visual display was presented 200 ms earlier than the tactile display. The dark "star" represents the tactile singleton (target) finger, and the light gray disks the non-singleton (distractor) fingers. The four visual items were Gabor patches presented at, relative to the stimulated fingers, corresponding locations. The visual target was the single left- vs. right-tilted Gabor patch, among the three vertical distractor Gabor patches. Observers' task was to discriminate the tactile target-frequency pattern (T1 vs. T2) by pressing the corresponding foot pedal. The maximum stimulus duration was 6 s. A feedback display was presented after the response. **(B)** On unisensory/multisensory-tactile trials, only tactile stimuli were presented.

2.2. Apparatus and stimuli

Both experiments were conducted in a sound-attenuated testing chamber, dimly lit by indirect incandescent lighting, with a Windows computer using Matlab routines and Psychophysics Toolbox extensions (Brainard, 1997; Pelli, 1997). The tactile and visual items were presented at spatially corresponding locations at vertically offset (i.e., a lower, tactile and an upper, visual) presentation planes (Figure 1A). Visual stimuli (and task instructions/ feedback) were projected onto a white canvas in front of the participant, using an Optoma projector (HD131Xe; screen resolution: 1,024 × 720 pixels; refresh rate: 60 Hz), mounted on the ceiling of the experimental booth. The canvas was fixed on a wooden frame and tilted about 20° toward the observer. The viewing distance was fixed at about 60 cm with a chin rest. Responses were recorded using foot pedals (Heijo Research Electronics, UK).

Participants placed their eight fingers (except the thumbs) on eight solenoid actuators (each of a diameter of 1.8 cm, with a distance of 2 cm between adjacent actuators; see also Assumpção et al., 2015; Chen et al., 2020). The actuators activated lodged metal tips, vibrating a pin by 2–3 mm upon the magnetization of the solenoid coils, controlled by a 10-Channel Amplifier (Dancer Design) connected to the computer with a MOTU analog-output card. Four vibro-tactile stimulations were presented to four fingers, two of each hand, with the tactile target delivered to one finger and tactile distractors to the three other fingers. Each distractor was a constant 150-Hz vibration, while the target was one of the following vibration patterns (see Figure 1C): target 1 (T1) was a 5-Hz square wave with 30% duty cycle, composed of 150-Hz vibrations, and target 2 (T2) a 5-Hz square wave with an average 60% duty cycle, also made up of 150-Hz vibrations. To make T2 distinguishable from T1, a blank gap of 200 ms was inserted between every two cycles in T2 (the mean frequency of T2 was thus 3.3 Hz).

Visual stimuli in multisensory, visuo-tactile, trials consisted of four (striped black and white; Michelson contrast of 0.96, spatial frequency of 2 cpd) and four empty circles, each subtending 1.8° of visual angle, presented on a gray background (36.4 cd/m^2). Of the four Gabor patches, one patch was an odd-one-out orientation item, deviating by +30° or −30° from the vertical: the visual "target"; and the other three were orientation-homogeneous, vertical visual "distractor" patches (see also Chen et al., 2021a). The visual Gabor and empty-circle items were presented at the eight "virtual" (i.e., collocated) finger positions on the upper display plane, with a distance of about 1.9° of visual angle between adjacent items. The "target" and "distractor" Gabor positions exactly matched the vibro-tactile target and distractor stimuli, i.e.,: the response-relevant tactile target position was signaled redundantly by a collocated visual Gabor singleton. Importantly, cross-modally redundant target-location signaling was realized with both predictive and non-predictive distractor contexts. This also applied to the pairing of a particular vibro-tactile target (T1 or T2) with a particular visual Gabor orientation (+30° or −30°); this pairing was fixed for a particular participant (and counterbalanced across participants). Keeping these conditions the same with both predictive and non-predictive distractor contexts was designed to rule out any potential influences of space- and identity/response-based crossmodal correspondences (e.g., Spence and Deroy, 2013) on the dependent measure: contextual facilitation.[1] During task performance, participants wore headphones (Philips SHL4000, 30-mm speaker drive) delivering white noise (65 dBA) to mask the (otherwise audible) sound produced by the tactile vibrations. The white noise started and stopped together with the vibrations.

2.3. Procedure

Experiments 1 and 2 only differed in the stimulus-onset asynchrony (SOA) between the visual and tactile displays. In Experiment 1, the *tactile* display was presented 350 ms prior to the visual display, similar to the setting in our previous work (Chen et al., 2020, 2021a). Conversely, in Experiment 2, the *visual* display was presented 200 ms before the tactile display. A pilot experiment (run in preparation for the current study) with the unisensory *visual* displays showed that the 200-ms presentation of the displays was sufficient to produce a contextual cueing effect, with four repeated target-distractor configurations. This is consistent with a previous study of ours (Xie et al., 2020), which demonstrated a contextual cueing effect with a 300-ms presentation, even though the search displays were more complex (consisting of 1 T-shaped target among 11 L-shaped distractors) and there were 12 repeated target-distractor configurations. Moreover, evidence from neuro-/electrophysiological studies indicate that the allocation of spatial attention diverges as early as 100–200 ms post-display onset between repeated and novel target-distractor configurations (e.g., Johnson et al., 2007; Chaumon et al., 2008; Schankin and Schubö, 2009).

2.3.1. Practice tasks

Participants first practiced the response mapping of the foot (i.e., response) pedals to the tactile targets (T1 or T2). The target-pedal assignment was fixed for each participant but counterbalanced across participants. The practice phase consisted of four tasks: (1) tactile target identification (32 trials); (2) tactile search (32 trials); (3) visual search (32 trials); and (4) multisensory search (64 trials, half of which presented only tactile targets and the other half redundantly defined, visuotactile targets). Participants had to reach a response accuracy of 85% in a given task before proceeding to the next task (all participants achieved this criterion with one round of training).

In the tactile target-identification task, one vibrotactile target (either T1 or T2 lasting 6 s) was randomly delivered to one of the eight fingers. Participants had to respond, as quickly and accurately, as possible by pressing the corresponding foot pedal to discriminate the tactile target. During this task, the tactile array was always accompanied by the correct target label, "T1" or "T2", on the screen, to aid identification of the tactile target (T1 vs. T2) and mapping it onto the required (left vs. right foot-pedal) response. In the tactile-search task, four vibrotactile stimuli, one target and three distractors, were delivered to two fingers of each hand. Participants had to identify T1 or T2 as quickly and accurately as possible by pressing the associated foot pedal. Given the experimental task proper consisted of redundant visuo-tactile displays, the visual-search practice was designed to familiarize participants with the visual target (and distractor) stimuli and, so, ensure that they would not simply be ignored on multisensory trials in the experiment proper (in which the task could be performed based on the tactile stimuli alone). In the visual search task, eight visual items (four Gabors and four empty circles) were presented on the screen. Participants were asked to identify the target Gabor orientation (tilted to the left or the right) as rapidly as possible by pressing the corresponding foot pedal.

In the practice of the search task under mixed, uni- and multisensory conditions, participants were presented with 50% unisensory tactile trials (identical to the tactile-search practice) and 50% multisensory visuotactile trials (presenting both one target and three distractors in each, the visual and the tactile, modality), randomly interleaved (see Figure 3). In Experiment 1, the visual

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[1] To avoid potential response incompatibility, the response to the tactile target T1 or T2 (either left or right) was mapped to the orientation of the tactile-matched visual singleton (left or right), across phases. That is, if T1 was paired to the left-tilted Gabor patch in the mixed, uni- plus multisensory phase for a given participant, the target T1 was assigned to the left key, and T2 to the right key for that participant.

items were presented 350 ms after the tactile stimuli; in Experiment 2, they were presented 200 ms prior to the tactile stimuli. Importantly, although the multisensory displays had two collocated targets singled out in the two sensory modalities, participants were expressly instructed to set themselves for tactile search, even though the visual stimuli (provided only on multisensory trials) could provide cues to solving for the tactile task. This instruction was meant to ensure that the "tactile" task set was identical across uni- and multisensory trials[2]—allowing us to examine for any beneficial effects of multisensory vs. tactile stimulation on statistical context learning. To reflect *tactile* search, the RTs were recorded from the onset of the tactile stimuli in both experiments.

2.3.2. Experimental tasks

Immediately following the practice, each participant performed two experimental phases: a pure unisensory phase and a mixed, uni- and multisensory phase. The unisensory phase presented only tactile trials, and the mixed phase included both tactile and visuotactile trials, randomly intermixed. And the repeated target-distractor configurations were identical for the tactile-only and visuotactile trials in the mixed phase. The trial procedure was the same as in the respective practice tasks (see Figure 3). Each trial began with a 600-Hz beep (65 dBA) for 300 ms, followed by a short fixation interval of 500 ms. A search display (tactile or visuotactile) was then presented until a foot-pedal response was made or for a maximum of 6 s. Participants were instructed to respond as quickly and accurately as possible to the tactile target. Following observers' responses, accuracy feedback with the word "correct" or "wrong" was presented in the center of the screen for 500 ms (Figure 3). After an inter-trial interval of 1,000 ms, the next trial began. Eight consecutive trials constituted one trial block, consisting of the presentation of each of the four predictive display configurations plus four non-predictive configurations, in randomized order. After every two blocks, double beeps (2 × 200 ms, 1,000 Hz, 72 dBA, separated by an 800 ms silent interval) cued the accuracy feedback, with the mean accuracy attained in the previous two blocks shown in the center of the screen for 1,000 ms.

Half of the participants started with the pure unisensory phase and the other half with the mixed uni- and multisensory phase; each phase consisted of the same number of trials [Experiment 1: 256 trials per phase, with 128 repetitions per (repeated/non-repeated display) condition for the pure unisensory phase, and 64 repetitions per condition for the mixed, uni- plus multisensory phase; Experiment 2: 320 trials per phase with 160 repetitions per condition for the pure unisensory phase and 80 repetitions per condition for the mixed phase], to equivalent numbers of trials with tactile information between the pure unisensory and the mixed, uni- and multisensory phase. We increased the number of trials in Experiment 2 in order to extend the opportunity for contextual learning, i.e.,: would the enhanced contextual facilitation by multisensory information become observable with more trials—i.e., repetitions of each predictive display arrangement—per learning "epoch"? Recall that each of the four predictive display arrangements is presented once per block, intermixed with four non-predictive displays in Experiment 1. So, in Experiment 2, an experimental epoch combined data across five blocks of trials (i.e., five repetitions of each predictive display), compared with four blocks (i.e., four repetitions of each predictive display) in Experiment 1.

2.4. Design

To balance stimulus presentations between the left and right sides, the search arrays always consisted of two distractors on one side, and one target and one distractor on the other side. There were 144 possible displays in total to be sampled from. For the repeated contexts, we randomly generated two different sets of four displays for each participant, one set for the pure unisensory phase (hereafter Set 1) and one for the mixed uni- and multisensory phase (Set 2). Separate sets of repeated displays were generated to minimize potentially confounding transfer effects across phases. For "repeated" displays (of both sets), the target and distractor positions were fixed and repeated in each phase. For "non-repeated" displays, by contrast, the pairing of the target location with the three distractor positions was determined randomly in each block; these displays changed across blocks, making it impossible for participants to form spatial distractor-target associations. Note, though, that target locations were repeated equally in non-repeated and repeated displays (see Figure 4). That is, in each block of four repeated and four non-repeated trials, four positions, two from each side, were used for targets in the repeated condition, and the remaining four positions (again two on each side) for non-repeated displays (we also controlled the eccentricity of the target locations to be the same, on average, for repeated and non-repeated trials; see Supplementary material for an analysis of the eccentricity effects). This was designed to ensure that any performance gains in the "repeated" conditions could only be attributed to the effects of repeated spatial distractor-target arrangements, rather than repeated target locations, in this condition (see, e.g., Chun and Jiang, 1998, for a similar approach).

2.5. Data analysis

Trials with errors or RTs below 200 ms or above three standard deviations from the mean were excluded from RT analysis. Mean accuracies and RTs were submitted to repeated-measures analyses of variance (ANOVAs) with the factors Modality (unisensory-tactile, multisensory-tactile, multisensory-visuotactile), Display (repeated vs. non-repeated), and Epoch (1–8; one experimental epoch combining data across four consecutive trial blocks in Experiment 1 and 5 blocks in Experiment 2). Greenhouse-Geisser-corrected values were reported when the sphericity assumption was violated (Mauchley's test, $p < 0.05$). When interactions were significant, least-significant-difference *post-hoc* tests were conducted for further comparisons. The contextual-cueing effect was defined as the RT difference between repeated and non-repeated displays. We conducted one-tailed t-tests to examine the

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[2] Of course, this was also the only set permitting the task to be performed consistently, without set switching, on both types of—randomly interleaved—trials.

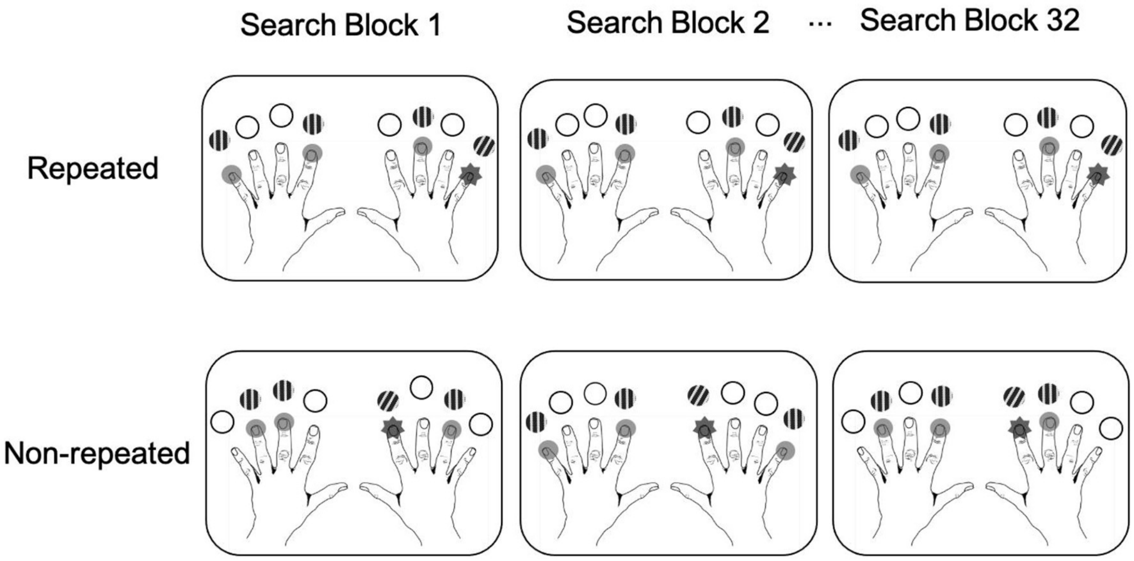

FIGURE 4
Schematic illustration of the distribution of targets in repeated and non-repeated displays across search blocks. In repeated displays, the target location was constant and paired with constant distractor locations; in non-repeated displays, only the target, but not the distractor, locations were held constant across blocks.

significance of the contextual-cueing effect (i.e., testing it against zero), given contextual cueing is, by definition, a directed effect: search RTs are expected to be faster for repeated vs. non-repeated search-display layouts (Chun and Jiang, 1998). We additionally report Bayes factors (Bayes inclusion for ANOVA) for non-significant results to further evaluate the null hypothesis (Harold Jeffreys, 1961; Kass and Raftery, 1995).

3. Results and discussion

3.1. Accuracy

The mean accuracies in Experiment 1 (in which the visual items were presented after the tactile items) were 90, 91, and 94%, for the unisensory-tactile, multisensory-tactile, and multisensory-visuotactile conditions, respectively. A repeated-measures ANOVA revealed no significant effects, all $ps > 0.31$, $\eta_p^2 s < 0.09$, $BF_{incl}s < 0.16$.

In Experiment 2 (where the visual items were presented before the tactile items), the mean accuracies were 93, 91, and 97% for the unisensory-tactile, multisensory-tactile, and multisensory-visuotactile conditions, respectively. A repeated-measures ANOVA revealed the main effect of Modality to be significant, $F_{(1.3,16.94)} = 7.63$, $p = 0.009$, $\eta_p^2 = 0.37$: accuracy was higher for multisensory-visuotactile trials compared to both unisensory-tactile and multisensory-tactile trials (two-tailed, $ps < 0.008$, $d_z s > 0.83$); there was no significant difference between the latter two conditions ($p = 0.37$, $d_z = 0.25$, $BF_{10} = 0.39$). Thus, accurately responding to the tactile target was generally enhanced by the preceding visual display (whether or not this was predictive). Further, accuracy was overall slightly higher for repeated (94.4%) vs. non-repeated (93.5%) displays, $F_{(1,13)} = 4.66$, $p = 0.05$, $\eta_p^2 = 0.26$, $BF_{incl} = 0.10$, though the Bayes factor argues in favor of a null effect. No other effects were significant, all $ps > 0.1$, $\eta_p^2 s < 0.16$, and $BF_{incl}s < 0.31$.

3.2. RTs

Trials with extreme RTs were relatively rare: only 0.4% had to be discarded in Experiment 1 and 0.5% in Experiment 2. Figure 5A depicts the correct mean RTs for repeated and non-repeated displays as a function of Epoch, separately for the unisensory-tactile, multisensory-tactile, and multisensory-visuotactile trials, for Experiments 1 and 2, respectively. By visual inspection, both experiments show a procedural-learning effect: a general (i.e., condition-non-specific) improvement of performance with increasing practice of the task. Importantly, in contrast to Experiment 1, there was a clear contextual-cueing effect (over and above the general performance gain) in the multisensory-visuotactile as well as unisensory-tactile and multisensory-tactile search conditions (witness the differences between the corresponding solid and dashed lines) in Experiment 2; in Experiment 1, by contrast, there appeared to be no cueing effect in the multisensory-visuotactile condition. Recall, the only difference between Experiments 2 and 1 was the order in which the visual and tactile (context) stimuli were presented on multisensory-visuotactile trials: the visual context preceded the tactile context in Experiment 2, whereas it followed the tactile context in Experiment 1.

3.2.1. Experiment 1

A repeated-measures ANOVA of the mean RTs in Experiment 1 revealed significant main effects of Display (repeated vs. non-repeated), $F_{(1,13)} = 11.24$, $p = 0.005$, $\eta_p^2 = 0.46$, and Epoch, $F_{(7,91)}$

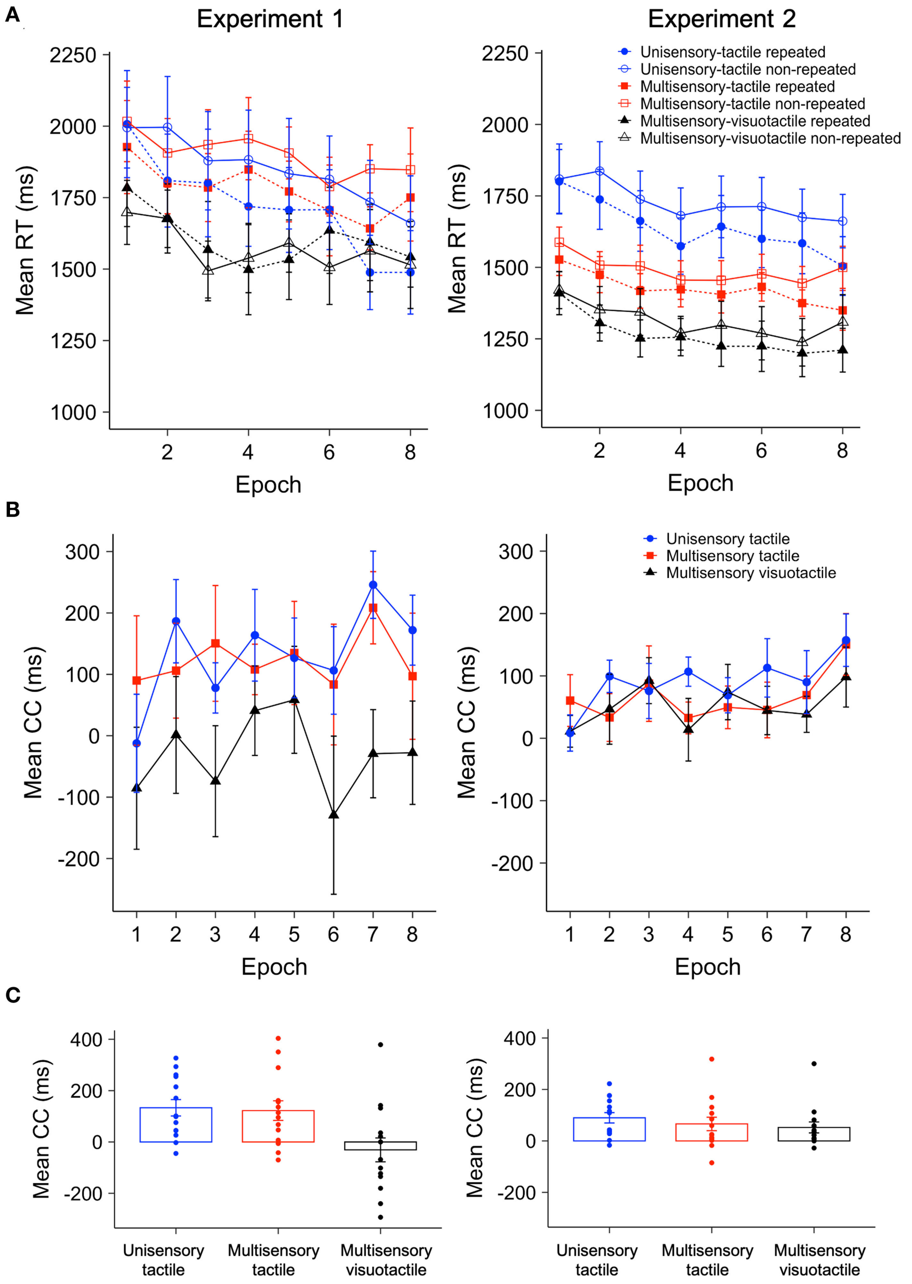

FIGURE 5
(A) Mean RTs for repeated and non-repeated contexts across epochs (one epoch collapses the RT data across four consecutive blocks in Experiment 1 and five blocks in Experiment 2) for unisensory-tactile, multisensory-tactile, and multisensory-visuotactile trials in Experiments 1 and 2. **(B)** Mean contextual-cueing (CC) effect as a function of epoch, for unisensory-tactile, multisensory-tactile, and multisensory-visuotactile trials in Experiments 1 and 2. **(C)** Mean contextual cueing (CC) and individual participants' data as a function of modality in Experiments 1 and 2. The error bars denote the within-subject standard error of the mean in panel A and the between-subject standard error of the mean in panels **(B, C)**.

$= 3.30$, $p = 0.004$, $\eta_p^2 = 0.2$. RTs were generally shorter to repeated vs. non-repeated displays, indicative of contextual cueing (mean contextual-cueing effect = 75 ms); and they decreased (linearly) across the task epochs, indicative of procedural task learning. The main effect of Modality was non-significant, $F_{(1.4,18.24)} = 0.97$, $p = 0.37$, $\eta_p^2 = 0.07$, $BF_{incl} = 0.63$. Importantly, the Modality × Display interaction turned out significant, $F_{(2,\,26)} = 5.35$, $p = 0.01$, $\eta_p^2 = 0.29$, due to the RT difference between repeated and non-repeated displays (i.e., contextual facilitation) being more pronounced for both unisensory-tactile and multisensory-tactile trials compared to multisensory-visuotactile trials (two-tailed, $ps < 0.047$, $d_z s > 0.59$), without a difference between the former two (purely tactile) conditions ($p = 0.82$, $d_z = 0.06$, $BF_{10} = 0.28$). No other interactions were significant (all $ps > 0.45$, $\eta_p^2 s < 0.07$, $BF_{incl} s < 0.1$). Of note, contextual facilitation was reliably greater than zero in the unisensory-tactile (133 ± 27 (SE) ms, one-tailed $t_{(13)} = 4.18$, $p <$

0.001, $d = 1.12$) and multisensory-tactile (122 ± 45 ms, $t_{(13)} = 3.20$, $p = 0.007$, $d = 0.86$) conditions, but not the multisensory-visuotactile condition [-31 ± 45 ms, $t_{(13)} = -0.66$, $p = 0.52$, $d = -0.18$, $BF_{10} = 0.18$; Figure 5C].

Despite the non-significant Modality main effect, responding appeared to be generally faster in the multisensory-visuotactile condition, and this may have curtailed any contextual-facilitation effect. Therefore, to more "fairly" compare contextual cueing among the three modality conditions, we normalized the cueing effect by dividing the mean RT facilitation ($RT_{non-repeted} - RT_{repeated}$) by the mean RT for the respective modality condition, for each observer. Not surprisingly, the comparisons again revealed contextual facilitation to be much smaller (and, in fact, absent) for the multisensory-visuotactile condition ($-2.6 \pm 2.7\%$) compared to both the unisensory-tactile ($6.5 \pm 1.3\%$) and multisensory-tactile ($7 \pm 2\%$) conditions (see Supplementary Figure S2), $F_{(2, 26)} = 5.91$, $p = 0.008$, $\eta_p^2 = 0.31$.

Additional comparisons confined to the very first epoch of learning revealed no significant context-based facilitation for any of the three modality conditions, $ps > 0.20$, $d_z\text{s} < 0.24$, $BF_{10}\text{s} < 0.58$. In other words, contextual facilitation in the unisensory-tactile and multisensory-tactile conditions required more than four repetitions of each predictive tactile context (i.e., the number of repetitions in Epoch 1) to evolve.

Experiment 1 thus showed that predictive tactile contexts alone could facilitate tactile search in both the pure unisensory and mixed, uni- and multisensory phases of the experiment, whereas redundant predictive visuotactile contexts (with the visual display following the tactile array) failed to facilitate tactile search. Note that, in the mixed multisensory phase, the purely tactile and the visuotactile contexts involved exactly the same predictive tactile item configurations. Accordingly, the absence of contextual facilitation on visuotactile trials, which contrasts with the manifestation of facilitation on purely tactile trials (where the two types of trial were presented randomly interleaved), indicates that it is not the lack of contextual learning that is responsible for lack of cueing on the former trials; instead, this is likely due to retrieval of successfully learnt contexts being blocked when the visual context is presented after the tactile search array—consistent with previous findings (Zang et al., 2015).

3.2.2. Experiment 2

A repeated-measures ANOVA of the mean RTs in Experiment 2 again revealed significant main effects of Display, $F_{(1,13)} = 15.82$, $p = 0.002$, $\eta_p^2 = 0.55$, and Epoch, $F_{(1.93,25.13)} = 5.14$, $p = 0.014$, $\eta_p^2 = 0.28$. RTs were faster to repeated vs. non-repeated displays (mean contextual-cueing effect = 70 ms), and task performance improved generally with time-on-task. However, different from Experiment 1, the main effect of Modality was also significant, $F_{(1.15,14.91)} = 5.29$, $p = 0.03$, $\eta_p^2 = 0.29$: responding was substantially faster on multisensory-visuotactile trials (1,286 ± 23 ms) compared to both multisensory-tactile (1,459 ± 19 ms) and unisensory-tactile trials (1,683 ± 31 ms; $ps < 0.024$, $|d_z|\text{s} > 0.68$), without any significant differences between the latter two conditions ($p = 0.12$, $d_z = 0.45$, $BF_{10} = 0.83$). There were no significant interactions, all p's > 0.11, $\eta_p^2\text{s} < 0.12$, $BF_{incl}\text{s} < 0.31$. Further one-sample t-tests revealed the contextual-facilitation effect to be reliable (i.e., greater than zero; see Figure 5C) and statistically comparable [$F_{(2, 26)} = 1.15$, $p = 0.33$, $\eta_p^2 = 0.08$, $BF_{incl} = 0.38$] in all three modality conditions [unisensory-tactile trials: 90 ± 23 ms, $t_{(13)} = 4.50$, $p < 0.001$, $d = 1.20$; multisensory-tactile trials: 66 ± 19 ms, $t_{(13)} = 2.53$, $p = 0.013$, $d = 0.68$; multisensory-visuotactile trials: 53 ± 9 ms, $t_{(13)} = 2.46$, $p = 0.014$, $d = 0.66$]. Again, given the differences in the general, baseline-RT levels among the three conditions and the facilitation effects scaling with the baseline RTs, we further examined the normalized facilitation effects (see Supplementary Figure S2). These were $5.8 \pm 1.4\%$, $4.5 \pm 1.6\%$, and $4.5 \pm 1.4\%$ for the unisensory-tactile, multisensory-tactile, and multisensory-visuotactile conditions, respectively, and did not differ among the three conditions, $F_{(1.37,17.82)} = 0.57$, $p = 0.51$, $\eta_p^2 = 0.04$, $BF_{incl} = 0.26$. This pattern indicates when the visual context is presented prior to the tactile context, it neither enhances nor suppresses contextual cueing. Again, comparisons within Epoch 1 revealed no significant contextual facilitation in any of the three conditions, $ps > 0.14$, $d_z\text{s} < 0.38$, $BF_{10}\text{s} < 0.92$—so, more than five repetitions of each predictive tactile configuration were required to engender a cueing effect.

The significant Modality effect is interesting: It was due to the preceding visual display generally enhancing both response speed and accuracy (see the accuracy results above). However, this effect (in both RTs and accuracy) is independent of whether the visual context is predictive or non-predictive of the target location in the tactile array, i.e., it does not impact the contextual-cueing effect (the Modality × Display interaction was non-significant). Thus, the visual display likely just acts like an additional "warning signal" (Posner, 1978) over and above the auditory beep and fixation cross at the start of the trial, boosting observers' general preparedness for processing the impending tactile array.

3.2.3. Experiment 1 vs. Experiment 2

A further ANOVA comparing the *normalized* contextual-facilitation effects between Experiments 1 and 2, with the within-subject factors Modality and Epoch and the between-subject factor Experiment (see Supplementary Figure S2), revealed a significant main effect of Modality, $F_{(2, 52)} = 5.6$, $p = 0.006$, $\eta_p^2 = 0.18$, owing to a much reduced contextual-facilitation effect for multisensory-visuotactile trials vs. both multisensory-tactile (mean difference = 4.8%) and unisensory-tactile trials (mean difference = 5.2%; $ps < 0.01$, $|d_z|\text{s} > 0.71$); there was no difference between the latter two conditions (mean difference = 0.4%, $p = 0.83$, $d_z = 0.06$, $BF_{10} < 0.1$). This pattern was mainly attributable to Experiment 1 (rather than Experiment 2), as attested by the Modality × Experiment interaction, $F_{(2, 52)} = 4.38$, $p = 0.018$, $\eta_p^2 = 0.14$. And there was no significant difference in the normalized facilitation effects between experiments in the unisensory-tactile and multisensory-tactile conditions ($ps > 0.33$, $|d|\text{s} < 0.37$, $BF_{10}\text{s} < 0.51$), but a significantly reduced effect in the multisensory-visuotactile condition in Experiment 1 vs. Experiment 2 ($p = 0.026$, $d = -0.89$). No other effects were significant, all p's > 0.38, $\eta_p^2\text{s} < 0.04$, $BF_{incl}\text{s} < 0.1$. The results pattern remained the same when examining the original (non-normalized) contextual-cueing scores (see Figure 5B). Thus, the multisensory-visuotactile condition engendered less (if any) contextual facilitation in Experiment 1 compared to Experiment 2. Given the analysis unit of an "Epoch" is somewhat arbitrary and, arguably, to examine for procedural learning effects, the cueing effect between the very first epoch of learning (in which participants had encountered the repeated arrangements only a few times) and the very last epoch (by which they had the maximum opportunity to acquire the contextual regularities) was compared by an ANOVA on the *normalized* contextual-facilitation effects with the within-subject factors Modality and Epoch (Epoch 1, Epoch 8) and the between-subject factor Experiment. The results revealed a significant main effect of Modality, $F_{(2,\,52)} = 4.87$, $p = 0.012$, $\eta_p^2 = 0.16$, and a significant main effect of Epoch (Epoch 1, Epoch 8), $F_{(1,26)} = 4.46$, $p = 0.04$, $\eta_p^2 = 0.15$, with a larger cueing effect in Epoch 8 than in Epoch 1 (mean difference = 4.9%). No other main effects or interaction effects were significant, all p's > 0.15, η_p^2s < 0.08, BF_{incl}s < 0.22. This result pattern is indicative of an increased effect of the contextual learning across the experiment for all three modality conditions, in both Experiments 1 and 2.

4. General discussion

The question at issue in the present study was to examine what context would be learned, and in which modality the context would be encoded and retrieved if both visual and tactile contexts are available in principle to guide tactile search. To address this, in two experiments, we compared the impact of multisensory, relative to unisensory, predictive contexts on the performance of a tactile search task. The two experiments differed in the order in which the visual and tactile contexts were presented on multisensory-visuotactile trials: the visual context followed the task-critical context in Experiment 1 but preceded it in Experiment 2. Critically, in the mixed uni- and multisensory phase of the task, we randomly intermixed tactile-only and visuotactile trials using identical predictive configurations in both trial types. Both experiments revealed reliable contextual cueing when the tactile context was shown alone, whether in a separate (unisensory) phase or randomly intermixed with visuotactile trials in the mixed (uni- and multisensory) phase, replicating previous findings (Assumpção et al., 2015, 2018). However, presenting both identically positioned visual items and the tactile target-distractor configuration together on multisensory trials did not enhance the contextual-cueing effect over and above the presentation of the tactile array alone, i.e.,: there was no redundancy gain from multisensory-visuotactile contexts. Indeed, the expression of the cueing effect was impeded when the visual display was presented after the tactile array in Experiment 1. We take the lack of a redundancy gain even under optimal conditions (with the visual display preceding the tactile array in Experiment 2) to indicate that, despite the availability of redundant, visual and tactile predictive item configurations, statistical learning of distractor-target contingencies is driven (solely) by the task-relevant, tactile modality.

Presenting the visual display after the tactile array on multisensory trials in Experiment 1 abolished the contextual-cueing effect. Given that the same predictive tactile configurations significantly facilitated tactile search on tactile-only trials in the multisensory (i.e., the mixed, uni- plus multisensory) phase of the experiment, the lack of a contextual cueing effect on multisensory trials may be owing to the (delayed) presentation of the visual display interfering with tactile-context retrieval, likely by diverting attention away from the tactile modality (see also Manginelli et al., 2013; Zang et al., 2015). Whatever the precise explanation, the differential effects between Experiments 1 and 2 agree with the hypothesis that which modality is selected for the encoding of contextual regularities is determined by the task at hand.

Recall that in the existing studies of crossmodal contextual cueing (Chen et al., 2020, 2021a,b, 2022a,b), search was either visual (Chen et al., 2020, 2021a, 2022b) or only visual predictive contexts were presented to inform tactile search (Chen et al., 2021b, 2022a). Those studies consistently showed that learning predictive distractor contexts in one modality can facilitate search in the other, target modality while highlighting the aptness of the spatiotopic visual reference frame for crossmodal spatial learning. A question left open by these studies was how statistical context learning develops in the presence of redundant context stimuli encoded in different reference frames in search of a tactile target—in particular, predictive visual and predictive tactile contexts sensed in spatiotopic and somatotopic frames, respectively. We take the pattern of findings revealed in the present study to provide an answer: The spatiotopic reference frame of the visual modality is not the default system for multisensory contextual learning. Rather, when the task requires a search for a tactile target, contextual memories are formed within the somatotopic frame of the tactile modality—even when the target location is redundantly predicted by both the tactile and the visual item configuration.

In the previous study, Chen et al. (2021a) had observed enhanced contextual cueing when the task-critical visual item configuration was preceded by predictive tactile contexts (vs. predictive visual contexts alone) in a *visual* search task. Extrapolating from this result, a multisensory contextual redundancy gain would also have been expected in the present study, at least when the visual display preceded the tactile array. Chen et al. (2021a) argued that presenting the tactile context prior to the visual context in the visual search task permitted the predictive tactile array to be remapped into spatiotopic-external coordinates, i.e., the reference system of the visual modality. Accordingly, the remapped tactile-predictive array could be combined with the visual-predictive display, enhancing visual contextual cueing over and above the level rendered by the unisensory visual context alone (Kennett et al., 2002; Heed et al., 2015; Chen et al., 2021b). By analogy, in the present study, encoding of the preceding visual configuration could conceivably have engendered visual-to-tactile remapping, thus adding to the cue provided by the task-relevant tactile arrangement to enhance contextual facilitation (based on the common somatotopic reference system). However, our results are at odds with this possibility: although the prior onset of the visual array boosted performance (accuracy and speed) in general, it did not enhance contextual cueing. A likely reason for this is an asymmetry in coordinate-frame remapping: while somatotopic ("tactile") coordinates can be efficiently remapped into spatiotopic-external ("visual") coordinates, there may be no ready routines for remapping spatiotopic-external coordinates into somatotopic coordinates (Pouget et al., 2002; Eimer, 2004; Ernst and Bülthoff, 2004). Given this, the present findings demonstrate a limit of multisensory signal processing in contextual cueing: multisensory redundancy gains require that both the visual-predictive and the tactile-predictive contexts can be coherently represented in a reference frame that is supported by the task-critical, target modality. Our results show that predictive visual contexts fail to meet this (necessary) condition when the task requires a search for a tactile target.

We acknowledge a possible limitation of the current study, namely: the fact that participants underwent only a relatively short multisensory phase of task performance. Recall that, even in this phase, the critical, multisensory-visuotactile displays occurred only on half the trials (the other half being designed to enforce a tactile task set, as well as providing a unisensory-tactile baseline condition against which to assess any multisensory-visuotactile redundancy gains). Thus, it cannot be ruled out that multisensory contextual facilitation of tactile search might be demonstrable with more extended training regimens (for indirect evidence of the facilitatory effect of consistent audio-visual training on the subsequent performance of a pure visual search task, see Zilber et al., 2014). Accordingly, with respect to the present visuotactile scenario, future work might examine whether tactile cueing of target-distractor regularities would be enhanced by concurrent visual-predictive items when an extended training schedule, perhaps coupled with a pre-/post-test design (cf. Zilber et al., 2014), is implemented.

In sum, when both visual-predictive and tactile-predictive contexts are provided in a tactile search task, the tactile context dominates contextual learning. Even giving the visual contexts a head-start does not facilitate tactile learning, likely because there are no ready routines for remapping the visual item configuration into the somatotopic coordinates underlying the tactile task. We conclude that the task-critical—i.e., target—modality determines the reference frame for contextual learning; and whether or not redundant predictive contexts provided by another modality can be successfully exploited (to enhance contextual cueing) depends on the availability of the requisite spatial remapping routines.

Data availability statement

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/73ejx/.

Ethics statement

The studies involving human participants were reviewed and approved by LMU Munich Faculty of Psychology and Pedagogics. The patients/participants provided their written informed consent to participate in this study.

Author contributions

SC: conception, experimental design, data collection, data analysis, results interpretation, and drafting. ZS, TG, and HM: conception, experimental design, results interpretation, and revision. GV: data collection, data curation, and methodology. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fcogn.2023.1124286/full#supplementary-material

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Different oscillatory rhythms anticipate failures in executive and arousal vigilance

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Introduction: Vigilance is the challenging ability to maintain attention during long periods. When performing prolonged tasks, vigilance failures are often observed, reflecting a decrease in performance. Previous research has shown that changes in oscillatory rhythms are associated with states of vigilance loss. The present study aimed to investigate whether changes in different oscillatory rhythms anticipate failures in two vigilance components: (a) executive vigilance –necessary to detect infrequent critical signals– and (b) arousal vigilance –necessary to maintain a fast reaction to environmental stimuli without much control–.

Methods: 37 young adults (age: M = 25.86; SD = 4.99) completed two experimental sessions in which high-density electroencephalography signal was recorded while they performed the Attentional Networks Test for Interactions and Vigilance – executive and arousal components, a task that simultaneously measures executive and arousal vigilance along with others attentional functions. Changes in delta, theta, alpha, beta, and gamma power before target onset were analyzed at the trial level in the executive and the arousal vigilance subtasks and as a function of the behavioral response.

Results: Changes in different oscillatory rhythms were observed prior to failures in executive and arousal vigilance. While increased alpha power in left occipital regions anticipated misses in the executive vigilance subtask, increased delta power in frontal-central regions anticipated very slow responses in the arousal vigilance subtask.

Discussion: The present results further support an empirical dissociation at the neural level between executive and arousal vigilance. Changes in alpha –in left occipital regions– and delta –in frontal-central regions– power might be identified as different brain states associated with loss in vigilance components when performing prolonged tasks.

KEYWORDS

frequency power, vigilance, attention, EEG, failures

1. Introduction

Sustaining attention for prolonged periods is a challenging although imperatively necessary ability for many daily life situations (Neigel et al., 2020). When performing prolonged activities (as, for instance, driving a car in a route, taking a lecture, or monitoring critical signals in security environments), sustained attention usually decreases with time-on-task, a phenomenon scientifically known as "vigilance decrement" (Davies and Parasuraman, 1982; Basner et al., 2008; Warm et al., 2008; Stearman and Durso, 2016; Hancock, 2017). In the lab, the vigilance decrement is observed as a progressive increase in vigilance failures, i.e., as a higher miss rate in detecting infrequent signals and slower reaction times (RT) as time-on-task progresses (Thomson et al., 2016). With the aim to elucidate how changes in brain states may be underlying vigilance loss, there has been extensive interest in examining electroencephalographic (EEG) oscillatory rhythms across vigilance tasks (Oken et al., 2006; Kim et al., 2017; Karamacoska et al., 2019; Reteig et al., 2019; Pershin et al., 2023). Nevertheless, to date, it has been particularly challenging to determine whether specific oscillatory rhythms anticipate different vigilance failures in prolonged tasks.

According to some theoretical proposals, vigilance should not be conceived as a single mechanism (Sarter et al., 2001; Sturm and Willmes, 2001; Oken et al., 2006; van Schie et al., 2021). Several models have been proposed to account for vigilance loss in prolonged tasks, most of them predicting a depletion in attentional resources and variations in physiological states of arousal (Thomson et al., 2015; Esterman and Rothlein, 2019; Schumann et al., 2022). According to Sarter et al. (2001), sustained attention can be described as the interaction of two independent neural mechanisms: (a) a top-down mechanism modulating vigilance responses to detect infrequent signals that is regulated by the cholinergic system, and (b) a bottom-up mechanism modulating the arousal levels of attention, which is regulated by noradrenergic's release from the locus coeruleus and might be necessary to modulate the behavioral responsiveness of vigilance. Importantly, Esterman and Rothlein (2019) highlight the critical role of an optimal state of physiological arousal to maintain vigilance, as hypoarousal could lead to reduced task-engagement while hyperarousal might induce distractibility.

Moreover, behavioral tasks measuring vigilance seem to assess different components of this process. While in signal-detection tasks like the Mackworth Clock Test (Mackworth, 1948) participants must detect a critical signal (e.g., a double jump of the clock's hand) that occurs quite rarely, in single RT tasks like the Psychomotor Vigilance Test (Lim and Dinges, 2008) participants are required to stop a single stimulus (i.e., a millisecond counter) as fast as possible, by pressing any available key. Noting the diversity in the theoretical frameworks and behavioral paradigms developed to investigate vigilance, in the last years it has been proposed that vigilance can be dissociated in two independent components, which are measured in different behavioral paradigms (Luna et al., 2018, 2020; Sanchis et al., 2020). On the one hand, executive vigilance (EV) is defined as the ability to monitor and detect infrequent critical signals by executing a specific response. The EV decrement is usually observed in signal-detection tasks like the Mackworth Clock Test as a drop in hit rate on critical signals (Mackworth, 1948; Robertson et al., 1997; Epling et al., 2016). On the other hand, arousal vigilance (AV) is conceived as the capacity to sustain a fast reaction to environmental stimuli without implementing much control on the response. In single RT tasks like the Psychomotor Vigilance Test, the AV decrement is commonly observed as a progressive increase in mean and variability of RT as time-on-task progresses (Drummond et al., 2005; Lim and Dinges, 2008; Basner and Dinges, 2011). Changes in AV have been observed as changes in variability of RT in short inter-stimulus intervals (Steinborn and Langner, 2012) and have been associated with "out of the task" states, wherein increased variability of RT and reduced accuracy is observed (Esterman et al., 2013).

Although extensive research has examined changes in EEG oscillatory rhythms across vigilance tasks, several frequency bands have been associated with vigilance loss, showing relatively inconsistent evidence across studies (Boksem et al., 2005; Oken et al., 2006; Braboszcz and Delorme, 2011; Molina et al., 2019). According to the cortical oscillations model of sustained attention proposed by Clayton et al. (2015), the ability to maintain attention for long periods is regulated by the interaction between theta oscillations in frontomedial cortical regions and alpha and gamma oscillations in posterior areas. Evidence from experimental studies shows that vigilance loss is associated with changes in delta (Braboszcz and Delorme, 2011; Hoedlmoser et al., 2011; Groot et al., 2021), theta (Gorgoni et al., 2014; Molina et al., 2019), alpha (Dockree et al., 2004; Boksem et al., 2005; Clayton et al., 2019; Molina et al., 2019), beta (Braboszcz and Delorme, 2011; Ramautar et al., 2013; Molina et al., 2019; Pershin et al., 2023), and gamma (Kim et al., 2017; Pershin et al., 2023) rhythms. Nonetheless, despite the observed variety of findings, there seems to be a general consensus that states of vigilance loss are associated with increased alpha power (Sadaghiani et al., 2010; Clayton et al., 2018; Benwell et al., 2019; Karamacoska et al., 2019).

In studies measuring EV with signal-detection tasks, changes in different frequency power have been associated with vigilance loss. Boksem et al. (2005) found that the EV decrement in a signal-detection task was accompanied by an increase in theta, alpha, and beta power. In studies measuring frequency power at the trial level, Dockree et al. (2004) analyzed changes in alpha power while participants completed the Sustained Attention to Response Task by Robertson et al. (1997). The authors observed a decline in alpha power in some trials prior to the one wherein the infrequent critical signal was correctly detected, a mechanism that might be interpreted as a state of increased vigilance prior to target detection. Groot et al. (2021) reported that, during mind-wandering periods (i.e., when attention is directed to internal irrelevant thoughts and out of the external task), alpha but also delta and theta power were increased, while beta power was reduced. In this vein, Arnau et al. (2020) observed increased alpha power in inter-trial periods during mind-wandering states. However, other studies failed to observe changes in alpha power across prolonged signal-detection tasks (Kim et al., 2017; Reteig et al., 2019).

Similarly, in studies measuring AV loss with single RT tasks, changes in different frequency power have been observed. In Hoedlmoser et al. (2011), the AV decrement observed across

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Abbreviations: EV, executive vigilance; AV, arousal vigilance.

a night of sleep deprivation was accompanied by a progressive increase in delta and theta power. Theta power has been also positively correlated with RT in the Psychomotor Vigilance Test and subjective ratings of fatigue after 40 h of sleep deprivation (Gorgoni et al., 2014). In a study measuring changes in AV along several weeks, Witkowski et al. (2015) found that slower RT were accompanied by a decrease in alpha power at rest. However, when frequencies' power were analyzed as the average change measured in each trial after the target stimuli in the Psychomotor Vigilance Test, similar but also opposite findings were observed. In particular, in Molina et al. (2019), slower RT were associated with increased theta, alpha, and beta power.

While several studies have examined changes in oscillatory rhythms associated with vigilance loss across long periods, as time-on-task (Boksem et al., 2005; Kim et al., 2017; Pershin et al., 2023), sleep deprivation (Hoedlmoser et al., 2011; Gorgoni et al., 2014; Hao et al., 2022), or even weeks (Witkowski et al., 2015), evidence seems to be relatively scarce regarding whether changes in frequency power can anticipate vigilance failures (Dockree et al., 2004; Chua et al., 2012; Molina et al., 2019; Arnau et al., 2020). Moreover, noting that vigilance might be supported by two independent components, it could be possible that EV and AV failures are anticipated by changes in different oscillatory rhythms. Previous research has supported an empirical dissociation of EV and AV at the physiological and neural levels by simultaneously measuring vigilance components, under the same participant's attentional state (Feltmate et al., 2020; Luna et al., 2020; Sanchis et al., 2020). In particular, whereas the EV decrement has been reduced by exercise intensity (Sanchis et al., 2020) and anodal transcranial direct current stimulation over the right fronto-parietal network (Luna et al., 2020; Hemmerich et al., 2023), AV has been modulated by caffeine intake (Sanchis et al., 2020) and fatigue due to ∼6 h of extensive testing (Feltmate et al., 2020). Critically, different event-related potentials have been observed for vigilance components in the same task period; whereas EV loss was associated with changes in P3 and slow-positivity in centro-parietal regions, AV loss was associated with changes in N1 and P2 in occipital regions (Luna et al., 2023).

The aim of the present study was to examine whether failures in EV and AV are anticipated by changes in power in specific oscillatory rhythms. To this end, participants completed the Attentional Networks Test for Interactions and Vigilance—executive and arousal components (ANTI-Vea; Luna et al., 2018) while high-density EEG signal was recorded. The ANTI-Vea combines three subtasks that are simultaneously and randomly completed within a single session, in particular: (a) the ANTI task by Callejas et al. (2004), a flanker paradigm combined with visual and warning cues that is suitable to assess the main effects and interactions of classic attentional networks functions (i.e., phasic alertness, orienting, and executive control); (b) a signal-detection subtask similar to the Mackworth Clock Test, suitable for measuring the EV decrement; and (c) a RT subtask similar to the Psychomotor Vigilance Test, suitable to assess the AV decrement (Luna et al., 2018, 2021a,b). Importantly, to increase the number of vigilance trials as well as the statistical power of our study, participants completed the same procedure in two repeated sessions.

To examine whether changes in oscillatory rhythms anticipate vigilance failures, delta, theta, alpha, beta, and gamma power were computed at the trial level prior to target onset and separately for EV and AV components. For each vigilance component, frequency power changes for all bands were analyzed in different vigilance states as a function of the behavioral response, i.e., as optimal performance (hits in EV and fastest RT in AV) or failures in performance (misses in EV and slowest RT in AV). Importantly, in a recent study conducted in our lab, differences in EV and AV states were associated with changes in different event-related potentials: while P1 amplitude in central-parietal regions was larger in hits than misses for EV, N1, and P2 amplitude in occipital channels was smaller in fastest RT than slowest RT for AV (Luna et al., 2023). We expect the present study to contribute in characterizing whether different neural states, described as changes in power in specific oscillatory rhythms, may be useful to anticipate specific vigilance failures when performing prolonged tasks without breaks.

2. Materials and methods

2.1. Participants

Forty healthy adults (34 women; age: $M = 25.96$; $SD = 4.96$), who were undergraduate or graduate students from the Universidad Nacional de Córdoba, Argentina, volunteered to participate in the present study. All of them had normal or corrected to normal vision and none of them self-reported any neurological or psychiatric illness. Prior to participation, participants signed an informed consent approved by the local ethics committee. The study was conducted according to the ethical standards of the 1964 Declaration of Helsinki (last update: Seoul, 2008) and was positively evaluated by a local ethics committee (Comité Institucional de Ética de Investigaciones en Salud of the Hospital Nacional de Clínicas, CIEIS HNC, Universidad Nacional de Córdoba, Argentina).

Five participants were excluded from data analysis due to the following reasons: three participants did not complete the second EEG session, one participant had an extreme error percentage in the ANTI trials (3 *SD* above the group mean), and one participant had an overly high mean power (3 *SD* above the group mean) in four frequency bands. Therefore, the final sample size of the present study was $N = 35$.

Sample size was a-priori estimated based on previous studies with 40 participants per group in which the decrement in hits across six blocks measured with the ANTI-Vea showed an effect size of $\eta_p^2 = 0.05$ (Luna et al., 2018, 2021a). Using G*Power 3.1.9.4 (Faul et al., 2007), power analysis showed that considering $\alpha = 0.05$ and $1 - \beta = 0.90$, the minimum sample size required to observe a decrease in hits across blocks considering an effect size of $\eta_p^2 = 0.05$, two sessions, and seven blocks, was of 35 participants (i.e., as the final sample included in data analysis). Given that in the present study participants performed two experimental sessions of seven blocks, the statistical power of our study was much higher than in previous studies with a lower number of trials (i.e., one session of six blocks in Luna et al., 2018, 2021a; Baker et al., 2021).

2.2. Behavioral task: ANTI-Vea

The experimental task was designed and run with E-Prime 2.0 software (Psychology Software Tools, 2012). The ANTI-Vea comprises three embedded subtasks that are randomly completed across three separated types of trials: (a) ANTI (60%), a flanker paradigm combined with auditory and visual cues suitable to assess the main effects and interactions of the classic attentional networks functions (i.e., phasic alertness, orienting, and executive control); (b) EV (20%), a signal-detection subtask similar to the Mackworth Clock Test to assess the EV component; and (c) AV (20%), a RT subtask that mimics the Psychomotor Vigilance Test to assess the AV component. Importantly, as depicted in Figure 1, stimuli timing was the same for the three type of trials. Further details on the procedure, stimuli, and instructions of the ANTI-Vea can be reviewed in previous studies (Luna et al., 2018, 2021a,b). Importantly, the ANTI-Vea has shown acceptable split-half reliability scores for hits in EV and mean RT in AV (i.e., mean r corrected by the Spearman-Brown prophecy above 0.90) and at least similar split-half reliability for phasic alertness, orienting, and executive control as the ANT (MacLeod et al., 2010) and ANTI (Ishigami and Klein, 2010) tasks (Luna et al., 2021b).

Participants were encouraged to fix on the fixation cross at all times. As depicted in Figure 1A, ANTI trials followed the procedure of the ANTI task (Callejas et al., 2004). In these trials, participants had to select the direction (left/right) pointed by the central arrow of a five-arrow horizontal string—which could appear either above or below the fixation point—, ignoring the direction pointed by the surrounding flanking arrows (see Figure 1B). Importantly, ANTI trials serve a double purpose in the ANTI-Vea. On the one hand, the main effects and interactions of classic attentional functions were measured in ANTI trials as follows: (a) to assess executive control, the direction of the target and flanking arrows were congruent in half of these trials and incongruent in the other half; (b) to assess phasic alertness, a tone (i.e., warning signal) could anticipate the target in half of these trials, whereas no tone was presented in the other half; and (c) to assess attentional orienting, the target's position (i.e., above/below the fixation point) could be preceded either by a valid (i.e., the same location in 1/3 of ANTI trials), an invalid (i.e., the opposite location in 1/3 of ANTI trials) spatial visual cue, or by no cue at all in the remaining 1/3 of ANTI trials. On the other hand, ANTI trials were considered as the noise events for the embedded signal-detection subtask in the ANTI-Vea, as the infrequent critical signal (i.e., a large vertical displacement of the target) was not present in ANTI trials.

EV trials had the same procedure than ANTI trials, except that the target was largely displaced (i.e., 8 pixels –px–) from its central position in the five-arrow horizontal string either upwards or downwards (see Figure 1A). To perform EV trials, participants were instructed to continuously monitor and detect the infrequent vertical displacement of the target (i.e., the critical signal of the embedded signal-detection subtask of the ANTI-Vea) by pressing the space bar key, ignoring in these cases the direction the target pointed to (see Figure 1B). Note that, if participants pressed the space bar key in the ANTI trials (i.e., the noise events of the signal-detection subtask), the response was categorized as a false alarm. Lastly, AV trials had the same timing than the ANTI and EV ones but, importantly, no warning signal nor visual cue was presented (i.e., the fixation point remained on the screen) and the target of the ANTI/EV trials was replaced by a millisecond counter (see Figure 1A). In AV trials, participants had to stop the millisecond counter as fast as possible by pressing any key from keyboard (see Figure 1B).

2.3. EEG data acquisition and pre-processing

A high density 128 channels BioSemi ActiveTwo system, controlled with the ActiView software (BioSemi, Amsterdam), was used for EEG signal recording. Electrical reference channels were set with two flat-type active-electrodes placed over the right and left mastoids. Signal was registered with a sampling rate of 1,024 Hz and keeping electrode's impedance below 1 Ohm.

EEG data pre-processing was conducted using EEGLAB v2020.0 toolbox (Delorme and Makeig, 2004) on MATLAB R2016a (The MathWorks, Inc.). Raw data format was converted to the EEGLAB format and resampled at 512 Hz. The signal was filtered between 0.5 and 45 Hz and then decomposed into 128 components (i.e., the same number of channels) using Independent Component Analysis. Using ADJUST v.1.1.1 (Mognon et al., 2011), artifactual components were automatically classified as horizontal eye movements ($M = 3.36$; $SD = 2.50$), vertical eye movements ($M = 6.96$; $SD = 5.46$), blinks ($M = 3.99$; $SD = 3.29$), and generic discontinuities ($M = 14.71$; $SD = 8.59$), and were removed from signal.

Data was epoched in 3,400 ms periods from the initial fixation point onset in each trial. Epochs were visually inspected to interpolate artifactual channels or to reject the entire epoch if it was too noisy (trials rejected by participant in the collapsed two sessions: $M = 10.30$; $SD = 17.46$). Finally, following Clayton et al. (2015), signal was filtered in five frequency bands as follows: delta (1–4 Hz), theta (4–8 Hz), alpha (8–14 Hz), beta (14–30 Hz), and gamma (30–45 Hz). Frequency power was calculated as the squared signal in each frequency band.

2.4. Procedure and design

Prior to the experimental sessions in the lab, participants performed a practice session outside the lab to familiarize themselves with the task by completing the online ANTI-Vea (https://www.ugr.es/~neurocog/ANTI/; Luna et al., 2021b). Then, aiming at increasing the number of vigilance trials for EEG analysis, each participant completed two experimental sessions at the lab (average time between sessions: $M = 11.94$ days; $SD = 16.10$). At the lab, participants received the standard instructions to complete each type of trial and performed one practice block of 40 (24 ANTI, 8 EV, and 8 AV) randomly presented trials, without visual feedback. Each experimental session comprised seven experimental blocks without any pause or visual feedback, consisting in 80 (48 ANTI, 16 EV, and 16 AV) randomly presented trials within each block. The ANTI trials had the following design: Warning signal

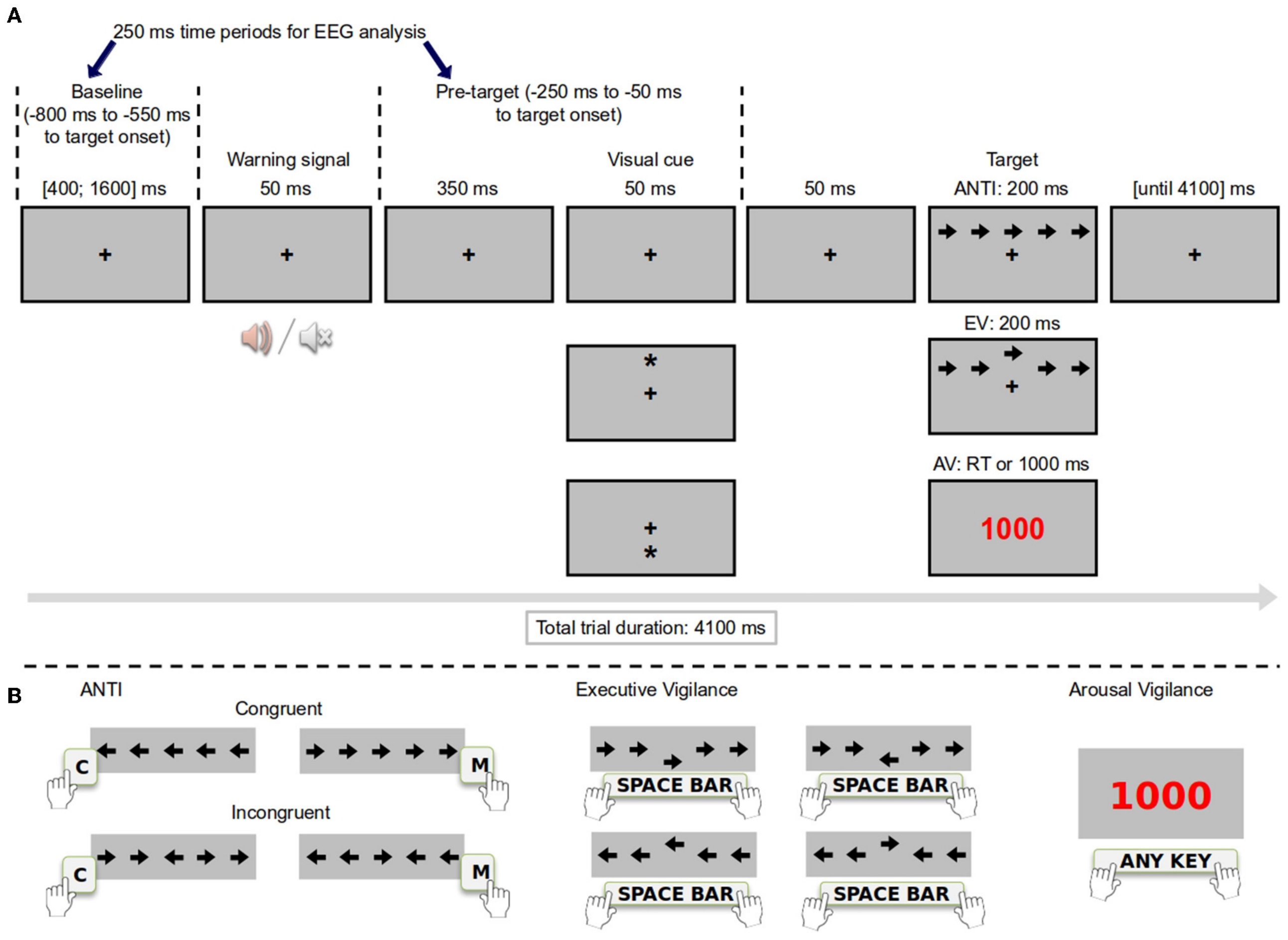

FIGURE 1
Experimental procedure and timing of ANTI-Vea trials. **(A)** Stimuli and timing for ANTI-Vea trials. Warning signal and visual cue were only present in ANTI and EV trials, although only counterbalanced in ANTI trials. **(B)** Correct responses in examples of the three type of trials of the ANTI-Vea. Responses were allowed since target onset until 2,000 ms in each type of trial.

(no tone/tone) × Visual cue (invalid/no cue/valid) × Congruency (congruent/incongruent). The 16 executive vigilance trials per block were randomly selected from any possible combination of the ANTI trials design.

2.5. Data analysis

Data analyses were conducted using RStudio 2022.07.1 Build 554 (RStudio Team, 2022) in R 4.1.2 (R Core Team, 2021). Aiming at increasing the number of EV/AV trials, both behavioral and EEG data was collapsed across sessions. Analyses of variance (ANOVA) were conducted with the afex package (Singmann et al., 2021) and planned contrasts were performed with the emmeans package (Lenth, 2021). Partial eta-squared (η_p^2) are reported as measure of the ANOVAs' effect size and Cohens' d as a measure of t-tests' effect size (Kelley and Preacher, 2012). All effect sizes are reported with the 95% confidence intervals around them (Cumming, 2014). In those ANOVAs wherein the sphericity assumption was violated (i.e., Mauchly's test $p < 0.05$), degrees of freedom are reported with Greenhouse-Geisser correction.

Behavioral data was plotted with Matplotlib (Hunter, 2007). EEG data figures were performed using ggplot2 (Wickham, 2016), purrr (Henry and Wickham, 2020), Rmisc (Hope, 2013), and magrittr (Bache and Wickham, 2020) packages in RStudio for time-frequency plots and MNE-Python code (Gramfort et al., 2013) for topoplots.

2.5.1. Behavioral data

Given that the main aim of the present study was to analyze vigilance failures, behavioral data analyses were focused on EV and AV components measured with the ANTI-Vea. Data analyses on the main effects and interactions of the classic attentional functions measured in the ANTI trials were analyzed and reported in a separated study aiming to analyze event-related potentials associated with attentional networks functions (Luna et al., 2023).

First, to analyze the EV and AV decrements across time-on-task, data was averaged as a function of blocks of trials. For EV trials, data were collapsed across warning signal, visual cue, and congruency conditions, following standard analysis of the ANTI-Vea (Luna et al., 2021a). The EV decrement was analyzed with a repeated-measures ANOVA, with hits rate as dependent variable and blocks (seven levels) as a within-participant factor. The AV decrement was analyzed with a repeated-measures ANOVA, with mean RT in AV trials as dependent variable and blocks (seven levels) as a within-participant factor. To determine the significance of a linear EV/AV decrement, planned comparisons of the polynomial linear component were performed.

Then, and more importantly for the goal of this study, to describe EV and AV failures, overall responses in EV and AV trials were categorized as follows. For EV trials, whereas hits (i.e., correct responses) described an adequate EV performance, misses (i.e., incorrect responses) were considered as EV failures. Overall AV performance was analyzed following standard scores usually computed in the Psychomotor Vigilance Test (Basner and Dinges, 2011). In particular, for AV trials, whereas fastest RT (i.e., first quintile of RT responses) described an adequate AV performance, slowest RT (fifth quintile of RT responses) were considered as AV failures. Trials with RT equal to 0 ms (i.e., "no responses") were removed from AV data analyses (0.38% of AV trials).

2.5.2. EEG data

To analyze whether changes in oscillatory rhythms might anticipate vigilance failures, power in frequency bands was analyzed in the 800 ms preceding the EV or AV target onset at the trial level. For each frequency band, mean power was calculated at two time periods of 250 ms in each trial (see Figure 1A): (a) baseline (−800 to −550 ms prior to target onset) and (b) pre-target (−300 to −50 ms prior to target onset). Power in frequency bands was computed in EV trials as a function of hits/misses and in AV trials as a function of fastest/slowest RT.

For EV analyses, given that the difference in trial count between hits and misses might biases the outcomes, we performed a randomized selection of EV trials to equate the number of hits and misses by participant (Cohen, 2014). Noting that hits decrease with time-on-task (see below section 3.1), following Reteig et al. (2019), first hits/misses trials were randomly selected in two task periods: between blocks 1 and 4 and between blocks 5 and 7. The randomized selection of trials was performed only for the largest subset of trials (i.e., hits or misses) in each task period to equate the smallest subset of trials and was repeated 1,000 times. Each subset of randomly selected trials was averaged and, after that, all 1,000 subsampled trials-averaged were averaged together. Finally, data was collapsed between task periods (i.e., blocks 1–4 and blocks 5–7) and averaged as a function of hits/misses.

Next, to determine whether the change in frequency power prior to target onset anticipates EV/AV performance, we computed a normalized change as a function of hits/misses for EV trials and as a function of fastest/slowest responses for AV trials. The normalized change was separately computed for delta, theta, alpha, beta, and gamma power as follows:

$$normalized\ change = \frac{pre-target\ mean\ power - baseline\ mean\ power}{baseline\ mean\ power}$$

Mean power in baseline and the normalized power change in the pre-target period of each frequency band were computed in nine a-priori defined channels across the whole brain, in particular: O1, Oz, O2, P3, Pz, P4, F3, Fz, F4. Then, analyses were separately conducted for EV/AV performance, by analyzing each frequency band in separated repeated-measures ANOVAs for baseline (with mean power as dependent variable) and pre-target (with normalized change power as dependent variable) periods, with channels of interest (nine levels) and performance (hits/misses for EV or fastest/slowest RT for AV) as within-participant factors. To determine whether the normalized power change in a specific frequency band was significantly different as a function of performance in a singular or multiple channels of interest, paired-wise comparisons were conducted.

3. Results

3.1. Behavioral performance

As reported in Luna et al. (2023) and similarly to previous studies with the ANTI-Vea (Luna et al., 2018, 2021a,b), the EV decrement was observed as a significant change in hits across time-on-task [$F_{(4.55,154.58)} = 9.70$, $p < 0.001$, $\eta_p^2 = 0.22$, $\text{CI}_{95\%}$ (0.11, 0.30); see Figure 2A]. Polynomial contrasts demonstrated a significant and negative linear component for hits across blocks [$t_{(34)} = -5.23$, $p < 0.001$, $\eta_p^2 = 0.45$, $\text{CI}_{95\%}$ (0.24, 1.00)]. Unexpectedly, however, the AV decrement was not observed in the present study and Luna et al. (2023) (see Figure 2B): the change of mean RT across blocks was not significant [$F_{(3.93,133.78)} = 0.46$, $p = 0.764$, $\eta_p^2 = 0.01$, $\text{CI}_{95\%}(0.00, 0.03)$].

A summary of EV and AV overall performance is presented in Table 1.

3.2. Frequency power

For the sake of clarity, the report of each ANOVA is focused on the interaction between channels and performance, which examines whether differences in power in channels of interest was modulated by vigilance performance. Main effects of channels (i.e., whether frequency power was different among channels of interest) and performance (i.e., whether frequency power was different as a function of the EV/AV response) are summarized in Tables 3, 5.

3.2.1. Executive vigilance

In the baseline period, mean frequency power in channels of interest was not significantly modulated by performance (i.e., hit or miss) for delta, theta, alpha, beta, or gamma band (see Table 2). Importantly, as depicted in Figure 3A and detailed in Table 2, in the pre-target period, the normalized change from baseline in channels of interest was significantly modulated by performance only for alpha power, but not for delta, theta, beta, or gamma band.

Pairwise comparisons in the pre-target period for alpha band determined that the normalized power change between trials with hits and misses was significantly different only in O1 [$t_{(34)} = -3.09$, $p = 0.004$, Cohen's $d = 0.53$, $\text{CI}_{95\%}$ (0.17, 0.88)], but not in the other channels of interest (all $ps > 0.110$). As can be observed in Figure 3B, the normalized change of alpha power was smaller in trials with hits than in trials with misses in the pre-target period.

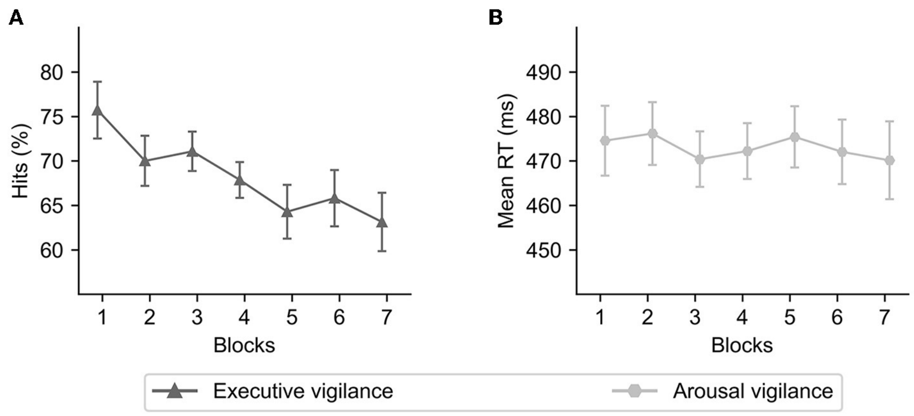

FIGURE 2
Executive **(A)** and arousal **(B)** vigilance performance as a function of time-on-task. Error bars represent 95% confidence intervals and were computed with the Cousineau-Morey method (Morey, 2008).

TABLE 1 Descriptive statistics of executive and arousal vigilance overall performance.

Component	Performance	*N*	Min	Max	*M*	95% CI
EV	Hits	164.11	68	212	73.26%	[67.11, 79.41]
	Misses	59.89	12	156	26.74%	[20.59, 32.89]
AV	Fastest RT	45	42	45	385 ms	[361, 409]
	Slowest RT	44	41	44	596 ms	[556, 636]

EV, executive vigilance; AV, arousal vigilance; N, absolute frequency per participant; Min, minimum; Max, maximum; M, mean; CI, confidence intervals; RT, reaction time; ms, milliseconds. N represents the mean number of trials in which that response was observed, with its respective Min and Max across participants. M represents the mean performance in that score, with its respective variability (i.e., 95% CI around the mean).

TABLE 2 Statistics of the interaction between channel and performance (i.e., hits vs. misses) factors analyzed in repeated-measures ANOVAs for executive vigilance.

Frequency	Period	*F*	df	*p*	η_p^2[95% CI]
Delta	Baseline	0.95	2.28, 77.42	0.399	0.03 [0.00, 0.05]
	Pre-target	1.02	2.88, 98.03	0.386	0.03 [0.00, 0.05]
Theta	Baseline	1.60	3.14, 106.91	0.192	0.04 [0.00, 0.08]
	Pre-target	0.60	3.18, 108.08	0.626	0.02 [0.00, 0.03]
Alpha	Baseline	2.67	2.29, 77.90	0.068	0.07 [0.01, 0.12]
	Pre-target	**3.73**	**4.84, 164.67**	**0.004**	**0.10 [0.02, 0.15]**
Beta	Baseline	0.89	4.57, 155.26	0.483	0.03 [0.00, 0.04]
	Pre-target	0.61	3.40, 115.77	0.628	0.02 [0.00, 0.03]
Gamma	Baseline	1.51	2.04, 69.48	0.227	0.04 [0.00, 0.07]
	Pre-target	0.45	5.28, 179.57	0.822	0.01 [0.00, 0.02]

df, degrees of freedom; CI, confidence intervals.
Significant outcomes are highlighted in bold.

As can be observed in Table 3, mean power in the baseline period and the normalized change in the pre-target period were not significantly different as a function of EV performance for delta, theta, alpha, and gamma bands. However, in the analyses for beta band, in the baseline period mean power was not significantly different between trials with hits and misses, but there was a significant difference in the pre-target period as a function of EV performance (see Table 3). In the pre-target period, beta power decreased from baseline in trials with hits (normalized change $M = -0.05$, 95% CI$[-0.07, -0.03]$) but not in trials with misses (normalized change $M = 0.00$, $[-0.04, 0.04]$).

3.2.2. Arousal vigilance

Similarly to analysis for EV, in the baseline period, mean frequency power in channels of interest was not modulated by performance (i.e., fastest or slowest RT) for delta, theta, alpha,

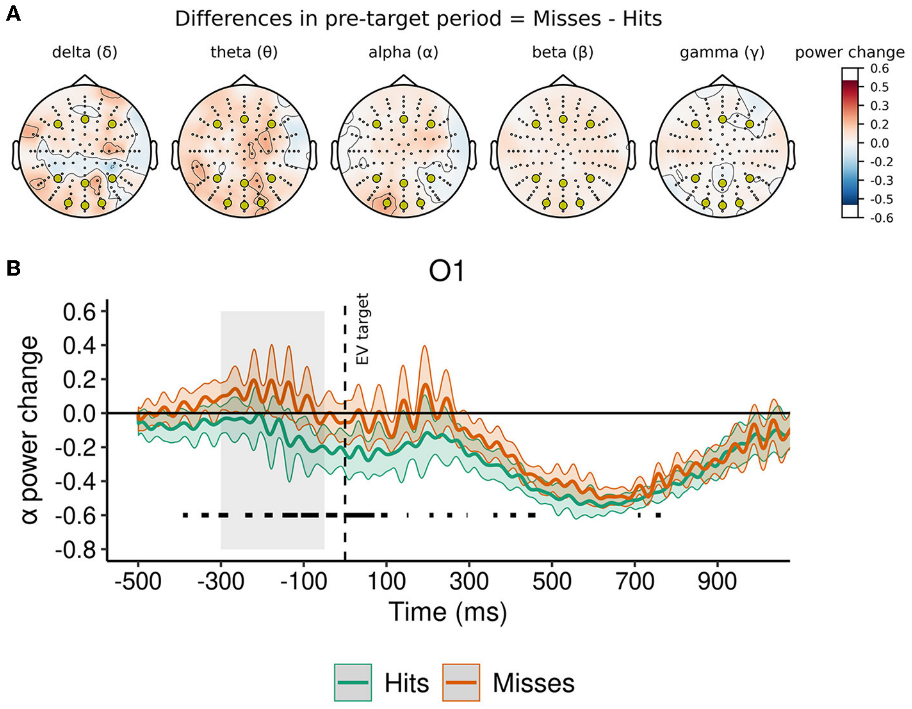

FIGURE 3
Normalized change in power of oscillatory rhythms in pre-target period as a function of executive vigilance performance. **(A)** Topoplots represent the difference between conditions in mean power change from baseline in the pre-target period for each frequency band. In all topoplots, a-priori channels of interest (O1, Oz, O2, P3, Pz, P4, F3, Fz, F4) are represented with yellow markers. **(B)** Alpha power change from baseline in O1. Significant differences ($p < 0.05$) between conditions at each time point are highlighted with a black line above the x-axis. The gray area prior to target onset (i.e., between −350 and −50 ms) represents the pre-target period. Shadowed traces around mean signal represent within-participant 95% CI of mean for that condition and were computed with the Cousineau-Morey method (Morey, 2008).

beta, and gamma band (see Table 4). As observed in Figure 4A and detailed in Table 4, in the pre-target period, the normalized change from baseline in a-priori channels of interest was not significantly modulated by performance in delta, theta, alpha, beta, and gamma band.

Interestingly, noting the differences observed in some regions in the high-density topoplot of delta band in Figure 4A, exploratory repeated-measures ANOVAs on these oscillatory rhythms were conducted. Exploratory analysis included as a tenth region of interest a set of five frontal-central channels (Cz, Fcz, and three adjacent channels) where clear differences were observed in a-priori analysis. In channels included in exploratory analysis, performance did not significantly modulate mean power in the baseline period [$F_{(1.05,35.85)} = 0.09$, $p = 0.776$, $\eta_p^2 = < 0.01$, $\text{CI}_{95\%}$ (< 0.01, < 0.01)], nor the normalized change from baseline in the pre-target period [$F_{(6.35,215.91)} = 1.83$, $p = 0.090$, $\eta_p^2 = 0.05$, $\text{CI}_{95\%}$ (0.00, 0.10)]. Following the differences observed in high-density topoplot (see green channels in Figure 4A), *post-hoc* comparisons showed that, in the pre-target period, the normalized delta power change was significantly smaller in trials with fastest RT than in trials with slowest RT only in exploratory frontal-central channels [$t_{(34)} = -2.12$, $p = 0.041$, Cohen's $d = 0.36$, $\text{CI}_{95\%}$ (0.01, 0.71)] (see Figure 4B), but not in the other a-priori channels of interest (all *ps* > 0.090).

As can be observed in Table 5, mean power in the baseline period and the normalized change in the pre-target period were not significantly different in any frequency band as a function of AV performance.

4. Discussion

The present study aimed at examining whether failures in two dissociated vigilance components, i.e., EV and AV, are anticipated by changes in power of different oscillatory rhythms. Although previous research has extensively examined changes in oscillatory rhythms associated with states of vigilance loss (Boksem et al., 2005; Oken et al., 2006; Braboszcz and Delorme, 2011; Molina et al., 2019), evidence reported so far seems to be relatively inconsistent about whether changes in specific frequency bands might serve as indicators of failures in different vigilance components (Boksem et al., 2005; Chua et al., 2012; Kim et al., 2017; Molina et al., 2019; Reteig et al., 2019; Groot et al., 2021). A critical limitation to identify whether different brain states are independently associated with EV or AV might rely in the difficulty to simultaneously assess vigilance components, thus observing failures in EV and AV under the same participants' attentional state. To overcome this limitation, in the present study, changes in power of several frequency bands were analyzed while participants completed the ANTI-Vea, a behavioral task that simultaneously measures EV and AV along with others classic attentional components in a single session (Luna et al., 2018, 2021a,b). Importantly, failures in vigilance components were

TABLE 3 Statistics of channel and performance (i.e., hits vs. misses) factors analyzed in repeated-measures ANOVAs for executive vigilance.

Frequency	Period	Factor	*F*	df	*p*	η_p^2[95% CI]
Delta	Baseline	Channel	3.48	1.15, 39.03	0.064	0.09 [0.02, 0.14]
		Performance	0.01	1, 34	0.916	<0.01 [0.00, 0.07]
	Pre-target	Channel	**5.19**	**3.53, 119.99**	**0.001**	**0.13 [0.05, 0.19]**
		Performance	0.88	1, 34	0.354	0.03 [0.00, 0.20]
Theta	Baseline	Channel	**6.75**	**3.51, 119.46**	**<0.001**	**0.17 [0.07, 0.23]**
		Performance	2.36	1, 34	0.134	0.06 [0.00, 0.27]
	Pre-target	Channel	**6.85**	**4.33, 147.20**	**<0.001**	**0.17 [0.08, 0.23]**
		Performance	1.44	1, 34	0.238	0.04 [0.00, 0.23]
Alpha	Baseline	Channel	**6.94**	**3.37, 114.72**	**<0.001**	**0.17 [0.08, 0.23]**
		Performance	0.75	1, 34	0.392	0.02 [0.00, 0.19]
	Pre-target	Channel	**3.40**	**4.80, 163.20**	**0.007**	**0.09 [0.02, 0.14]**
		Performance	1.53	1, 34	0.225	0.04 [0.00, 0.23]
Beta	Baseline	Channel	**11.23**	**4.27, 145.09**	**<0.001**	**0.25 [0.15, 0.32]**
		Performance	0.14	1, 34	0.707	<0.01 [0.00, 0.13]
	Pre-target	Channel	1.11	5.08, 172.80	0.358	0.03 [0.00, 0.05]
		Performance	**5.59**	**1, 34**	**0.024**	**0.14 [0.00, 0.36]**
Gamma	Baseline	Channel	**7.24**	**2.40, 81.63**	**<0.001**	**0.18 [0.08, 0.24]**
		Performance	0.66	1, 34	0.422	0.02 [0.00, 0.18]
	Pre-target	Channel	1.38	5.48, 186.27	0.227	0.04 [0.00, 0.07]
		Performance	0.08	1, 34	0.778	<0.01 [0.00, 0.12]

df, degrees of freedom; CI, confidence intervals.
Significant outcomes are highlighted in bold.

TABLE 4 Statistics of the interaction between channel and performance (i.e., hits vs. misses) factors analyzed in repeated-measures ANOVAs for arousal vigilance.

Frequency	Period	*F*	df	*p*	η_p^2[95% CI]
Delta	Baseline	0.09	1.05, 35.69	0.773	0.01 [<0.01, <0.01]
	Pre-target	1.49	5.67, 192.75	0.187	0.04 [0.00, 0.07]
Theta	Baseline	0.56	2.52, 85.71	0.612	0.02 [0.00, 0.02]
	Pre-target	1.43	4.43, 150.70	0.222	0.04 [0.00, 0.07]
Alpha	Baseline	0.87	3.74, 127.01	0.476	0.03 [0.00, 0.04]
	Pre-target	0.79	4.35, 147.73	0.542	0.02 [0.00, 0.04]
Beta	Baseline	0.39	3.91, 133.10	0.814	0.01 [0.00, 0.01]
	Pre-target	0.73	5.07, 172.54	0.602	0.02 [0.00, 0.03]
Gamma	Baseline	0.90	3.17, 107.76	0.447	0.03 [0.00, 0.04]
	Pre-target	0.82	5.72, 194.32	0.548	0.02 [0.00, 0.04]

df, degrees of freedom; CI, confidence intervals.

anticipated by changes in different oscillatory rhythms. Whereas, increased alpha power in left occipital areas and beta power (independently of the brain region) anticipated misses in EV, increased delta power in frontal-central regions was observed prior to slowest responses in AV.

The cortical oscillations model of sustained attention posits that alpha oscillations play an important role in task-irrelevant cortical areas (as, for instance, auditory cortices during a visual task) aiding to sustain attention by suppressing distracting information from the environment (Clayton et al., 2015). However, when alpha oscillations are present in task-relevant areas (as in visual cortex when performing a visual signal-detection task), alpha oscillations may impair attentional focus (Clayton et al., 2015). In the present study, changes in alpha oscillations were specifically observed in task-relevant areas, i.e., left occipital regions (O1). As predicted by Clayton et al. (2015), when performing prolonged tasks without

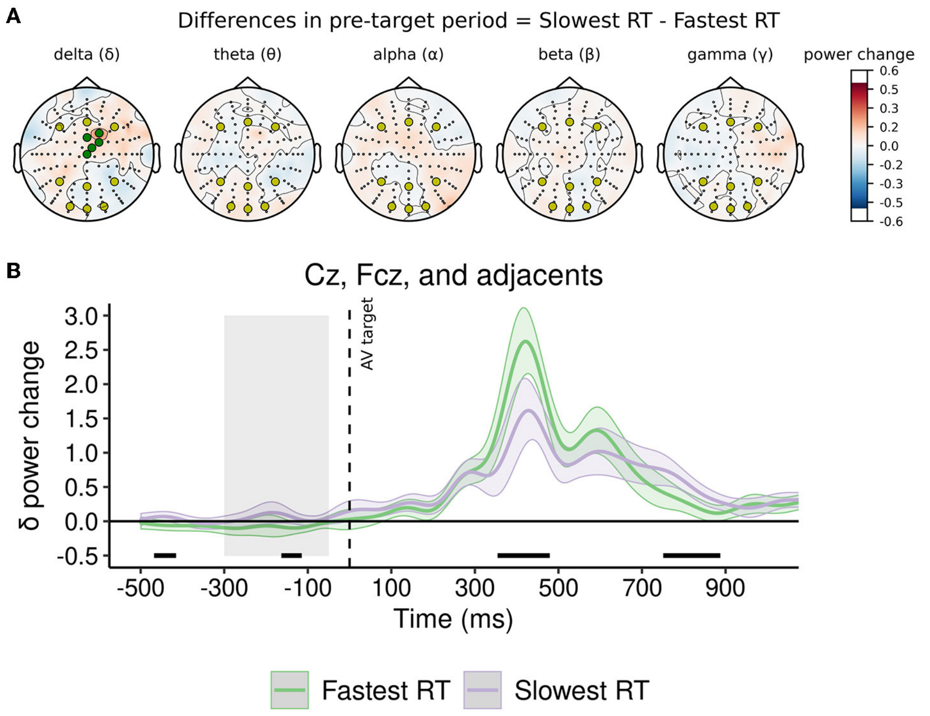

FIGURE 4
Normalized change in power of oscillatory rhythms in pre-target period as a function of arousal vigilance performance. **(A)** Topoplots represent the difference between conditions in mean power change from baseline in the pre-target period for each frequency band. In all topoplots, a-priori channels of interest (O1, Oz, O2, P3, Pz, P4, F3, Fz, F4) are represented with yellow markers. In topoplot for delta, exploratory channels of interest (Fcz, Cz, and adjacent channels) are represented with green markers. **(B)** Delta power change from baseline in frontal-central channels. Significant differences ($p < 0.05$) between conditions at each time point are highlighted with a black line above the x-axis. The gray area prior to target onset (i.e., between −350 and −50 ms) represents the pre-target period. Shadowed traces around mean signal represent within-participant 95% CI of mean for that condition and were computed with the Cousineau-Morey method (Morey, 2008).

breaks, increased alpha power in task-relevant areas as the occipital cortex in a visual signal-detection task should hinder vigilance performance, leading to vigilance failures due to the loss of attentional focus. Critically, as proposed by the cortical oscillations model of sustained attention (Clayton et al., 2015), in the present study increased alpha power in left occipital regions was observed prior to target onset in trials with failures in EV (i.e., misses on the critical signal), impairing attentional focus in the ongoing EV subtask. In line with the model proposed by Clayton et al. (2015) and the present results, previous research has found that stabilizing alpha oscillations at 10 Hz through transcranial alternating current stimulation over occipital regions prevents the EV decrement in visual detection tasks (Clayton et al., 2019), thus further supporting the prediction that increased alpha oscillations in occipital regions might lead to states of reduced EV. Importantly, the present outcomes also fit well with previous research in which increased alpha power was associated with states of EV loss (Dockree et al., 2004; Boksem et al., 2005; Arnau et al., 2020; Groot et al., 2021).

Interestingly, similar to the pattern observed in alpha rhythms, increased beta power prior to target onset was observed in trials with misses in the EV subtask. However, in contrast to alpha rhythms, beta power changes seem to be region–independent, as the observed effect was not modulated by channels of interest. The observed results in beta rhythms seem to be in contrast with some previous research in which reduced (rather than increased, as in the present study) beta power was associated with states of vigilance loss (Braboszcz and Delorme, 2011; Pershin et al., 2023). Note that beta rhythms seem to not play a critical role in the cortical oscillations model of sustained attention (Clayton et al., 2015). Taking all this into account, we consider that the observed outcomes in beta rhythms should be taken with caution and, most importantly, further explored in future research. Some indices combining beta with alpha and also theta rhythms have been proposed to account for changes in vigilance in prolonged periods (Kamzanova et al., 2014; Coelli et al., 2018). Future studies may further address whether changes in beta rhythms associated with EV performance in the ANTI-Vea are rather independent of the cortical region and/or if changes in beta power are related with other frequency bands, as alpha and theta.

Regarding failures in AV, only increased delta power in frontal-central regions was observed prior to AV target onset in trials with slowest RT. Note that the cortical oscillations model of sustained attention seems to be specifically developed for vigilance in visual detection tasks (Clayton et al., 2015), in which the EV component is mainly involved. Indeed, the model proposed by Clayton et al. (2015) does not predict changes in delta rhythms associated with sustained attention. Importantly, while in the present study changes in AV were measured in a single and daytime session, note that previous research has mainly examined changes in oscillatory rhythms associated with AV loss in sleep deprivation periods

TABLE 5 Statistics of channel and performance (i.e., fastest RT vs. slowest RT) factors analyzed in repeated-measures ANOVAs for arousal vigilance.

Frequency	Period	Factor	F	df	p	η_p^2[95% CI]
Delta	Baseline	Channel	**5.89**	**1.31, 44.69**	**0.013**	**0.15 [0.06, 0.21]**
		Performance	0.01	1, 34	0.926	<0.01 [0.00, 0.06]
	Pre-target	Channel	0.49	5.66, 192.50	0.808	0.01 [0.00, 0.02]
		Performance	0.16	1, 34	0.687	<0.01 [0.00, 0.14]
Theta	Baseline	Channel	**8.33**	**3.42, 116.30**	**<0.001**	**0.20 [0.10, 0.26]**
		Performance	1.52	1, 34	0.226	0.04 [0.00, 0.23]
	Pre-target	Channel	0.27	4.44, 150.80	0.911	<0.01 [<0.01, <0.01]
		Performance	0.05	1, 34	0.820	<0.01 [0.00, 0.10]
Alpha	Baseline	Channel	**7.07**	**3.09, 105.15**	**<0.001**	**0.17 [0.08, 0.24]**
		Performance	1.65	1, 34	0.207	0.05 [0.08, 0.24]
	Pre-target	Channel	1.07	4.43, 150.51	0.378	0.03 [0.00, 0.06]
		Performance	0.40	1, 34	0.533	0.01 [0.00, 0.16]
Beta	Baseline	Channel	**10.54**	**4.33, 147.12**	**<0.001**	**0.24 [0.14, 0.31]**
		Performance	2.93	1, 34	0.096	0.08 [0.00, 0.29]
	Pre-target	Channel	1.68	4.98, 169.28	0.143	0.05 [0.00, 0.08]
		Performance	0.02	1, 34	0.897	<0.01 [0.00, 0.08]
Gamma	Baseline	Channel	**7.05**	**2.74, 93.00**	**<0.001**	**0.17 [0.08, 0.24]**
		Performance	1.79	1, 34	0.189	0.05 [0.00, 0.24]
	Pre-target	Channel	0.21	5.08, 172.69	0.958	<0.01 [<0.01, <0.01]
		Performance	0.01	1, 34	0.943	<0.01 [0.00, 0.05]

df, degrees of freedom; CI, confidence intervals.
Significant outcomes are highlighted in bold.

(Hoedlmoser et al., 2011; Chua et al., 2012; Gorgoni et al., 2014) or in several sessions across weeks (Witkowski et al., 2015). Moreover, it is important to note that some previous studies analyzing changes in frequency bands in single RT tasks have not analyzed delta power (Witkowski et al., 2015; Molina et al., 2019). However, in studies measuring AV with single RT tasks in which delta power was analyzed, increased delta power was associated with vigilance loss states (Hoedlmoser et al., 2011; Chua et al., 2012; Gorgoni et al., 2014). Whereas Hoedlmoser et al. (2011) and Gorgoni et al. (2014) observed that AV loss during the course of sleep deprivation was accompanied by increased delta –but also others frequency bands– power, Chua et al. (2012) found that delta power in frontal-central regions (i.e., Fz) was positively correlated with lapses (i.e., responses slower than 500 ms in the Psychomotor Vigilance Test) during sleep deprivation. Therefore, the present outcomes seem to fit well with those observed in Chua et al. (2012), supporting that increased delta power in frontal-central regions might be identified as a neural state associated with AV loss states at the trial level.

The present outcomes further support an empirical dissociation at the neural level between EV and AV as two independent vigilance components. There is a long-standing discussion concerning whether vigilance is better understood as a set of independent brain mechanisms rather than as a single component (Sarter et al., 2001; Sturm and Willmes, 2001; Oken et al., 2006; van Schie et al., 2021). According to some conceptual and theoretical reviews, the concept of vigilance has been used to describe different mechanisms associated with alertness and sustained attention (Oken et al., 2006; van Schie et al., 2021). Following, Oken et al. (2006), vigilance can define either: (a) the ability to maintain attention in prolonged periods to perform the task at hand, (b) a hypervigilance state to increase alertness for detecting potential threats from the environment and avoid dangers, or (c) the arousal levels of attention, which oscillate during the sleep-wake cycle. Although it has been proposed that the arousal levels of attention would not be associated with a particular behavioral response (Sarter et al., 2001; Oken et al., 2006), empirical research has demonstrated that changes in arousal and readiness can be observed by measuring AV through simple RT tasks, like the Psychomotor Vigilance Test (Lim and Dinges, 2008; Basner and Dinges, 2011). In particular, the slowness in readiness has been associated to AV loss in prolonged RT tasks, which seems to be related with mental fatigue although unaffected by temporal preparation after warning signals presented at different foreperiods (Langner et al., 2010).

Previously, McIntire et al. (2014) observed some dissociable effects of transcranial direct current stimulation on vigilance components, although EV and AV were measured at different moments of the session through different behavioral tasks. During a night of sleep deprivation, it was observed that while stimulation reduced the drop in hits in the Mackworth Clock Test for EV, stimulation did not affect mean RT in the Psychomotor Vigilance Test for AV (McIntire et al., 2014). In the last years, by simultaneously measuring vigilance components in a single session under the same participants' state, the proposed conceptual dissociation between EV and AV has been supported by empirical evidence at the behavioral (Luna et al., 2022a,b), physiological (Feltmate et al., 2020; Sanchis et al., 2020), and neural (Luna et al., 2020, 2023; Hemmerich et al., 2023) levels. In particular, at the behavioral level, the EV but not the AV decrement was modulated by the cognitive load of the task at hand (Luna et al., 2022a) and was associated with the decrement in cognitive control across time-on-task (Luna et al., 2022b). At the physiological level, while only the AV decrement was mitigated by caffeine intake, exercise intensity modulated only EV performance (Sanchis et al., 2020). Moreover, Feltmate et al. (2020) observed that after performing ∼6 h of cognitive tasks, fatigue particularly impaired AV but not EV. Finally, at the neural level, anodal transcranial direct current stimulation over the right fronto-parietal network mitigated the EV but not the AV decrement (Luna et al., 2020; Hemmerich et al., 2023) and different event-related potentials have been associated with changes in EV and AV (Luna et al., 2023). In the present study, a novel dissociation between EV and AV at the neural level was observed: whereas increased alpha power in left occipital regions anticipates failures in EV, increased delta power in frontal-central regions anticipates failures in AV.

Lastly, it is important to note that the present study is not exempt of some limitations. Unexpectedly, the typical AV decrement, usually observed as an increase in mean RT across blocks, was not found in our data. A similar outcome was observed in a previous study conducted with the ANTI-Vea, in which participants completed the task while receiving anodal/sham transcranial direct current stimulation and EEG signal was recorded (Luna et al., 2020). Indeed, in the present study, the AV decrement was also not observed in *SD* of RT across blocks [$F_{(3.81,129.41)} = 0.58$, $p = 0.667$, $\eta_p^2 = 0.02$, CI95%(0.00, 0.03)]. Nonetheless, given that the aim of the present study was to analyze brain states at the trial level associated with states of vigilance loss, analyses on AV were focused on other classic scores usually computed in single RT tasks, i.e., as fastest and slowest RT (Basner and Dinges, 2011). In this vein, previous research has associated different brain states described by changes in BOLD signal (Drummond et al., 2005) and theta, alpha, and beta power (Molina et al., 2019) with the speed of responses in the Psychomotor Vigilance Test. Fastest and slowest RT are two of the most used scores in the Psychomotor Vigilance Test (Basner and Dinges, 2011). Indeed, a typical score to assess AV changes in RT tasks is the lapses' rate, a score that averages responses ≥500 ms in the Psychomotor Vigilance Test (Basner and Dinges, 2011) or ≥600 ms in the ANTI-Vea (Luna et al., 2018, 2021a,b). Note that these thresholds are relatively similar to the mean of slowest RT observed in the present study (see Table 1). To assess differences in AV states by a similar number of trials, we decided to compare changes in frequencies' power as a function of trials with slowest vs. fastest RT. Therefore, despite the AV decrement was not observed in the present study, we consider that fastest and slowest RT are appropriate scores for describing AV states.

Finally, it should be acknowledged that changes in delta power associated with AV states were not observed in a-priori analyses, as no significant differences in frequency power were observed between trials with fastest and slowest RT in the nine a-priori defined channels. However, it must be noticed that differences observed in delta power in central regions as a function of fastest/slowest RT trials were not observed in high-density topoplots for theta, alpha, beta, and gamma bands. Therefore, although changes in delta power in central regions seems to be specifically associated with AV states, we recognize that these outcomes should be further explored.

To conclude, the present study provides novel and high-powered evidence about changes in different oscillatory rhythms as predictors of failures in vigilance components. For EV, misses in a signal-detection subtask were anticipated by increased alpha power in left occipital regions prior to the infrequent critical signal onset. For AV, slowest responses in a single RT subtask were anticipated by increased delta power in frontal-central regions prior to target onset. The increment in alpha power associated with EV loss seem to fit well with the role of alpha oscillations in visual detection tasks accounted by the cortical oscillations model of sustained attention of Clayton et al. (2015). The increment in delta power associated with AV loss in frontal-central regions are in line with previous findings observed in single RT tasks (Chua et al., 2012). The present outcomes further support an empirical dissociation at the neural level between EV and AV as two independent components. Changes in alpha power left occipital regions in EV and in delta power in frontal-central regions in AV might serve as indicators of vigilance loss states across prolonged periods.

Data availability statement

The datasets presented in this article are not readily available because expressed consent to publicly share data collected was not required to participants. Requests to access the datasets should be directed to FL (fluna@unc.edu.ar) and PB (pablob@conicet.gov.ar).

Ethics statement

The studies involving human participants were reviewed and approved by Comité Institucional de Ética de Investigaciones en Salud of the Hospital Nacional de Clínicas, CIEIS HNC, Universidad Nacional de Córdoba, Argentina. The patients/participants provided their written informed consent to participate in this study.

Author contributions

FL: conceptualization, data curation, formal analysis, investigation, methodology, resources, software, validation, visualization, writing—original draft, review, and editing. MA: conceptualization, data curation, formal analysis, investigation, methodology, software, validation, visualization, and writing—review and editing. EM-A: conceptualization, formal analysis, methodology, software, supervision, validation, and writing—review and editing. AI: conceptualization, resources, software, validation, and writing—review and editing. JL: conceptualization, formal analysis, methodology, project administration, software,

supervision, validation, and writing—review and editing. PB: conceptualization, data curation, formal analysis, funding acquisition, investigation, methodology, project administration, resources, software, supervision, validation, visualization, and writing—review and editing. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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Lateralized connectivity in the alpha band between parietal and occipital sources when spatial attention is externally and internally directed

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Introduction: External spatial attention refers to the selection of currently present information at a specific external location, whereas internal spatial attention refers to the selection of stored information in short-term memory initially presented at a specific location. Electroencephalographic (EEG) and magnetoencephalographic (MEG) studies revealed that these two types of selection may involve the same underlying neural mechanism as in both cases a reduction in posterior alpha power was observed. However, the posterior alpha rhythm may consist of different components, which can even be related to opposite behavioral effects. Furthermore, the employed paradigms to study both selection processes are often quite different, which makes a proper comparison cumbersome.

Methods: In the current EEG experiment, two-stimulus displays were preceded or followed by non-spatial pre-cues and post-cues that specified the target, thereby minimizing procedural differences.

Results: Results of time-frequency analyses revealed that in both cue conditions the selection of relevant information was associated with an increase of ipsilateral vs. contralateral posterior alpha power. An opposite effect, an increase in contralateral vs. ipsilateral posterior power was observed in the theta-band, but only in the pre-cue condition. This activity was related to stimulus onset and likely reflects target selection. To assess attention-related connectivity, we separated posterior alpha power in a medial-parietal and two parieto-occipital sources and computed alpha phase coherence between these sources. Results revealed that in both cue conditions, increased ipsilateral vs. contralateral connectivity in the alpha band was present between the medial-parietal and parieto-occipital sources.

Discussion: The results seem to reflect a modulation from parietal to occipital areas, providing support for the view that internal and external spatial attention share a common neural mechanism.

KEYWORDS
internal spatial attention, external spatial attention, EEG, lateralized alpha power, lateralized theta power, source connectivity

1. Introduction

A common observation when people try to recall a visual scene from memory is that they move their eyes upwards or even close them. This behavior may be interpreted as an attempt to reduce interference from the outside world, thereby facilitating the retrieval from memory. Recent studies employing electroencephalography (EEG) or magneto-encephalography (MEG) suggest that the alpha rhythm (∼8–14 Hz) reflects a related brain mechanism that suppresses neural activity that otherwise might disrupt ongoing processing and appropriate behavior (e.g., see Klimesch et al., 2007; Jensen and Mazaheri, 2010; Foxe and Snyder, 2011; Benedek et al., 2014). Several studies in the last decade (e.g., Van der Lubbe et al., 2014; Poch et al., 2017; Cona et al., 2020; for a review see Frey et al., 2015) revealed that focal changes in alpha power (and/or phase) are present not only when selecting visual information from a location in the outside world—i.e., external spatial attention, but also when retrieving information from visuospatial memory—i.e., internal spatial attention. There is quite some discussion on whether these changes are comparable, as they are often demonstrated within different experimental paradigms (e.g., see Kizilirmak et al., 2022). Furthermore, recent studies suggest that observed effects in the alpha band actually have different generators in occipital and parietal cortex that may fulfill different functional roles (e.g., see Sokoliuk et al., 2019; Rodriguez-Larios et al., 2022; Zhigalov and Jensen, 2022). This makes a comparison of results even harder. Apart from the alpha band, some studies indicated that in the case of external spatial attention an additional focal effect may be present in the theta band (∼4–8 Hz; e.g., see Van der Lubbe et al., 2014), which was interpreted as signal enhancement (Harris et al., 2017) or encoding of the relevant information (Hanslmayr et al., 2009). The question may be raised whether this potential effect in the theta band is present in both external and internal spatial attention conditions.

In the current EEG study, a previously employed paradigm of Van der Lubbe et al. (2014) was modified to minimize the differences between conditions related to external and internal spatial attention. Furthermore, by using the generalized eigenvalue decomposition (GED) technique (e.g., see de Cheveigné and Arzounian, 2015), we separated occipital and parietal sources of alpha activity, and examined whether connectivity between these sources during spatial orienting of external and internal attention is comparable.

The terms external and internal attention were introduced by Chun et al. (2011), and since then have been used in several influential papers (e.g., see Kiyonaga and Egner, 2013; Van Ede and Nobre, 2021). External attention refers to the selection and modulation of sensory information, while internal attention relates to the selection, modulation, and maintenance of internally generated information (Chun et al., 2011). Van der Lubbe et al. (2014) decided to use the terms external and internal “spatial” attention to zoom in on the process of selection of information at a location either present at that specific moment, or selection of earlier presented information at a specific location (i.e., retrieval from visuospatial memory).

Inspired by the studies of Griffin and Nobre (2003), Nobre et al. (2004), and Kuo et al. (2009), Van der Lubbe et al. adapted a paradigm introduced by Hommel (2002) that enables to contrast external and internal spatial attention in highly similar conditions, and study focal changes in EEG alpha power over time. Visual search or memory search displays were used containing four stimuli, two squares and two circles, all presented in different colors. Each stimulus was presented in one quadrant of a centrally positioned frame. In the pre-cue condition, the color of the frame indicated 1 s before the search display which stimulus, the one with the same color, was the target. The target required a left or right response depending on the shape (circle or square) of the stimulus. In the post-cue condition, the stimulus display was presented first, then masked for 33 ms (to avoid sensory after effects), and finally, after another 967 ms the color of the frame indicated which of the previously presented stimuli was the target. The study also included a simultaneous cue condition, wherein the colored frame was presented together with the four-stimulus display. Focal changes in alpha, beta, and theta power over time were assessed by computing lateralized power spectra (LPS; see Van der Lubbe and Utzerath, 2013), which are ipsilateral vs. contralateral power indices in specific frequency bands. An advantage of using the LPS index is that overall hemispherical differences in power unrelated to the location of the stimulus are subtracted out (see Section 2). Van der Lubbe et al. (2014) observed an increase in alpha power at ipsilateral vs. contralateral sites relative to the relevant stimulus location above posterior brain areas both in the pre-cue, the simultaneous cue, and the post-cue conditions (see also Poch et al., 2017). Furthermore, they observed that the Simon or spatial correspondence effect (i.e., the tendency to react toward the side of the task-irrelevant spatial location of a stimulus; Simon, 1969, 1990; Van der Lubbe et al., 2012), was present in all conditions, even when the stimulus was retrieved from memory. Finally, increased contralateral vs. ipsilateral power was observed in the theta band in all cue conditions. The LPS and behavioral findings led them to conclude that internal and external spatial attention may share their underlying mechanism.

In a recent study, Willems (2020) tried to replicate and extend these results with the same set of stimuli and responses. However, no convincing lateralized changes in alpha power were observed in two post-cue conditions, one with the same time interval as Van der Lubbe et al. (2014), and another condition with a longer time interval of 3,000 ms. Evaluation of post-experimental interviews revealed that several participants in the post-cue conditions adopted a strategy that no longer required retrieval from visuospatial memory. Instead, after being presented with the stimulus displays, they simply memorized that circles (or squares) had color C1 (e.g., green) and C2 (e.g., red), and upon presentation of the frame they selected the proper response (e.g., left) when the frame had color C1/C2, and if it had a different color they selected the alternative response (e.g., right). This strategy no longer requires any retrieval from visuospatial memory and may be responsible for the absence of clear lateralized changes in alpha power in the two post-cue conditions. The motivation to develop this strategy could have been induced by the use of the condition with a longer time interval between display onset and the post-cue, which might make retrieval from visuospatial memory more difficult. Based on these observations, several modifications in the employed paradigm were implemented in the current study that should reduce the likelihood of using this alternative strategy (see below).

In their review, Frey et al. (2015) suggested that the same oscillatory processes support different types of attention such as spatial attention, which is often examined with the Posner paradigm, and internal attention, which is often examined with a delayed match-to-sample task or a Sternberg task. However, different paradigms may imply different processing demands (see Kizilirmak et al., 2022), and results of recent studies indicate that oscillations in the alpha band may actually reflect different functions. In their EEG study, Sokoliuk et al. (2019) distinguished between a source in visual cortex and another source in parietal cortex that showed different sensitivities to experimental manipulations. They used unimodal (i.e., only visual or somatosensory stimuli) and multimodal (both visual and somatosensory stimuli) tasks. The occipital source showed a decrease in power when more attention had to be directed to visual stimuli, while the parietal source appeared more sensitive to the overall amount of attention required, as the power decrease was largest when attention had to be divided across modalities or locations. These observations suggest that it may be crucial to disentangle these different generators of alpha activity (see also Zhigalov and Jensen, 2020, 2022).

Rodriguez-Larios et al. (2022) measured MEG while participants performed a visual working memory task. Participants had to remember a first directional cue that pointed either to the upper-left, upper-right, down-left, or down-right corner. After a delay interval they had to indicate, depending on a second stay or switch cue, either the same or the opposite direction of the first cue. In one condition, distracting directional cues were presented during the delay interval, while in another condition, no distractors were presented. The use of independent component analyses (ICA) at a single-subject level revealed the existence of two dissociable alpha components during the delay interval relative to a fixation period, referred to as Alpha 1 and Alpha 2. Alpha 1 increased during the delay interval and became larger when distractors occurred during this interval. Alpha 1 power was also positively related with accuracy. Alpha 2, however, showed a decrease during the delay interval, it decreased when distractors occurred, and it showed a negative relation with accuracy. Alpha 1 was argued to be related to the effective inhibition of distractors, while Alpha 2 was suggested to be more related to overall lapses of attention or mind wandering. Although these different patterns were observed during the retention interval, and therefore do not tell much about memory retrieval,[1] these findings underline the view that effects on the posterior alpha rhythm may reflect the contribution of rather different underlying neurophysiological processes that may even have opposite effects on behavior.

In the present study, we separated posterior alpha activity, now in both an external and an internal spatial attention task, by using the GED technique (de Cheveigné and Arzounian, 2015). Separate GED analyses were performed to obtain spatial filters for a priori specified medial-parietal, and lateral-occipital sources of alpha activity, in line with the findings of Sokoliuk et al. (2019). After separating these activities, we estimated the communication between these areas by computing the inter-site (here source) phase coherence (ISPC; Lachaux et al., 1999), which can be considered as a new and critical test for assessing the similarities between external and internal spatial attention. Additionally, in line with the study of Van der Lubbe et al. (2014), we determined the LPS index and expected to observe increased ipsilateral vs. contralateral power in the posterior alpha band both in the external and the internal spatial attention conditions. Effects in the posterior theta band were additionally explored, as a contralateral increase in theta power has been observed in the case of both external and internal spatial attention (Harris et al., 2017; see also Van der Lubbe et al., 2014). Furthermore, we expected to replicate the previously observed Simon effect in both conditions.

To avoid the use of alternative strategies in the internal spatial attention task as indicated above, and to reduce the tendency to make eye movements, we made some adaptations to the tasks used by Van der Lubbe et al. (2014). First, we extended the type of possible stimuli from two to four, and also increased the number of response options from two to four. This should reduce the likelihood of the aforementioned alternative strategy, as the simple rule of relating two colors to a single response will no longer work. At the same time, we also minimized the load on visuospatial memory by reducing the number of stimuli on a trial from four to two. This should make the maintenance and subsequent retrieval from visuospatial memory easier. We employed a long time-interval of 2,000 ms between offset of the stimulus array and onset of the post-cue to ensure that the observed effect is not due to residual activity in a sensory buffer. Finally, instead of using a large frame as pre- or post-cue, which might promote lateral eye movements, the cue was simply a change in color of the surrounding of the fixation cross. This may be an additional incentive for participants to keep their eyes at fixation.

2. Methods

2.1. Participants

Twenty-seven healthy volunteers (21 females) participated in the experiment, mostly in return for course credits. Their mean age was 22.8 years (SD 7.4) and they were all right-handed, which was assessed with Annett (1970)'s handedness questionnaire. Most of the participants were students at the University of Twente, but two participants were recruited using convenience sampling and participated on a voluntarily basis. Due to the COVID-19 pandemic, the Dutch government implemented strict measures to minimize the risk of spreading the virus. When entering the lab, participants were asked to disinfect their hands and they received FFP2 face masks. During the preparation for the experiment, both the participants and the researchers were wearing FFP2 masks. However, during the experiment, participants were allowed to take off their masks. The participants and researchers kept their distance whenever possible and windows in the lab were regularly opened, allowing the air to circulate. Participants were explicitly asked not to take part if they were experiencing any COVID-19 symptoms or if they had been in close contact with someone who was infected. All participants had normal or corrected-to-normal vision, which was checked with the Freiburg Vision Test (Version 3.10.5), and they all showed normal color vision, which was examined with the Ishihara test. None of the participants reported a history of neurological or psychiatric disorders. Informed written

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[1] It might be argued that participants perform this task by simply keeping their attention at the corner indicated by the directional cue rather than by keeping the direction in visuospatial memory. Furthermore, the distractors might then also be seen as interfering with the attentional focus rather than interfering at the level of visuospatial memory.

consent was obtained from each participant before the start of the experiment, in accordance with the Declaration of Helsinki. The study was approved by the Ethics Committee of the Faculty of Behavioral, Management, and Social Sciences at the University of Twente (request number 210676).

Due to error rates exceeding two standard deviations above the sample mean, data of one participant was excluded from the pre-cue and post-cue datasets, and data of another participant was excluded from the pre-cue dataset. Thus, the pre-cue dataset included 25 subjects, and the post-cue dataset —26 subjects.

2.2. Task, stimuli, and procedure

The stimuli and examples of trials for the pre-cue and post-cue conditions are shown in Figure 1. The task is a modified version of the cued spatial selection task employed by Van der Lubbe et al. (2014). Each trial of the task began with a fixation cross displayed at the center of the computer screen, together with an auditory warning stimulus. We used a fixation stimulus recommended by Thaler et al. (2013) as this stimulus is most effective in maintaining central fixation. The height and width of the fixation stimulus were set at 6 mm (0.46°). On every trial, two geometrical shapes were displayed simultaneously on the computer screen, one on the left and other on the symmetrical right side of the fixation cross (at 3.8°). The left and right shapes always differed in two dimensions: shape and color. The stimulus was either a circle, a diamond, a star, or a triangle, displayed in either blue, green, red, or purple (RGB: 43,87,154; 36,126,77; 191,65,35; and 141,63,135, respectively). The stimuli were presented on a light gray background (RGB: 127, 127, 127). The shapes were each 8 mm (0.6°) high and wide. The shapes, colors, shape and color locations, and target visual field were all counterbalanced. The trial order was randomized per participant. The participant's task was to give a response depending on the shape in the color indicated by the central visual cue. Thus, the cue signaled which of the two shapes was the target stimulus on a given trial. The cue was a small circle—either blue, green, red, or purple—referring to the target color, and was displayed behind the fixation cross. The height and width of the cue were 6 mm (0.46°).

The color cue appeared either before or after target onset, which constituted the two main task conditions: the pre-cue and the post-cue conditions. In the pre-cue condition, the color cue was presented 1,000 ms before the two shapes, and was present until target onset. In this condition, the shapes were displayed until a response was made. In the post-cue condition, the order of the cue and shapes was flipped, which implies that the target had to be retrieved from visual short-term memory. The exact stimulus sequence in the post-cue condition was as follows: after an initial fixation period of 800 ms, the two shapes were displayed for 1,000 ms, then they were both masked, and after 2,000 ms the color cue appeared and remained present until a response was made. In both cue conditions, the inter-trial time varied randomly between 700, 1,000, and 1,300 ms.

Participants were instructed to base their responses on the shapes indicated by the color cues. A standard QWERTY keyboard was used to gather the responses. They had to press the "A" key with the left middle finger, the "Z" key with the left index finger, the "/" key with the right index finger, and the "‘" key with the right middle finger. Half of the participants responded with the left hand to circles ("A") and diamonds ("Z"), and the right hand to stars ("‘") and triangles ("/"). The other half of the participants were assigned the opposite hand mapping. Both reaction times and response accuracy were determined. A new trial started automatically after the response, or after 3,000 ms if the participant did not respond.

The pre-cue and post-cue conditions were administrated in one session, and were separated by a break. The order of these conditions was counterbalanced between participants. At

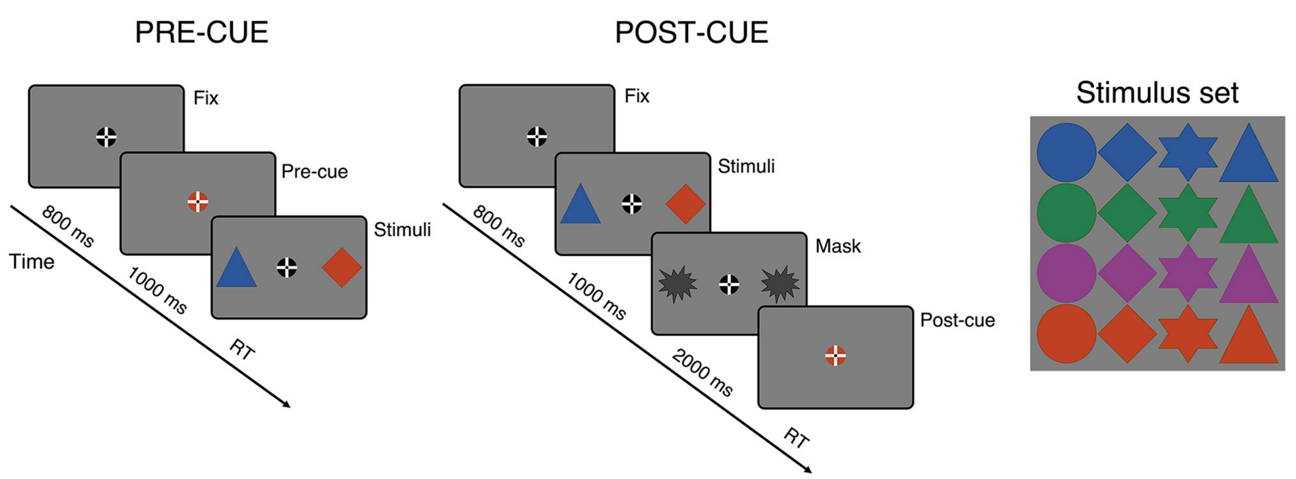

FIGURE 1
The task stimuli and examples of trials from the two main conditions of the experiment. Participants' task was to respond to the shape displayed in the color indicated by a central color cue. The color cue could appear either before or after target onset, which constituted the *pre-cue* and *post-cue* conditions, respectively. In the pre-cue condition, the color cue was presented 1,000 ms before the two-stimulus displays. In the post-cue condition, the order of the cue and the stimulus display was reversed, thus, the relevant information had to be selected from visuospatial memory. The post-cue was presented 3,000 ms after onset of the stimulus display.

the start of each condition, participants received written, and subsequently verbal instructions describing the task. They were asked to respond as quickly, but especially as accurately as possible. They were also instructed to keep central fixation and were explained why proper fixation was necessary during the EEG measurements. Participants were seated in an armchair in a dimly lit room at a distance of ~75 cm in front of a 22$'$ LED monitor. Each condition started with a practice block consisting of 32 trials in total in which participants received accuracy feedback after each response. Each of the two experimental conditions consisted of 480 experimental trials administered in five blocks of 96 trials, and lasted up to 1 h. No feedback was given during the experimental trials. Participants were allowed to take a short break between the blocks. Presentation software (Neurobehavioral Systems, Inc., Version 20.1) was used for experimental control and sending external triggers to the EEG amplifier.

Response time (RT) and accuracy data were submitted to a 2 × 2 repeated-measures ANOVA with Cue Condition (pre-cue, post-cue) and Spatial S-R Correspondence (corresponding, non-corresponding) as within-subjects factors. Trials with responses longer than 3 SD above the mean (in overall 1.8 % of trials) and trials with an incorrect response were excluded before the RT analysis.

2.3. EEG data recording and preprocessing

The EEG was recorded using an ActiChamp amplifier and BrainVision Recorder® (1.21.0403) software (Brain Products, Munich, Germany). Electrodes were placed on standard scalp sites according to the extended 10–20 system at 63 locations mounted in an elastic cap (Braincap, Brain Products GmbH). The online reference electrode was located at the TP8 electrode position. The ground electrode was placed at the forehead. The horizontal and vertical electro-oculogram (hEOG and vEOG) were measured by using electrodes located above and below the left eye and at the outer canthi of the left and right eye. Electrode gel and standard procedures were applied to keep the electrode impedance below 10 kΩ. The data sampling rate was 1,000 Hz. An online high cutoff filter of 200 Hz and a notch filter of 50 Hz was applied.

Offline EEG preprocessing was done using BrainVision Analyzer® software (version 2, Brain Products, Munich, Germany). Data were re-referenced to the mean signal from all electrodes, and filtered with 0.1–90 Hz band-pass filters and a 50 Hz notch filter (Butterworth zero-phase filters, attenuation of 12 dB/octave). The EEG from the pre-cue condition was split into segments from −1,800 to 2,000 ms relative to target onset, and baselined to the first 200 ms before target onset. For the post-cue condition, the segments were split from −3,800 to 2,000 ms relative to cue onset, and baselined to the first 200 ms before cue onset. Trials with an incorrect response, or with reaction times shorter than 200 ms or longer than 2,000 ms were automatically excluded from further analyses.

To exclude trials with horizontal eye movements, segments were marked if the horizontal EOG activity (right minus left EOG) exceeded ±35 μV and/or voltage steps between adjacent data points exceeding 16 μV. The marked segments were inspected manually and removed if a distinctive "boxcar" shape, characteristic for saccadic eye movements, was found in these marked segments within about 500 ms after target onset in the pre-cue condition and after cue onset in the post-cue condition. On average 25 segments (3%) per participant (range 1–121) were removed due to horizontal eye movements toward the target. Surprisingly, less eye movements were found in the pre- than post-cue condition, on average in 7 vs. 19 segments ($p = 0.001$).

ICA was carried out to correct the EEG for ocular artifacts and other activity that had non-cortical origins. Next, after resetting the baseline, the segmented data were checked for residual artifacts using BrainVision Analyzer semi-automatic artifact rejection method. Rejected were segments with absolute amplitudes ≥250 μV, minimum-maximum voltage differences within 1 second time-interval ≥300 μV, and voltage steps between adjacent data points ≥75 μV. If necessary, the rejection criteria were adjusted according to signal characteristics in individual subjects (such as unusually small or large EEG amplitudes). On average 4 % of segments per participant (SD 6, range 0–23%) were excluded due to artifacts at this step. The averages of accepted segments were 416 segments (range 293–472) for the pre-cue condition and 371 segments (range 224–457) for the post-cue condition.

2.4. Time-frequency decomposition

Subsequent analyses were performed in Matlab (version R2020a, The Mathworks, Natick, MA, USA) using custom written code based on published scripts (Cohen, 2014, 2017, 2022) and the Brainstorm toolbox (Tadel et al., 2011). To decompose the EEG signal into its time-frequency representation, the Fast Fourier Transformation (FFT) power spectrum of the single-trial data was convolved with complex Morlet wavelets, defined as: $e^{i2\pi ft}e^{-t^2}/(2\sigma^2)$, where i is the complex operator, t is the time, f is the frequency from 1 to 30 Hz in 30 logarithmically spaced steps, and σ is the width of each frequency band defined as $\sigma = n/(2\pi f)$, where n is the number of wavelet cycles. The wavelet cycles varied from 3 to 8 in logarithmically spaced steps, to obtain an optimal trade-off between temporal and frequency resolutions (Trujillo and Allen, 2007). After the convolution operation, instantaneous EEG power and phase were extracted from the resulting signal by taking, respectively, the squared magnitude and phase angle at each time-frequency point.

2.5. Lateralized power spectra (LPS)

To examine attention-related ipsilateral–contralateral modulations of local power in the alpha-and theta-bands, we calculated lateralized power spectra (LPS), based on the method described by Van der Lubbe and Utzerath (2013); see also Van der Lubbe et al. (2014, 2019). The LPS indices were calculated by a double subtraction of all symmetrical electrode pairs at each time-frequency point. First, the ipsilateral–contralateral subtraction was calculated separately for segments with targets in the left and right visual fields, then scaled by the sum of activation from both hemispheres (ipsilateral + contralateral), and averaged, according to the formula:

$$LPS_{(f_n,\,t)} = \frac{W_{(f_n,\,t)|ipsi} - W_{(f_n,\,t)|contra}}{W_{(f_n,\,t)|ipsi} + W_{(f_n,\,t)|contra}}$$

where W_{ipsi} is trial-averaged power from the ipsilateral channel (relative to target visual field), W_{contra} is power from the contralateral channel; both measured within the same frequency-band (f_n) and time-window.

The LPS values vary from -1 to $+1$. A positive LPS value indicates larger power at the ipsilateral site relative to the contralateral site, which may reflect a stronger desynchronization of a given frequency band at the contralateral site; zero indicates no hemispherical difference, thus no directing attention-related modulations. For visualization, the obtained LPS topographies were plotted on a map of the left hemisphere. The topographical results (see Section 3) showed that in the pre-cue condition, alpha-band LPS peaked at PO3/4; whereas in the post-cue condition, the lateralized alpha power was slightly more anterior, with a peak at P5/6. Therefore, further analysis of alpha LPS was restricted to these sites.

To evaluate the statistical significance of the LPS in the pre-cue and post-cue conditions, nonparametric permutation tests with cluster-level correction for multiple comparisons were performed (Nichols and Holmes, 2002; Maris and Oostenveld, 2007). One thousand iterations of randomization were created for each analysis. At each iteration, the time-frequency points from an interval of 1,300 ms—beginning from target onset in the post-cue condition, and from cue onset in the post-cue condition—were randomly shuffled. Next, a two-tailed *t*-test was performed for each time-frequency point against the null-hypothesis of no change from the baseline interval (-500 to 0 ms). The obtained *t*-value maps were corrected at cluster-level with the significance threshold set at $p = 0.01$. Additionally, for statistical evaluation of possible differences between the two cue conditions, LPS values were measured as mean activity in the time-frequency windows indicated in the permutation test results: 400–600 ms and 10–14 Hz for the pre-cue condition, and 550–750 ms and 10–14 Hz for the post-cue condition. The obtained alpha LPS values were submitted into a repeated-measures ANOVA with Cue Condition (pre-cue, post-cue) as a within-subjects factor (The comparisons between two cue conditions were done with $N = 25$).

2.6. Source-level inter-site phase coherence

To examine attention-related contra-ipsilateral modulations of functional connectivity in the alpha-band, we analyzed source-level inter-site phase coherence (ISPC). Before the analysis, the surface Laplacian filter (or current source density or current scalp density, CSD) was applied to the single-trial data. The Laplacian is commonly used to increase spatial selectivity and minimize volume conduction effects (Cohen, 2015a,b).

2.6.1. Spatio-spectral source separation

Source separation was performed by means of the GED technique. The GED is a multivariate source separation method that maximally separates two matrices of channel covariance between a priori specified signal features (Parra and Sajd, 2004; de Cheveigné and Arzounian, 2015; Cohen, 2022). The first matrix is the channel covariance of the relevant signal. Our a priori specified frequency band was the alpha, thus the signal matrix (S) was derived from data narrow band-filtered in this frequency band (cf. Nikulin et al., 2011; Zuure and Cohen, 2021). The filter was centered at 11 Hz, and the Gaussian spectral full-width at half-maximum (FWHM) range was set at 3 Hz. The second matrix is the channel covariance of the reference signal. Our reference matrix (R) was derived from the broadband (unfiltered) EEG. The S and R matrices were computed for a 100–600 ms time-window, relative to target onset in the pre-cue condition and cue onset in the post-cue condition. Additionally, the phase-locked part of the signal was removed from the single-trial data before the GED analyses to avoid potential stimulus-evoked transient artifacts (Cohen, 2018).

Separate GED analyses were performed to obtain spatial filters for the a priori specified medial-parietal, and the left and right lateral-occipital sources of alpha activity. To separate the medial-parietal source, the covariance matrices were calculated for each participant on condition-averaged data, and then the result was group-averaged. A similar procedure has been previously used for group-level ICA analyses (Calhoun et al., 2001, 2009). Next, the GED was applied to the group-averaged matrices, which produced a set of 63 spatial components of group-level condition-averaged alpha activity, where each component was a weighted combination of all 63 channels. The GED components are specified by their eigenvalues that show the S/R ratio indicating the importance of each component, and eigenvectors that provide the parameters of the spatial filters (i.e., the sensor weights constituting each spatial component). The medial-parietal source was isolated by the first of the 63 components—showing the largest S/R ratio, and this component was retained for ISPC analysis (cf. Zuure et al., 2020; Cohen, 2022). To obtain spatial filters for the lateral-occipital (visual) sources, the described GED analysis was performed separately for data from trials with targets presented in the left and right visual fields (pooled across all other conditions). In both analyses, the first component again isolated the medial-parietal source, as in the first GED analysis above, and the second largest components isolated, respectively, the left and right lateral parieto-occipital areas (see Section 3). These second-largest components were retained for the ISPC analysis.

To create visualizations of the components' anatomical distributions, forward models of the three selected GED components were correlated with a lead field matrix containing the coefficients relating the source space to the sensor-level EEG topography (Hild and Nagarajan, 2009; Cohen and Gulbinaite, 2017). The forward models were computed by multiplication between the eigenvector and covariance matrix S (Haufe et al., 2014). The lead field matrix was generated in the Matlab

Brainstorm toolbox using a Boundary Element Method (BEM) model. The obtained correlation coefficients were visualized on the standard cortical surface (MRI model: ICBM 152; see Section 3).

2.6.2. Inter-site phase coherence (ISPC)

To assess attention-related functional connectivity, ipsilateral and contralateral (relative to the target visual field) alpha-band ISPC was computed between the isolated medial-parietal and lateral parieto-occipital sources. ISPC estimates the consistency between band-specific phase angle values at two sites or areas of activity (Lachaux et al., 1999; Cohen et al., 2008). In the literature, this measure has also been referred to as inter-site phase connectivity, inter-site phase clustering, inter-channel phase synchrony (ICPS), and inter-site phase-locking value (PLV). Component-specific time-series data, i.e., the component eigenvectors multiplied by the single-subject EEG signal, were decomposed into their time-frequency representations through convolution with complex Morlet wavelets (as described above for sensor-level analysis), separately for all retained components. This yielded medial-parietal and lateral parieto-occipital time-frequency activity for each task condition per participant. Next, alpha-band (10–14 Hz) ISPC was computed by taking the length of the mean vector of differences between the distribution of obtained phase angle differences over trials, for each time-frequency point of the spatially-filtered signal, according to the formula:

$$ISPC_{(f_n,t)} = \left| \frac{1}{k} \sum_{t=1}^{k} e^{i(\Phi_x(f_n,t) - \Phi_y(f_n,t))} \right|$$

where, x and y stands for two distinct sources, Φ indicates instantaneous phase at single time-frequency point (f_n, t). The resulting ISPC index varies between 0 and 1, where 0 indicates no phase synchrony between two sites (i.e., random distributed phases), and 1 indicates a fully consistent phase synchrony between two sites. The obtained ISPC values were baseline-corrected as the percentage of change at each frequency band relative to a pre-stimulus baseline (−700 to 200 ms).

Statistical significance of the differences between ipsilateral and contralateral alpha ISPC over time was examined using non-parametric permutation testing with cluster-level correction for multiple comparisons (Nichols and Holmes, 2002; Maris and Oostenveld, 2007). First, t values were computed for every time-point of the ipsilateral vs. contralateral difference, which produced clusters of significant time-points (threshold set at $p = 0.05$). Next, the direction of the hemispheric difference was randomly shuffled in 1,000 iterations, to create a distribution for the null-hypothesis of no ipsilateral–contralateral difference. At each iteration, a two-tailed t-test was performed for each time-point of the shuffled data against the null-hypothesis, and the size of the largest time-point cluster of significant t values was determined, to obtain a distribution of maximal cluster sizes expected under the null-hypothesis. Finally, the significant time-point clusters from the true t-statistic map of the ipsilateral vs. contralateral difference were determined by setting a threshold at the level of the 95th percentile of the null-hypothesis distribution, which indicated as significant any cluster that was equal to or larger than the 95% of the distribution of null hypothesis cluster sizes ($p = 0.05$ was applied here because the ISPC effects did not reach the more conservative 0.01 threshold). Additionally, to test for possible differences between the cue conditions, alpha ISPC was measured as mean activity in the time-windows approximated to the epochs indicated in the permutation results: 400–600 ms for the pre-cue condition, and 550–750 ms for the post-cue condition; and submitted into a 2 × 2 repeated-measures ANOVA with Hemisphere (ipsilateral, contralateral) and Cue Condition (pre-cue, post-cue) as within-subjects factors.

To examine whether the observed ISPC reflected a non-zero lag connectivity, the circular "V-test" was conducted, which is a nonparametric method that allows to determine if an observed phase angle difference is significantly different from a specified phase angle (Zar, 1999; Cohen, 2014). Following the implementation by Van Driel et al. (2012), first the distributions of single-trial alpha-band (10 Hz) phase-angle-differences over time were computed. This was done separately for the left and right ipsilateral connections between the medial-parietal and parieto-occipital sources, in both cue conditions, for each participant. Next, the V-test was performed over trials within-subjects, against "0" and "π," on the randomly sampled phase-angle data (cf. Cohen, 2014). This was done for the time-intervals in which significant differences between ipsilateral and contralateral alpha-band ISPC were observed (380–590 ms in pre-cue condition, and 540–780 ms in post-cue condition). The obtained V-test results were averaged across participants (cf. Van Driel et al., 2012). Confirmation of the null hypothesis would indicate a difference between the tested phase angle directions; thus, a non-significant p-value indicates that the phase angle lag is not zero or π. A non-zero or non-π lag difference implies true inter-regional connectivity, whereas zero-lag or π-lag connectivity might reflect a volume conduction artifact or real zero-phase-lag inter-site synchrony (Roelfsema et al., 1997; Bastos et al., 2015a).

3. Results

3.1. Response time and accuracy

RT and accuracy data are displayed in Figure 2. RT in both cue conditions were rather slow, which may be ascribed to the relatively difficult four-choice response task and the emphasis on accuracy. The average RT was 858 ms (SD 127) in the pre-cue condition and 860 ms (SD 188) in the post-cue condition, $F < 1.0$, n.s. The two cue conditions differed in accuracy, indicating that the retrieval from visual memory in the post-cue condition (88 %, SD 6) was more difficult than the selection of the visual target in the pre-cue condition (94 %, SD 4), $F_{(1,\,24)} = 37.78$, $p < 0.001$, $\eta_p^2 = 0.61$.

Furthermore, RTs were faster by about 25 ms, $F_{(1,\,24)} = 30.82$, $p < 0.001$, $\eta_p^2 = 0.56$, and accuracy higher by about 2 %, $F_{(1,\,24)} = 13.08$, $p = 0.001$, $\eta_p^2 = 0.35$, in trials with S(stimulus)-R(Response) spatial correspondence (C) than non-corresponding (N) trials. Interestingly, in RTs, significant was also the interaction between Cue Condition and Spatial S-R Correspondence, $F_{(1,\,24)} = 4.89$, $p = 0.037$, $\eta_p^2 = 0.17$, indicating that the S-R correspondence effect in the post-cue (35 ms), $F_{(1,\,24)}$

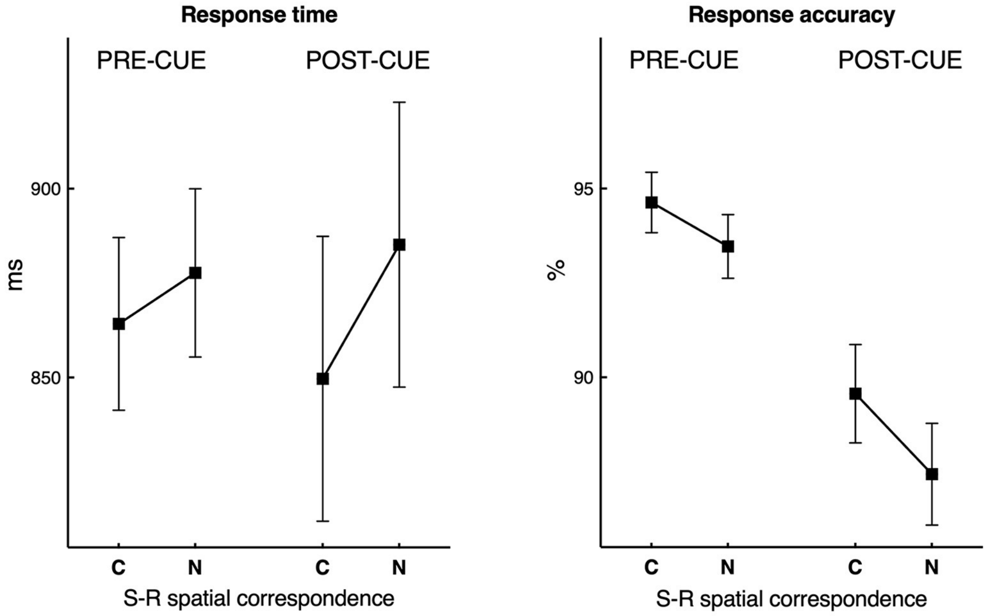

FIGURE 2
Response time and response accuracy data obtained in the pre-cue and the post-cue conditions as a function of correspondence (C) and non-correspondence (N) between the (former) location of the relevant stimulus (S) and the response (R) side. The vertical bars are standard errors of the mean.

$= 26.35$, $p < 0.001$, $\eta_p^2 = 0.52$, was significantly larger than in the pre-cue condition (14 ms), $F_{(1, 24)} = 4.47$, $p = 0.045$, $\eta_p^2 = 0.16$. In accuracy, the interaction was not significant, $F = 1.35$, n.s. In sum, the behavioral results replicated the findings of Van der Lubbe et al. (2014), with the additional result of a larger spatial correspondence effect in the post-cue condition on RT.

3.2. Lateralized power spectra

Figure 3 shows time-frequency representations of the LPS results for the two cue conditions. Black contours outline the time-frequency regions in which power lateralization was significant with permutation testing with a threshold set at $p \leq 0.01$ (cluster-level corrected for multiple comparisons). The head maps show topographies of LPS in windows approximated to the significant time-frequency areas. As can be seen in Figure 3, there was a significant transient lateralization of power in the alpha-band (∼10–14 Hz) in both cue conditions, indicating that contralateral alpha power was smaller than ipsilateral alpha power. In the pre-cue condition the alpha lateralization peaked at about 400–600 ms after target onset; whereas in the post-cue condition the peak was at about 550–750 ms after onset of the cue. The topographies show that the alpha LPS effects were present over parieto-occipital areas, with a slightly more anterior focus in the post-cue condition. The magnitude of the lateralization within the 200 ms time windows that was determined based on the outcome of the permutation tests was slightly larger in the pre-cue than in the post-cue condition, $F_{(1, 24)} = 5.42$, $p = 0.029$, $\eta_p^2 = 0.18$.

In the pre-cue condition, we also observed an LPS effect in the theta-band (∼4–6 Hz), indicating an early target-related contralateral vs. ipsilateral increase of theta power over occipital areas, at about 100–300 ms after target onset. No such effect was present in the post-cue condition. A similar effect was actually observed by Van der Lubbe et al. (2014). This theta-band lateralization may reflect an event-related activity evoked in the process of target selection (cf. Bastos et al., 2015b; Landau et al., 2015). To establish whether this effect is indeed event-related, we separated phase-locked LPS ("evoked" activity) from non-phase-locked LPS ("induced" activity) by subtracting the evoked activity (ERP) from single-trial total power (for details see Cohen and Donner, 2013; Asanowicz et al., 2021). The results of the LPS analyses on evoked and induced activity are displayed in Figure 4. Increased contralateral vs. ipsilateral theta power was only present in evoked activity, whereas increased ipsilateral vs. contralateral alpha power was only present in induced activity. The results therefore indicate that the effect in the theta-band in the pre-cue condition is strongly related to the onset of the two-stimulus display.

3.3. Inter-site phase coherence

To isolate the a priori specified source components of alpha-band activity, we used a multivariate spatio-spectral GED decomposition technique (see Section 2 for details). The results of the source separation are displayed in Figure 5A. The medial-parietal areas for both hemispheres were isolated in the first, strongest GED component. The second-largest component isolated the parieto-occipital areas, ipsilateral to the left and right targets, respectively. Next, we analyzed source-level inter-site phase coherence (ISPC) to estimate attention-related ipsilateral and

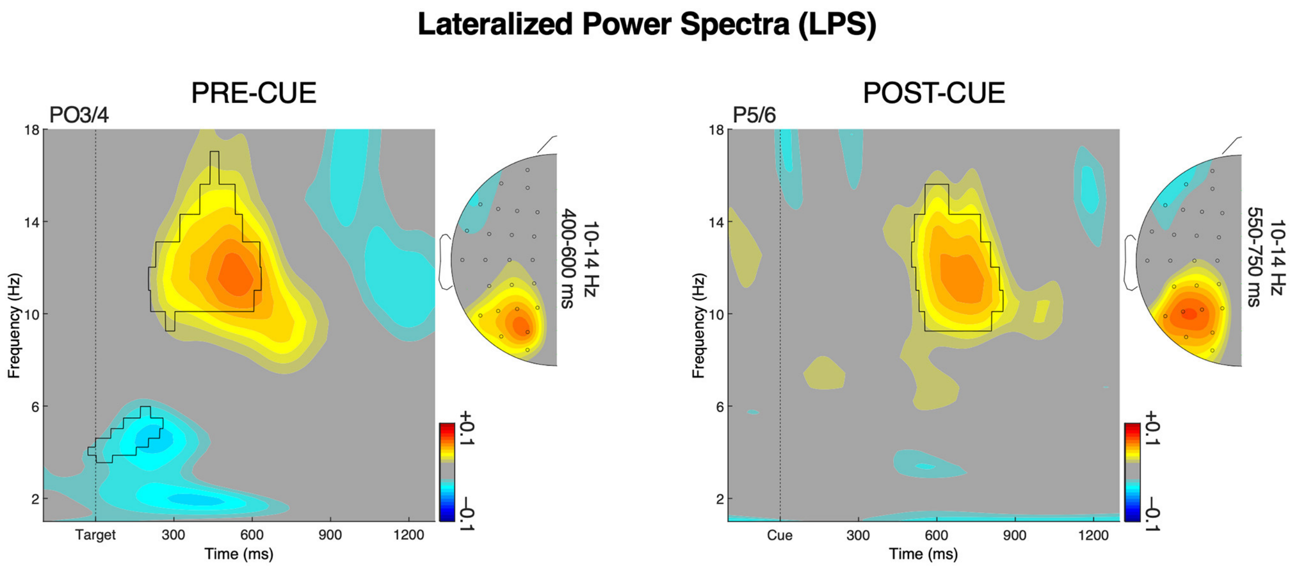

FIGURE 3
Lateralized power spectra (LPS). The time-frequency plots show the LPS from the indicated posterior electrode pairs for the pre-cue and post-cue conditions [**(Left, Right panels)**, respectively]. Black contours outline the time-frequency regions in which power lateralization was significant in the permutation tests (cluster-level corrected for multiple comparisons). Time-zero in the pre-cue condition is the target onset, and in the post-cue condition it is the cue onset. The head maps show topographies of the alpha-band LPS (10–14 Hz) in the indicated time-windows, plotted on the left hemisphere. The maps are min-max scaled, with positive values in red and negative in blue. The head view is from above. The results show a significant transient lateralization of power in the alpha-band (∼10–14 Hz) in both cue conditions, indicating that contralateral alpha power was smaller than ipsilateral alpha power. The results additionally show that contralateral theta power was larger than ipsilateral theta power shortly after target onset in the pre-cue condition.

contralateral modulations of alpha phase synchrony between the obtained components.

Figure 5B shows the time-course of alpha-band (10–14 Hz) ISPC between the medial-parietal and parieto-occipital sources for the ipsilateral and contralateral hemispheres, relative to the target visual field, in the two cue conditions. Gray fills indicate the time-windows in which the ipsilateral-contralateral difference was indicated significant in permutation tests at $p \leq 0.05$ (cluster-level corrected for multiple comparisons).

The results showed significant target-related lateralizations of alpha-band ISPC in both cue conditions, indicating an increase of inter-regional alpha coherence ipsilateral vs. contralateral to the target visual field. The effect was significant from 380 to 590 ms in the pre-cue condition, and from 540 to 780 ms in the post-cue condition, which corresponds with the direction and timing of the LPS effects in the two cue conditions. The ANOVA showed that the magnitude of ISPC lateralization did not differ between the two cue conditions, $F < 1.0$, n.s., while confirming significance of the main effect of the ipsilateral-contralateral difference, $F_{(1,\,24)} = 21.60$, $p < 0.001$, $\eta_p^2 = 0.47$ (Hemisphere × Cue was also not significant, $F < 1.0$, n.s.).

Lastly, given the relatively short distances between the sources, we decided to check whether the observed ipsilateral ISPC reflected a non-zero lag connectivity, which would disfavor an explanation in terms of volume conduction. The group-level circular V-test p values (see Section 2 for details) were 0.48 for zero and 0.51 for π in the pre-cue condition, and 0.67 for zero and 0.32 for π in the post-cue condition. These results indicate that the observed phase angle differences are not distributed around zero or π. Therefore, it may be concluded that the observed ipsilateral vs. contralateral alpha-band phase synchrony between the medial-parietal and parietal-occipital sources is unlikely to be due to volume conduction.

4. Discussion

Earlier studies revealed strong support for similarities between the selection of visual information from a location in the external world (external spatial attention) and the retrieval of former "visual" information from memory (internal spatial attention). This support comes from the application of various approaches, such as assessing gaze bias (Van Ede et al., 2020; Van Ede and Nobre, 2021), examining stimulus-response correspondence effects like the Simon effect (Hommel, 2002; Van der Lubbe et al., 2014), and using neuroimaging measures like fMRI (e.g., Zhou et al., 2022), EEG (e.g., Magosso et al., 2021), and MEG (e.g., Cona et al., 2020). Our primary focus in the current paper was directed at EEG activity in the alpha band, as numerous studies revealed that there is an inverse relation between alpha power and the allocation of visuospatial attention (e.g., Worden et al., 2000; Thut et al., 2006; Van der Lubbe and Utzerath, 2013; Bacigalupo and Luck, 2019; Asanowicz et al., 2021), and comparable effects have been observed in studies focusing on the retrieval from visuospatial memory on the basis of non-spatial cues (e.g., see Van der Lubbe et al., 2014; Poch et al., 2017). Although the previous support based on alpha modulations seemed straightforward (Van der Lubbe et al., 2014), the demonstration of effects within rather different experimental paradigms (see Kizilirmak et al., 2022), and the

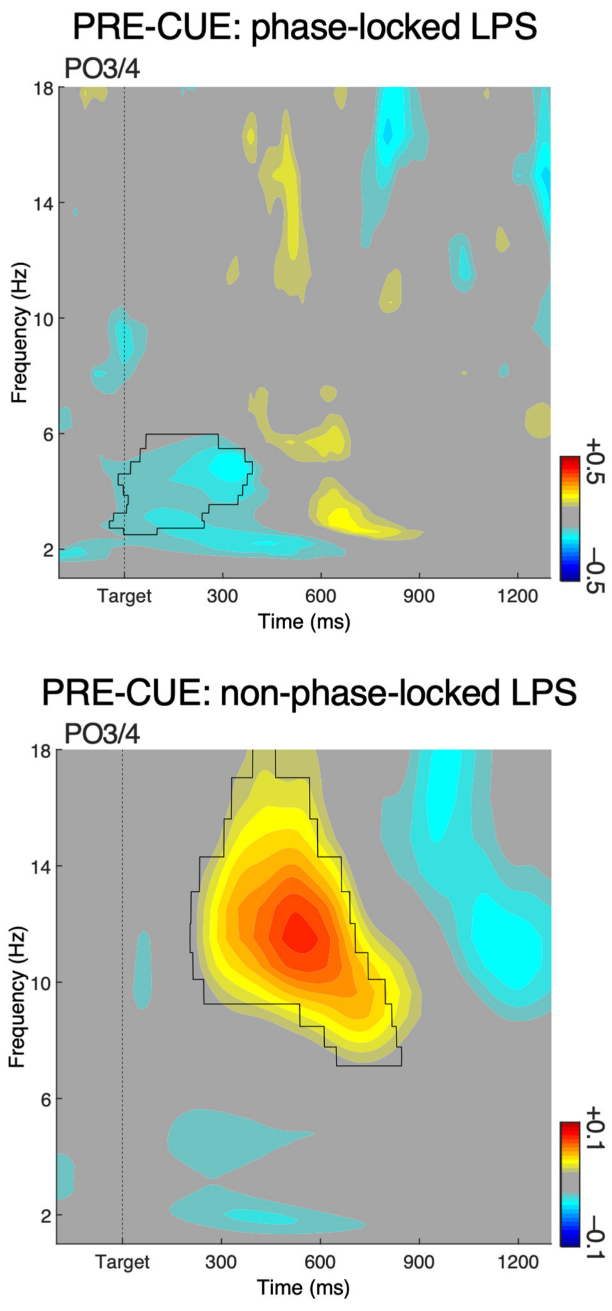

FIGURE 4
A separation of phase-locked **(upper panel)** lateralized power spectra (LPS) and non-phase-locked LPS **(lower panel)** in the pre-cue condition for the most relevant parieto-occipital electrode pair. The results show that the effect in the theta band (cf. the left panel of Figure 3) can be considered as evoked, while the effect in the alpha band can be considered as induced.

observation that the posterior alpha rhythm may actually consist of different components (Sokoliuk et al., 2019; Rodriguez-Larios et al., 2022) have complicated a straightforward answer. Moreover, a later study by Willems (2020) was not able to provide support for similar effects in the alpha band, which may be due to the employment of alternative strategies that sidestep the use of visuospatial memory. Finally, on the basis of a few studies it may be suggested that similar effects in the case of external and internal spatial attention could be present in the theta band as the relevant stimulus still needs to be selected/encoded (see Hanslmayr et al., 2009; Van der Lubbe et al., 2014; Harris et al., 2017).

We modified a previously employed task (in Van der Lubbe et al., 2014) to further reduce experimental differences and diminish the possibility of using alternative strategies. Furthermore, we used the GED technique to separate the posterior alpha rhythm in two parieto-occipital and one medial-parietal sources, and examined whether the phase coherence between these sources shows comparable effects in conditions that should highlight external and internal spatial attention. First, however, we will focus on the expected replication of previous behavioral and LPS results.

The behavioral data (see Figure 2) confirmed the presence of a Simon effect, the tendency to respond to the (former) side of the target shape, both in the pre-cue and the post-cue conditions, which replicates the results of Hommel (2002) and Van der Lubbe et al. (2014). Observing this effect strongly suggests that the location of the relevant shape plays a role not only when the shape is present, which is the common Simon effect, but also when it is retrieved from memory. This observation suggests that a spatial code is generated or reactivated when retrieving the shape. According to Van der Lubbe et al. (2012), this reflects the shift of attention toward the location of the previously presented shape. These data also nicely correspond with the gaze biases related to memorized locations reported by Van Ede et al. (2020). Thus, these behavioral data are in line with the view that external and internal spatial attention share a common mechanism. The behavioral data additionally indicated that retrieval from memory is more difficult than selection from the external world, as more errors were made in the post-cue condition. This observation may actually be taken as an argument why there is a need to reduce interference when retrieving information from memory.

As indicated before, the alpha rhythm seems to fulfill this role of reducing interference (Jensen and Mazaheri, 2010; Foxe and Snyder, 2011; Klimesch, 2012; Benedek et al., 2014). This interference may not only arise from external stimuli (i.e., distractors) but also from internal neural activity (see Rodriguez-Larios et al., 2022). Our results on lateralized alpha power, assessed with the LPS index, clearly replicated previous findings with non-spatial post-cue conditions (Van der Lubbe et al., 2014; Poch et al., 2017). A reduction in contralateral vs. ipsilateral posterior alpha power was indeed observed in both the pre-cue and the post-cue conditions (see Figure 3), suggesting that neural activity related to the irrelevant side was suppressed and/or neural activity related to the relevant side was facilitated. Thus, we were able to show that these effects can also be observed in the case of a longer time interval in the post-cue condition. The magnitude of the lateralized effect appeared slightly larger in the pre-cue condition. This observation, however, should not be interpreted as increased relevance of this underlying process for external spatial attention, as very likely the temporal variability of the suppressive effect is simply smaller in the pre-cue condition (for a similar argument, see Van der Lubbe et al., 2014), which implies overall a larger lateralized effect. Nevertheless, we observed some small differences in the topographies of the lateralized effects, with a slightly more anterior focus in the post-cue condition. This potential difference

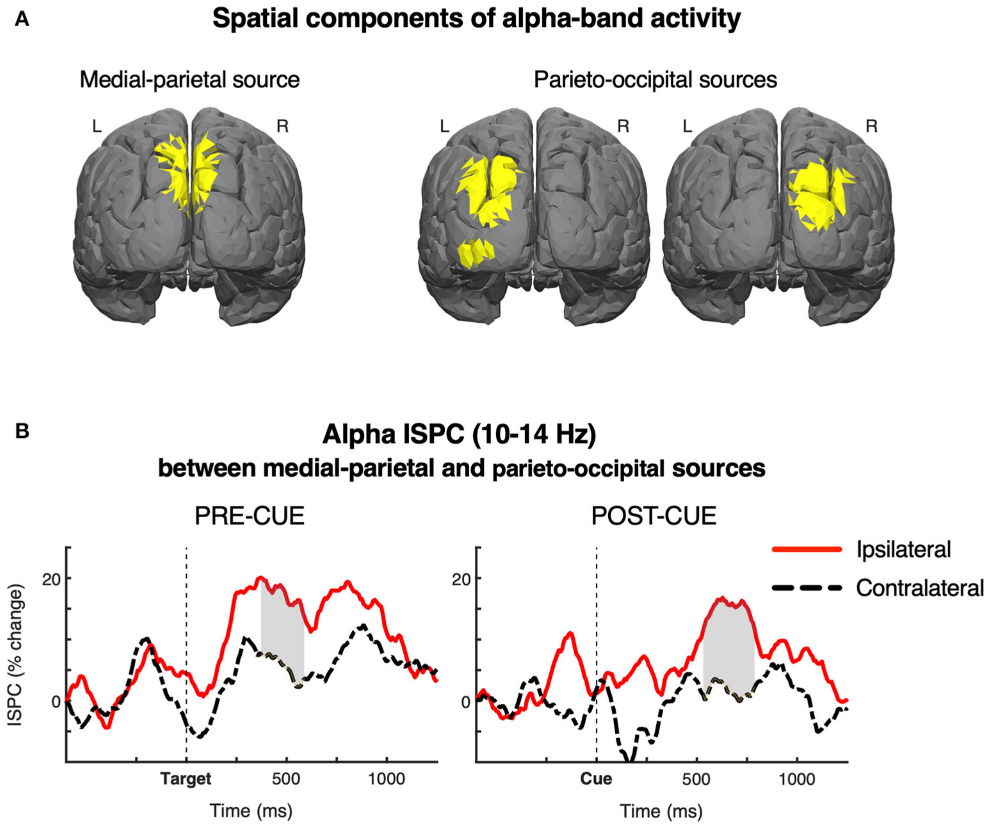

FIGURE 5
(A) Source components of the alpha-band activity. The components were isolated using a multivariate spatio-spectral filtering based on the generalized eigenvalue decomposition (GED), and visualized by correlating the obtained forward models with a leadfield matrix generated using a standard boundary element method model in the Matlab Brainstorm toolbox. The medial-parietal areas were isolated in the first, strongest GED component. The second-largest component of GED isolated the parieto-occipital areas, ipsilaterally to the left and right targets, respectively. **(B)** Source-level alpha inter-site phase coherence (ISPC) between the medial-parietal and parieto-occipital sources for the two conditions. Dashed black and solid red lines show the ISPC, respectively from the ipsilateral and contralateral hemispheres, relative to the target visual field. Gray fills indicate the time-points where the ipsilateral-contralateral difference was significant in permutation tests (cluster-level corrected for multiple comparisons). The depicted results showed significant target-related lateralizations of alpha ISPC in both cue conditions, suggesting that both external and internal spatial attention entail an increased inter-regional connectivity ipsilaterally to the target visual field.

was not statistically assessed, so, we decided not to speculate on possible reasons. The current LPS results appear more pronounced than in the study of Van der Lubbe et al. (2014). This may be due to the implemented changes in the current experiment, an increase in the number of participants, and improved procedures for analyzing the EEG. Clearly, the absence of lateralized effects in the study of Willems (2020) seems to be an exception, also for the longer time interval between the stimulus display and the post-cue, and is indeed likely due to the use of alternative strategies.

With the use of the GED technique, it was possible to separate the posterior alpha rhythm in a medial-parietal source and two sources in lateral parieto-occipital cortex (see Figure 5A). The subsequently estimated source activities in the alpha band were used to examine the connectivity between the medial-parietal and the parieto-occipital sources for the two cue conditions, where we contrasted ipsilateral with contralateral connectivity (see Figure 5B). In both cue conditions, we observed increased ipsilateral vs. contralateral connectivity in the time windows wherein the LPS effects were demonstrated, suggesting that this small parieto-occipital network plays a role in reducing interference. These findings provide new support for the idea that external and internal spatial attention share their underlying mechanism. Still, some extra caution is needed when interpreting connectivity results. As the sources are quite close to each other, one could argue that the observed effects might reflect the effect of volume conduction across the scalp. In principle, this potential problem is much smaller when determining source-connectivity (Schoffelen and Gross, 2009) as the GED procedure should separate different activity patterns (de Cheveigné and Arzounian, 2015; Cohen, 2022), especially in conjunction with the Laplacian spatial filtering that additionally attenuates volume conduction artifacts (Cohen, 2015a,b). Importantly, in both cue conditions, the observed ipsilateral inter-site phase coherence had non-zero phase-lags, which provides further evidence for true connectivity (A spurious effect due to volume conduction would display a zero phase-lag). Moreover, similar alpha-band connectivity between the parietal and occipital areas has previously been demonstrated for external spatial attention (Siegel et al., 2008; Capotosto et al., 2015; Liu et al., 2016; Lobier et al., 2018), which aligns with the present results. Thus, to conclude, our findings indicate that the connectivity between medial-parietal and parieto-occipital areas is highly comparable when selecting visual information in the outside world and when retrieving "visual" information from our inner world.

We also observed larger theta power at contralateral vs. ipsilateral sites but only in the pre-cue condition (see Figure 3, left panel; see also Van der Lubbe et al., 2014). Increased posterior theta power has been interpreted as a reflection of target encoding (Hanslmayr et al., 2009) and signal enhancement (Harris et al., 2017), while a recent study suggested that it is also related to conflict detection in the Stroop task (Haciahmet et al., 2023). Results of another recent study (Marturano et al., 2021) indicate that the lateralized effect in the theta band[2] may actually be the spectral counterpart of the N2pc (e.g., Eimer, 1996) or PCN (posterior contralateral negativity) component (e.g., Van der Lubbe et al., 2001), which is a lateralized ERP component usually interpreted as the allocation of attention to a lateral target (e.g., see Bacigalupo and Luck, 2019; Forschack et al., 2022). The idea that the observed effect in the theta band is related to the N2pc would imply that it concerns an evoked rather than an induced effect. To explore this issue further, separate LPS analyses were performed on evoked (phase-locked) and induced (non-phase-locked) activity. Results of these analyses (see Figure 4) confirmed that the contralateral vs. ipsilateral increase in posterior theta power concerns an evoked effect, which favors the idea that this effect may be the spectral counterpart of the N2pc.

The N2pc was present in both external and internal spatial attention conditions in the study of Kuo et al. (2009). They presented four-stimulus displays that were either preceded by a to-be-searched target or followed by a match-to-sample target. One possibility is that the signal-to-noise ratio in the current study was too low to clearly demonstrate effects in the theta band in the post-cue condition. Indeed, in the study of Van der Lubbe et al. (2014), increased contralateral vs. ipsilateral theta was present in both the pre-cue and the post-cue conditions (see their Table 2), which aligns with the findings from Kuo et al. (2009). Nevertheless, with match-to-sample targets (where the target is repeated) the retrieval process is rather easy as it only requires recognition, while in the current study, only the color of the target is cued, which makes the retrieval process more similar to recall. Furthermore, in the current study the time interval between offset of the stimulus display and onset of the post-cue was twice as long (2 vs. 1 s) as in the study of Van der Lubbe et al. (2014). The latter difference may be responsible for the absence of the theta effect in our post-cue condition. Future studies may very well-confirm the earlier findings from Van der Lubbe et al. (2014), who demonstrated increased contralateral vs. ipsilateral theta power in the post-cue condition.

The demonstration of highly comparable data patterns, either based on behavioral or neuroimaging measures in conditions, that highlight external and internal spatial attention may not be considered as decisive. Strong support, however, would be obtained if interference of processing in medial-parietal cortex (e.g., with transcranial magnetic stimulation [TMS]) would deteriorate both external and internal spatial attention. The report on neglect patients from Bisiach and Luzzatti (1978) relates to this suggestion. They described two patients that suffered from unilateral neglect following brain injuries in the right hemisphere. The patients were not only ignoring the left part of their visual field but were also unable to recall buildings from the left side of a famous square in Milan when they were imagining to be at a specific spot, even though they could recall those previously ignored buildings when they imagined to move to the opposite side of the square. The problems of these patients are obviously not related to visual impairments but to the ability of attending to both actual and virtual space, and seem related to damage in right parietal cortex. These early findings already support the idea that there is overlap between spatial attention and the possibility to recall from visuospatial memory, and that parietal areas play a crucial role in these selection processes.

In our introduction, we mentioned that in the study of Willems (2020) several participants may have used a strategy [if the post-cue has color C1/C2 then response R1 (left), else response R2 (right)] that no longer relied on visuospatial memory, which was held responsible for the absence of a clear lateralized effect on posterior alpha power in the post-cue condition. For the current study, such a strategy seemed unlikely, as a four-choice task was employed. However, also in the current experiment, participants could have invented an alternative strategy. Upon presentation of the two-stimulus display, they might pre-select the two out of four possible responses related to the two stimuli [e.g., left middle finger (R1), and right index finger (R3)], and relate one of the responses with its color [e.g., R1–C1 (e.g., blue)]. At the moment of presenting the post-cue, they might notice if the color (C1) relates to R1, and if not they could simply respond with R2. As our data displayed the expected effects, it seems that this strategy was not or at least not often employed. Nevertheless, there are probably several conditions wherein the retrieval of previously presented visual information may no longer involve visuospatial memory. One reason why this might happen is when the amount of visual information presented is simply too much and the viewing time is too short. This was obviously the case in the experiments reported by Sperling (1960) and maybe also in the study of Willems (2020). It may also be easier to recode the information presented, for example, by using semantic labels. In such a case, participants would still be able to properly report the presented information but no longer rely on a visuospatial representation. This idea might imply that the capacity of visual working memory may be even smaller than the average of four elements as proposed by Cowan (2001).[3]

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[2] Interestingly, Bastos et al. (2015b) revealed that in the primate visual system, feedforward effects (from primary sensory to higher order areas) are related to the theta band, so, one could argue that our effect, and results focusing on the N2pc/PCN are related to projections from lower to higher visual brain areas. Van der Lubbe et al. (2014) already suggested that "the PCN may be characterized as a reflection of evoked posterior increased contralateral θ power" (p. 187). Van der Lubbe et al. (2016) additionally confirmed that the amplitudes of early visual ERP components strongly relate to modulations in alpha and theta power.

[3] Recent studies actually indicate that a conceptualization of the capacity in terms of number of elements (i.e., the so-called slot theory) is inappropriate (e.g., Ma et al., 2014) and may better be formulated in terms of available resources, as it appears that the preciseness of memories (e.g., a specific color, orientation or size) decreases in a gradual way, and not in an all or none manner.

Although the current study focused on short-term memory, it is relevant to know that there are indications that some of the observed effects may extend to episodic memory, which is an important component of long-term memory. Waldhauser et al. (2016) used an approach that resembles the approach employed by Kuo et al. (2009). During an initial encoding phase, participants were instructed to either simply judge the size of a laterally presented object or to memorize the object. After an intermediate distractor task to prevent rehearsal of the previously presented objects, participants took part in the retrieval phase. During that phase, they were presented with old and new objects that were now centrally presented. They first had to indicate whether the presented object was old or new, and subsequently were asked whether the object was presented to the left or the right. EEG[4] measured during the retrieval phase showed a reduction in alpha and beta power contralateral to the encoding position of the old objects, in line with the idea that retrieval of the object reactivated sensory information during the encoding phase. They also revealed that repetitive TMS that interfered with this contralateral change deteriorated memory retrieval, suggesting that this contralateral reduction is indeed crucial for the ability to recall whether a presented object was old or new. Thus, even after a very long time interval, retrieval may involve visuospatial memory. This observation suggests that the distinction between short-term memory and long-term memory is less strict or at least different than commonly assumed (for relevant theoretical ideas, e.g., see Oberauer, 2013).

An issue ignored in the current study is individual differences. There are not only individual differences in the capacity of visuospatial memory but also individual differences in peak frequency of the alpha rhythm. In the study of Klimesch et al. (1993) participants that had better memory performance had a higher individual alpha frequency, while bad performers displayed a larger reduction in alpha power than good performers. A distinction between different individual alpha rhythms might therefore provide even clearer results. In this respect, the study of Rodriguez-Larios et al. (2022) is also very informative, as they reported that not all of the participants showed their Alpha 1 or Alpha 2 effects. Clearly, future studies will need to focus more on individual differences (e.g., see Pahor and Jaušovec, 2016).

In conclusion, the current study provides new support for the view that external and internal spatial attention rely on a shared neural mechanism. This mechanism may be related to a medial-parietal to parieto-occipital local network as connectivity between these areas was demonstrated in conditions that highlight external and internal spatial attention. Results from previous studies—an increase in ipsilateral vs. contralateral posterior alpha power and a Simon effect in both attention conditions—were replicated, while an increase in contralateral vs. ipsilateral posterior theta power could only be demonstrated for external spatial attention.

Data availability statement

Data files used for the analyses reported in the paper will be made available from the DANS repository under: https://doi.org/10.17026/dans-x2m-ug9s.

Ethics statement

The studies involving human participants were reviewed and approved by the Ethics Committee of the Faculty of Behavioral, Management, and Social Sciences at the University of Twente (request number 210676). The patients/participants provided their written informed consent to participate in this study.

Author contributions

The study was conceptualized by RV and DA. Design and implementation was performed by RV, DA, and BP. The data were acquired by IJ. IJ, BP, DA, and RV analyzed different aspects of the data. BP and DA wrote the first and final draft of the Sections 2, 3. RV wrote the first and final draft of the the Sections 1, 4 and finalized the draft for submission. All authors commented on earlier drafts. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

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[4] One could argue that this second instruction is actually responsible for these lateralized effects as participants may already be orienting toward the remembered side, this alternative account, however, does not explain the observations from their second experiment.

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How attention and knowledge modulate memory: The differential impact of cognitive conflicts on subsequent memory—A review of a decade of research

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In order to cope with cognitive conflicts, attention and knowledge are required. In some conditions, cognitive conflicts can boost subsequent memory and in other conditions, they can attenuate subsequent memory. The goal of the present study is to provide a narrative review of studies from the last decade in which Stroop or flanker conflicts, task switching, perceptual disfluency or semantic incongruence were manipulated at study. We propose an integrative framework considering attentional mechanisms and knowledge structures. Attentional mechanisms can refer to conflict resolution, which is required to explain the memory benefit for incongruent stimuli in Stroop and Flanker paradigms. Attentional mechanisms can also refer to attention allocation, which is required to explain the memory cost for targets and the memory benefit for task-irrelevant distractors in task-switching paradigms. Moreover, attention allocation policies can also account for the inconsistent results for perceptual disfluency manipulations. Prior knowledge is required to explain effects of semantic congruency and incongruency: Information that is expected, or congruent with prior knowledge, is better remembered, namely by pre-existing schemata. Moreover, information that is unexpected or incongruent with prior knowledge attracts attention and is better remembered. The impact of prior knowledge on memory performance thus results in a U-shape function. We integrate the findings according to this framework and suggest directions for future research.

KEYWORDS

cognitive conflict, attention allocation, consolidation theory, schema-congruence theory, conflict-monitoring hypothesis, load theory of selective attention

1. Introduction

From the moment we wake up, we employ attention and prior knowledge to reach goals and navigate successfully through the day. For every task like getting dressed, making coffee, and driving to work, we focus on task-relevant information while ignoring task-irrelevant information. Different task-sets can sometimes conflict when we need to do two tasks at the same time (e.g., getting dressed and monitoring the time) or when we switch tasks. Cognitive conflicts can also arise when a situation does not match with our prior knowledge (e.g., handling a new coffee machine), or with our expectancies (e.g., the postman bringing our parcels at a specific time) or when we face incongruent information (e.g., green traffic light signals us to drive but a pedestrian is crossing the street). In the laboratory, cognitive conflicts are induced with dual-task paradigms, task-switching paradigms, violation of expectancies (prediction errors), perceptual disfluency or incongruent trials in conflict paradigms as the Stroop or the Flanker task. During task performance, all these conflicts slow us down compared to the condition without (Rogers and Monsell, 1995; Wylie and Allport, 2000; Egner and Hirsch, 2005; Bugg, 2008; Kalanthroff and Henik, 2014). Although the effects on immediate task performance are similar, the consequences on long-term memory vary substantially. The aim of this review is to examine the underlying cognitive mechanism promoting memory costs and gains.

We review research from the past decade on the effects of different encoding manipulations involving conflict on subsequent memory. For inclusion, a basic requirement was that a study included trial-unique stimuli and a kind of conflict in the study phase and that memory for these stimuli was measured in a subsequent test phase. We integrate these findings in a framework that consists of two overarching main factors—*attentional mechanisms* and *knowledge structures*. Attentional mechanisms can explain memory effects produced by dividing attention, task switching, Stroop and Flanker like conflicts, the attentional boost effect and perceptual disfluency. This part of our framework is mainly based on the load theory of selective attention, explaining memory costs (Lavie, 2005, 2010), enriched with attentional enhancements, explaining memory benefits (Diemand-Yauman et al., 2011; Swallow and Jiang, 2013; Krebs et al., 2015; Ptok et al., 2021; LaPointe et al., 2022). Knowledge structures can explain memory effects produced by prior knowledge (i.e., schemata), novelty and prediction errors. This part of our framework is based on schema theory of memory and consolidation theories (von Restorff, 1933; Wang and Morris, 2010; Van Kesteren et al., 2012; Gilboa and Marlatte, 2017). Accordingly, memory performance follows a *U*-shape function with information that is congruent with prior knowledge and information that it novel at the endpoints of a continuum (Greve et al., 2019; Quent et al., 2021).

In 2012 the first study was published in the line of research which is the core of our review. In their seminal study, Richter and Yeung (2012) investigated how cognitive control influences memory encoding by applying a task-switching procedure in the study phase. In 2015, two studies investigated the impact of Stroop like conflicts on subsequent memory performance (Krebs et al., 2015; Rosner et al., 2015a). As these studies found opposing effects on memory, the debate about why *more* cognitive control can result in both, memory gains and memory losses, was launched and inspired multiple follow-up studies. Thus, our review focuses primarily on relevant behavioral studies from the past decade.

The literature of the other paradigms is somewhat older. The research on divided attention started in the 90's and originated from the episodic and declarative memory research (Tulving, 1985; Gardiner and Parkin, 1990; Craik et al., 1996; Naveh-Benjamin et al., 1998; Yonelinas, 2002). The studies mainly focused on the comparison of encoding and retrieval effects, and on different types of memory. The research on knowledge structures has some origins in the educational and learning literature. Exploring which learning condition leads to the most efficient way of acquiring and consolidating new information is an important research topic since decades (Craik and Lockhart, 1972; Slamecka and Graf, 1978). This research also focused on pre-existing knowledge (i.e., schemata; Bjork and Allen, 1970; DeWinstanley and Bjork, 2004; Cepeda et al., 2006; Roediger and Karpicke, 2006; Rohrer and Taylor, 2007). Interestingly, the literature on perceptual disfluency also stems from the educational literature, as some researchers investigated the hypothesis that making study materials perceptually more difficult can promote sustainable learning (Hirshman et al., 1994; Sungkhasettee et al., 2011). The effects stemming from challenging encoding conditions were subsumed under the label "desirable difficulties" (Bjork and Bjork, 2011, 2020), emphasizing that more effort at study leads to better learning and memory (Craik, 2002; Staresina and Davachi, 2006; Staresina et al., 2009; Bjork and Kroll, 2015). The reviewed literature on knowledge structures moreover has its roots in neuroscientific and animal research on memory formation, consolidation and reconsolidation (Moscovitch et al., 2005; Tse et al., 2007; Wang and Morris, 2010; McClelland, 2013).

We present a narrative review of several lines of rather heterogenous research which we integrated in a framework. However, the main focus of our review remains on the task-switching and conflict studies which directly tested the implications of the load theory of selective attention and the conflict-monitoring hypothesis (Lavie and Cox, 1997; Botvinick et al., 2001, 2004; Lavie, 2005). The other paradigms have been reviewed elsewhere (Bennett and Flach, 1992; Wang and Morris, 2010; Van Kesteren et al., 2012; Swallow and Jiang, 2013; Weissgerber and Reinhard, 2017). An overview of the reviewed studies is presented in a Supplementary Table 1.

In the first part, we provide theoretical considerations associated with attentional mechanisms and we include studies that used different paradigms to induce cognitive conflicts. Figure 1 depicts an overview of different conflict paradigms. We use the term conflict for all types of encoding conditions which impair immediate task performance, such as dividing attention, task-switching, attentional boost, Stroop and Flanker conflicts and perceptual disfluency. In the second part, we provide theoretical considerations associated with knowledge structures and review research on the impact of prior knowledge and information that is incongruent with pre-existing knowledge and thus unexpected. We include studies that manipulated schema-congruence, novelty or induced prediction error. Our main goal was to review the relevant behavioral literature to provide an integrative view of the findings. As mentioned above, we started with the task switching and Stroop conflict studies, but then realized that the related paradigms need to be considered for the sake of completeness.

2. Attentional mechanisms

Allocating attention influences what we remember, and what we remember guides our attention (Becker and Rasmussen, 2008; Chun and Johnson, 2011). Attention and memory are therefore inextricably intertwined. Interactions between the two became the focus of research in the last decade and this field of research is

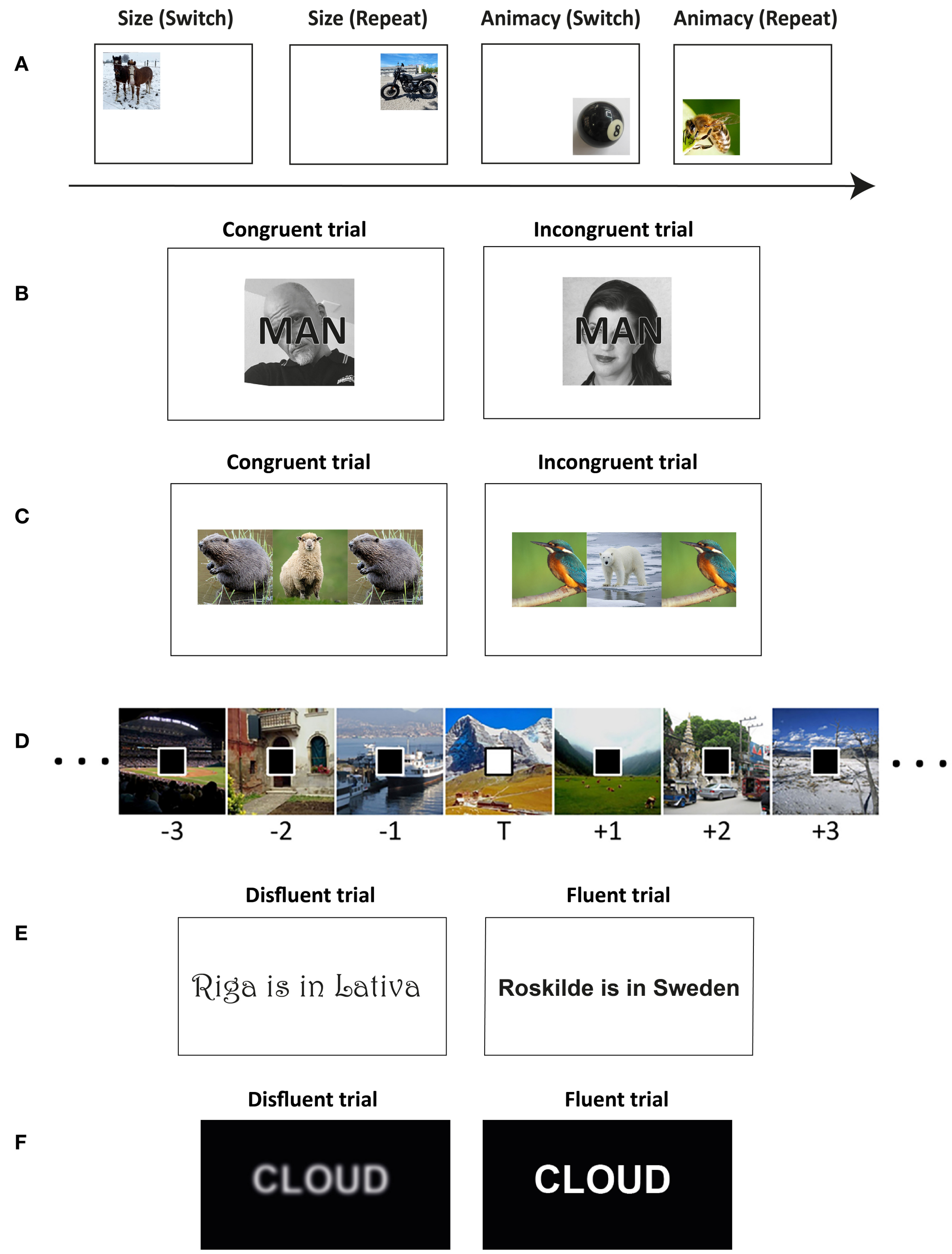

FIGURE 1
Conflict paradigms. **(A)** Task-switching paradigm (cf., Muhmenthaler and Meier, 2019). **(B)** Stroop like conflict (cf., Krebs et al., 2015). **(C)** Flanker like conflict (cf., Muhmenthaler and Meier, 2021b). **(D)** Stimulus sequence in an attentional boost study (cf., Swallow and Jiang, 2010). Examples for disfluency manipulations: **(E)** Meier and Muhmenthaler, 2021. **(F)** Rosner et al., 2015b.

growing quickly. We will start with theoretical considerations about attentional mechanisms with a focus on the load theory of selective attention as this theory proved especially suitable for explaining the effects of a variety of encoding manipulations on memory (Lavie, 2005, 2010).

A basic premise of many attention theories is that attention is limited in capacity (Driver, 2001; Oberauer, 2019). In order to attain one's goals through focused and goal-oriented behavior, attention must be selective (Otten et al., 2002). Combining early and late selection processes, the theory of selective attention states that perceptual and cognitive processing demands determine the selectivity of attention (Lavie, 2005; Tsal and Benoni, 2010). An intriguing assertion of this theory is that perceptual processing and cognitive processing both have capacity limits but exhausting these capacities would lead to opposing effects on selective attention. As perceptual processing operates automatically and perceptual resources are used obligatorily, task-irrelevant distractors are processed automatically when perceptual load is low. When perceptual load is high, however, the perceptual processing capacity is already exhausted by processing task-relevant information, thus leading to higher selective attention toward targets (Lavie, 2005; Swallow and Jiang, 2013). Thus, perceptual load—defined by stimulus set characteristics (number, similarity, and type of presented items) and stimulus quality (e.g., readability, size, and color; Lavie et al., 2009)—*enhances* selective attention. When perceptual load is low, a second, higher-order control mechanism that actively inhibits attention to irrelevant distractors comes into play (Lavie, 2010). The efficiency of this control mechanism depends on the level of load on cognitive control functions such as working memory (Lavie, 2010; Tsal and Benoni, 2010). When cognitive load is low, attention is focused on task-relevant information and task-irrelevant distractors can be inhibited, thus selective encoding is intact. When the cognitive load is high, however, control functions are already absorbed by the target task which enables distractor intrusions, resulting in "broad" attention. Thus, cognitive load—defined by the proportion of time during which a given activity absorbs control functions (Barrouillet et al., 2007)—*reduces* selective attention.

2.1. Perceptual load

In line with the load theory of selective attention, several studies showed that various manipulations of perceptual load in a target task affect the processing of distractors (Lavie, 1995; Lavie and Cox, 1997; Brand-D'Abrescia and Lavie, 2007; Forster and Lavie, 2009). The theory predicts that higher perceptual load reduces distractor processing and thus interference from conflicting distractor information (i.e., attention is more selective). Most relevant for the present review, studies showed corresponding effects on subsequent memory (Jenkins et al., 2005; Lavie et al., 2009; Greene et al., 2017; Nussenbaum et al., 2017).

For example, Greene et al. (2017) investigated the impact of perceptual load on memory by showing video clips and measuring eye movements. Based on the premise that perceptual load reduces memory accuracy, they investigated whether the memory impairments under high perceptual load resulted from inattentional blindness or from failure to visually inspect stimuli (due to capacity limits; Lavie et al., 2009). Their participants viewed a video depicting a theft either under high or low perceptual load and then had to identify characters from the video in photographic line-ups. High perceptual load impaired participants' ability to identify the peripheral character (witness) but not the central character (thief). There was no effect of perceptual load on number of ocular fixations on the witness, time to first fixation or total visit duration. The authors concluded that memory impairments under high perceptual load were due to attentional failures rather than differences in visual search. Thus, this study confirmed that the perceptual load is the key factor in determining the locus of selective attention. When capacity limits of perception are reached, distractors in the periphery are not encoded, resulting in selective attention and memory.

Nussenbaum et al. (2017) also investigated the impact of perceptual load by exploring the effects of distractor number and content on memory. The authors asked participants to identify a target image from among zero, one, or three distractor images and to categorize the target as "alive" or "not alive." A subsequent recognition memory test assessed memory for target and distractor images. Results of Experiment 1 showed that target memory was *worse* in the one-distractor condition (low perceptual load) than in the three-distractor condition (high perceptual load). In contrast, distractor memory was *better* in the one-distractor condition, suggesting that a higher perceptual load leads to more selective memories (better target and worse distractor memory). Experiment 2 extended these findings by showing that a single distractor hurts target encoding more than three distractors, especially when the response associated with the distractor conflicts with the response to the target. This suggests that distractor number and content matters: As the number of distractors increases, distractor interference decreases probably due to the higher perceptual load leading to more selective attention to the target and better inhibition of distractors. In other words, when perceptual load is low, cognitive load takes effect. In the next sections we will discuss the effects of cognitive load on memory in more detail.

2.2. Cognitive load

Cognitive load is high when demands on cognitive control processes increase, for example due to working memory load or dual-task interference (Oberauer, 2019). Studies using divided attention or task-switching paradigms showed corresponding effects on memory (Lavie, 2005, 2010; Swallow and Jiang, 2013; Dubravac and Meier, 2022). In the following we will review these studies.

2.2.1. Divided attention

Effects of divided attention during encoding on later memory performance are widely documented. It is well-established that engaging in two tasks simultaneously results in impaired long-term memory (Gardiner and Parkin, 1990; Craik et al., 1996, 2018; Naveh-Benjamin et al., 1998; Fernandes and Moscovitch, 2000; Greene and Naveh-Benjamin, 2022). In a typical divided-attention procedure, participants have to perform two discrimination tasks at the same time, often involving different modalities (e.g., Middlebrooks et al., 2017; Greene and Naveh-Benjamin, 2022). The main finding is that dividing attention results in costs on subsequent memory performance.

For example, in a study by Craik et al. (2018), participants had to perform a word/non-word discrimination task in the primary task. In the divided-attention condition, half of the participants had to perform either a visual or an auditory continuous choice reaction time task. Across five experiments, the results demonstrated that dividing attention resulted in lower memory than full attention. The authors reasoned that dividing attention during encoding affected multiple types of processing, resulting in an overall degraded memory trace, rather than interrupting any particular process. In other terms, keeping two concurrent tasks active required additional control processes, which impaired target memory (Lavie, 2010; Oberauer, 2019).

In a study by Uncapher and Rugg (2005), the mechanisms underlying dividing attention were investigated by manipulating secondary task difficulty. In the primary task, an animacy decision to visually presented study words had to be performed, the second task was to perform either an easy or a hard auditory monitoring task. The authors demonstrated that memory was worse when the words were encoded under hard secondary condition compared than under easy secondary task condition. This finding further corroborates that enhancing the cognitive load (hard vs. easy secondary task) reduces subsequent target memory.

2.2.2. Task switching

The task-switching paradigm has been developed to address the mechanisms of cognitive control by comparing task switch and task repetition trials (Rogers and Monsell, 1995; Wylie and Allport, 2000). On switch trials, reaction times are usually slower and error rates higher. These switch costs are highly robust and reflect the cognitive load imposed by the requirements to update the new task set and to select the appropriate task (Kiesel et al., 2010).

To investigate the memory consequences of task switching, Muhmenthaler and Meier (2019) presented participants with trial unique stimuli which they had to classify according to one of two categorization tasks in alternating runs (ABBA). In Experiment 1, participants classified images of animals as a mammal or bird (task A) and images of objects as a musical instrument or kitchen utensil (task B). These stimuli were univalent because animals could only be classified according to task A and objects could only be classified according to task B. In Experiment 2, participants classified images along the dimensions size (task A) and animacy (task B), thus turning them into bivalent stimuli (see Figure 1A). In both experiments, a subsequent surprise memory test assessed participants memory of the stimuli. The results showed that memory was worse for stimuli presented on switch trials. Critically, the effect size was three times larger in Experiment 2 with bivalent stimuli. The results indicate that task-set reconfiguration (present in both experiments) and between-task interference (present in Experiment 2) both impair subsequent memory performance. Critically, increasing cognitive load by additional between-task interference increased the switch cost for target memory, in line with the load theory of selective attention (Lavie, 2010).

Richter and Yeung (2012) investigated the question whether reconfiguration or between-task interference were critical for subsequent memory effects. The authors suggested that reconfiguration would divert resources away from encoding, thereby resulting in a general memory decline for task-relevant and task-irrelevant information. However, between-task interference would result in lower memory for *task-relevant* items, but higher memory *for task-irrelevant* items, due to residual attention to the former task (Monsell and Driver, 2000; Yeung et al., 2006). In order to disentangle between these possibilities, the authors used compound stimuli which consisted of pictures and words and participants had to switch between classifying them. Thus, each trial consisted of task-relevant (target) and task-irrelevant (distractor) information. In line with the between-task interference account, task switching impaired memory for task-relevant information but improved memory for task-irrelevant information. The authors concluded that task switching reduced the ability to encode the targets selectively and to inhibit distractors, that is, task switching resulted in impaired *memory selectivity*.

Chiu and Egner (2016) further developed the idea that between-task interference was crucial for the memory benefit for distractors in switch trials. Toward this goal, they examined two distractor categories. In one group, participants switched between two classification tasks, the distractors were relevant in one task and irrelevant in the other task. In the other group, the distractors (objects in the background) were never task-relevant (thus they were unrelated background pictures). The results showed better memory for distractors that were relevant in one task on switch compared to repeat trials, aligning with the finding by Richter and Yeung (2012): When the task switches, attention is broad, resulting in distractor intrusion. When the task is repeated, attention can be focused on the targets, thus less distractor intrusion. In contrast, the condition with the *always* irrelevant distractors resulted in lower memory for the distractors in switch compared to repeat trials. The authors concluded that the higher cognitive load in switch trials reduced memory performance. As the distractors were never task-relevant, this decline in memory performance must be seen as a memory *cost* and not as a successful strategy to inhibit distractors.

Building on this work, Dubravac and Meier (2022) directly tested whether higher cognitive load would lead to less selective memories. They employed a similar procedure as Richter and Yeung (2012, 2015) with picture-word pairs as stimuli. According to the time-based resource sharing model (Barrouillet et al., 2007), cognitive load is higher when attention demanding activities co-occur in time and concurrently draw on limited cognitive control resources that are necessary for selective attention (Lavie, 2010). Following this definition, the authors varied the cognitive load by manipulating task predictability, preparation time, and stimulus presentation duration in cued (Exp. 1–3) and alternating runs task switching procedures (Exp. 4 and 5). In all experiments, task switching resulted in lower memory selectivity. Shorter preparation times, however, reduced memory selectivity only in the cued task switching paradigm (cf., Richter and Yeung, 2012; Exp. 1), but not in the alternating runs task switching paradigm. This result suggests that a cue triggers preparatory processes that—given a long enough cue-to-stimulus interval (i.e., preparation time)—alleviate cognitive load during the classification task as task-set reconfiguration processes were preponed. Shorter stimulus presentation durations also reduced memory selectivity. With shorter stimulus presentation durations, the stimuli had to be held active in working memory to solve the classification task, thus increasing cognitive load during the task. This effect occurred mostly on switch trials, when cognitive load was already high. This further corroborates that co-occurring cognitive load results in lower memory selectivity.

Together, the studies about divided attention and task switching showed that cognitive load plays an important role. In *low* cognitive load conditions, such as full-attention or task repetitions, attention can be focused on the targets, which leads to improved target memory and lower distractor intrusion. In contrast, in *high* cognitive load conditions, such as divided-attention or task switching, the control mechanism is absorbed by other control processes which leads to broader attention (Lavie, 2010; Dubravac and Meier, 2022). Consequently, target memory is reduced and irrelevant distractors intrude. Further studies on divided attention and task-switching are in line with this interpretation and support the cognitive load theory (Gardiner and Parkin, 1990; Craik et al., 1996, 2018; Richter and Yeung, 2015; Brito et al., 2016; Dubravac and Meier, 2020; Muhmenthaler and Meier, 2021a, 2022; Greene and Naveh-Benjamin, 2022; Naveh-Benjamin et al., 2022).

2.3. Transient shifts of attention and cognitive load

2.3.1. Conflict stimuli

Another line of research investigated the impact of conflict stimuli on subsequent memory performance (Krebs et al., 2015; Rosner et al., 2015a; Jiménez et al., 2020; Muhmenthaler and Meier, 2021a,b). It was suggested that detecting conflicts can enhance target memory (Botvinick et al., 2001, 2004; Yeung et al., 2004; Carter and Van Veen, 2007). According to the conflict-monitoring hypothesis, detecting conflicts leads to a transient upregulation of selective attention in anticipation of the next trial, resulting in improved conflict resolution (Egner and Hirsch, 2005). In this line of research, conflict stimuli are defined as stimuli which involve simultaneously active, competing representations which point to different responses (Egner et al., 2007; Bugg, 2008). Conflicts usually slow down immediate task performance and increase the error rates.

According to the load theory, selective encoding should be optimal under low cognitive load (Lavie, 2010; Tsal and Benoni, 2010). As conflict resolution *increases* the demands on control processes, the load theory would predict *lower* memory performance for conflict stimuli, however the opposite is true. Several studies showed that due to an up-regulation of attention, conflict stimuli can enhance later memory performance (Krebs et al., 2015; Rosner et al., 2015a; Ptok et al., 2019; Davis et al., 2020; Muhmenthaler and Meier, 2021a,b). Importantly, this up-regulation is only possible when the processing demands are low, that is, under low cognitive load. When the processing demands increase, interference is stronger than the attentional enhancement, thereby eliminating any beneficial effects on memory. In the following paragraphs, we review studies which investigated conflict stimuli in the light of these considerations.

Krebs et al. (2015) investigated the impact of conflict stimuli on memory using a variant of a face-word Stroop task. In the study phase, the participants performed a gender discrimination task with male or female faces which were overlaid with the words *man*, *house*, or *woman*, thus congruent, neutral, and incongruent face-word stimuli were created (see Figure 1B). At study, the participants had to judge the gender of the face while ignoring the superimposed word. In the subsequent recognition memory test, faces from incongruent trials resulted in better memory performance than faces from congruent or neutral trials. The authors concluded that the results were in line with the conflict-monitoring hypothesis postulating that interfering information triggers a top-down reinforcement and enhances attention to the targets (Egner and Hirsch, 2005; Egner et al., 2007).

In a replication attempt, Jiménez et al. (2020) found no evidence for conflict-enhanced memory. However, they found a memory benefit for incongruent trials following incongruent trials, suggesting that conflict over two successive trials might be necessary to boost target memory. In the studies by Krebs et al. (2015) and Jiménez et al. (2020), the same procedure was used, but only Krebs et al. (2015) found conflict-enhanced memory. A reason might be that the inter-stimulus interval was up to 7 s in Krebs et al. study, as the experiment was conducted in the scanner, and 1 s only in the Jiménez et al. (2020) study. In the latter study, conflict effects may have been eliminated due to reduced processing time, that is, due to a high cognitive load. The cognitive load of a task can be seen as a function of the proportion of time during which it captures attention, thus impeding other attention-demanding processes, such as conflict resolution (Barrouillet et al., 2007). If conflict resolution is hampered, no memory benefit of conflicts can be expected.

In a study by Rosner et al. (2015a), the participants had to read the red word in a pair of red and green spatially interleaved words. They were instructed to read the red word aloud and to ignore the green distractor. Half of the items were congruent (the interleaved words had the same identity), and the other half were incongruent (the interleaved words had different identities), thus the incongruent condition triggered a conflict. Following the reading phase, participants completed a surprise recognition memory test. The results showed better memory performance for incongruent trials. The authors interpreted their findings in terms of selective attention processes which improved memory for incongruent trials.

Davis et al. (2020) extended this line of research by investigating context effects in a series of experiments. Overall, they replicated the results by Rosner et al. (2015a) and moreover demonstrated that blocked lists of congruence led to stronger subsequent conflict effects than mixed lists. This is in line with the finding that the memory benefit for incongruent trials occurred only on successive incongruent trials (Jiménez et al., 2020). A noteworthy feature of the studies by Rosner et al. (2015a) and Davis et al. (2020) was that the cognitive load was low, as the task consisted of word reading, which is almost automatic (Walczyk, 2000). Moreover, the participants had plenty of time to complete the task. If cognitive resources would have been more depleted by the tasks, conflict resolution would have been hampered, thereby probably eliminating any memory effects.

Evidence for this claim stems from a recent study, in which the research by Rosner et al. (2015a) and Davis et al. (2020) was extended. In this study, the cognitive load was enhanced by combining the conflict stimuli with a task-switching procedure (Muhmenthaler and Meier, 2021a). Task switching enhances the cognitive load due to the required task-set reconfiguration and the between-task interference (Rogers and Monsell, 1995; Wylie and Allport, 2000). The authors used similar stimulus materials as Rosner et al. (2015a), but instead of word reading, they applied a semantic word classification task which further enhanced the cognitive load. In the congruent condition, the two interleaved words derived from the same category whereas in the incongruent condition, the two words derived from different categories, thus they triggered a conflict. The results showed that the incongruent condition did not lead to better recall performance than the congruent condition. The authors then reduced the cognitive load in a follow-up experiment by presenting blocked instead of mixed lists (cf., Davis et al., 2020). The results revealed better memory for incongruent targets. Moreover, the results replicated the effect that a series of incongruent trials might help to boost memory (Davis et al., 2020; Jiménez et al., 2020).

Ptok et al. (2019) investigated whether a memory benefit for conflict stimuli also occurs in a semantic priming procedure. In a series of experiments, they first showed a prime involving two words (e.g., "Kate / male"), then they showed a stimulus (e.g., "Kate"). The participants had to categorize the name by gender, then they assessed recognition memory for the stimuli. The authors varied the processing stages of the conflict and investigated whether a stage-specific focus of control demands would influence later memory. A memory benefit only occurred when the conflict lied on the semantic categorization stage (e.g., by showing "Kate / male" as prime) and not on the response stage (e.g., by showing "Kate / right"). They concluded that a memory benefit can be produced by semantic incongruency priming, but only when the primes induce a conflict at the semantic categorization stage. Moreover, in line with our account, they suggested that conflict-enhanced memory can only be observed when demands of the task are low and allow a degree of automaticity in responding.

In a recent study, the memory-enhancing effect was demonstrated by using a Flanker like conflict at study (Muhmenthaler and Meier, 2021b). In the congruent condition, participants saw three pictures, a target in the middle, and two identical flankers. Importantly, all the pictures were from the same stimulus category (e.g., three mammals). In the incongruent condition, participants saw also three pictures, but the target was from one category and two identical flankers were from another category (e.g., a mammal in the middle and two birds, see Figure 1C). Participants had to categorize the target and ignore the flankers. Different pictures in both conditions were used to eliminate potential effects of fluency or perceptual load, as in this setting, the perceptual difficulty was similar in both conditions. The results showed that the incongruent condition led to better target memory than the congruent condition, indicating that the memory-enhancing effect generalized from the Stroop to the Flanker task.

Together, the results of these studies show that cognitive conflicts such as Stroop or Flanker conflict can enhance subsequent memory performance for targets. When conflict is detected, attention is transiently enhanced, rather than reduced, as the load theory of attention would predict (Botvinick et al., 2004; Olivers and Meeter, 2008; Lavie, 2010). However, the attention enhancement is only viable under low cognitive load, that is, when processing of the target task is within the capacity limits of attention. In the next section we review the literature on the attentional boost effect. Similar to Stroop and Flanker like conflics, we propose that the emergence of the attentional boost effect strongly depends on the cognitive load imposed by the target tasks.

2.3.2. The attentional boost effect

Due to the limited capacity of attention, the load theory of attention suggests that increasing attention to one task should reduce attention to another task, as in typical divided attention studies (Naveh-Benjamin et al., 1998; Lavie, 2010; Craik et al., 2018; Greene and Naveh-Benjamin, 2022). However, several studies showed that attending to a relevant target can actually boost the perceptual processing of concurrent, but unrelated information, referred to as the attentional boost effect (Swallow and Jiang, 2010, 2013). It has been proposed that detecting a target produces a transient up-regulation of attention which enhances memory for items in close spatiotemporal proximity to the target (LaPointe et al., 2022).

In a seminal study, Swallow and Jiang (2010) asked participants to perform two continuous tasks at the same time. For one task the participants saw a series of scenes, one at a time at the center of the screen. Participants had to encode the scenes for a subsequent memory test. For a second task a stream of squares appeared superimposed over the scenes (see Figure 1D). The participants pressed a key as quickly as possible whenever a specified target square appeared. The square was completely unrelated to the scene. Then, a recognition test assessed memory for the scenes. The results showed better memory for the scenes presented with a target square than those presented with a distractor square. The authors concluded that increasing attention to a target can lead to widespread increases in perceptual processing, which enhances memory for them.

In Experiment 2 of the same study, the authors showed that auditory targets also facilitated image-encoding, thereby demonstrating that the attentional boost effect is not modality specific. In Experiment 3, the participants were instructed to memorize the scenes and to *ignore* the squares (single-task condition). In this experiment, the results showed no attentional boost effect and demonstrated that the effect depended on performing the target-detection task. Together, the authors concluded that the attentional boost effect might reflect the opening of an attentional gate, which enhances perceptual processing. In other words, target detection leads to a transient shift of attention, which later enhances memory for the unrelated scenes.

Although the occurrence of an attentional boost effect is widely documented (Swallow and Jiang, 2013; Mulligan et al., 2014; LaPointe et al., 2022), the exact underlying mechanism is not clear. As in classic divided attention studies, two tasks are performed simultaneously, but the outcomes are different. Like the conflict studies, the outcomes may depend on the processing demands associated with the two tasks, that is, on the cognitive load. In typical divided attention experiments, the tasks are rather complex, for example engaging in two discrimination tasks in different modalities (cf., Naveh-Benjamin et al., 1998; Craik et al., 2018; Greene and Naveh-Benjamin, 2022). The cognitive load is high, and up-regulations of attention are not viable. In contrast, in typical attentional boost experiments, the tasks are very simple, for example pressing the space button when a specific square appears. Processing is within the capacity limits of attention, as the tasks do not involve any higher-order control processes. The cognitive load is low and up-regulations of attention are viable (Oberauer, 2019).

This assumption is corroborated by the finding that the *single-task condition* of Experiment 3 (without targets) of the above-mentioned study led overall to better memory than the *dual-task condition* of Experiment 1 (Swallow and Jiang, 2010). Specifically, memory performance of the single-task condition was at the same level as the *peak* (when a target was detected) of the dual-task condition. This implies that overall, the dual-task condition *impaired* memory performance compared to the single-task condition, aligning with other dividing attention studies which consistently showed memory costs (Craik et al., 2018; Greene and Naveh-Benjamin, 2022). In other terms, the attentional boost lifts performance up to the level of full attention performance but not beyond (LaPointe et al., 2022). However, as the cognitive load is low in attentional boost studies, it does not exceed working memory limits, and thus target detection can lead to trial-to-trial attentional enhancements.

More evidence for this claim stems from a further experiment of the study mentioned above (Swallow and Jiang, 2010; Experiment 5). In this experiment, the target squares could be green or red. In the *simple-detection* condition, participants pressed the spacebar whenever *either* a red or a green square appeared. In the *arbitrary-mapping* condition, participants pressed one key for red squares and another key for green squares. In the simple-detection task, an attentional boost effect occurred. Critically, in the arbitrary-mapping condition, no attentional boost effect emerged. It was eliminated due to increased processing demands associated with response selecting, that is, due to high cognitive load.

2.3.3. Perceptual disfluency

Studies of perceptual disfluency on memory have their roots in the desirable difficulties account. This account suggests that making things harder to learn can *improve* subsequent learning and memory (Bjork and Bjork, 2011; Maddox and Balota, 2015; Bjork and Yue, 2016). Several studies provided evidence that perceptual disfluency, despite slowing down immediate task performance, can improve subsequent memory performance. Similar to conflict and attentional boost studies, it has been proposed that disfluency results in a transient up-regulation of attention, in order to decode the stimulus and to optimize performance (LaPointe et al., 2022). However, the impact of perceptual disfluency on memory is more inconsistent, with some studies leading to better, other to worse memory and other showing no effects. Similar to the attentional boost effect and the conflict literature, cognitive load could moderate the relationship between perceptual disfluency and memory. When cognitive load is low, an up-regulation of attention is possible, and disfluency can act as a desirable difficulty (LaPointe et al., 2022). In contrast, when cognitive load is high, there are not enough resources available to decode the disfluent stimuli, and disfluency reduces subsequent memory (Eitel et al., 2014; Meier and Muhmenthaler, 2021). In the next paragraphs, we review the literature on perceptual disfluency in the light of these considerations.

In a study by Sungkhasettee et al. (2011) the participants had to study inverted and upright words. Participants were instructed that they had to read each word and to encode the words for a later test. The authors found that inverted words were more often recalled than upright words and interpreted the results in terms of processing fluency and desirable difficulties. The cognitive load imposed by word reading was low and almost automatic, thus perceptual disfluency enhanced later memory (Walczyk, 2000).

Diemand-Yauman et al. (2011) investigated whether disfluent fonts can lead to better memory than fluent fonts. In two experiments, the participants were asked to learn facts about three species of aliens, they had 90 s to memorize 21 features. The authors found that that information presented in hard-to-read fonts was better remembered than information which was presented in easy-to-read fonts. They interpreted that hard-to-read fonts can operate as a desirable difficulty, which engendered deeper processing strategies (Bjork and Bjork, 2011). We assume that due to enough processing time, perceptual disfluency enhanced later memory.

Further studies showed that the memory-enhancing effect of disfluent fonts is not as robust as the study by Diemand-Yauman et al. (2011) implied. Seufert et al. (2017) provided evidence for a boundary condition associated with perceptual disfluency: They manipulated the disfluency level of the fonts and the results showed significant differences, indicating that there is an optimal level of disfluency on performance that leads to increased engagement. These results provided evidence that there is a breaking point of disfluency where the perceptual load begins to be too high, and disfluency begins to be an "undesirable difficulty."

Meier and Muhmenthaler (2021) provided evidence for a reversed effect of disfluent fonts on subsequent recognition memory. The authors presented different statements of the type "a *City* is in a *Country*" in fluent (e.g., **Arial**) or disfluent fonts (e.g., Harrington), and the participants had to decide whether these statements were true or not as fast and as accurate as possible (see Figure 1E). A following recognition test revealed that fluent statements were better remembered than disfluent statements. Due to high cognitive load in the study phase (deciding whether a geographic statement was true and selecting the appropriate response under time pressure), there were not enough working memory resources available to decode the disfluent statements, thus disfluency acted as an undesirable difficulty which reduced subsequent memory.

Rosner et al. (2015b) investigated the impact of clear and blurred words on recognition memory performance (see Figure 1F). The participants had to read the words aloud in their own pace. In several experiments, the results showed that blurred words were better remembered than clear words. The authors concluded that the blurred words up-regulated cognitive control, which enhanced encoding and later memory. This is in line with our account, as the cognitive load imposed by word reading was low and enough working memory resources were left to decode the blurred words.

A somehow unexpected result was reported by Yue et al. (2013). The authors investigated the impact of blurred and clear words on metacognitive predictions and subsequent memory. In five experiments, the participants had to read blurred or clear words and they had to give a judgement of learning (JOL) after each word. The participants had plenty of time for encoding the words. Thus, due to low cognitive load, we would expect an advantage for the blurred words. However, the opposite was true, the authors reported overall a small memory benefit for clear words. The JOLs were higher for the perceptually fluent items, as more easily processed information is usually predicted to be more retrievable in the future (Hirshman et al., 1994; Rhodes and Castel, 2008). Giving JOLs activates pre-existing knowledge, or schemata (Staresina et al., 2009; Meier and Muhmenthaler, 2021). This mechanism probably enhanced memory for the clear words. Thus, it seems that the metacognitive judgements overwrote the subtle effects of perceptual disfluency, despite enough available working memory resources. The study showed that small changes in the experimental designs can change or even reverse the memory effects, thus implying that it is noteworthy to thoroughly consider each manipulation.

In a recent study, LaPointe et al. (2022) combined two attentional manipulations that cause transient shifts of attention on memory, perceptual disfluency and attentional boost. They tested the hypothesis that the two attentional manipulations produce redundant effects on recognition. The participants had to read blurred or clear words while they had to monitor for and respond to target signals, whereas they had to ignore distractor signals. The results showed memory-enhancing effects for both, perceptual disfluency as well as for the target signals, however, the two factors did not interact. The authors concluded that the attentional effects for disfluency and target detection ware additive and not redundant. Moreover, the authors reasoned that sufficient resources in response to target detection and to blurry words were available in this setting, which may have led to an additive memory effect of the two variables. It would be interesting to investigate these memory effects when the resources would approach or exhaust capacity limits. To investigate combined attentional manipulations at different levels of cognitive load may be an avenue for future research (cf., Muhmenthaler and Meier, 2021a).

Together, perceptual disfluency is basically associated with a memory enhancement, but the effect depends on the level of cognitive load, and it requires an optimal level of disfluency (Seufert et al., 2017). The disfluency effect is not as robust as for example the detrimental effects of task switching and dividing attention on subsequent recognition memory. For example, Eitel et al. (2014) as well as Rummer et al. (2016) reported null effects, despite applying the same experimental manipulation as Diemand-Yauman et al. (2011). Further research is necessary to explore the impact of other possible moderators in the relationship between perceptual disfluency and later memory.

2.4. Attentional mechanisms: Preliminary discussion

Because attention is limited in capacity, it must be selective in order to reach the most relevant goals (Driver, 2001; Oberauer, 2019). According to the load theory of attention, responding to a target should increase demands on control processes, thereby reducing memory. However, the literature on Stroop and Flanker like conflicts, the attentional boost effect and disfluency showed that responding to relevant targets can transiently enhance attention, rather than reduce it, as the load theory of attention would predict (Lavie, 2005; Olivers and Meeter, 2008). When specific targets appear, transient attentional enhancements are triggered, in order to optimize performance. Importantly, the cognitive load imposed by the target task can act as a moderator in this relationship. Under high cognitive load, the capacity limits of attention are reached, and up-regulations of attention are not viable. Under low load, however, trial-to-trial adaptions are viable, thereby leading to memory gains.

Noteworthy, the tale is not that simple and other factors may further modulate subsequent memory. For example, the study by Yue et al. (2013) showed that adding a simple action to the experimental procedure can reverse the effects on subsequent memory. Giving a metacognitive judgment (i.e., a judgment of learning after each trial), reversed the memory effects in this study. Metacognition is based on prior knowledge, or schemata, which is the main topic of the next section. This results of this study foreshadows that the two components of our framework, attention and knowledge, can interact.

Of importance with regard to many studies in this field is the lack of baselines. For example, in the task-switching studies, we do not know whether task repeating enhances or decreases memory performance compared to a baseline condition. Memory performance resulting from *task-repeating* trials (trials from a task-switching block) should be compared with single-task trials (trials from a task block in which one task has to be performed repeatedly). Specifically, task-repeating trials compared to single-task trials could enhance memory due to a more cautious responding style, or due to higher motivation (Woodward et al., 2003). In contrast, task-repeating trials might decrease memory compared to single-task trials due to the higher cognitive load in these trials. Although not having direct evidence for this claim, we assume that the latter would apply. The lack of baselines is also an issue in the other conflict paradigms. The assessment of baselines should be considered in future research.

3. Knowledge structures

Our brains are optimized to remember relevant information and to quickly discard irrelevant information (Van Kesteren and Meeter, 2020). Our brains are also optimized to remember events that differ from previous experiences, as adaptation to the ever-changing world is critical for survival (Duszkiewicz et al., 2019). We preferentially attend to irregularities in the environment which may signal an upcoming danger (Reggev et al., 2018). Together, it is adaptive to remember information that is *congruent* as well as *incongruent* with prior knowledge, reflecting regularities and irregularities in the environment. It is therefore no surprise that these types of information are better remembered than neutral information.

3.1. Prior knowledge and novelty

Knowledge is represented in schemata which can be defined as networks of interconnected neocortical representations of prior knowledge. They are established in early childhood, continue to develop throughout life and allow us to make efficient judgements in an economical and adaptive way (Iran-Nejad and Winsler, 2000). Schemata exert powerful influences over how events are perceived and interpreted (Gilboa and Marlatte, 2017). They maximize the efficiency of new learning, expand memory capacity, and enable inferential processing (Bonasia et al., 2018). Events that are congruent with pre-existing schemata lead to better memory than incongruent events and this effect is stronger after a delay (Hennies et al., 2016). The *schema theory on memory* predicts that schemata enhance memory due to efficient encoding and accelerated consolidation processes (Tse et al., 2007; Wang and Morris, 2010; Van Kesteren et al., 2012; McClelland, 2013).

Novelty is also known to enhance memory (von Restorff, 1933; Bonasia et al., 2018), with the distinctiveness of a stimulus thought to improve later memory. Memory for a special event (e.g., the first kiss) can be much better than memory for events that occur repeatedly (Poppenk et al., 2010). Novelty involves responding to information that is not expected or predicted in a given context on the basis of prior experience (Van Kesteren et al., 2012; Quent et al., 2021).

According to these considerations, memory performance follows a *U*-shape, but the two ends are associated with different expressions of memory. Schema-congruence produces generalized semantic memories, reflecting schemata, whereas incongruence/novelty produces detailed episodic memory, reflecting the encoding of a "snapshot" (Quent et al., 2021). Both types of memory are mediated by different brain structures (Van Kesteren et al., 2012). In the next section, we review articles which investigated the impact of schema-congruence and novelty on subsequent memory performance.

In a study by Cortese et al. (2019), congruence effects for color-word associates were investigated in a Stroop color naming task. The participants had either to name the color of a font, or to read the word which was presented in a specific color. The words appeared in a congruent (e.g., *ocean* in *blue*), neutral (e.g., *lawyer* in *green*), or incongruent (e.g., *banana* in *blue*) manner. Then, the participants had to recall the words. The results of the memory test revealed that words which have been shown in the congruent condition were more often recalled than words from the incongruent which were more often recalled than the neutral condition. This pattern of results appeared in both tasks, font color naming and word reading. Together, the results showed the expected *U*-shape function of memory, but they also revealed that the effect for schema-congruence was stronger than the effect for novelty.

The congruence effect was further investigated by Van Kesteren M. T. R. et al. (2013). The authors explored the impact of subjective congruence involving different modalities. The participants had to study simultaneously presented combinations of visual motifs, visual object words and tactile fabric samples which were either congruent (e.g., jacket and leather) or incongruent (e.g., umbrella and lace). A recognition memory test for the motifs was administered either immediately, after 1 day, or after 2 days. The results showed that congruent stimuli were better remembered after a consolidation interval, but not in the immediate test. These results demonstrated that a consolidation phase may be a precondition for schema-congruence effects.

Van Kesteren M. et al. (2013) also investigated the impact of subjective schema-congruence on later recognition memory. They used pairs of objects and scenes at encoding, and the participants indicated how congruent they found these pairs and were tested on recognition memory for these associations 1 day later. For example, a congruent pair was *tennis court—tennis racket* and an incongruent pair was *classroom—soup ladle* (see Figure 2A). The authors found a monotonic increase in memory performance with increasing congruency ratings. They did not find evidence of enhanced memory for incongruent information. The authors interpreted the results as confirmation of the schema theory of memory (Wang and Morris, 2010; McClelland, 2013).

In the studies mentioned above, the schemata reflected prior knowledge of the participants. Hennies et al. (2016) investigated whether a schema can be induced *experimentally*, through several training sessions. The participants learned facts over six sessions during 2 weeks. Then, they learned new facts which were related or completely unrelated to the schema they had established. Directly after these sessions, memory for all facts was tested in a two-alternative task. The results revealed better memory for schema-congruent information, thus providing evidence that a schema can be established within a few days, and that this schema can lead to memory gains.

Ortiz-Tudela et al. (2017) used a change-detection task to explore long-term consequences of schema congruence. They manipulated the congruence between a changing object and a background scene (see Figure 2B). For example, they showed a cow in the prairie (congruent with prior knowledge) or a cow on the street (incongruent with prior knowledge), the background-only and the background-plus-target images were presented in rapidly alternating sequences to generate a flickering appearance. The participants had to press a specific button when they detected an object in the foreground. Across three experiments, the data showed that incongruent events were faster detected than congruent events. However, the results of the memory test revealed that schema-congruent events led to better recognition memory performance than incongruent events, providing evidence that the schema effect generalizes to other tasks, such as the change-detection task.

Bonasia et al. (2018) let their participants watch narrative film clips which contained events that were either congruent with prior knowledge or not. Memory for the events was tested either immediately or after one week. Both congruence with prior knowledge and incongruence/novelty enhanced memory for events, though incongruent events were recalled with more errors over time. The authors concluded that novel and congruent information both enhanced memory but were processed *via* distinct mechanisms. The findings confirmed that memory performance was a *U*-shape function of congruence (Van Kesteren et al., 2012). The authors emphasized that they could demonstrate these effects with more naturalistic events than usually used in the labs, such as film clips.

Greve et al. (2019) investigated the impact of expectations (i.e., schema-congruent) and events that conflict with schemata (i.e., schema-incongruent) on subsequent memory. Across four experiments, schemata were established by training relationships between randomly paired objects (e.g., a shoe and an umbrella). Thus, as in the Hennies et al. (2016) study, the schemata were induced experimentally and did not reflect prior knowledge. The participants learned which of two types of objects had a higher value (the rule). In congruent conditions, the rule remained constant across trials; in incongruent conditions, the rule reversed after the penultimate trial, that is, before the final critical trial that was later tested; in unrelated conditions, rules reversed several times. Thus, in congruent and incongruent conditions, the schema was either violated (incongruent condition) or not (congruent condition) on a critical trial. Better memory was found for both congruent and incongruent trials, relative to unrelated trials, producing memory performance that followed a *U*-shape function of congruence.

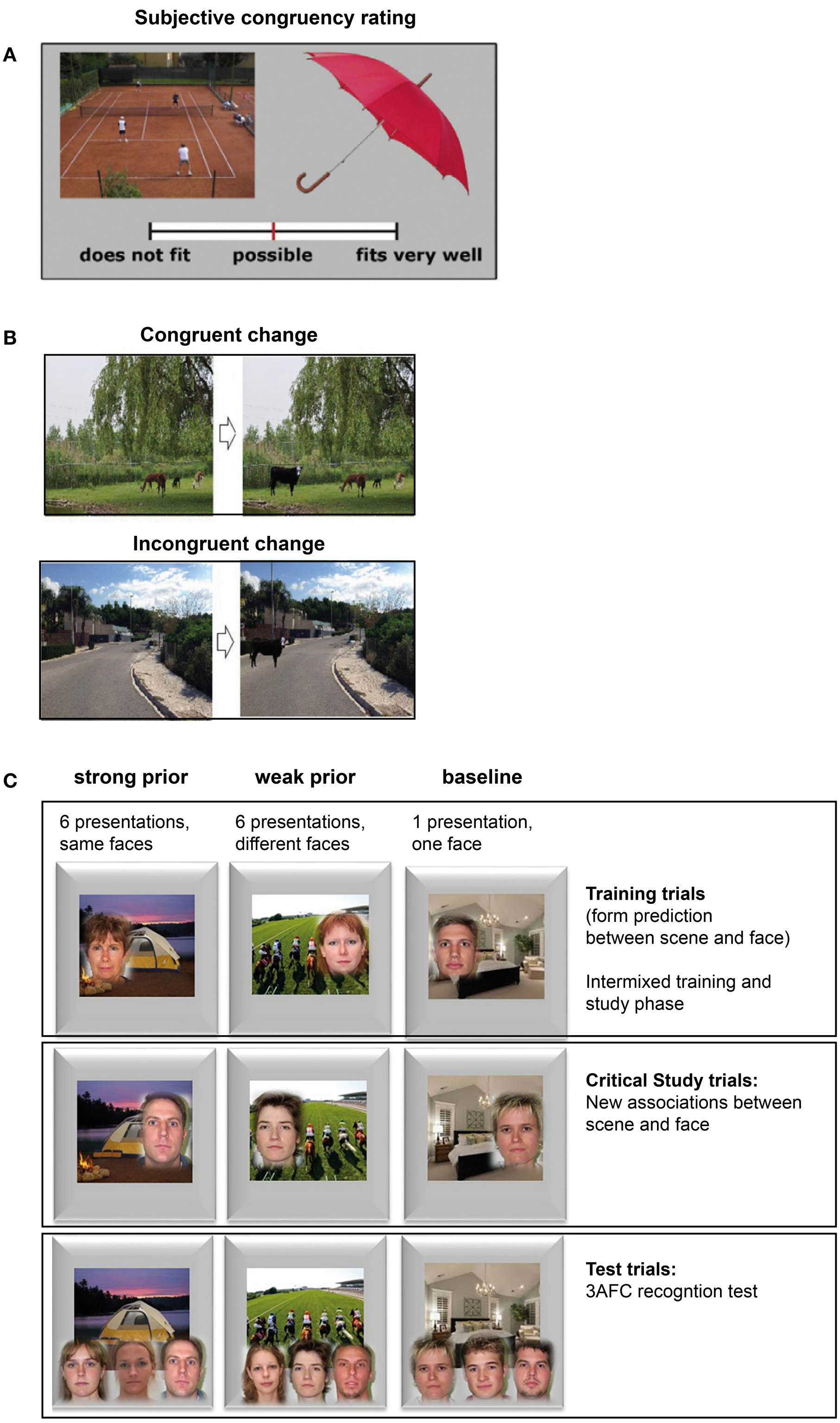

FIGURE 2
Paradigms based on knowledge structures. **(A)** Congruency rating task (cf., Van Kesteren M. et al., 2013). **(B)** Change detection task (cf., Ortiz-Tudela et al., 2017). **(C)** Establishment of weak and strong priors (cf., Greve et al., 2017).

The congruence advantage but not incongruence advantage was mediated by post-encoding processes, whereas the incongruence advantage, but not congruence advantage, emerged even if the incongruent information was irrelevant to the schema. The authors concluded that schemata augmented memory in multiple ways, depending on the match between novel and existing information.

Reggev et al. (2018) examined the role of experimental distinctiveness as a potential explanation for the memory benefits for novel and schema-congruent information. Across two experiments, they used word pairs which were either familiar (e.g., yellow banana) or novel (e.g., yellow zebra). The participants had to decide whether the word pairs were congruent or not. In a later test phase, recognition memory for the nouns was assessed. The results revealed that novelty was sensitive to its experimental proportions: improved memory for novelty was observed when novel word pairs were relatively rare. Memory levels for schema-congruent items, in contrast, were completely unaffected by experimental proportions, highlighting their insensitivity to list-based distinctiveness. The authors concluded that novel and congruent items both enhanced memory but were processed *via* partially distinct mechanisms.

The results of these studies provide evidence for a robust effect of schema-congruence on subsequent memory. The schema theory of memory explains this effect with an accelerated integration of new information into a pre-existing schema (Tse et al., 2007; Wang and Morris, 2010; McClelland, 2013; Durrant et al., 2015). Despite applying different tasks at study and applying different memory tests, the memory advantage for schema-congruent information materialized. The results also showed that a consolidation phase increases the effects, or may even be necessary for an effect to materialize (Van Kesteren M. T. R. et al., 2013; Hennies et al., 2016). The studies also provided evidence for a memory advantage for novel or unexpected events, but the results revealed that this effect is less robust than the schema effect on memory. The effect depends on the precise nature of the encoding and retrieval tasks, such as context effects (Reggev et al., 2018). In the next section we review research on the impact of prediction errors, which can be seen as a special case of novelty.

3.2. Prediction error

When our experience violates our predictions, it is adaptive to update our knowledge, in order to make better and more accurate predictions in the future (Bein et al., 2021). Theoretical models propose that such prediction errors should be encoded as distinct memory traces, reflecting the encoding of a "snapshot," to prevent that previous memories interfere (McClelland, 2013; Quent et al., 2021). For example, the "Predictive Interactive Multiple Memory Signals" (PIMMS) framework is a framework for understanding how prior knowledge and prediction errors affect memory formation (Greve et al., 2017, 2019). According to this framework, the brain is assumed to contain hierarchical representations of the world, where representations at one level of the hierarchy predict the activity of representations in lower levels. The difference between those predictions and the sensory evidence from lower levels comprises the prediction error, which is assumed to drive learning between levels, so as to improve predictions and minimize prediction errors in the future (Friston, 2005). PIMMS offers a framework for considering how prediction errors might vary in the world, and therefore be manipulated experimentally in the laboratory.

Greve et al. (2017) examined the implications of the PIMMS framework. Specifically, the authors examined whether a prediction error reflects the divergence between the prior probability (from previous experiences) and sensory evidence (from the current experience). The hypothesis was tested across five experiments, in terms of peoples' ability to encode a single presentation of a scene-item pairing as a function of previous exposures to that scene and/or item. Memory was tested by presenting the scene together with three choices for the previously paired item, in which the two foil items were from other pairings within the same condition as the target item. For example, the prior expectation was manipulated by training the participants to associate a scene with one or more unfamiliar faces (see Figure 2C). In order to induce a strong prior, a scene was repeatedly paired with the same face during training (high prediction error condition). In order to induce a weak prior, a scene was paired with different faces (low prediction error condition). Critical study trials used novel faces, evoking a higher degree of prediction error when the prior was strong. In a subsequent test phase, associative memory for faces paired in the critical study trials was assessed. The results showed, as hypothesized, better memory for the new scene-face pairing in the high prediction error condition compared to the low prediction error condition, that is, memory was best when the expectation was violated. The prediction error hypotheses were supported in all experiments. The prediction errors reflected the divergence between the prior probability and current sensory evidence, and the authors concluded that the PIMMS framework provided the most parsimonious account of the pattern of results.

In a study by Quent et al. (2021), the relationship between object-location memory and expectedness was investigated across four experiments. In an immersive virtual reality, participants explored a virtual kitchen with the instruction to count and memorize the locations of 20 objects that were scattered across the room. In the following recall phase, the participants reentered the kitchen (now without the 20 original objects), were given one object, and were asked to place it at its previously seen location. Once placed, the object disappeared, and the process was repeated for the remaining 19 objects. Recall was followed by a recognition test, performed outside the virtual reality. Each trial showed one studied object in three alternative locations, one of which was correct. Importantly, the target and two foil locations were matched in expectancy according to the normative ratings, so using prior knowledge to guess the location could not help performance. The results demonstrated better memory for highly expected and unexpected locations relative to neutral locations. The results showed that memory followed a U-shaped function of the expectancy of an event, with better memory for highly expected or highly unexpected object locations.

In a study by Bein et al. (2021), the authors investigated the impact of experience violations on memory, by repeatedly exposing participants to pairs of objects. During a prediction learning phase, the participants were presented with a stream of objects that included neighboring pairs of objects that followed each other back to back, thereby evoking predictions. Then, they violated this prediction in half of these pairs during the critical violation phase by replacing the second object in the pairs with a novel object. The following item memory test required participants to discriminate between identical old items and similar lures. The results revealed that the prediction errors enhanced recognition memory: Participants correctly identified more old items as old when those items violated expectations during learning, compared with items that did not violate expectations. Importantly, this memory enhancement was only observed when participants later showed intact memory for the related pairs which were used to establish the predictions. The authors concluded that the advantage for prediction errors was dependent on the strength of the predictions, defined as the participants' memory of the original pair. In a follow-up experiment, the authors reduced prediction strength by lowering associative binding during encoding and found that the memory advantage for violations was diminished.

Ortiz-Tudela et al. (2018) provided empirical evidence for the claim of Bein et al. (2021), suggesting that strong predictions are a necessary precondition to facilitate memory. The authors explored expectation violations by means of a validity paradigm. Across seven experiments, the authors showed participants arrows which pointed to specific directions. The participants were told that the relevant arrow (presented in a specific color) would point to the location at which the upcoming word stimulus was most likely to appear. On most of the trials, the stimulus appeared at the predicted location (expectation match), and on several trials, the stimulus appeared at another location (expectation mismatch). The authors reported evidence for a null effect of expectation violations on memory formation. We assume that using this task, the predictions were not strong enough to facilitate memory for violations (Bein et al., 2021). The spatial task might not have produced strong predictions, or priors, as the arrows my rather have been used as spatial cues. In other terms, the expectations were not based on previous memories, which may be a precondition for a memory gain.

3.3. Knowledge structures: Preliminary discussion

Taken together, the studies on schema-congruence, novelty and prediction errors provide robust evidence for the hypothesis that memory performance follows a *U*-shape, with better memory for schema-congruence at one end and incongruence/novelty at the other end of a continuum. The quality of these memories is however different and mediated by different brain structures (Van Kesteren et al., 2012; Greve et al., 2017). Schema-congruence is associated with generalized semantic memory and less hippocampal activity, whereas incongruence/novelty is associated with detailed episodic memory and enhanced hippocampal activity (Quent et al., 2021). The *U*-shape reflects the opposing demands of benefitting from reoccurring regularities to enable efficient encoding on the one hand, and on the other hand, of accommodating surprising information and irregularities in the environment, which is essential for flexible adaptation to an ever-changing environment (Greve et al., 2019).

4. General discussion

In the present article, we reviewed the differential impact of cognitive conflicts on subsequent memory. We distinguished between conflict conditions which required more vs. less cognitive control or attentional resources and conflict conditions related to the (in-)congruence of the study materials with existing schemata. Thus, we distinguished conflicts based on attentional mechanisms and conflicts based on knowledge structures.

For attentional conflicts, the reviewed studies suggest that conflicts associated with dividing attention and task switching hurt subsequent memory, due to enhanced cognitive load (Lavie, 2010). Performing two tasks simultaneously loads working memory and this leads to interference effects, which later results in reduced memory performance compared to a full-attention condition (Naveh-Benjamin et al., 1998; Greene and Naveh-Benjamin, 2022). Task switching also hurts subsequent target memory. Specifically, stimuli which have been shown in switch trials lead to worse memory than stimuli which have been shown in repeat trials, as the cognitive load is enhanced in switch trials due to higher processing demands (Muhmenthaler and Meier, 2019, 2022). The high cognitive load in switch trials leads to a "broad" attention, thereby leading to distractor intrusion (Lavie, 2005, 2010; Richter and Yeung, 2012, 2015; Chiu and Egner, 2016; Dubravac and Meier, 2022). Neuroimaging studies demonstrated that processing associated with task switching and divided attention are mostly associated to activity in the prefrontal regions and related networks in parietal regions, highly depending on specific features of the task at study (Reynolds et al., 2004; Johnson and Zatorre, 2006; Johnson et al., 2007; Niendam et al., 2012; Grange and Houghton, 2014).

In contrast, conflicts associated with Stroop or Flanker like conflicts, the attentional boost effect and perceptual disfluency can enhance subsequent memory (Swallow and Jiang, 2010; Diemand-Yauman et al., 2011; Sungkhasettee et al., 2011; Mulligan et al., 2014; Krebs et al., 2015; Muhmenthaler and Meier, 2021b). By going through the literature, the present review provided evidence that such transient trial-to-trial attentional shifts are however only viable under low cognitive load. Under high load, the capacity limits of attention are exhausted and attentional enhancements are not viable. Thus, for attentional conflicts, we identified the presence of specific target stimuli, combined with the level of the cognitive load, as the critical variables, that determine the direction of a specific effect for a particular conflict manipulation. According to the conflict-monitoring hypothesis, the dorsal anterior cingulate cortex signals the regulative components in the dorsolateral prefrontal cortex the detections of conflicts, thereby creating a feedback loop between the two components (Botvinick et al., 2004). A neuroimaging study showed that the memory benefit of incongruent trials of a Stroop like conflict was associated with activity in these structures, thereby providing neuronal evidence for the conflict-monitoring hypothesis (Krebs et al., 2015). Whether these structures are also involved in attentional boost and disfluency effects is not documented and may be a topic for future research.

For conflicts at the level of knowledge structures, a somewhat different pattern emerged. Both high congruence and high incongruence with prior knowledge can benefit memory, leading to a *U*-shape function, with better memory performance for information that is congruent with an existing schema, and better memory performance for novel or unexpected information at both ends. The quality of these memories is different. Schema-congruence is associated with generalized semantic memory whereas incongruence/novelty is associated with detailed episodic memory (Quent et al., 2021). The quality of these memories is also mediated by different neuronal structures. The neuroscientific model "schema-linked interactions between medial prefrontal and medial temporal lobe" (SLIMM) proposes a time-dependent shift from medial temporal lobe to neocortical representations (Van Kesteren et al., 2012; Quent et al., 2021). Within SLIMM, the medial prefrontal cortex is to detect the congruency of new information with existing information in neocortex. Greater congruence leads to greater medial prefrontal cortex activity, which is assumed to potentiate direct connections between neocortical representations. In contrast, associations that are incongruent with a schema or highly novel are encoded in the medial temporal lobe, comprising hippocampus, perirhinal and entorhinal cortices, and parahippocampal gyrus (Van Kesteren et al., 2012; Quent et al., 2021).

The different patterns within each domain underline the usefulness of the conceptual distinction of our framework. The conceptual distinction is represented in the involvement of different neural substrates, as specified above. Overall, attentional conflicts activate areas in the prefrontal cortex and related networks (Johnson et al., 2007; Niendam et al., 2012). In conditions of conflict resolution, the anterior cingulate cortex has been located as critical structure which signals the dorsolateral prefrontal cortex to recruit attentional resources when conflicts are detected (Botvinick et al., 2004). In contrast, for conflicts at the level of knowledge structures, the medial temporal lobe with the hippocampus and the medial prefrontal cortex are the most relevant structures (Van Kesteren et al., 2012; Quent et al., 2021). Thus, the two parts of our framework, attention and knowledge, are not only distinct in terms of theories, paradigms and outcomes, but also in terms of the involved neuronal structures.

Recently, Craik and Bialystok (2006) proposed a similar distinction in a framework to address cognitive changes across the lifespan. Specifically, they distinguished between "cognitive representation" and "control" as factors that have different lifespan trajectories, with stronger decline across adult age in control than in representations. Combining the ideas from Craik and Bialystok with our distinction of cognitive conflict domains, an avenue for future research would be to test the hypothesis that conflicts at the level of attentional mechanisms would result in stronger age-effects than conflicts at the level of knowledge structures.

Another avenue for further research is to investigate the combined impact of attention and knowledge on subsequent memory. We are not aware of studies which manipulated both, prior knowledge and cognitive control demands, with the purpose to explore their interactions. In the attentional section, we emphasized the importance of cognitive load at study. However, we believe that cognitive load would not moderate memory effects resulting from knowledge or expectation manipulations, as these manipulations do not load on working memory, and thus they do not stress the limited attentional resources. As mentioned above, these manipulations are mediated by different neuronal structures (Van Kesteren et al., 2012; Quent et al., 2021). The same might apply for perceptual load. Thus, we would not expect interactions between cognitive/perceptual load and knowledge manipulations, but rather additive effects (e.g., processing schema-congruent information in a full-attention condition could maximally improve memory). However, there is evidence that pre-existing schemata can overwrite the memory-enhancing effects of attentional manipulations. In the study by Yue et al. (2013), the pattern of results showed that higher metacognitive judgments for fluent words eliminated the expected memory benefit for disfluent words. The results imply that the effects of schema-congruence might be stronger than the effects produced by perceptual disfluency. It is also conceivable that in some conflict studies, schema congruence somehow counteracted the incongruence effects (e.g., a yellow banana is schema-congruent, but a blue banana is Stroop-incongruent, both promoting better memory), thereby maybe explaining the rather small memory effects produced by Stroop like conflicts (Jiménez et al., 2020; Muhmenthaler and Meier, 2021a). The example nicely shows that it is important to carefully design these experiments. Together, there is evidence that the two components, attention and knowledge, can however interact and further affect memory.

It can be considered as a limitation of the present review that we did not discuss the types of memory tests that were used. Rather, we focused on the encoding manipulations. We are aware that the specific test may influence the resulting memory effects. However, so far, the evidence indicates that the direction of memory effects is quite robust across different memory tests. For example, in the domain of task switching, different methods have resulted in convergent evidence (Richter and Yeung, 2012; Muhmenthaler and Meier, 2019, 2021a), as switch costs on memory generalized across recognition, confidence ratings and free recall as memory measures. Nevertheless, addressing the impact of cognitive conflict systematically across different memory tests may be an interesting avenue for future research (e.g., free recall vs. recognition; explicit vs. implicit memory, etc.).

Another limitation may be that we mainly focused on studies which used short study-test intervals, although we are aware that the study-interval may also play a crucial role for memory effects, due to post-encoding and consolidation processes (Wang and Morris, 2010; Van Kesteren et al., 2012; Durrant et al., 2015). The literature on schema-congruence showed that a consolidation phase may even be a precondition for some memory effects (Van Kesteren M. T. R. et al., 2013). Nevertheless, systematically addressing the impact of different retention intervals (and more generally, time for consolidation) may be a fruitful line for future research.

5. Conclusion

In this review, we distinguished between cognitive conflicts at the level of attentional mechanisms and at the level of knowledge structures. We illustrated that in both domains the specific encoding situation must be taken into account to foresee whether the particular conflict results in a loss or gain for subsequent memory performance. Besides of ordering these phenomena on a theoretical level, these insights may help to create study situations to optimize learning. Toward this goal it might be relevant to investigate the combined effect of beneficial cognitive conflicts identified in this review.

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MM, MD, and BM wrote the manuscript. All authors approved the final manuscript for submission.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

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Temporal attention

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Attention, that is, the ability to focus processing resources on a specific part of sensory input, is often thought of as being mainly allocated in space, toward specific objects or modalities. However, attention can also be allocated within time. Temporal attention can be induced implicitly, that is, through learning of temporal regularities between at least two events, or explicitly, by the active instruction to attend to a specific time point. Moreover, temporal attention can be induced *via* external rhythmic stimulation. Many studies throughout the last 120 years have shown that, irrespective of the way temporal attention is induced, it facilitates processing of upcoming events. Based on early findings measuring reaction time, researchers initially assumed that temporal attention primarily accelerates motor processing. Within the last 20 years, however, more and more evidence has emerged supporting the view that temporal attention facilitates perceptual processing. Moreover, temporal attention may even interact with other attentional domains such as spatial attention or feature-based attention. In the present article, we summarize classical as well as current findings and theoretical accounts on how temporal attention affects perceptual processing. Furthermore, we sketch current challenges and open questions in research on temporal attention.

KEYWORDS

temporal attention, foreperiod, perceptual processing, temporal cueing, entrainment, cross-modal processing, spatial attention, feature-based attention

Time as attention domain

Attention has the basic function of focusing processing resources on a subset of sensory input (e.g., Johnston and Dark, 1986; Desimone and Duncan, 1995; Summerfield and Egner, 2009; Carrasco, 2011). It can be assumed that without that selective mechanism, humans would be overloaded by sensory input and would therefore be unable to interpret the world in a meaningful way or act in a goal-directed manner. A large body of experimental research has shown that attention can operate in different domains such as space, sensory modalities or sensory features (for reviews, see, e.g., Johnston and Dark, 1986; Carrasco, 2011; Klein and Lawrence, 2012), and on different levels of representations, ranging from low-level sensory representations such as orientation to higher-level object-like representations (e.g., Johnston and Dark, 1986; Chen, 2012). One of the most fundamental attention domains is that of time (see also Lawrence and Klein, 2013), though it has been much less studied than others. Allocating attention in time, typically referred to as *temporal attention*, can be broadly defined as the allocation of processing resources toward a specific time point (e.g., Lawrence and Klein, 2013; Nobre and Rohenkohl, 2014). This process was described as early as 1874 in pioneering work by Wundt as "*vorbereitende Spannung der Aufmerksamkeit*" (preparatory tension of attention) (Wundt, 1874, p. 737). This allocation of attention within time is typically based on some type of implicit or explicit temporal contingency between successive sensory stimuli, with one stimulus allowing anticipation of the subsequent one(s). Like the allocation of attention to locations in space, temporal attention leads to a processing benefit for stimuli presented at the attended moment in time (e.g., Lange et al., 2003; Correa et al., 2006a; Bausenhart et al., 2007; Rolke, 2008; Jepma et al., 2012; Rohenkohl et al., 2012).

Temporal attention's mechanisms and beneficial effects have already been summarized in several reviews published over the last decade (e.g., Nobre and Rohenkohl, 2014; Nobre and Van Ede, 2018). The goal of the present article is not to provide a fully comprehensive overview of all facets of temporal attention research;[1] instead, we will focus on the *effect*(s) of temporal attention on stimulus processing. Herein, we will put particular emphasis on perceptual processing and the main lines of research in this subfield of temporal attention research: In the first two sections, we will provide a summary of the most common experimental paradigms as well as a recap of the history of temporal attention research. In doing so, we will also point out some of the methodological differences among experimental paradigms. In the third and the fourth sections, we will focus on the facilitatory effects of temporal attention on perceptual processing, and we will discuss both, studies examining effects of temporal attention within and across modalities and studies examining its interactions with other attention domains. In the fifth section, we will then summarize current theories on the mechanisms by which temporal attention specifically facilitates perceptual processing. We will conclude our review by outlining some unresolved issues and providing an outlook on possible future directions in temporal attention research.

Experimental paradigms in temporal attention

Several experimental approaches or paradigms have been developed throughout the history of temporal attention research (see Figures 1A–C; for an overview see Rolke and Ulrich, 2010; Lawrence and Klein, 2013; Nobre and Rohenkohl, 2014). Even though these paradigms differ in several aspects and may even involve distinct mechanisms, all of them rely on the (explicit or implicit) temporal contingency between two or more successive sensory stimuli. The common observation in all these paradigms is that the processing of a stimulus presented at a temporally attended moment in time is facilitated relative to the processing of a stimulus presented at a temporally unattended or less-attended moment in time (see Figure 1D).

The oldest and most basic type of experimental paradigm in the study of temporal attention is the *foreperiod paradigm*: In this paradigm, a warning signal is presented before an imperative stimulus which typically requires some kind of overt response (typically a speeded keypress). The crucial manipulation is the length of the temporal interval between warning signal and imperative stimulus, which is called the foreperiod. In the *blocked* (or *constant*) *foreperiod paradigm*, the foreperiod remains constant within a block of trials but varies across blocks; in the *variable foreperiod paradigm*, the foreperiod varies randomly from trial to trial. In both variants, the effect of temporal attention on processing of the imperative stimulus is quantified as the difference between at least two different foreperiod lengths, that is a "short" foreperiod, which is often around or <1 s long, and a "long" foreperiod between 1 and 4 s (e.g., Rolke and Hofmann, 2007; Steinborn et al., 2010; for studies including wider foreperiod ranges see, e.g., Klemmer, 1956; Bertelson and Tisseyre, 1969; Müller-Gethmann et al., 2003), with reaction time (RT) to the imperative stimulus being the most common dependent variable. Interestingly, the result pattern observed when measuring RT differs strikingly between blocked and variable foreperiod paradigms (for a review, see, e.g., Niemi and Näätänen, 1981). In the blocked foreperiod paradigm, an increase in foreperiod length typically leads to an increase in RT, except for very short foreperiods between 50 and 150 ms (e.g., Klemmer, 1956; Karlin, 1959; Bertelson and Tisseyre, 1969; Müller-Gethmann et al., 2003). In contrast, in the variable foreperiod paradigm, RT typically decreases with increasing foreperiod length (e.g., Drazin, 1961; Los and Van den Heuvel, 2001; Los et al., 2001; Los and Heslenfeld, 2005; Steinborn et al., 2008; see also Niemi and Näätänen, 1981). Furthermore, this descending foreperiod-RT function in the variable foreperiod paradigm is qualified by sequential effects (e.g., Los and Van den Heuvel, 2001; Los et al., 2001; Van der Lubbe et al., 2004; Steinborn et al., 2008; for a review see, e.g., Los, 2010). Specifically, when analyzing the variable foreperiod effect not only as a function of the current foreperiod N, but also as a function of the foreperiod in the previous trial $N-1$, it has been shown that RT for a short-foreperiod trial N is longer if the foreperiod in trial $N-1$ was long than if it was short (e.g., Los et al., 2001; see also Los et al., 2014). Furthermore, it has been shown that the slope of the foreperiod-RT function in the variable foreperiod paradigm depends on the type of foreperiod distribution: Specifically, a descending foreperiod-RT function is typically observed if foreperiods are sampled with equal probability (so-called aging foreperiod distribution). In contrast, if the probability of short foreperiods is increased (so-called non-aging foreperiod distribution) so that the subjective overall probability of the different foreperiods is equalized, the foreperiod effect on RT is strongly attenuated or even eliminated (e.g., Baumeister and Joubert, 1969; Nickerson and Burnham, 1969; Näätänen, 1971).

The discrepancy in results across blocked and variable foreperiod paradigm, that is, an ascending as opposed to a descending foreperiod-RT function, has been attributed to different underlying mechanisms: Specifically, the RT increase in the blocked foreperiod paradigm is assumed to be the consequence of an imperfect time-keeping ability (Treisman, 1964; Gottsdanker, 1975; Näätänen and Merisalo, 1977) in the sense that the estimation of longer foreperiods tends to be less accurate (Treisman, 1964; Gibbon, 1977). Therefore, even though the temporal contingency between warning signal and imperative stimulus remains constant in each block of trials, long foreperiod blocks nonetheless come with a higher degree of temporal uncertainty because the exact moment in time the imperative stimulus will be presented can be estimated less accurately. This results in less efficient focusing attention to that moment in time in blocks with a (rather) long

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[1] We will not cover functional imaging and electrophysiological studies focusing on the process of orienting attention in time itself and its associated neural correlates. In that respect, interested readers are referred, for instance, to Coull (2004) and Mento et al. (2015; for a summary, see also Nobre and Van Ede, 2018). Furthermore, we will also not further discuss recent theoretical and computational approaches that link temporal attention to learning mechanisms; in that respect, we refer to the insightful work of Los et al. (2017; see also Salet et al., 2022).

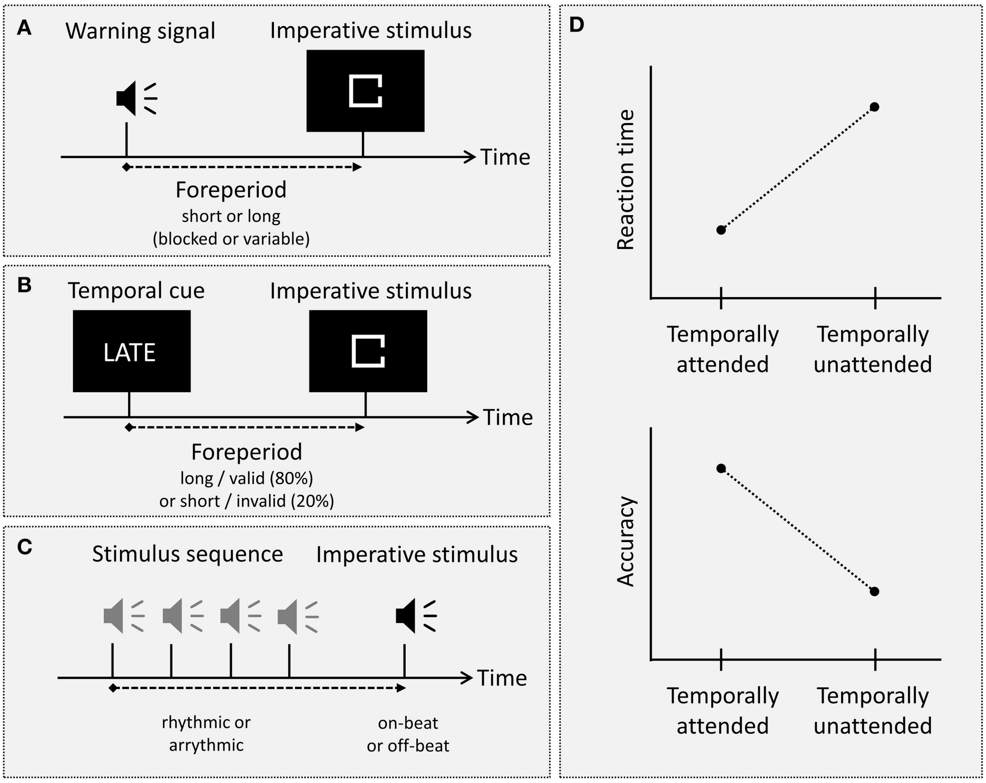

FIGURE 1
(A–C) Illustration of three major types of experimental paradigms in temporal attention research, that is, the foreperiod paradigm, probabilistic temporal cueing, and a typical rhythmic temporal attention paradigm. In the foreperiod paradigm **(A)**, a warning signal precedes the imperative stimulus by either a (blocked or variable) short or long foreperiod. In probabilistic temporal cueing **(B)**, an explicit temporal cue either validly or invalidly announces the occurrence of the imperative stimulus after a short or long foreperiod. In a typical rhythmic temporal attention paradigm **(C)**, the imperative stimulus is preceded by a rhythmic (or an arrhythmic) stimulus sequence and/or is presented either in accordance with the rhythm ("on-beat") or not ("off-beat"). **(D)** The typical beneficial effect of temporal attention, exemplified for the dependent variables reaction time and accuracy: Responses are faster and more accurate for temporally attended as compared to temporally unattended stimuli.

foreperiod than in blocks with a (rather) short foreperiod.[2] In the variable foreperiod paradigm, participants experience a different type of temporal uncertainty because of the foreperiod's variation from trial to trial (see also Klemmer, 1956). Here, the exact moment of occurrence of an imperative stimulus in a current trial is uncertain because it cannot be predicted directly on grounds of the previous trial. Nonetheless, the temporal onset of the imperative stimulus can still be anticipated using the passage of time during the foreperiod. Specifically, the more time that has elapsed since the presentation of the warning signal without the imperative stimulus being presented, the higher the probability that the imperative stimulus will be presented at the next moment in time (also referred to as hazard function). As a result of this increasing stimulus occurrence probability, more and more resources will be allocated toward the next possible moment in time, leading to faster responses with increasing foreperiod length (see also Niemi and Näätänen, 1981). Although the hazard function can account for the classic variable foreperiod effect, it does not provide a straightforward explanation for the above-described sequential effect because it is agnostic to the contribution of previous trials to the foreperiod effect. Consequently, some authors have advocated the ideas that different, and potentially independent, mechanisms might be involved in the two effects. For instance, in a developmental study, Vallesi and Shallice (2007) showed that sequential effects were present in participants of a young age (4–5 years old), whereas the variable foreperiod effect itself appeared some years later. Moreover, Mento and Tarantino

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[2] In the context of the foreperiod paradigm, it is important to consider that warning signals do not only trigger temporal attention, but also lead to a temporary increase in response readiness, most often termed as (phasic) *alertness* (e.g., Posner and Petersen, 1990; Weinbach and Henik, 2012; but see Lawrence and Klein, 2013, for a different terminology). Although alertness has been shown to lead to similar effects on perception (e.g., Matthias et al., 2010) and response selection (e.g., Weinbach and Henik, 2012) as those reported for temporal attention, both concepts are nonetheless dissociable (for a systematic discussion see Weinbach and Henik, 2012): For instance, functional imaging evidence suggests that temporal attention and alertness are associated with differential brain activation patterns (Hackley et al., 2009). Furthermore, it has been shown that one diagnostic effect of alertness, that is, an increase in congruency effects in response-conflict tasks, is mainly restricted to short FPs (e.g., Weinbach and Henik, 2013), and that alerting effects are observed even when the temporal contingency between a warning signal and the imperative signal required for temporal attention to emerge is eliminated (Weinbach and Henik, 2013; see also Lawrence and Klein, 2013).

(2015) reported that children were able to combine different hints to focus attention in the variable foreperiod paradigm only at an age above 8 years. These studies suggest that the sequential effect—in contrast to other temporal attention mechanisms—might reflect rather implicit or automatic mechanisms, which might be available at an early age. Further support for this assumption came from electrophysiological studies (e.g., Mento, 2017) and studies comparing temporal attention effects between healthy and atypical populations (e.g., Mento et al., 2019). In contrast to a dual-process account, other authors have advocated the idea of a single mechanism accounting for the classic variable foreperiod effect and the sequential effect. For instance, Los and Van den Heuvel (2001) assumed that both effects are caused by trace conditioning mechanisms (see also Los and Heslenfeld, 2005). Furthermore, Los et al. (2014, 2017); see also Mattiesing et al., 2017) more recently proposed that both effects are signatures of memory traces stored in long-term memory, which are formed and continuously updated on grounds of past experiences. Importantly, and irrespective of the difference in the assumed mechanisms within the variable foreperiod paradigm and the difference between variable and blocked foreperiod paradigms, both paradigms share the common feature that they entail what might be called a “gradual” manipulation of temporal attention: Due to the temporal contingency between warning signal and imperative stimulus, the presentation of the warning signal initiates an intentional (or unintentional) process of resource allocation toward the temporal onset of the imperative stimulus, but the degree of this allocation will depend on the precision of the temporal estimation process as well as the degree of temporal uncertainty.

A second type of paradigm temporal attention research, which has become increasingly popular over the last 20 years, is the *temporal orienting paradigm*. In this type of paradigm, participants are provided with explicit information about which foreperiod to expect or which interval to attend. The most widely used variant is the *(probabilistic) temporal cueing paradigm*, which can be characterized as a temporal analog of the spatial cueing paradigm (e.g., Posner et al., 1973). In this paradigm, a temporal cue provides explicit information about when to expect the imperative stimulus. Like in the spatial cueing paradigm, the crucial manipulation in temporal cueing paradigms is the validity of the cue: Specifically, the temporal information provided by the cue is correct (valid) in about 75–80% of the trials, whereas it is incorrect (invalid) in the remaining trials. Furthermore, in some studies, valid temporal cues have been contrasted against neutral cues, that is, cues that do not indicate a specific foreperiod (e.g., Coull et al., 2016; Korolczuk et al., 2018). For instance, Coull et al. (2016) compared blocks in which the cue indicated one of four foreperiods with 100% validity with blocks in which the cue did not provide any information about the foreperiod. Typically, the temporal information provided by the cue is symbolic (e.g., one of two colors or forms, with one indicating a short and the other one indicating a long foreperiod; Miniussi et al., 1999; Jepma et al., 2012; Rohenkohl et al., 2014), but verbal information has also been used (e.g., Correa et al., 2006a,b). Furthermore, the cued temporal information (i.e., whether the cue indicates a short or a long foreperiod) can been presented in a way that the information changes from trial to trial (e.g., Miniussi et al., 1999; Correa et al., 2004) or remains constant for a given block of trials (e.g., Correa et al., 2004, Experiment 2; Correa et al., 2006a). Using a slightly different approach, some researchers have employed a *temporal Hillyard paradigm* (e.g., Lange et al., 2003; Lange, 2009; see also Lange and Röder, 2010), which, like temporal cueing, can be characterized as an analog to selective attention paradigms being used in other attentional domains (e.g., Hillyard et al., 1973). In this paradigm, participants are presented with either a short or long temporal interval, each being marked by an onset and offset marker. In separate blocks, participants are instructed to attend to offset markers following one temporal interval (e.g., only the short interval), and to respond to those offset markers that deviate in a basic feature (e.g., their pitch) from the other offset markers. Accordingly, the critical manipulation in this paradigm is not stimulus occurrence probability (i.e., the probability that the imperative stimulus will appear after a short or long foreperiod), but the task-relevance of a specific time point (i.e., whether the offset marker is presented at the to-be-attended time point or at an unattended time point).

Like the foreperiod paradigms, temporal orienting paradigms typically lead to a processing benefit: For instance, RT to imperative stimuli following valid temporal cues is faster than RT to imperative stimuli following invalid temporal cues (e.g., Correa et al., 2010).[3] This temporal cueing effect has been observed irrespective of whether the cued temporal information varies from trial to trial (e.g., Griffin et al., 2002; Correa et al., 2004, Experiment 2) or remains constant within a block (e.g., Correa et al., 2004; Experiment 2; Correa et al., 2006a), although there is some evidence that block-wise cueing effects might be larger for some tasks (Correa et al., 2004). Similarly, in the Hillyard paradigm, a processing benefit is observed for stimuli presented at the to-be-attended time point relative to those presented at an unattended time point (e.g., Lange et al., 2003). One crucial difference between temporal orienting paradigms and foreperiod paradigms is that in the former paradigms an imperative stimulus can occur at an unattended moment in time, either because participants attend to a different interval or because it is presented earlier than indicated by the temporal cue; in contrast, in the latter paradigms a rather gradual allocation of attention can be assumed and attention is not explicitly oriented away from any time point. Furthermore, probabilistic temporal cueing differs from foreperiod paradigms as explicit temporal information is conveyed by the cue, whereas in the foreperiod paradigms the temporal information is conveyed rather implicitly, that is, through the temporal regularity that exists between the warning signal and the imperative signal.

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[3] It should be noted that, in temporal cueing, the performance benefit for imperative stimuli following valid temporal cues is not necessarily symmetric across foreperiods. Instead, it is typically weaker (or even absent) for the (relative) longer of two foreperiods (see, e.g., Coull and Nobre, 1998; Correa et al., 2006a). This asymmetry in temporal cueing has been explained in terms of a re-orienting of attention in the invalid long foreperiod condition (e.g., Coull and Nobre, 1998): If the cue invalidly announces a short foreperiod, but the imperative stimulus does not occur after that foreperiod, participants may strategically re-orient attention toward the next possible moment of stimulus presentation, that is the long foreperiod. Consequently, an imperative stimulus in the invalid long foreperiod condition might be attended to a similar degree as one in the valid long foreperiod condition.

Another experimental paradigm that has become increasingly popular over the last few years is the *rhythmic temporal attention paradigm*. The key principle of this paradigm is to present a rhythmic sequence of signals rather than a single warning signal (or cue) before an imperative stimulus. The imperative stimulus is then presented in temporal accordance with the rhythm ("on-beat") or at a different time point ("off-beat"). Furthermore, depending on the specific study, it is either presented as part of the rhythmic sequence (e.g., Lange, 2009; Rohenkohl et al., 2012) or at its end, with an additional variable foreperiod being interspersed between the rhythmic sequence and the imperative stimulus (e.g., Sanabria et al., 2011; Triviño et al., 2011; Breska and Deouell, 2014, 2017). In this type of paradigm, it has been shown that processing of stimuli presented in accordance with the rhythm is facilitated (e.g., Sanabria et al., 2011; Bolger et al., 2013; but see Elbaz and Yeshurun, 2020). Like temporal orienting paradigms, rhythmic temporal attention paradigms thus entail a comparison of conditions in which an imperative is either presented at an attended moment in time or at an unattended moment in time. However, deviating from all other temporal attention paradigms, the anticipatory process that drives attention toward a specific time point is not based on a discrete preceding event (a cue or warning signal), but on a repetitive sequence of stimuli. Consequently, rhythmic paradigms have been given a special status in temporal attention research, and some studies suggest that they may involve mechanisms that are distinct from discrete temporal attention paradigms such as the foreperiod or the temporal cueing paradigm (e.g., Capizzi et al., 2012; De la Rosa et al., 2012; Lakatos et al., 2013; Samaha et al., 2015; but see Correa and Nobre, 2008, on potentially shared mechanisms; see also Breska and Deouell, 2017).

Finally, although not in the focus of the present overview, there are paradigms in which temporal information is not provided in isolation but is correlated with stimulus-specific information. This *specific temporal expectancy* (Thomaschke and Dreisbach, 2013), also referred to as *time-based event-related expectation* (e.g., Ball et al., 2022) is typically studied using a variant of the variable foreperiod paradigm. In this variant, each foreperiod is correlated with a specific type of stimulus (or response) so that the foreperiod predicts not only the temporal onset of a stimulus but also what kind of stimulus will be presented or which response it will require (e.g., Wagener and Hoffmann, 2010; Thomaschke and Dreisbach, 2015; Thomaschke et al., 2018; Ball et al., 2022). The typical result is that performance is better for frequent time-stimulus combinations than for infrequent ones (for an overview see Thomaschke and Dreisbach, 2015).

In sum, different experimental paradigms have been developed to investigate the influence of temporal attention on stimulus processing, and these paradigms may also come along with different processing requirements and effects on the processing of temporally attended stimuli. The next sections provide an overview of the results obtained in temporal attention research.

A recap of the history: From motor preparation to temporal attention

The experimental investigation of temporal attention dates back to the end of the 19th century and the beginning of the 20th century, when experimental psychologists first investigated how the presentation of a warning signal influences responses to a subsequent imperative stimulus (e.g., Wundt, 1874; Woodrow, 1914; Klemmer, 1956; Bevan et al., 1965; for an excellent summary of these early studies see Niemi and Näätänen, 1981). For instance, in a seminal series of experiments, Wundt (1874) observed that the time needed to report the impact of a falling bullet was substantially reduced when the impact was preceded by a warning signal and that this effect was larger for larger drop heights. Following this first observation, various studies showed that properties of the foreperiod such as its length, the employed range and its variability across trials affect performance in response to the imperative stimulus. Thereby, these studies demonstrated the important role that temporal uncertainty plays in the allocation of temporal attention (for a summary see Niemi and Näätänen, 1981).

Apart from attempts to further characterize the factors that influence (the size of) the foreperiod effect, many subsequent studies focused on the theoretically important question of which mental processes within the stimulus-response processing chain are affected by variations in the foreperiod length, and temporal attention in general (for overviews see, e.g., Hackley and Valle-Inclán, 2003; Müller-Gethmann et al., 2003; Rolke and Ulrich, 2010; see also Correa, 2010). Originally, the predominant view was that variations in foreperiod length affect relatively late mental processes such as response preparation and response execution (e.g., Näätänen, 1971; Sanders, 1980; see also Teichner, 1954)—a view that was tightly linked to the idea that a warning signal mainly reduces the time needed for the execution of an action but does not influence stimulus processing itself. This idea was, for instance, advocated by Näätänen (1971) in the so-called *motor readiness model*. Some evidence for this motoric view comes from studies including measures of motor processes such as response force, which has been shown to be sensitive to foreperiod length (e.g., Mattes and Ulrich, 1997). Furthermore, indirect support for a motoric locus comes from studies that show that the effect of foreperiod on RT interacted neither with the effects of stimulus degradation (Frowein and Sanders, 1978), nor with those of visual stimulus intensity (Raab et al., 1961), nor those of stimulus-response compatibility (e.g., Posner et al., 1973; Frowein and Sanders, 1978). Since these latter variables are assumed to affect perceptual processing and response selection, respectively, the lack of an interaction with foreperiod length was interpreted as evidence that foreperiod length does not operate on either of these processes, but rather on late, motoric processes (e.g., Sanders, 1980).

Challenging the view that variations in foreperiod length exclusively operate on late, motoric processes, subsequent studies have provided evidence in favor of a pre-motor locus (Posner, 1978; Hackley and Valle-Inclán, 2003; Müller-Gethmann et al., 2003; Bausenhart et al., 2006; Hackley et al., 2007). Originally, the notion of a pre-motor locus was advocated on grounds of the observation that the foreperiod effect in choice RT tasks can come along with a speed-accuracy tradeoff (SAT), that is, a reduction in RT is accompanied by an increase in error rate (e.g., Posner et al., 1973). Since SAT effects are assumed to reflect an adjustment of the response criterion rather than a change in the speed of information processing, this finding was interpreted as evidence for a central locus of the foreperiod effect. However, the observation of a SAT has not been replicated in other RT studies (e.g., Müller-Gethmann et al., 2003; Los and Schut, 2008), and more recent research suggests that it seems to be confined to rather short foreperiods and/or situations in which participants receive trial-by-trial RT feedback (Han and Proctor, 2022; see also Lawrence and Klein, 2013). More direct evidence for a pre-motor locus came from psychophysiological studies examining temporal attention effects on the latency of the lateralized readiness potential (LRP) in the event-related potential (ERP). These studies have revealed that temporal attention reduces the latency of the stimulus-locked LRP as an index of the duration of pre-motor processes but has no or little effect on the response-locked LRP as an index of the duration of motor processes (e.g., Müller-Gethmann et al., 2003; Hackley et al., 2007; Seibold and Rolke, 2014b). Furthermore, consistent with a pre-motor locus, several studies also hint at temporal attention effects on central functions such as memory (e.g., Jones and Ward, 2019; but see Kulkarni and Hannula, 2021). For instance, Jones and Ward (2019) observed better recognition memory for items presented with a fixed temporal spacing during the retention interval than for items presented with a variable temporal spacing, suggesting that temporal attention may lead to an encoding benefit or to a more stable representation of encoded items.

Following up the notion of a pre-motoric influence, other researchers have advocated the idea that temporal attention may influence even early, perceptual processing (e.g., Bausenhart et al., 2007; Rolke and Hofmann, 2007; Rolke and Ulrich, 2010), taking up Wundt's original idea that warning signals serve the preparation of attention (Wundt, 1874). Some early, preliminary evidence in this regard was already provided by Niemi and Lehtonen (1982; Experiments 1 and 2) who observed that foreperiod effects on RT were stronger for low-intensity visual stimuli than for high-intensity stimuli (but see Niemi and Lehtonen, 1982, Experiment 3), a finding that was later replicated by Jepma et al. (2012) in the temporal cueing paradigm. Since stimulus intensity affects already early sensory processing (e.g., Kaskey et al., 1980; for an overview see also Nissen, 1977), these results hint that temporal attention may operate on early, perceptual processing. More direct evidence came later from studies employing tasks that put high demands on perceptual processing such as discrimination of (masked) stimuli (Rolke and Hofmann, 2007; Rolke, 2008; Seifried et al., 2010; Rohenkohl et al., 2012; Vangkilde et al., 2012; Cravo et al., 2013), temporal order judgment (Correa et al., 2006b; Bausenhart et al., 2008), and rapid serial stimulus presentation (Martens and Johnson, 2005; Nieuwenstein et al., 2005; Shen and Alain, 2011). Furthermore, evidence for an effect on perceptual processing came from studies employing more direct measures of perceptual processing such as d-prime (Rolke and Hofmann, 2007; Cravo et al., 2013), early components within the ERP (e.g., Lange et al., 2003; Doherty et al., 2005; Correa et al., 2006a; Rolke et al., 2016; Seibold et al., 2020; Balke et al., 2022), or the BOLD responses measured over sensory areas in functional imaging studies (e.g., Hackley et al., 2009; Bueti et al., 2010).

Despite the accumulating evidence that temporal attention can affect perceptual processing, it should also be noted that some studies did not reveal such an early effect (e.g., Griffin et al., 2002; Elbaz and Yeshurun, 2020, Experiment 2; Miniussi et al., 1999; Rudell and Hu, 2001; Wilsch et al., 2020). For instance, Miniussi et al. (1999), who employed trial-by-trial cueing of the temporal onset of a visual target in a simple detection task, did not observe any effect of temporal attention on sensory ERPs such as the P1 and N1; instead, the earliest effect of temporal attention in that study was on the P300 and thus rather late within visual processing (see also Rudell and Hu, 2001; Hackley et al., 2007). Furthermore, several recent studies did not provide evidence for an effect of rhythmic temporal attention on perception (e.g., Elbaz and Yeshurun, 2020; Wilsch et al., 2020). For instance, Elbaz and Yeshurun (2020) asked participants to give a non-speeded judgment about the orientation of a masked visual target presented at the end of a rhythmic or arrhythmic auditory sequence. At variance with a perceptual benefit reported in other rhythmic temporal attention studies (e.g., Rohenkohl et al., 2012; Breska and Deouell, 2017), these authors did not observe higher accuracy in reporting targets presented in-phase with the rhythm as compared to targets presented out of phase. Instead, they observed a lower guessing rate in the rhythmic as compared to the arrhythmic condition, but irrespective of whether the target was presented in-phase or out of phase. Furthermore, Wilsch et al. (2020) measured the combined effect of spatial and rhythmic temporal cueing in a cross-modal setting and did not observe higher discrimination accuracy for targets following a rhythmic context as compared to targets following a random context.

The exact reasons for why perceptual effects of temporal attention are observed in some studies but not in others, are not entirely clear so far. In principle, several factors might play a role: First, the effect of temporal attention may depend on the type of task that has to be performed on the imperative stimulus (see, e.g., Los and Horoufchin, 2011, for such a suggestion; see also Davranche et al., 2011, for neurophysiological evidence) and, specifically with respect to perceptual processing, the effect of temporal attention may become apparent only in perceptually demanding tasks (see also Correa et al., 2006a, for this suggestion). As noted above, most studies providing evidence for an influence of temporal attention on perceptual processing entailed manipulations or tasks that render perceptual processing of the imperative stimulus rather difficult—such as presenting the imperative stimulus only very briefly and/or superimposing it with a mask. Furthermore, some studies showed that the effect of temporal attention was most pronounced in those conditions that were especially difficult (e.g., Rolke, 2008; Seifried et al., 2010; see also Balke et al., 2022). For instance, Rolke (2008) observed the largest temporal attention effect on discrimination of masked visual stimuli in the condition with the shortest target presentation time before masking (or the lowest target contrast). This result indicates that temporal attention facilitates perceptual processing specifically (or even only) in suboptimal sensory conditions. Second, apart from task properties, another factor that might play a role is whether temporal attention is investigated in isolation or jointly with other attention domains such as spatial attention. In the latter situation, it could be possible that the potential of temporal attention in facilitating perceptual processing is masked by the influence of the other attention domain, especially if this domain is more informative or easier to use for attentional orienting (see also Seibold et al., 2019). Interestingly, some evidence that the effect of one attention domain can depend on how easy it can be processed and/or used relative to another domain has been observed for the comparison of spatial and feature-based attention in vision: Whereas it was originally argued that selective effects of feature-based attention arise later than those of spatial attention (e.g., Hillyard and Münte, 1984), more recent research has shown that feature-based attention can also precede or overcome effects of spatial attention, for instance in visual search tasks in which stimuli cannot be selected solely on grounds of location (e.g., Hopf et al., 2004; Seiss et al., 2009). Finally, and specifically with respect to rhythmic temporal attention, it has been suggested that specific methodological factors may play a role in observing a specific benefit for in-phase targets—such as the degree of temporal uncertainty or whether rhythm and target are of the same modality (see Elbaz and Yeshurun, 2020, for a discussion of these methodological factors). Hence, the lack of a temporal attention effect on measures of perceptual processing does not *per se* invalidate the view that temporal attention affects perception but rather indicates that effects of temporal attention may vary depending on the specific (task) context (see also Correa et al., 2005).

Finally, and at variance with the prevailing notion that temporal attention primarily leads to processing benefits, some studies have shown that temporal attention can also impair stimulus processing (e.g., Correa et al., 2010; Korolczuk et al., 2018; Menceloglu et al., 2021). An illustrative example in that respect is a study by Correa et al. (2010), who investigated the effect of temporal cueing on congruency effects in an arrow flanker task (Experiment 1) and in a setup combining the Simon task with a Spatial Stroop task (Experiment 2). In line with the notion that temporal attention affects perceptual processing, Correa et al. (2010) observed that the size of the Spatial Stroop effect, indicative of perceptual conflict, was smaller in the valid than in the invalid temporal cueing condition. However, and most importantly, the opposite result pattern emerged in the arrow flanker task and the Simon task: Here, the size of the congruency effect, indicative of response conflict, was larger in the valid in comparison to the invalid temporal cueing condition. This basic finding of a larger congruency effect in the Simon and the arrow flanker task has been replicated in subsequent studies (Korolczuk et al., 2018; Menceloglu et al., 2021; but see Menceloglu et al., 2017, for no such effect in a letter flanker task). Furthermore, Korolczuk et al. (2018) reported that the time required to stop an already planned response to an imperative stimulus was longer when the temporal onset of the imperative stimulus was predictable than when it was unpredictable. From a theoretical point of view, these findings, in particular those of Correa et al. (2010), are interesting because they do not only show that temporal attention may directly affects aspects of response selection such as response activation (see Korolczuk et al., 2018), but also suggest that temporal attention can operate on stimulus processing *via* distinct, parallel mechanisms rather than a single mechanism.

Taken together, the available empirical evidence argues against the traditional view assuming a sole motoric influence and instead supports the view that temporal attention can have multi-faceted effects on the stimulus-response processing chain, including higher-level, cognitive, and late motor processing, but also early, perceptual processing.

Temporal attention effects in different modalities and across modalities

As in research on spatial attention (e.g., Driver and Spence, 1998), the question of how temporal attention affects stimulus processing, and in particular perceptual processing, has been investigated in different modalities and, to some extent, also in multi-modal settings (Ball et al., 2018a,b) as well as in cross-modal settings (Lange and Röder, 2006; Bolger et al., 2013; Mühlberg et al., 2014; Mühlberg and Soto-Faraco, 2019).

In general, unimodal studies have revealed benefits of temporal attention on perceptual processing not only in the visual modality (e.g., Doherty et al., 2005; Correa et al., 2006a; Rohenkohl et al., 2012; Seibold and Rolke, 2014b), but also in the auditory (Lange et al., 2003, 2006; Lange and Röder, 2006; Rimmele et al., 2011), and tactile modalities (Lange and Röder, 2006; Van Ede et al., 2011). These perceptual effects of temporal attention in different modalities have been particularly clearly demonstrated in studies including the measurement of early components of the ERP such as the visual P1, the visual and auditory N1, and the visual N2posterior-contralateral (N2pc), which are directly linked to perceptual processing. For instance, Seibold and Rolke (2014b) showed that targets in a visual search task elicited a more pronounced and earlier arising N2pc in a blocked foreperiod context when the search display appeared after a short foreperiod compared with a long one. Furthermore, Lange et al. (2003) showed that temporally attended auditory stimuli in a temporal Hillyard paradigm elicited a more pronounced auditory N1 than temporally unattended ones. Complementing these findings, studies in which a time-frequency decomposition was applied to the electroencephalogram have shown that temporal attention is accompanied by changes in the amplitude or phase of pre-stimulus neural oscillations in different modalities (e.g., Rohenkohl and Nobre, 2011; Van Ede et al., 2011; Cravo et al., 2013; Todorovic et al., 2015). For example, Van Ede et al. (2011) observed a modulation of the amplitude of neural oscillations within somatosensory areas shortly before the expected onset of the imperative stimulus, and this modulation was spatially specific, that is, contralateral to the expected location of the stimulus. Importantly, these changes in pre-stimulus neural oscillations have been shown to correlate with the amplitude of post-stimulus neural activity in some studies (e.g., Cravo et al., 2013; Todorovic et al., 2015), indicating that they may form the basis for the perceptual processing benefit observed in ERP studies.

Despite the clear evidence for a beneficial effect of temporal attention in different modalities, the results from unimodal studies nonetheless hint to modality-specific differences in the locus of these effects, which becomes evident for the comparison of vision and audition. Specifically, early perceptual effects of temporal attention on auditory processing have been consistently reported in most studies (Lange et al., 2003, 2006; Lange and Röder, 2006; Lange, 2009, 2012; Rimmele et al., 2011; Herbst and Obleser, 2019; but see Lampar and Lange, 2011, Experiment 1) including those using a wide variety of temporal attention manipulations such as temporal orienting (Lampar and Lange, 2011, Experiment 2; Lange et al., 2003; Lange and Röder, 2006), rhythms (Sanders and

Astheimer, 2008; Rimmele et al., 2011), and blocked foreperiods (Seibold et al., 2011b), and even when temporal information is conveyed only implicitly by stimulus features (Herbst and Obleser, 2019).[4] In contrast, temporal attention effects on early visual processing have been observed less consistently. For instance, as mentioned above, an ERP study by Miniussi et al. (1999) aimed at investigating the effect of temporal cueing in a simple detection task did not reveal an effect of temporal attention before rather late visual processing (i.e., on the P300; see also Griffin et al., 2002, Experiment 2). In contrast, subsequent studies using other types of tasks and temporal attention manipulations did reveal temporal attention effects on early visual processing (e.g., Correa et al., 2006a; Rohenkohl and Nobre, 2011; Seibold and Rolke, 2014b; Balke et al., 2022). For instance, Correa et al. (2006a), who employed blocked temporal cueing in a visual discrimination task, observed a larger P1 and a shorter-latenced N2 for temporally attended stimuli in comparison to temporally unattended ones. Furthermore, Seibold and Rolke (2014b) as well as Balke et al. (2022), both of whom used a blocked foreperiod manipulation in combination with a visual search task, observed amplitude enhancements and latency reductions for both the visual N1 and the N2pc, showing that temporal attention can affect early visual processing.

Apart from methodological differences, one potential reason for these discrepant results could be that the visual and auditory modality are differentially sensitive to temporal attention effects (see also Ball et al., 2022). This view is consistent with the general idea that the auditory modality may be more sensitive to temporal information (e.g., Repp and Penel, 2002; Bratzke et al., 2012), whereas the visual modality may be more sensitive to spatial information (e.g., Bertelson and Aschersleben, 1998; Park et al., 2003). Due to this potential differential sensitivity, temporal attention may affect early auditory processing by default and independent of other factors such as the type of task participants have to perform or the way temporal attention is induced; in contrast, and as already noted above, temporal attention may affect early visual processing only when demands on perceptual processing are high enough (see also Correa et al., 2006a), meaning that early visual processing might be already less optimal. However, given that the above-described comparison is indirect (i.e., across studies), further systematic research is necessary to unravel differential influences of temporal attention on perceptual processing in different modalities.

Apart from unimodal contexts, some recent studies (e.g., Ball et al., 2018a,b) have also addressed multimodal contexts, focusing on the question of whether temporal attention effects differ between unimodal and multimodal stimuli. In these studies, near-threshold (uni- or multimodal) targets were embedded within sequences of audio-visual, visual, or auditory distractors, with the target appearing either early (short foreperiod) or late (late foreperiod) in the sequence. The effect of temporal attention was examined by varying the foreperiod probability across blocks, with either the short or the long foreperiod being more likely in each block. In this context, the effect of temporal attention on discrimination accuracy was larger for multi-modal (audio-visual) stimuli than for unimodal (visual or auditory) stimuli. Importantly, this larger temporal attention effect was observed not only on a group-level (Ball et al., 2018b), but also when comparing the multi-modal condition with the best unimodal conditions for each participant (Ball et al., 2018a). Although multi-modal superiority in temporal attention can be explained in several ways (Ball et al., 2018a; see also Ball et al., 2021), one possible reason for multi-modal superiority would be that temporal information from different modalities can be combined to boost perceptual processing (see also Ten Oever et al., 2014).

Finally, going beyond the question whether temporal attention facilitates perceptual processing in different modalities, some studies have even addressed the possibility of cross-modal transfer, that is, that temporal attention effects can transfer from one (attended) modality to another (unattended) modality (e.g., Lange and Röder, 2006; Bolger et al., 2013; Mühlberg et al., 2014; Menceloglu et al., 2019; Mühlberg and Soto-Faraco, 2019). One example for cross-modal transfer in temporal attention is provided by Lange and Röder (2006) who presented auditory or tactile stimuli being separated either by a short or long foreperiod. The authors instructed participants to attend to one modality and, within that modality, only to stimuli presented after a specific (e.g., short) foreperiod. They observed an enhanced auditory N1 for temporally attended stimuli irrespective of the modality to which participants attended. In contrast to these results, two subsequent studies employing an audio-visual context (Mühlberg et al., 2014) or a visuo-tactile context (Mühlberg and Soto-Faraco, 2019) did not reveal evidence for cross-modal transfer of temporal attention effects. In line with these latter findings, Menceloglu et al. (2019) observed no interaction of temporal attention with modality-specific attention in a study employing auditory and visual stimuli. Temporal attention in this study was tested by presenting blocks with different probabilities for short and long foreperiods and asking participants to attend only to one foreperiod per block. Modality-specific attention was tested by varying the probability for stimulus occurrence in each modality, thereby biasing attention toward the more probable modality. Although beneficial effects of both temporal attention and modality-specific attention were observed, their effects did not interact, arguing for independent influences of temporal attention and modality-specific attention on stimulus processing.

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[4] It should be noted that, although these studies have typically revealed amplitude enhancements for early ERPs, some studies (e.g., Lange, 2009) also revealed amplitude reductions. Lange (2013) suggested that the observation of amplitude enhancements as opposed to amplitude reductions may depend on the specific type of manipulation that is used: Specifically, she suggested that varying task-relevance (i.e., instructing participants to attend to a specific interval) as it is done in the temporal Hillyard paradigm is a more direct manipulation of attention and will be reflected amplitude enhancements, whereas varying the probability that a stimulus will appear at all as it is done when comparing rhythmic and arrhythmic temporal sequences induces stimulus expectations, and these expectations will lead to amplitude reductions (see also Summerfield and Egner, 2009, for an insightful review on attention vs. stimulus expectations). However, given that other studies investigating effects of rhythmic temporal attention have shown amplitude enhancement for early ERPs (e.g., Doherty et al., 2005; Rimmele et al., 2011), and given that both amplitude reductions and enhancements were observed at a similar processing level (i.e., on the auditory N1), we do not further differentiate between expectation-based and task-relevance based effects in temporal attention at this point.

The reasons for these discrepant findings regarding cross-modal effects in temporal attention have yet to be clarified. However, one factor that could play a role is the way in which modality-specific attention was induced in different studies: For instance, in the Menceloglu et al. (2019) study, the primary (more frequent) modality was kept constant for each participant, and modality-specific attention was induced implicitly, that is, by presenting stimuli in one modality in most trials. In contrast, in the study of Lange and Röder (2006), the to-be-attended modality alternated between blocks, and participants were explicitly instructed to focus on one single modality. Accordingly, for cross-modal transfer to occur, modality-specific attention may have to be explicitly task-relevant.

In sum, there is clear evidence that temporal attention facilitates perceptual processing in different modalities (e.g., Lange et al., 2003; Van Ede et al., 2011; Seibold and Rolke, 2014b). Furthermore, perception of multimodal stimuli seems to benefit to a stronger extent from temporal attention than unimodal stimuli (e.g., Ball et al., 2018a), which indicates that processing of temporal regularities may be combined across different sensory modalities. Finally, there is some evidence for cross-modal transfer in temporal attention (Lange and Röder, 2006; but see Menceloglu et al., 2019). Yet, whether these findings on temporal attention within and across modalities reflect modality-specific mechanisms or a supramodal mechanism underlying temporal attention, as it has been discussed in the context of spatial attention (see, e.g., Driver and Spence, 1998), remains a matter of debate.

Temporal attention in combination with other attention domains

Not surprisingly, the discovery that temporal attention affects perceptual processing of the imperative stimulus has also fueled research addressing the interplay of temporal attention with other attention domains. Most of the evidence for this interplay comes from studies investigating temporal attention in combination with spatial attention (e.g., Griffin et al., 2002; Doherty et al., 2005; Lamy, 2005; Rohenkohl and Nobre, 2011; Seibold et al., 2020). In contrast, much less research has been conducted on the interplay of temporal and feature-based attention (but see Kingstone, 1992; Warren et al., 2014; Rolke et al., 2016; Grubert and Eimer, 2018).

Studies combining temporal and spatial attention in the visual modality have revealed mixed results. One set of studies suggests that temporal and spatial attention may exert independent effects on stimulus processing (Coull and Nobre, 1998; Griffin et al., 2002; Lange et al., 2006; MacKay and Juola, 2007; see also Tal-Perry and Yuval-Greenberg, 2022). For instance, Coull and Nobre (1998) employed probabilistic cueing of either the temporal onset or the spatial location of a visual stimulus and measured PET and fMRI to compare the activation of brain areas involved in temporal and spatial attention. These authors observed overlapping neural signatures of the two attention domains, but also a clear differential lateralization of these signatures, indicating that temporal and spatial attention may affect stimulus processing in distinct ways. Even more direct evidence for the idea of independent effects was observed by Griffin et al. (2002, Experiment 1) in an ERP study with a similar experimental setup. Replicating previous findings on spatial attention, they observed effects of spatial attention appearing within the first 100 ms, whereas the effect of temporal attention arose later and was less focused on visual areas. These results led the authors to conclude that spatial and temporal attention modulate visual processing in distinct ways.

In contrast to this conclusion, however, other studies have provided evidence for interactions between temporal and spatial attention (e.g., Milliken et al., 2003; Doherty et al., 2005; Rohenkohl and Nobre, 2011; Seibold et al., 2020). For instance, Doherty et al. (2005) asked participants to monitor a moving disc that disappeared behind an occluder at some point during the trial, and participants had to respond to the presence of a dot upon the re-appearance of the disc. Temporal and spatial attention were varied by making the trajectory of the disc either spatially predictable, temporally predictable, spatio-temporally predictable, or unpredictable in both domains. The main finding of that study was that spatially predictable disks evoked a more pronounced P1 as an index of early visual processing, and this effect was amplified by additional temporal predictability (see also Rohenkohl and Nobre, 2011, for a similar finding). Based on these observations, Doherty et al. (2005) proposed that temporal attention boosts facilitatory (or selective) effects of other attention domains (i.e., spatial attention).

In contrast to the vast array of studies in the visual modality, relatively few studies exist addressing the interplay of temporal and spatial attention in the auditory modality (e.g., Lange et al., 2006; Rimmele et al., 2011; Lange, 2012). For instance, Lange et al. (2006) combined manipulations of temporal and spatial attention in a temporal Hillyard paradigm in which the offset marker of either a short or long interval was presented either to the left or right ear and participants were asked to attend only to stimuli presented within one ear and after one temporal interval. Analogous to previous studies, Lange et al. (2006) observed an enhanced auditory N1 for both temporally attended stimuli and spatially attended stimuli. Crucially, the effects of both domains were additive, indicating that temporal and spatial attention modulate early auditory processing in an independent manner. Moreover, Rimmele et al. (2011) showed that not only did temporal attention and spatial attention exert independent effects on auditory processing, but the effects of temporal attention also preceded those of spatial attention. Hence, the existing studies conducted in the auditory modality suggest that temporal attention and spatial attention are uncoupled.

Although the exact conditions required for observing interactive effects of temporal and spatial attention have yet to be clarified, several factors other than stimulus modality may play a role: One important factor may be the type of manipulation that is used to induce spatial attention and/or temporal attention. For instance, Olk (2014) contrasted arrow cues with more abstract symbolic cues and showed that only the former ones, most probably eliciting involuntary spatial attention (e.g., Ristic and Kingstone, 2006), interacted in their effect with temporal cues, but this was not the case for the latter, abstract ones, which probably elicited only voluntary spatial attention. Furthermore, a recent modeling study by Tal-Perry and Yuval-Greenberg (2022) did not reveal an interaction of spatial cueing with the variable foreperiod effect. Consequently, observing interactions between time and space may also depend on the way spatial and/or temporal attention is manipulated. Another factor that may play a role in whether temporal and spatial attention are combined to facilitate stimulus processing might be the nature of the target object. In the study of Doherty et al. (2005), temporal and spatial attention were not induced by single cues but instead by the trajectory of a single moving object. Since the processing of a movement requires integration of spatial locations across time, it may well be that processing of temporal and spatial information in this type of setup is strongly bound. One final important factor for combining spatial and temporal attention may be the (relative) informativeness of a specific attention domain with respect to the upcoming stimulus and/or how difficult it is to extract and use the information in this domain for attentional preparation. This idea is rooted in the observation that for multi-dimensional stimuli (i.e., stimuli that have to be selected on grounds of several attention domains such as location and color), selection does not seem to follow a fixed temporal order (i.e., selection of location before selection of color), but instead seems to depend on how easy discrimination is in each domain (e.g., Hansen and Hillyard, 1983; for a discussion of this possibility in the context of temporal attention see Seibold et al., 2019). Accordingly, in a case where spatial information is the more informative cue for attentional preparation, this information would be weighted stronger and thereby dominate the effect of the cue in another (i.e., temporal) domain. From a theoretical point of view, this latter possibility is interesting because it would suggest that different types of attentional cues can be used in a flexible manner for preparation, depending on which information is most useful.

Though less often examined than the interaction between temporal and spatial attention, there do exist several studies addressing potential interactions of temporal and feature-based attention. One pioneering study in this respect was conducted by Kingstone (1992) who combined probabilistic temporal cueing with feature-based cueing. Specifically, participants were presented with a combined cue that contained information about both the likely temporal onset of a subsequent imperative stimulus and the type of stimulus. Kingstone (1992) observed that the RT to the imperative stimulus was fastest when both the temporal and the feature cue were valid and slowest when one cue was valid but the other one was invalid, indicating that participants formed combined expectations about the "what" and the "when" of stimulus occurrence.

Further available evidence shows that temporal attention can indeed support selective feature-based processing (e.g., Lakatos et al., 2013; Warren et al., 2014; Grubert and Eimer, 2018; Seibold et al., 2020), which becomes particular clear in the following exemplary studies: First, in a functional imaging study by Warren et al. (2014), participants were presented a Gabor grating that regularly changed its orientation in a clockwise rotation, and they had to detect a change in the Gabor's frequency. To measure the effect of temporal attention, the authors varied the temporal predictability of the particular Gabor orientation at which the frequency change could occur. Warren et al. (2014) showed that, in the temporally predictable condition, orientation-sensitive voxels in the primary visual cortex were tuned toward the orientation at which the frequency change was expected to occur, and this tuning occurred shortly before the actual Gabor grating was presented. Thus, temporal attention was used to tune the neuronal system toward specific stimulus attributes which were important for the expected perceptual task. The second study to show temporal attention supporting feature-based processing was an ERP study on visual search. Grubert and Eimer (2018) showed that the pre-activation of target templates can be temporally aligned with the expected onset of the target. Specifically, these authors observed that probe stimuli that (1) were interspersed between subsequent visual search episodes and (2) shared the target's color elicited an N2pc, but this was only the case if they were presented shortly before the expected onset of the next search display. Finally, Seibold et al. (2020) measured the conjoint effect of temporal, spatial and feature-based attention on early visual processing. In that study, temporal attention was varied by means of probabilistic temporal cueing, whereas spatial and feature-based attention were varied by instructing participants to attend and respond to stimuli at a specific location and in a specific color. Replicating early effects of spatial and feature-based attention, Seibold et al. (2020) observed that both spatial and feature-based attention led to an enhanced visual N1. Importantly, this enhancement was observed only in temporally valid trials, that is, when the stimulus was presented at the expected moment in time. Hence, the results of these three studies show that temporal attention can tune selective effects of other attention domains in the sense that spatially selective and feature-selective processing can start before the actual onset of a stimulus.

Perceptual mechanisms of temporal attention

In this section we will discuss three prominent mechanisms that have been proposed to explain how perceptual processing may benefit from temporal attention (see also Nobre and Van Ede, 2018).

The presumably least disputed and—given attention mechanisms in general—most natural account of the mechanisms underlying temporal attention effects on perceptual processing is *sensory enhancement*. Sensory enhancement can be roughly described as an improvement in the quality of neural signatures of incoming sensory information (e.g., Downing, 1988; Chun and Wolfe, 2001; Carrasco et al., 2002). According to this account, knowing the temporal onset of a stimulus temporally increases neural responses to incoming sensory signals and thereby facilitates perceptual processing of stimuli presented within the attended time window (e.g., Correa et al., 2006a). Support for this assumption comes from several lines of evidence: First, the early effects of temporal attention in most ERP studies demonstrating these effects appeared as ERP amplitude enhancements, and this pattern emerged across different paradigms, including blocked foreperiods (e.g., Seibold and Rolke, 2014b), different types of temporal orienting (e.g., Correa et al., 2006a; Lange et al., 2006), and rhythmic temporal attention paradigms (e.g., Doherty et al., 2005; Rimmele et al., 2011). Such ERP amplitude enhancements have frequently been linked to an amplification of the incoming sensory signal, similar to what has been proposed in the context of other attention domains such as spatial and feature-based attention (e.g., Hillyard and Anllo-Vento, 1998; Ling et al., 2009; Hopf et al., 2012; Mishra et al., 2012; but see Makeig et al., 2002, for other potential mechanisms that may give rise to ERP amplitude enhancements). A second line of evidence for sensory enhancement by temporal attention comes from the observation that temporal attention leads to a temporary upregulation of neural activity in sensory areas, as revealed in functional imaging studies (e.g., Coull and Nobre, 1998; Hackley et al., 2009). Finally, even though it is indirect, evidence for signal enhancement also comes from studies showing that temporal attention may increase the signal-to-noise ratio (e.g., Bausenhart et al., 2010; Rohenkohl et al., 2012; Balke et al., 2022). For instance, Balke et al. (2022) measured the effect of temporal attention on spatial selection of a target in pop-out visual search under different target salience conditions. They observed that, not only did temporal attention lead to an earlier-occurring N2pc as index of target selection, but this latency reduction was also more pronounced when the target was less salient. On grounds of the assumption that the signal-to-noise ratio is lower for low-salience targets, the stronger temporal attention effect in the low-salience condition could be explained by an increase in the signal-to-noise ratio, and this increase may be caused by an enhancement of the signal.

Directly related to signal enhancement and most probably a mechanism that can be considered a subordinate one is a mechanism that we will refer to as *pre-activation of selective processing* (e.g., Correa et al., 2010; Warren et al., 2014; Grubert and Eimer, 2018). This mechanism has been proposed particularly in the context of the interplay of temporal attention with other attention domains. Specifically, extending the idea of signal enhancement, the core assumption here is that temporal attention not only facilitates neural processing in sensory areas in general but may also lead to a selective pre-activation or tuning of areas that decode specific, task-relevant features. Thus far, the strongest empirical evidence for this approach has been provided by the studies discussed above showing that temporal attention is already inducing (or enhancing) spatially selective and feature-selective processing in sensory areas shortly before the expected onset of an imperative stimulus (e.g., Van Ede et al., 2011; Warren et al., 2014; Grubert and Eimer, 2018).

Second, several authors have also proposed that temporal attention affects the speed of early perceptual processing (e.g., Rolke, 2008; Seifried et al., 2010; Seibold et al., 2011a,b). This *sensory acceleration* mechanism is rooted in the so-called *law of prior entry* (Titchener, 1908), a general mechanism of attention that has also been investigated in the context of other attention domains (e.g., Scharlau, 2004; Vibell et al., 2007; Weiß and Scharlau, 2012; for a review see, e.g., Spence and Parise, 2010). According to this principle, attention increases the speed of neural processing within modality-specific sensory areas, and this leads to an earlier (conscious) perception of a stimulus. In the context of temporal attention, the existence of such a mechanism has been supported by research showing a reduction of perceptual latency for stimuli presented at temporally-attended moment in time (Seifried et al., 2010) as well as by research showing a reduction in latency for early ERPs such as the N1 (e.g., Hackley et al., 2007; Seibold and Rolke, 2014b), the visual N2 and the auditory mismatch negativity (e.g., Correa et al., 2006a; Seibold et al., 2011b), and the N2pc (Seibold and Rolke, 2014b; Rolke et al., 2016; Balke et al., 2021, 2022). The questions of whether and how sensory acceleration might cause signal enhancement (or vice versa), what the connection between these possible mechanisms is, and whether these mechanisms come into play specifically in temporal attention have yet to be answered.

Finally, a third mechanism which has become especially prominent in temporal attention research over the past years concerns the changes in the dynamics of ongoing neural oscillations, in particular the power and the phase of these neural oscillations (see Nobre and Van Ede, 2018; see also Herbst et al., 2022). A power change caused by temporal attention refers to a change in the overall amplitude of ongoing oscillations shortly before the expected onset of a stimulus (see, e.g., Rohenkohl and Nobre, 2011). This amplitude change is typically explained in terms of how synchronized the neural activity is. Specifically, increases (or decreases) in the power of ongoing oscillation are assumed to be the consequence of an increase (or decrease) of neural synchronization. Although changes in oscillatory power have been observed across different frequency bands, oscillations in the range of the alpha-band are particularly frequently cited in the context of temporal attention (e.g., Rohenkohl and Nobre, 2011; Herbst and Obleser, 2017; for an overview, see also Van Diepen et al., 2019). For instance, Herbst and Obleser (2017) who varied the variability of the foreperiod distributions in a probabilistic temporal cueing task observed that the condition with lowest temporal uncertainty (i.e., the smallest range of possible foreperiods) was associated with an increase in alpha power shortly before the expected moment of stimulus onset. Importantly, the reported power changes went along with a processing benefit. A change of the phase of neural oscillation, which has become popular under the term *entrainment*, refers to the alignment of the phase of neural oscillations (i.e., the maximum amplitude of oscillations in either direction) to an externally induced stimulation, which leads to optimized stimulus processing (e.g., Schroeder and Lakatos, 2009; Mathewson et al., 2010; Stefanics et al., 2010). Empirical research on entrainment has provided evidence for temporal attention-induced entrainment of slow oscillatory activity in the delta range (e.g., Lakatos et al., 2008; Cravo et al., 2013; Breska and Ivry, 2020; Daume et al., 2021; see also Herbst et al., 2022), as well as some evidence for entrainment in higher frequency bands (e.g., Besle et al., 2011; Spaak et al., 2014; Samaha et al., 2015; Solís-Vivanco et al., 2018).

Given that most studies that provided evidence for entrainment in the context of temporal attention used rhythmic stimulation, the question arises: is entrainment a mechanism that is specific to rhythmic temporal attention, or is it a more general mechanism that also plays a role in settings in which temporal attention is induced by a single stimulus, for instance a warning signal or a temporal cue? Even though this question has been addressed in few studies (e.g., Breska and Deouell, 2017; Herbst and Obleser, 2019; Daume et al., 2021), the results obtained so far suggest that entrainment may indeed be a general mechanism of temporal attention. For instance, Daume et al. (2021) recorded the magnetoencephalogram in a task in which a visual disc moved continuously at constant speed, disappeared, and then reappeared again from behind an occluder. The timing of the disc's re-appearance was jittered around the interval that was required to pass the occluder based on its length and the disc's speed. To assess the effect of temporal attention, the authors compared neural activity in two tasks, a temporal task, in which participants had to judge whether the disc had appeared too early or too late, and a luminance task, in which participants had to judge whether the disc was brighter or darker than before disappearing. Despite using a non-rhythmic stimulation, Daume et al. (2021) observed a higher phase-reset of low-frequency oscillations in accordance with the expected temporal onset of reappearance of the disc in the temporal task as compared to the luminance task. This finding of phase-reset in a non-rhythmic stimulation context shows that entrainment is not a mechanism that is necessarily specific to rhythmic temporal attention but may instead reflect a general mechanism that also underlies beneficial effects of temporal attention in non-rhythmic contexts.

Summary and future directions

In sum, the research on temporal attention discussed up to this point in the review clearly shows that temporal attention is a core domain of attention that not only affects stimulus processing across sensory modalities but also interacts with other domains. Furthermore, like other attention domains (see, e.g., Spence and Parise, 2010; Carrasco, 2011), temporal attention may facilitate stimulus processing *via* several mechanisms, depending on factors such as the specific stimulus, task context, and the specific way temporal attention is induced. Yet, even though steady progress has been made in the last decade of research on temporal attention, particularly on the subjects of rhythmic temporal attention and the role of neural oscillations in temporal attention (e.g., Breska and Deouell, 2017; Herbst et al., 2022), several questions remain unanswered, and new questions have arisen. In the remainder of this review, we will highlight some of the most pressing questions.

First, as became evident reviewing the research across the different sections, temporal attention (like other attention domains) has been studied in different paradigms, with some of them being rooted in the historical tradition of temporal attention research (i.e., foreperiod paradigms) and others being adopted from other attention domains (i.e., temporal orienting paradigms). On the one hand, this paradigmatic diversification has resulted in a wide body of research on temporal attention, contributing to its establishment in the broader landscape of attention. On the other hand, as illustrated for example by the case of the interaction between temporal and spatial attention in vision, this paradigmatic diversification has also led to partially incompatible results, which remain difficult to interpret. The incompatibility of these results may be due in part to the unintentional measurement of partially distinct types of temporal attention in different paradigms (see also Nobre et al., 2007; Lawrence and Klein, 2013). Some empirical evidence for this claim is provided by a direct comparison of the results from studies that used closely similar tasks but different temporal attention manipulations (e.g., De la Rosa et al., 2012, in comparison to Capizzi et al., 2012) or from those studies directly comparing different types of temporal attention manipulations (e.g., Olk, 2014), and also from studies examining the brain structures involved in different temporal attention paradigms (e.g., Coull et al., 2000; Triviño et al., 2016; see also Coull and Nobre, 2008). For instance, De la Rosa et al. (2012) showed that probabilistic temporal cueing effects in a primary task were strongly reduced if participants had to perform a concurrent working memory task, whereas Capizzi et al. (2012) showed that effects of rhythmic temporal cueing remained intact when adding a secondary task. These results as well as those from other studies (e.g., Rohenkohl et al., 2011; Triviño et al., 2011) have led some researchers to draw a distinction between *automatic temporal attention*, which is assumed to be induced by rhythmic temporal cueing, and *controlled temporal attention*, which is assumed to be induced in probabilistic temporal cueing (see also Nobre et al., 2007; for another classification scheme see, e.g., Schroeder and Lakatos, 2009). Similar distinctions between *unintentional* and *intentional* processes or *bottom-up* and *top-down* processes have also been made in the context of the variable foreperiod paradigm (see Los and Van den Heuvel, 2001; Los and Heslenfeld, 2005; Vallesi and Shallice, 2007; Mento and Tarantino, 2015). Here, dissociative result patterns arguing for the contribution of either one or the other process were obtained when comparing different participant populations (Mento et al., 2019; Mento and Granziol, 2020; Duma et al., 2021) or when considering neurophysiological indicators (Mento, 2017; Duma et al., 2020). Classifications like this could serve as a starting point for a more systematic comparison of different temporal attention manipulations, which may in turn help to gain a better understanding of some of the discrepant results that have been reported in the temporal attention literature.

A second, somewhat related research opportunity for future research would be a further systematic investigation of the mechanisms that underlie temporal attention. In particular, it remains unclear whether some of the mechanisms outlined above such as sensory acceleration are distinct mechanisms that can occur independently of other mechanisms or whether they are simply the consequence of other mechanisms, such as entrainment or signal enhancement. Furthermore, as already noted above, it is also still unclear to what extent (some of) these mechanisms operate only under very specific task contexts and/or temporal attention conditions. Here, more systematic research is needed to gain a better understanding of how general these specific mechanisms may be. As mentioned above, some progress has been made with respect to entrainment, which has been demonstrated not only in rhythmic temporal attention tasks but also in non-rhythmic tasks (e.g., Daume et al., 2021), indicating that this mechanism may be more general than originally thought.

In addition to a further examination of experimental paradigms and underlying mechanisms, future research could also put a stronger focus on how temporal attention affects perceptual processing in different unimodal and cross-modal contexts. As described in the previous section, research on temporal attention has been predominantly focused on vision at the expense of other modalities. Consequently, future research could include a systematic investigation of temporal attention effects in other modalities, in particular the tactile modality, as well as different cross-modal and multisensory settings. Another pressing question in this field is the existence of modality-specific differences in the effects of temporal attention, particularly with respect to the comparison of audition and vision. Here, future research should also include a direct comparison of temporal attention effects in different modalities within the same study.

Finally, another promising avenue for future studies could be to investigate how specific temporal attention might aid perceptual processing. For example, one important mechanism in the context of voluntary (or top-down) attention is *suppression* (or inhibition) of irrelevant stimuli, features, or locations (see, e.g., Geng, 2014). Whereas, suppression has become a major topic in research on spatial attention, and in particular in the context of visual search (see, e.g., Cosman et al., 2018; Gaspelin and Luck, 2018; Feldmann-Wüstefeld et al., 2021), this mechanism has been covered by few studies in the context of temporal attention (but see Los, 2004; Seibold and Rolke, 2014a; Amit et al., 2019; Balke et al., 2021; Gresch et al., 2021; Xu et al., 2021, 2022). For instance, Balke et al. (2021), who examined the effect of a blocked foreperiod manipulation on spatial selection and suppression of a salient distractor in visual search did not observe evidence for an influence of temporal attention on the distractor positivity (or P_D), an ERP assumed to index distractor suppression. In contrast, Xu et al. (2021), observed reduced attentional capture if a salient distractor was presented at a location that was frequently occupied by a distractor, and this reduction was stronger if the location was presented more frequently after a specific foreperiod. This result suggests that suppression of stimuli at particular locations can become more effective if the location can be predicted on grounds of temporal information. Following these results, it may be interesting to investigate more systematically the conditions under which temporal attention may support suppression in general and whether temporal attention may (also) serve suppression of specific features.

Taken together, research from the last few years has substantiated the idea that temporal attention, like spatial and feature-based attention, influences perceptual processing of stimuli in different modalities. Furthermore, there is growing evidence of interactions across attention domains: temporal attention can influence the beneficial effects of other attention domains on perceptual processing. Of course, there are still many open questions, such as whether temporal attention is effective in the same way in different modalities and in which conditions temporal attention interacts with other attention domains. Nonetheless, the current state of research clearly shows that the temporal domain is an essential factor in understanding attention in general and must be taken into account in the saddling and interpretation of studies.

Author contributions

VS wrote the first draft of the manuscript. BR and JB wrote sections of the manuscript. All authors reviewed and edited the manuscript and they read and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

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Influences of inattention on perceived self-efficacy, stress, and depression

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Introduction: Going through life mindlessly appears to produce feelings of boredom and depression, suggesting that cognitive deficits can lead both directly and indirectly to emotional problems. Under this hypothesis, there are numerous possible routes from attention to affective issues, including through the sense of self-efficacy – a judgement about our ability to successfully achieve goals – and the experience of psychological stress. The present study assessed potential pathways from everyday inattentiveness, through the presumed intermediate experiences of diminished self-efficacy and psychological stress, to the experience of depressive affect.

Method: In two studies we collected questionnaire responses from large groups of participants ($N = 188$, Study 1; $N = 399$, Study 2), assessing individual differences in trait inattention, attention-related errors, self-efficacy, stress, and depression.

Results: Via path analyses we tested several predictions: 1) the frequency of attention lapses predicts depressive affect; 2) attention-related errors mediate effects of inattention in predicting both self-efficacy and stress; 3) self-efficacy and stress directly influence depressive affect. The results indicate the proposed Attention-to-Affect models provide good fit overall. They also indicate a reversal of the causal flow, while consistent with traditional views, does not adequately fit the data.

Discussion: That the Attention-to-Affect models provide good fit for the data is consistent with the view that everyday inattention contributes to the emotional distress that creates depression. While this view is contrary to the typical conception of attention problems as consequences of depression, it is consistent with our own previous findings. Accordingly, our results continue to suggest it is important for future research to further validate this pathway and to consider directly remedying attentional issues as a potentially important part of depression treatment strategies.

KEYWORDS

inattention, attention lapses, self-efficacy, stress, depression

Influences of inattention on perceived self-efficacy, stress, and depression

Absentminded lapses of attention and a general tendency not to be fully mindful of our present experiences are common threads running through most of our daily lives. Although generally taken to be rather trivial events, they can sometimes have dramatic and even life-threatening consequences (Reason, 1984). Indeed, not paying attention to the critical task of driving the train may have been the primary cause of a major commuter train crash near Chatsworth, California, when an engineer became too focused on sending text messages from his cell phone to either notice or respond to operating signals telling him he should stop the train (National Transportation Safety Board, 2008). Such examples highlight the dramatic consequences that disruptions in attention can have when they happen to occur at particularly inopportune moments, but, given the pervasiveness of attention lapses in everyday life, particularly inopportune moments may not be a necessary component in this process. Indeed, the cumulative effects of inattention may have the potential for less obvious and dramatic, but ultimately more persistent emotional consequences. In our earlier work we presented data consistent with the view that seemingly minor episodes of everyday inattention, and their resulting minor cognitive and behavioral errors, are potentially important factors in the development of depression (Carriere et al., 2008). The present paper extends this earlier argument by testing two follow-up hypotheses that the influences of everyday inattention on depression are mediated, in part, by (1) one's general perceived self-efficacy and (2) one's overall experience of psychological stress.

To examine the potential influence of inattention on depression we (Carriere et al., 2008) previously conducted a large-scale questionnaire study and used structural equation modeling to test and compare two hypotheses: (1) that chronic cognitive failures in attention are an initial cause of depression (the Attention-to-Affect hypothesis), and (2) the reverse hypothesis that depression causes non-specific attention and memory failures (the Affect-to-Attention hypothesis). These hypotheses are both based on the commonly held belief that there is a causal connection between attention and depression, but their corresponding models differ with respect to how this causal link plays out in everyday life. To evaluate these hypotheses each model was compared against the null hypothesis, addressing the question of whether either an Attention-to-Affect or Affect-to-Attention model was capable of explaining the observed relations among the relevant variables. These analyses showed *only* the Attention-to-Affect model provided good fit for the data, by modeling attention lapses and associated everyday mistakes as a common cause of both memory failures and depression (Carriere et al., 2008). This causal flow is consistent with findings from MacLeod and colleagues (MacLeod et al., 2002), but contrasts with the traditional view of how emotion and attention are linked. Indeed, although not often articulated as an explicit hypothesis (owing, presumably, to the ubiquity of this view), the more common perspective is that emotions influence our attention. Indeed, for example, this appears to be the received view in most research on attentional biases in anxiety and depression (e.g., MacLeod et al., 1986; Gotlib et al., 1988; Dalgleish and Watts, 1990; Mogg et al., 1995; see also a review in Ingram et al., 2008) and on the attentional effects of encountering emotionally salient information (e.g., biases toward negative emotions; Fenske and Eastwood, 2003). With respect to the latter view, however, it is worth noting that attention has also been shown to have reciprocal effects on later emotional evaluation of stimuli (Fenske and Raymond, 2006).

Self-efficacy as a potential mediator of attention-to-affect

Given the above, we now seek to elaborate on the Attention-to-Affect hypothesis by evaluating another path by which attention lapses could influence our affective state—one's sense of self-efficacy. Self-efficacy is a judgement about our ability to successfully achieve our goals—usually in reference to a specific situation or activity (Bandura, 1977). Our sense of self-efficacy reflects both the expectation that we are capable of performing a task and that most others are not substantially *more* capable of performing the same task (Davis and Yates, 1982). Such beliefs apply not only to special skills or talents, but also to our ability to perform everyday tasks or cope with novel life challenges. These latter beliefs reflect a generalized sense of self-efficacy (Tipton and Worthington, 1984), which could be undermined in the face of chronic attention lapses interfering with our ability to perform even simple everyday tasks.

Reduced self-efficacy involves the belief that one's failures are the result of an inherent lack of ability rather than a consequence of situational factors or simply insufficient effort (Bandura, 1997). The obvious parallels between such beliefs and the counterproductive, negative affect-laden biases that are typical of depression support Bandura's claim that depression is a potential outcome of decreased self-efficacy. In particular, as he noted, if through our sense of inefficacy we come to believe our successes are the result of luck and failures represent our actual ability, depression will result in part because we devalue our accomplishments and overvalue our failures. Ultimately, because failure at a task is seen as more meaningful than success, even when initial failures are followed with later successes, we may still be less willing than others to engage in the same task again. On this view, reduced self-efficacy is thought to create cognitive biases that produce depression, and potentially influence attention by continually directing it away from information critical to successful task completion (Bandura, 1997). Furthermore, given that our sense of self-efficacy is relative to the ability of others, depression, and an unwillingness to even attempt tasks, is an especially likely result when we expect others would not have difficulty completing the same tasks with which we have had trouble (Davis and Yates, 1982).

According to the Attention-to-Affect hypothesis (Carriere et al., 2008) frequent attention lapses lead to attention-related errors that interfere in task success and should *precede* reductions in self-efficacy for that task. Given that attention lapses occur even in simple, everyday activities—particularly those we know are not problematic for most people—it follows that these continual mistakes could produce a generally diminished sense of self-efficacy and may then lead to disengagement from everyday behavior in general and eventually to depression. The present research thus applied the Attention-to-Affect hypothesis to illuminate more precisely the structure of these relations. In particular, we sought to evaluate the potential causal flow from attentional errors—specifically attention lapses and subsequent attention-related errors—through to decreased self-efficacy and on to a proneness to experience depression.

Study 1

To examine the hypothesis that perceived self-efficacy mediates the association between failures of basic cognitive mechanisms and depression, we conducted a path analysis using structural equation modeling in AMOS (Arbuckle, 2005) to examine the relations among five self-report questionnaires. Frequency of attention lapses and associated cognitive errors were assessed *via* the Mindful Attention Awareness Scale–Lapses Only and the Attention-Related Cognitive Errors Scale (MAAS-LO and ARCES: Carriere et al., 2008). We assessed depression *via* the Beck Depression Inventory—Second Edition (BDI-II: Beck et al., 1996). Self-efficacy was measured *via* the Generalized Self Efficacy scale (GSE: Schwarzer and Jerusalem, 1995).

Based on previous findings (Carriere et al., 2008) and the present arguments, we predicted the MAAS-LO would explain a significant amount of the variance in the BDI-II, while the ARCES would function as a mediator between the MAAS-LO and GSE, such that the ARCES would not explain a significant amount of the variance in the BDI-II once relations with the MAAS-LO and GSE were accounted for. The ARCES is the key variable in this case, since it allows us to place specific a priori constraints on the model, which are necessary for evaluating causal claims with correlational data, and it is most clearly recognized as an effect of inattention (i.e., it is non-sensical to instead argue that attention-related errors cause the inattention that was necessary for them to have occurred). With these same data we also evaluated the more conventional Affect-to-Attention hypothesis that negative affect creates cognitive biases which influence attention (e.g., through reduction of attentional capacity by rumination) and our perceived self-efficacy.

Method

Participants

Participants were from an international sample of 188 respondents who completed all five of the questionnaires below *via* our public attention lapses research website. Participants included in the analyses completed all four questionnaires and had no more than two missing responses for each questionnaire; 158 participants had zero missing responses. Participants received no compensation for completing the questionnaires, aside from the information already available to them on our website. Not all participants opted to provide demographic information and given that this study was conducted online we have no information for these participants beyond their questionnaire responses. Of those participants who opted to provide their demographic information, which represents the majority of the sample, there were 85 males and 97 females with a mean age of 36.1, ($SD = 13.3$; $n = 178$).

Measures

After first receiving the initial demographics questionnaire, the four questionnaires below were completed in random order across participants. In addition, the individual items within each questionnaire were randomly ordered, such that no two participants were likely to receive the exact same ordering of questionnaires and items. To accommodate occasional missing responses, item mean scores were calculated by averaging across the responses provided.

The 12-item Mindful Attention Awareness Scale–Lapses Only (MAAS-LO; Carriere et al., 2008) was selected as the measure of attention lapses. MAAS-LO items, such as "I find it difficult to stay focused on what's happening in the present," ask about inattentive behavior in everyday situations using a six-point Likert scale ranging from *almost never* (1) to *almost always* (6). Responses indicating a greater frequency suggest a greater propensity toward everyday attention lapses. It is important to explicitly acknowledge here that, aside from removal of three items and the scale being direct-scored rather than reverse-scored, the MAAS-LO is identical to the MAAS as originally developed by Brown and Ryan (2003).

A revised version of the Attention-Related Cognitive Errors Scale (ARCES; Cheyne et al., 2006) was incorporated as an assessment of notable cognitive and behavioral outcomes of attention lapses. The revised ARCES (Carriere et al., 2008) is a 12-item questionnaire measuring the frequency with which one experiences a variety of cognitive failures, for example: "I have absent-mindedly misplaced frequently used objects, such as keys, pens, glasses, etc." The ARCES employs a five-point Likert scale from *never* (1) to *very often* (5).

The 10-item General Self-Efficacy scale (GSE; Schwarzer and Jerusalem, 1995) was selected as a measure of one's perceived self-efficacy. The GSE includes items such as "I can always manage to solve difficult problems if I try hard enough" and "I am confident that I could deal efficiently with unexpected events," and uses a four-point Likert scale ranging from *not at all true* (1) to *exactly true* (4).

We used the Beck Depression Inventory (BDI-II: Beck et al., 1996) to measure depression. The BDI-II is a 21-item scale that was designed to address the diagnostic criteria for depression outlined in the DSM-IV (American Psychiatric Association, 1994). The BDI-II asks participants to select from a list of statements the one that best describes how they have been feeling throughout the last 2 weeks. Accordingly, the BDI-II includes statements such as "I am so sad or unhappy that I can't stand it" to indicate depression, and related normal mood statements such as "I don't criticize or blame myself more than usual."

Data analysis

It is worth highlighting that our research is based on correlational data, and it is a well-known truism that correlations do not guarantee the presence of causation (Fiedler et al., 2011). Likewise, the absence of temporal precedence as is typically found in correlational study designs further complicates matters and in the case of 3-variable mediation models it is impossible to statistically distinguish multiple competing causal hypotheses (Kline, 2015). While these statements offer good guidance about the need to exercise caution when working with causal models and correlational data, they are also often misconstrued as making the stronger claim that we can never reasonably infer causation from correlational data or use cross-sectional correlational data to examine any form of mediation model. As argued by Simon (1954), it is indeed quite reasonable, though certainly not conclusive, to infer the causal ordering of a set of correlational data as long as we (1) include at least one additional relevant variable, and (2) can apply specific a priori assumptions that constrain which of these variables do *not* involve a direct causal relation. This method, though argued only with respect to equations involving three variables by Simon (1954), is foundational to the process of path analyses in structural equation modeling—the statistical technique employed in our previous paper and again here. Shipley

(1999) also makes a very similar argument with respect to structural equation modeling, noting that "although correlation does not imply causation, *causation does necessarily imply particular types of zero-order correlations* or partial correlations as well as other constraints on the patterns of covariation between the variables" (p. 377; emphasis added). This is to say that, given a sufficiently complex set of variables and clear causal hypotheses about how they ought to relate to one another, it is possible to test these hypotheses by comparing a theoretically constrained covariation matrix against the observed patterns of covariation. If the causal hypotheses are true, the theoretically derived covariation matrix should correspond to the observed covariations, and yield good model fit. When competing causal hypotheses require different patterns of covariation, it likewise becomes possible to test which hypotheses, if any, provide sufficient correspondence to the observed covariation matrix. Of course, in this case, the otherwise valuable inclusion of temporal precedence to the order of data collection would make testing differing causal hypotheses on the same data more problematic. Therefore, assuming the amount of time and research funds is held constant, a researcher's study design options are limited to either collecting a single larger sample of data, which is generally preferable for minimizing chance model over specification, or multiple smaller samples of data including different patterns of temporal precedence. The former approach was precisely the case for our previous research on the Attention-to-Affect model (Carriere et al., 2008), which sufficiently explained the observed covariations among the tendency to be inattentive, to make attention-related mistakes in everyday life, and to experience depression. The similarly theoretically constrained covariation matrix of the Affect-to-Attention model, which involved a reversal in the causal role of depression, could not adequately explain the observed covariation matrix, however, suggesting its underlying causal hypothesis was flawed. In the present study we will employ the same analysis method, while continuing to expand the complexity of the potential mediation process, and exploit similar expectancies of covariation and non-covariation among the cross-sectionally measured constructs in order to evaluate and contrast the Attention-to Affect and Affect-to-Attention models. All analyses were run using jamovi version 2.3.21. Path analyses were performed *via* the pathj module with normal Maximum Likelihood estimation and structural equation models discussed in the Latent Variable Path Analyses in the Supplementary Figures S1, S2 were run *via* the SEMLj module, also using normal Maximum Likelihood estimation.

Results and discussion

Consistent with our earlier findings (Cheyne et al., 2006; Carriere et al., 2008), the ARCES and MAAS-LO were found to have good distributional and psychometric properties. All measures showed a good range of scores, no significant deviations from normality in skewness and kurtosis, and demonstrated very satisfactory internal consistency (see Supplementary Table S1, for detailed information).

Pearson Product-Moment correlation coefficients are presented in Table 1. All observed coefficients were moderate to large. As predicted, the ARCES and MAAS-LO were positively correlated, and both were negatively correlated with the GSE. Furthermore, as predicted, the GSE was also negatively associated with the BDI-II. Overall, the correlations between attentional and mood measures replicated our previous findings (Carriere et al., 2008).

TABLE 1 Study 1: pearson product-moment correlations of all measures ($N = 184$).

	ARCES	GSE	BDI-II
Mindful Awareness of Attention-Lapses Only	0.66^{**}	-0.20^{*}	0.55^{**}
Attention-Related Cognitive Errors		-0.29^{**}	0.48^{**}
Generalized Self-Efficacy			-0.49^{**}
Beck Depression Inventory-II			

$^{*}p = 0.005$, $^{**}p < 0.001$.

Our primary interest was in extending our knowledge about the role that everyday attention lapses play in the onset of depression. Hence, a model was constructed which allowed self-efficacy to mediate the relations from everyday inattention (the MAAS-LO) and attention-related errors (the ARCES) to depression (the BDI-II). To evaluate the conventional Affect-to-Attention hypothesis we simply reversed the causal paths involving depression, producing a model in which depression predicts inattention and self-efficacy.

The Attention-to-Affect model assessed the simultaneous effects of the MAAS-LO and GSE on the BDI-II, using the MAAS-LO as an exogenous variable predicting the ARCES, GSE and BDI-II. The GSE was also entered as a mediator of the effect of the MAAS-LO on the BDI-II, and the ARCES was entered as a mediator of the effect of the MAAS-LO on the GSE. This model provided reasonably good fit indices for the data, χ^2 (1, $N = 188$) $= 2.05$, $p = 0.152$, $CFI = 0.995$, $TLI = 0.972$, $RMSEA = 0.075$, consistent with the Attention-to-Affect hypothesis, *via* changes in self-efficacy. As anticipated based on previous findings (Carriere et al., 2008) the direct path between the ARCES and BDI-II was found to be non-significant ($b^* = 0.105$, $p = 0.156$) and omitted. The only notable shortcoming of this model was that the path from the MAAS-LO to the GSE was not significant ($b^* = -0.027$, $p = 0.772$). This is because almost the entirety of the MAAS-LO's relation with the GSE was mediated by the ARCES in this sample—consistent with the hypothesis that one's everyday inattention leads to a decreased sense of self-efficacy primarily through the attention-related cognitive errors that also result from attention lapses. Accordingly, a more parsimonious model, removing the non-significant path from the MAAS-LO to the GSE, is shown in Figure 1A and provides very good fit indices for the data, χ^2 (2, $N = 188$) $= 2.15$, $p = 0.341$, $CFI = 0.999$, $TLI = 0.998$, $RMSEA = 0.020$. For all direct and indirect effects see Supplementary Table S2.

The traditional Affect-to-Attention model was tested next. This model treats the BDI-II as an exogenous variable predicting both the MAAS-LO and GSE, and with the MAAS-LO mediating the relation of the BDI-II and ARCES. The Affect-to-Attention model, which implicitly directs most research, does not specifically predict a *direct* influence of self-efficacy on attention-related errors—that

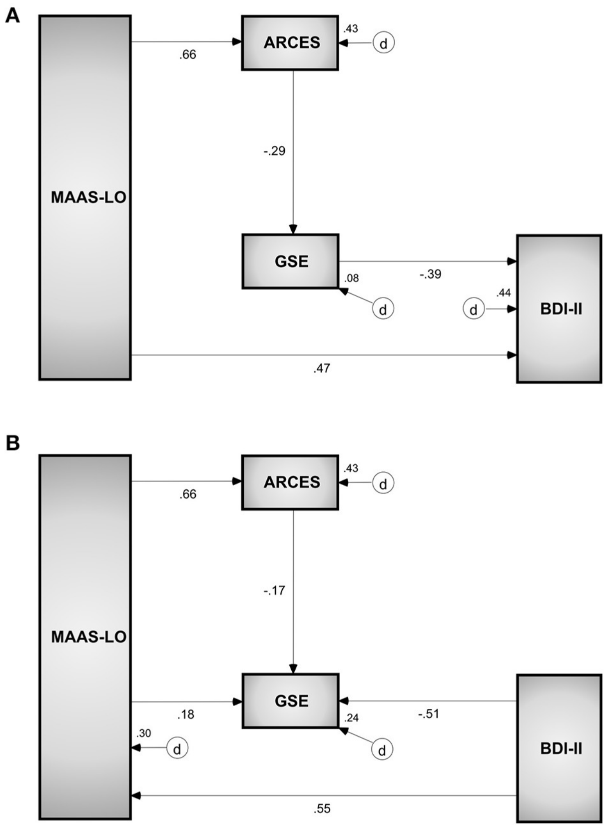

FIGURE 1
(A) Attention-to-affect path model with path coefficients for self-reported attention (MAAS-LO), error (ARCES) and self-efficacy (GSE) measures predicting depression (BDI-II). **(B)** Affect-to-attention path model with self-reported depression (BDI-II) predicting attention (MAAS-LO) and self-efficacy (GSE).

is, without influencing attention lapses first—so in the Affect-to-Attention model that we tested the direction of the path between the ARCES and GSE remained consistent with the model shown in Figure 1A. Accordingly, the Affect-to-Attention model shown in Figure 1B matches Figure 1A, except that all paths directly connecting with the BDI-II are reversed. This model provided much poorer fit indices for the data, χ^2 $(1, N = 188) = 6.61$, $p = 0.010$, $CFI = 0.976$, $TLI = 0.854$, $RMSEA = 0.173$. For all direct and indirect effects see Supplementary Table S3. Importantly, this model, which most closely matches the conventional Affect-to-Attention hypothesis, clearly does a poor job of representing the obtained pattern of relations and thus the implication is that conventional theories may be inadequate. Furthermore, this model requires a direct connection from the MAAS-LO to the GSE in order to better explain the observed covariance matrix, so is also potentially less parsimonious than the Attention-to-Affect model. A large source of variance left unaccounted for in the traditional Affect-to-Attention model involved the relation of the BDI-II with the ARCES, suggesting that, ultimately, the Affect-to-Attention model is not as effective in predicting the occurrence of attention-related errors as the Attention-to-Affect model is in predicting depression.

Given the present findings, the Attention-to-Affect model, shown in Figure 1A, best and most parsimoniously fits the data. Within this model, the MAAS-LO predicts all cognitive and affective variables, with the GSE mediating relations between the MAAS-LO and BDI-II. Moreover, in conjunction with attention-related cognitive errors (e.g., going to the fridge to get some milk, and instead taking out the juice), such attention lapses may be viewed as influencing our affective wellbeing *via* their impact on our sense of self-efficacy. Thus, the present findings once again highlight that maintaining an awareness of our actions is an important contributor not only to the outcomes of everyday activities, but potentially to our long-term emotional wellbeing.

The Attention-to-Affect model naturally focuses more on cognitive factors (i.e., attention lapses, attention-related errors, and self-efficacy) rather than affective factors (depression). Although we have thus far stressed these cognitive routes, we also acknowledge the existence of other affective mechanisms on the route from attention lapses to depression. One such potential linking mechanism is stress, particularly given that attention lapses can interfere with our ability to perform our normal, everyday activities and through such interference we may become stressed.

Study 2: evaluating a theoretical model linking attention lapses and stress

Here we adopt Selye's (1984) belief that the term *stress* should be reserved to describe physiological or emotional outcomes while the term *stressors* should be used to describe those events that cause stress. This view of stress as an emotional outcome is compatible with the work of Lazarus (1993), who argued that stress can be seen as a cognitive-emotional trait which persists across situations and remains present even in the absence of external stressors. On this view, there may sometimes be no identifiable situational cause for an individual's stress; that is, stress is simply an abundance of psychological tension, arousal, and other non-specific emotional distress resulting from no particularly salient cause. A similar view was put forward by Selye (1984), who argued that stress is a potential consequence of almost all physical, cognitive, or emotional activity—that essentially everything we do or feel has the potential to produce a stress response in the body, even if only to a very minimal extent. Accordingly, to say someone is feeling stressed reflects the individual's recognition of an *abundance* of the stress response rather than a truly categorical change in his or her present state. Expanding on the views of Lazarus (1993), we view *stress* more generally as a host of unpleasant affective traits, such as general irritability, that attention lapses may help to create, and which then potentially produce further emotional distress in the form of depressive affect. The challenge, accordingly, is to determine how stress fits into the Attention-to-Affect model.

Stress and attention

Viewing stress as emotional reactivity to life in general parallels the mechanism described in "daily hassles" research (e.g., Monroe, 1983; McIntyre et al., 2008), which provides an indirect link between attention lapses and stress. We would take this view one step further, however, to suggest that chronic inattention *directly* serves as a stressor, perhaps in part because inattention makes it more difficult to accomplish a variety of everyday and otherwise easy-to-accomplish tasks (e.g., reading; Smallwood and Schooler, 2006). In this way, if everyday inattention happens to have extraordinary consequences, such as narrowly avoiding a plane crash, it will likely produce sufficient stress to become noticeable, and those consequences would be easily identifiable as a stressor. If we experience an extraordinary abundance of minor attention-related errors in everyday life, however, then over time our stress level could also become sufficiently elevated to be perceived as bothersome, but we would not be able to identify the cause of our stress. Of course, it is also possible that inattention requires neither extraordinary nor minor consequences in order to produce stress, since the act of being inattentive could itself serve as a stressor—as was suggested by Selye (1984).

Stress and depression

Unlike the link between attention and stress, the link between stress and depression is well documented. Indeed, stress has long been identified as an important contributor to disease in general and is thought to be an especially important factor in the initial onset and later relapse into depression (Depue, 1979). Most notably, the contributions of stressors to depression are fundamental to the diathesis-stress model of depression. Such models are based on the theory that dysfunctional beliefs or behaviors tend to produce depression only when accompanied by stressful life events (for an interesting evaluation of diathesis-stress models of depression, see Robins and Block, 1989). The development of depression after stressful events may be further influenced by the tendency to experience chronic stress (Hammen et al., 2009), and even mild levels of daily stress appear to have the potential to produce mood disturbance over short timescales (DeLongis et al., 1988). These latter examples are more akin to the experience of general psychological stress we argue could be related to inattention and help bridge the gap with the diathesis-stress model of depression. A similar general experience of personal and interpersonal psychological stress was included in a structural model used by Shen and Takeuchi (2001) to understand the development of depression among Chinese American immigrants, and they found strong prediction of depression *via* stress. It is worth noting that decisions made while one is depressed may also create an environment in which the probability of experiencing future stress is increased (Hammen and Shih, 2008), thus creating a potentially vicious emotional downward spiral. Such complex interactions suggest a feedback model should be necessary to explain the relation between stress and depression but, interestingly, a reanalysis of Shen and Takeuchi's (2001) data set has shown that the reciprocal effect of depression is limited at best (Kline, 2005) and so a unidirectional relation will likely be sufficient in most cases. While none of the above studies have considered the potential role of everyday inattention in producing stress, it is nonetheless clear that, if the Attention-to-Affect hypothesis is correct, stress should play a pivotal role as a mediator between inattention and depression.

Self-efficacy and coping with stress

Attempts to minimize or avoid the effects of stress on our lives have been collectively described as *coping*, and generally fall into two categories: emotion focused coping, whereby the individual attempts to reappraise the situation in a more benign way, and problem focused coping, where the individual attempts to change the situation (Coyne and Lazarus, 1980; Coyne et al., 1981). Both coping strategies are based on an initial appraisal of the situation and one's abilities, followed by subsequent reappraisals after initial outcomes have been assessed. As such, a stress–coping feedback loop is created, in which the apparent causal flow is dependent on how early one breaks into the process (Coyne et al., 1981). Since general self-efficacy is essentially a belief in our ability to handle unforeseen situations, it makes sense to think it would play an important role at the earliest stages of this cycle. Indeed, a number of recent studies have shown the importance of self-efficacy in reducing stress and maintaining general mental health (e.g., Jerusalem and Hessling, 2009; Rees and Freeman, 2009; Nauta et al., 2010) or preventing job stress and burnout (Schwarzer and Hallum, 2008) when individuals encounter stressful situations. Thus, consistent with previous findings (Study 1), we again hypothesized that attention-related errors would predict self-efficacy, which, based on existing coping theory, we initially hypothesize would then also predict stress. It is worth noting, however, that the Attention-to-Affect hypothesis itself affords no specific causal predictions on which of stress and self-efficacy should be impacted prior to the other so the reverse causal relation is certainly possible.

Predictions and models

Guided by the above theoretical and empirical considerations, and to further investigate routes through which attention lapses and their associated errors influence depressive affect, Study 2 used stress as a partial mediator of these relations. In this study inattention was again measured *via* the Mindful Attention Awareness Scale–Lapses Only (MAAS-LO; Carriere et al., 2008) and errors resulting from inattention were measured by the Attention-Related Cognitive Errors Scale (ARCES; Carriere et al., 2008). As well, self-efficacy was again measured *via* the General Self-Efficacy scale (GSE; Schwarzer and Jerusalem, 1995). For this study stress and depression were both measured *via* their respective subscales on the Depression Anxiety Stress Scales (DASS; Lovibond and Lovibond, 1995). Based on our previous findings using the BDI-II as our measure of depression, we predicted the MAAS-LO would explain a significant amount of the variance in DASS-Depression, and that stress would play an important role as a partial mediator of this relation. Furthermore, as a measure of behavioral consequences of inattention, the ARCES should act primarily as a partial mediator between the MAAS-LO and DASS-Stress. As a result, the ARCES would not explain a significant amount of variance in DASS-Depression once relations with the MAAS-LO and DASS-Stress were accounted for. In this case, both the ARCES and DASS-Stress play a critical role in allowing key a priori causal constraints to be placed on the model.

Method

Participants

Participants were 399 undergraduate students (137 males) from the University of Waterloo, who completed a series of online questionnaires examining cognitive functioning and general emotional experience, including the measures of interest for this study. The data were compiled over three consecutive terms, in order to reach a sufficient sample size. Of those participants who provided their age, the mean was 20.8 ($SD = 4.85$; $n = 392$). The selected participants completed all four questionnaires and had no more than two missing responses from any questionnaire; 355 participants had zero missing responses. As compensation for their time, participants received partial course credit.

Measures

The majority of the questionnaires used in Study 1 were retained for the present study. These included the MAAS-LO, ARCES, and GSE. New to the present study was the short form of the Depression Anxiety Stress Scales (DASS; Lovibond and Lovibond, 1995). All questionnaires were completed in random order across participants, and item mean scores were calculated for each questionnaire in order to accommodate the occasional occurrence of response omissions.

Stress and depression were measured *via* the relevant subscales of the DASS, as it provides good discrimination between depression, anxiety, and stress, and has been shown to provide good long-term stability for each subscale (Lovibond, 1998). The 21-item DASS includes 7 questions for each subscale, asking about one's experiences over the past week, and is scored using a Likert scale ranging from *did not apply to me at all* (0) to *applied to me very much, or most of the time* (3). Response values are typically doubled in the short form of the DASS, in order to retain total score compatibility with the long form which has 42 items. No doubling was necessary for the present study, however, given that mean item scores were used and that the scaling is irrelevant for correlations in any case. To measure negative affect (depression) the DASS includes statements such as "I felt downhearted and blue" and "I couldn't seem to experience any positive feelings at all," while to measure tension (stress) it includes statements such as "I found myself getting agitated" and "I tended to over-react to situations." While the DASS does not attempt to address the DSM-IV (or DSM-V) criteria for depression (American Psychiatric Association, 1994, 2013), its depression subscale has nonetheless been shown to correlate strongly with the previous gold standard measures, the BDI (Lovibond and Lovibond, 1995; Antony et al., 1998) and BDI-II (Gloster et al., 2008), the latter having been used in our previous models. Furthermore, for the purposes of the present study, we find the clearer focus of the DASS on the simpler and more common experience of negative affect particularly appealing as we expect inattention should predict emotional distress in general, regardless of its severity.

Results and discussion

Once again, all measures were found to have good distributional and psychometric properties, with only the GSE reflecting a larger degree of kurtosis (though still acceptable, for full details see Supplementary Table S4). Pearson Product-Moment correlation coefficients are presented in Table 2. All coefficients were significant, and the majority was moderate to large. As predicted by the theory discussed earlier, both the MAAS-LO and ARCES show strong relations with DASS-Stress. Furthermore, consistent with the findings of Study 1 and the present theory, the GSE showed strong relations with the DASS-Stress, and DASS-Depression.

Attention-to-affect model

The hypothesis that attention lapses, and their resulting cognitive errors, would predict depression in part through their influence on our general sense of self-efficacy and stress level was addressed using structural equation modeling. Based on stress coping theory and recent findings that self-efficacy helps prevent a variety of negative mental health outcomes, this also involved adding a direct causal path from the GSE to DASS-Stress. Consistent with the hypothesis that attention lapses produce depression *via* changes in self-efficacy and stress, this model, shown in Figure 2A, provided good fit for the data, χ^2 (2, $N = 399$) $=$ 4.29, $p = 0.116$, $CFI = 0.996$, $TLI = 0.980$, $RMSEA = 0.054$. In this model the ARCES was found to partially mediate the relation of the MAAS-LO with DASS-Stress, with both direct and indirect effects remaining significant (see Supplementary Table S5 for all direct and indirect effects).

In Study 1 we found the relation of the MAAS-LO with the GSE was fully explained by the ARCES. The present model therefore assumed this path should be omitted. The results showed the largest residual covariance was between the MAAS-LO and the GSE, although it was within acceptable levels and, if added, the direct path coefficient was small and not significant ($b^* = -0.10$, $p = 0.119$), resulting in only a small improvement in overall model fit. Accordingly, through this replication we can now be confident

TABLE 2 Study 2: pearson product-moment correlations of all measures ($N = 399$).

	ARCES	GSE	Stress	Depression
Mindful Awareness of Attention-Lapses Only	0.53	−0.18	0.49	0.49
Attention-Related Cognitive Errors		−0.21	0.42	0.33
Generalized Self-Efficacy			−0.30	−0.35
DASS-Stress				0.66
DASS-Depression				

All $p < 0.001$.

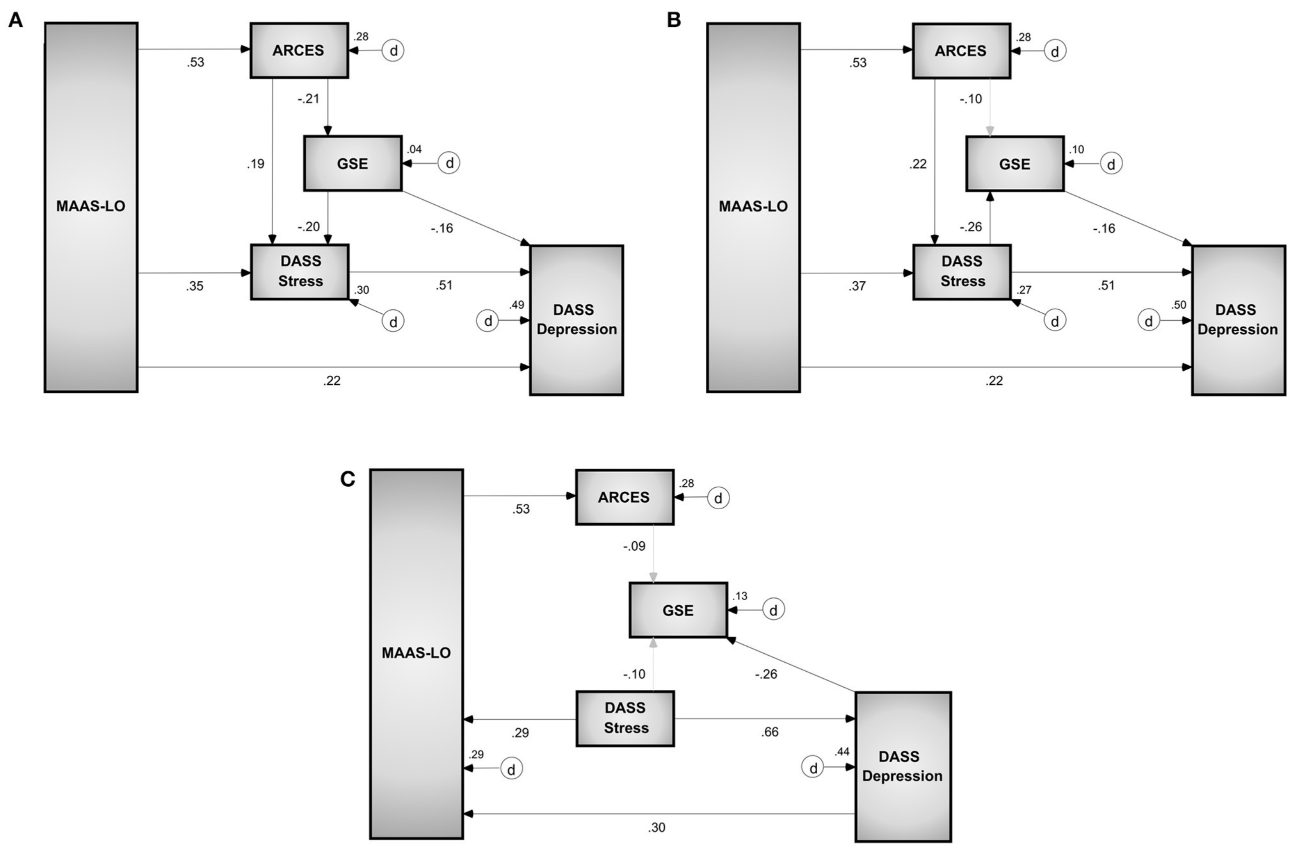

FIGURE 2
(A) Attention-to-affect path model with path coefficients for self-reported attention (MAAS-LO), error (ARCES), stress (DASS-stress), and self-efficacy (GSE) measures predicting depression (DASS-depression). **(B)** Attention-to-affect model with stress predicting self-efficacy. **(C)** Affect-to-attention path model with self-reported depression predicting attention and self-efficacy. Non-significant paths are shown in gray.

that the effect of inattention on self-efficacy is due to the fact that attention lapses lead us to make attention-related errors when completing everyday tasks. That is, it is only when we notice the negative effect inattention has on completing tasks that our sense of self-efficacy is diminished. Interestingly, there was a more parsimonious way to account for the residual covariance of the MAAS-LO and GSE in this model, which also adds an alternative view of the relation between inattention and self-efficacy. Based on the reviewed stress coping theory, for the Figure 2A we placed a path from the GSE to DASS-Stress. Reversing this direction, however, allows both the ARCES and DASS-Stress to function as mediators of the relation between the MAAS-LO and GSE, and is consistent with the alternative view that stress is likely to first contribute to a diminished sense of self-efficacy. Shown in Figure 2B, this revised Attention-to-Affect model has relatively little impact on most path coefficients, though the direct effect of the ARCES on the GSE is substantially diminished and only marginally significant (see Supplementary Table S6 for all direct and indirect effects). This model is, notably, equally consistent with the Attention-to-Affect hypothesis that attention lapses produce depression *via* changes in self-efficacy and stress, and provided good fit for the data, χ^2 (2, N = 399) = 1.27, p = 0.529, CFI = 1.00, TLI = 1.01, $RMSEA$ = 0.000. Thus, the present data are most consistent with the view that both inattention-induced stress and attention-related mistakes in everyday life generally lead one to experience a reduced sense of self-efficacy, prior to the onset of depression.

Affect-to-attention model

The present study substantially increased the complexity of our proposed Attention-to-Affect model and, accordingly, also any proposed alternative Attention-to-Affect models. It is perhaps unlikely that even among experts in clinical psychology a single alternative model could be easily agreed upon based on the existing literature alone. Some alternative models may rival the fit of our Attention-to-Affect model, but we suggest they would likely do so by subtly misrepresenting the currently held theories about how attention and affect are related with respect to self-efficacy and stress. Of course, other alternative Affect-to-Attention models may represent theory well, but provide poorer model fit and thus continue to call the underlying theory into question. It is not our claim to have addressed all potential alternatives, nor our desire to overfit the data in order to produce the best possible alternative model as a comparison. Rather, we have attempted to test our best estimate of a theoretically defensible alternative Affect-to-Attention model for all the measures included in this study.

In Figure 2C we present an Affect-to-Attention model that treats DASS-Stress as the sole exogenous variable, and provided poor overall fit to the observed data, χ^2 (3, $N = 399$) = 19.2, $p < 0.001$, $CFI = 0.972$, $TLI = 0.905$, $RMSEA = 0.116$. In this model the direct path from DASS-Stress to the GSE is not significant ($p = 0.081$), suggesting most of its influence is *via* DASS-Depression (see Supplementary Table S7 for all direct and indirect effects). Removing this path does not, of course, substantially improve model fit. Indeed, the largest sources of residual covariance in this model are in the relations between the ARCES, DASS-Stress, and DASS-Depression. Critically, we are not aware of any theoretical reason to believe either stress or depression could produce attention-related cognitive errors except indirectly through their effect on attention or perhaps some other more action-oriented third variable, which, notably, the more parsimonious Attention-to-Affect model does not require. Accordingly, there is no theoretically-defensible way to add direct causal connections from either stress or depression to attention-related cognitive errors. Overall, the present findings suggest that, once again, the Affect-to-Attention model is not as effective in predicting attention-related errors as the Attention-to-Affect model is in predicting depression and, therefore, the importance of everyday inattention to the experience of depression is worthy of greater consideration.

General discussion

Starting from our earlier model (Carriere et al., 2008), which postulated attention lapses as a common cause of a sequence of cognitive, behavioral, and affective outcomes, the present studies were designed to investigate additional potential intermediates between attention lapses and affective dysfunction. The addition of self-efficacy as a collection of beliefs about our ability to perform everyday tasks (Study 1) and stress as an individual tendency to experience an unpleasant abundance of tension, arousal and general emotional distress (Study 2) allowed the expansion of our causal model from initial attention failures to affective outcomes. Once again, the present findings provided good support for the view of attention lapses as a cause of depression. The present findings are also consistent with a view of attention lapses as potential stressors in their own right—capable of producing stress without the consequent major life events more typically identified as causes of stress. There remains the distinct possibility, however, that everyday inattention also increases the likelihood that we encounter difficult life situations, and so stressful life events are still an important component of the attention–stress relation. As we did not specifically inquire about these participants' experiences of stressful life events, the present findings cannot directly address this question.

It is worthwhile highlighting once again that relation of the MAAS-LO with the GSE was fully explained by the ARCES in both Study 1 and Study 2. Accordingly, we conclude that reduced self-efficacy results from attention lapses that lead us to make attention-related errors when completing everyday tasks, although stress may play an important role in this process as well. That is, it is only when we either experience stress or notice the negative effect inattention has on completing tasks, that our sense of self-efficacy is diminished. Thus, future studies looking to model the effect of inattention on self-efficacy should incorporate at least attention-related errors as a mediator of this relation.

A limitation of the present models is that they rely solely on relations observed among self-report questionnaire assessments of attention and emotion. This limitation is a consequence of the present research focusing on trait-level tendencies of the individual. That is, we investigated general tendencies to be inattentive, stressed, or depressed in everyday life regardless of specific situations one might encounter. Taking these general tendencies to the level of specific situations may not be easily accomplished because any given situation will inevitably introduce its own complexities. Such complexities are often unexpected and difficult to account for, and thus correct interpretation of the results is made more difficult. For example, it is probable that, although attention lapses may serve an important role in the etiology of depression, depressed affect will be associated with self-focused depressive rumination that provides additional attentional load and is associated with mind-wandering away from important characteristics of the task at hand (Smallwood et al., 2003, 2007)—and this process is likely to play out over both long and short timescales. Indeed, real-life scenarios are likely to involve reciprocal relations between trait inattention, trait and situational stress, and depression where a general tendency toward inattention increases the likelihood of stressful events occurring (such as in the example of the distracted Chatsworth train engineer from the introduction) and stressful events then lead to depression, both of which create new cognitive loads that make maintaining attention even more difficult, and the cycle repeats. Likewise, stressful life events could be responsible for the creation of short-term attentional difficulties and depression, and once these events are resolved the other effects will not necessarily persist. Notably, the studies discussed here, being focused solely on trait-level cognitive and emotional processes, do not preclude any such possibilities. What they do suggest is that everyday attentiveness should serve as a protective factor, minimizing the potential impact of traumatic life events, and that with careful control and interpretation, future studies might benefit from including measures of attention lapses and an attempt to better understand the long-term cognitive, emotional and behavioral interactions that play an important role in the onset and maintenance of depression. In the end, that several different studies have produced similar findings, even while involving different mediators of the link between attention lapses and affective distress, should make their findings more compelling. Accordingly, the results obtained in the present studies should be interpreted as a stimulus for additional research on the potential involvement of seemingly harmless lapses of attention in everyday life in the development of highly problematic emotional states.

It is worth noting that, although progressing down a somewhat different causal path, the present line of research is perfectly compatible with the findings of Killingsworth and Gilbert (2010). Through an experience sampling study these researchers have shown a significant relation between mind wandering in everyday situations and future happiness, with happiness reportedly decreasing following reports of mind wandering. The effect of mind wandering on happiness was independent of, and larger than, the emotional effect of the activity performed at the time. Furthermore, mind wandering was associated with decreased happiness when the content of that thought was either neutral or negative, relative to the same activity performed without mind wandering. Interestingly, mind wandering also did not lead to an increase in happiness when the topic was pleasant. Together these findings suggest mind wandering has a general negative effect on our emotional state, which may be only partially counteracted by thinking about pleasant topics. Of course, other slightly more recent work similarly examining the connection between mind wandering and negative mood produced contradictory results, where sadness predicted mind wandering but mind wandering did not predict sadness on short timescales (Poerio et al., 2013). Again, though similar in some respects to the present studies, these findings are based on reports of mind wandering rather than attention lapses or attention-related errors, and momentary happiness rather than depression over longer timescales. Nevertheless, there are obvious links between inattention and mind wandering, and between depression and momentary happiness, and so it is good to see some potential convergence on the same general conclusion in these separate lines of research.

On the whole, the present results provide good support for the hypothesis that chronic attention lapses may set in motion processes that ultimately lead to depression, including those working *via* their influence on our stress level. This process is likely to be, in part, also mediated by the effects of failures of attention on our general sense of our ability to accomplish both mundane and novel tasks. These models redefine the role of attention lapses in our everyday emotional experiences and are all the more compelling as a result of the consistency with which attempts to address the various causal relations underlying these experiences produce similarly well-fitting models. As a result, the present findings suggest that direct attentional training exercises could be an important, and presently underutilized, method of preventing onset or relapse of depressive episodes. Of course, the present research is only an initial foray in this domain, and it is worth noting again that the present models contrast the most simplified views of the interrelations among attention, self-efficacy, stress, and depression. We do not believe there is any debate over the validity of these interrelations, nor the belief that causality is involved in these relations. The Affect-to-Attention models we tested therefore represent what we believe are the most typical theoretical perspectives taken when discussing causation among these particular variables; those models failed to explain the data. The Attention-to-Affect models likewise represent our preferred alternative theoretical perspectives on the causal roles, after recognizing that seemingly minor inattentiveness is certainly capable of producing a wide range of major life consequences and that at least one study has already suggested it is possible to treat major depressive disorder with only attention training (Papageorgiou and Wells, 2000). But it is undoubtedly the case that in the real world the interrelations are more complex than any of our models allow. Indeed, the primary causal flow may be from attention-to-affect in general, but from affect-to-attention (particularly from stressors to inattention) in many real-world scenarios. It is clear there is a great deal of additional research to do with regard to testing even more potential pathways from either attention-to-affect or affect-to-attention (e.g., anxiety is likely to be another important variable) and we will not fully understand the connection between attention and affect without considering more complex alternatives from all plausible causal perspectives.

Data availability statement

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

Ethics statement

The studies involving human participants were reviewed and approved by University of Waterloo Research Ethics Board. The patients/participants provided their written informed consent to participate in this study.

Author contributions

JSAC is the main author, responsible for most data analysis, and writing. AN was responsible for data collection in Study 2 and assisting in developing the theoretical models. JAC and DS was a supervisor and assisted with proofreading and conceptual development of the models. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fcogn.2023.1125197/full#supplementary-material

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Visual search and the inhibitions of return

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In the early 1980's independent research streams were launched by two of the 20th century's leading attention researchers. Anne Treisman's research program is best-known for distinguishing empirically between serial and pop-out search and for proposing feature integration theory and the idea of an attentional operator, that sequentially inspects items or groups of items when search is difficult. Among his many contributions to psychological science, Michael Posner is well-known for pioneering simple model tasks that made the allocation of visual attention in space amenable to scientific inquiry. When one version of the Posner cuing paradigm was used to explore visuospatial orienting it was serendipitously discovered that an "inhibitory" effect occurred in the aftermath of events that captured visuospatial attention involuntarily. This "inhibitory" phenomenon became known as Inhibition of Return (IOR), and, as implied by its name, the underlying mechanisms were thought to bias attention away from previously explored places. These two research programs were linked in 1988 when Raymond Klein exploited the distinction between pop-out and serial search to test and verify Posner's proposal that this inhibition might be a novelty seeking mechanism that could improve search efficiency. Subsequent research has identified at least two different inhibitory mechanisms that bias attention toward novelty. We present evidence using several diagnostics (central vs. peripheral targets, joint consideration of speed and accuracy, and the locus of slack logic embedded in the psychological refractory period effect) to illustrate the dual natures of IOR. The input form operates on a salience map that influences what will capture our attention, while the output form operates on a priority map that influences what behaviors (including orienting) are likely to be executed. The input form is generated when the reflexive oculomotor system is suppressed while the output form is generated when this system is not suppressed. We believe that both forms of IOR can serve the novelty seeking (and search facilitating) function proposed by Posner and others. Yet, many questions remain unanswered. Some of the many remaining puzzles will be described and we hope that our review will stimulate research to solve them.

KEYWORDS
visual search, inhibition of return, orienting, attention, novelty seeking

Introduction

This story begins in the early 1980s when two of the late 20th century's leading attention researchers, Michael Posner and Anne Treisman, made seminal empirical discoveries for which they offered exciting interpretations, aspects of which remain topical today ∼40 years later. This review is not intended to provide comprehensive coverage of all the seminal contributions of these luminaries. Rather we will focus on our thoughts about the concept of inhibition of return (IOR) and how IOR provides a bridge between Treisman's "glue" and Posner's "beam."

Treisman and Gelade (1980) distinguished empirically between serial and pop-out search. Reaction time to find the target in serial search increases roughly linearly with the number of items in the display whereas search reaction time to find the target in pop-out search is relatively unaffected by the number of distractors (see Figure 1A). To help explain these findings and to provide one method for minimizing the possibility of illusory conjunctions, Treisman proposed feature integration theory, wherein spatial attention was the metaphorical "glue" that integrated otherwise free-floating features that fell within the metaphorical "beam" of attention (see Figure 1B). Whereas Treisman (e.g., Treisman and Schmidt, 1982; for a review see Treisman, 1988) supported this theory via converging operations, later, various aspects of the theory were discarded (e.g., Wolfe et al., 1989; Treisman, 1993; see Humphreys, 2016 for a review). Nevertheless, the idea of an attentional operator, that sequentially inspects items or groups of items when search is difficult, has endured (highlighted in the inset of Figure 1B).

Our focus on Treisman's work is not dependent on the correctness of feature integration theory, but rather is rooted in how the metaphorical spotlight behaves when search is difficult, as illustrated in solid lines of Figure 1A. The functions are not only nearly linear, but the slope of the target present trials is ∼1/2 of that for target absent trials. It is generally agreed that the kind of difficult search task illustrated here is characterized by a serial, self-terminating inspection of items or small sets of items in the array. The efficiency of such a sequential inspection strategy would be improved if there were a mechanism to discourage reinspections of already inspected items or regions in the array. Whereas, such a serial inspection might depend on eye movements, as are necessary for some search tasks (such as searching for Waldo/Wally, in the series of picture books by Martin Handford), using targets defined by the absence of a feature (inspired by Treisman and Souther, 1985) as illustrated in Figure 2A, Klein and Farrell (1989) demonstrated that when search is covert (because eye movements were not permitted), the pattern of results was more or less the same as when eye movements were permitted (Figure 2B). Thus, with stimuli like these, the inspections can be attributed to covert shifts of the attentional spotlight highlighted in Treisman's model [inset in panel (B) of Figure 1].

In the late 1970's, in a series of presentations at meetings of the Psychonomic Society, Posner and colleagues developed and exploited two model tasks, often referred to as "Posner cuing tasks," for exploring the endogenous and exogenous allocation of the "beam" of attention in space (see also, Posner et al., 1978). This work was punctuated by two papers published in 1980 (the same year as Treisman and Gelade): Posner (1980) and Posner et al. (1980). While somewhat lacking in ecological validity, such model tasks are useful because they enable isolation of mental operations for investigation. In the Posner cuing paradigms the onset of a cue precedes the target, often by an interval (the cue-target onset asynchrony, or CTOA) that might be varied. Usually targets are presented at two alternative peripheral locations. In one variant, used to explore endogenous control of spatial attention (see Figure 3A), the cue is presented centrally and is informative about the upcoming target's location. Using the model task for exploring exogenous control of attention (see Figure 3B), wherein cues are uninformative and presented in the periphery, Posner and Cohen (1984) discovered that an initial facilitation at a cued location was followed by inhibition there (see Figure 3C), an effect that has come to be known as inhibition of return (IOR; Posner et al., 1985).

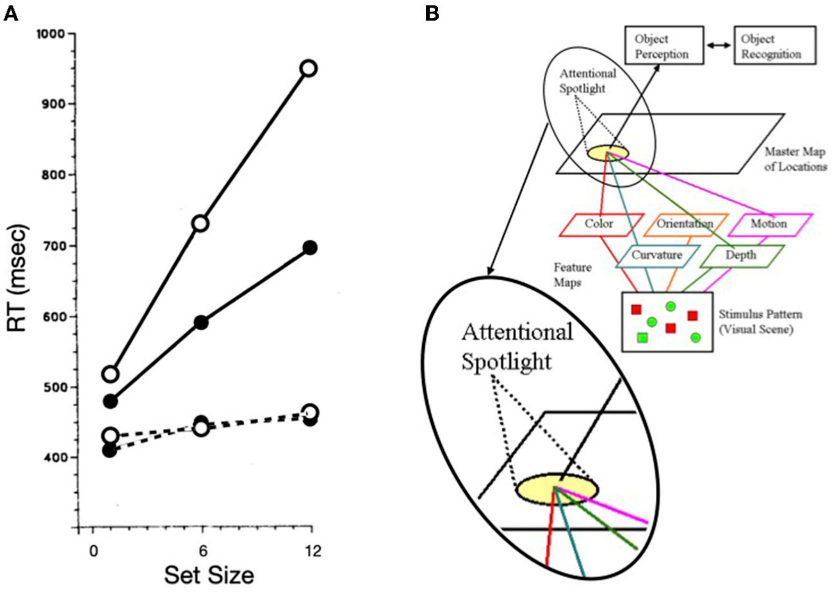

FIGURE 1
(A) Search reaction time as a function of set size for a difficult search task (solid lines) and an easy (or pop-out) search task (dashed lines). Target absent trials are plotted as open circles; target present trials as solid circles (redrawn from Treisman and Souther, 1985). **(B)** A schematic illustration of Feature Integration Theory with an inset used to highlight the "spotlight" of attention.

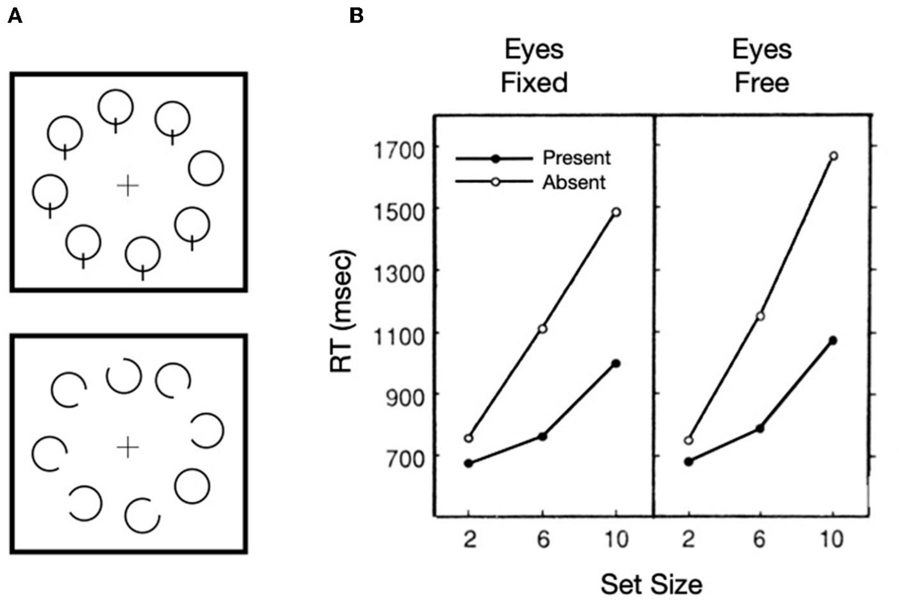

FIGURE 2
Sample target present stimuli **(A)** and results **(B)** from Klein and Farrell (1989, Exp. 2).

These streams of research, pioneered by Treisman and Posner, were linked in 1988 when Klein (1988) tested and verified Posner's proposal that this inhibition might be a novelty seeking mechanism that could improve search efficiency. Two related themes will be explored in this paper. Firstly, as noted by Hilchey et al. (2014b) the causes and effects of the inhibitions identified in the aforementioned papers from Posner's lab (Posner and Cohen, 1984; Posner et al., 1985) were different, suggesting that there might be (at least) two forms of inhibition[1]. In the next section, we will review converging evidence for this possibility and we will show that one form of inhibition operates early in the processing stream to affect the salience of possible targets while another form operates late in the processing stream to bias responding (including eye movements) away from previously attended objects and locations. Secondly, Posner et al. (1985), for whom IOR was a bias against re-orienting toward a previously inspected location, explicitly suggested that "such a bias would have obvious advantages in scanning for visual targets" presumably because IOR could operate to discourage reinspections. In the following section we will review research demonstrating that IOR operates during search and we will propose that both forms of inhibition could serve the novelty seeking function attributed to it/them in Posner's seminal papers. Finally, there are many interesting and unanswered questions about IOR and IOR in visual search, some of which will be introduced in the final section of our review.

Converging evidence for two inhibitory after-effects

The possibility of two inhibitory aftereffects of orienting can be found in the two seminal papers from Posner's lab. As described in Hilchey et al. (2014b), whereas Posner and Cohen (1984) proposed that repeated stimulation was the cause and the effect was to slow the detection of the target, Posner et al. (1985) proposed that oculomotor activation was the cause and the effect was a response bias. Unfortunately, this dramatic difference in cause and effect was not signaled by Posner et al. (1985) who named the(ir) inhibitory aftereffect, inhibition of return (IOR) while presenting their work as a continuation of Posner and Cohen's. As we will show in the next sections we strongly agree that there are two different inhibitory phenomena with different effects upon processing. Repeated stimulation of an input pathway does generate a relatively short-lived sensory adaptation effect (e.g., Boehnke et al., 2011) that can be difficult to distinguish from the IORs described below, and might be considered to be a third inhibitory aftereffect. Three diagnostics that permit researchers to distinguish whether an inhibitory effect is operating early or late in the processing of targets will be described briefly below. These are more thoroughly described in Klein and Redden (2018), Redden et al. (2021) and in the various papers wherein the original research was reported. It is noteworthy that all of the evidence described in the next three sections was obtained using versions of Posner's model task for exploring exogenous orienting.

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[1] Some readers might be uncomfortable calling both forms of inhibition "inhibition of return" as we do in this review. One reason for such discomfort can be found in Hilchey et al. (2014b). After dissociating "late" input and output forms of inhibition, they recommended: "that this late 'inhibitory' cueing effect (ICE) be distinguished from IOR because it lacks the cause (oculomotor activation) and effect (response bias) attributed to IOR when it was named by Posner et al. (1985)".

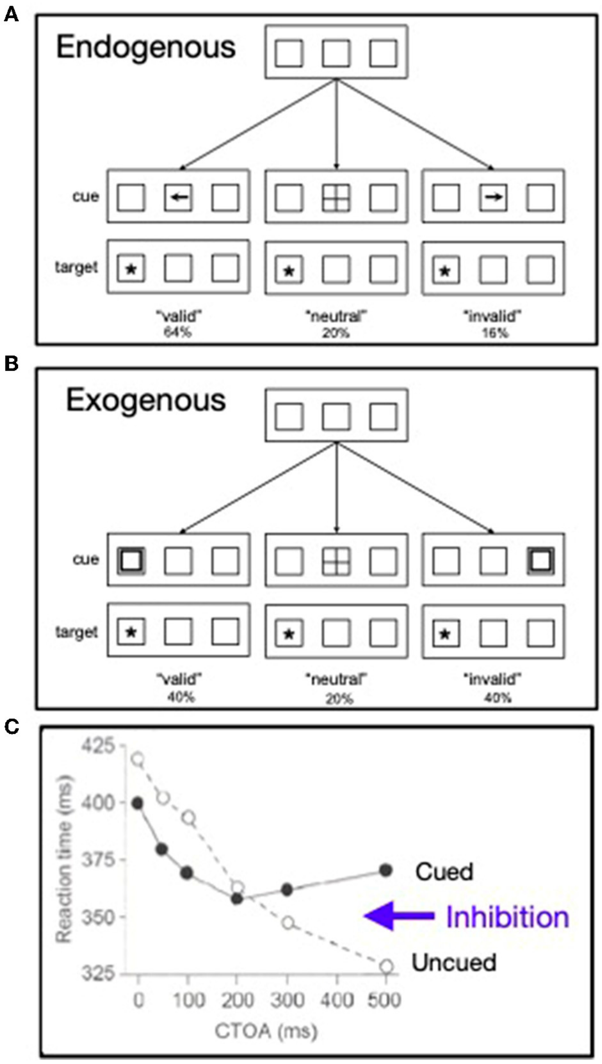

FIGURE 3
(A) Paradigm pioneered by Posner for exploring the endogenous control of attention in visual space. **(B)** Paradigm pioneered by Posner for exploring the exogenous control of attention in visual space. **(C)** From the exogenous cuing paradigm the pattern of response times is shown as a function the cue-target onset asynchrony (CTOA) (redrawn from Posner and Cohen, 1984).

Measuring aftereffects with spatial responses to peripheral targets vs. central arrows

The idea to compare responses toward inhibited locations using peripheral targets vs. central arrows was pioneered by Rafal et al. (1994) and Abrams and Dobkin (1994). The logic is straightforward. If the effect is a response bias away from the inhibited location then it shouldn't matter if localization responses are made to centrally presented arrows or peripheral targets. On the other hand if the inhibition is about the encoding of information presented at the originally cued location, the effect should be present with peripheral targets and absent with central targets.

The most thorough application of this diagnostic was rationalized and presented in Taylor's dissertation (as reported in Taylor and Klein, 1998, 2000)[2]. The methods and results from Taylor's 24 variants on Posner's model task are presented in Figure 4. Two dramatically different effects can be seen here: When eye movements were made either to the first or second stimulus (conditions outlined in green), measured IOR, if observed with peripheral targets was also observed with central targets suggesting an effect that is a straightforward response bias. In striking contrast, when no eye movement was executed to either stimulus (conditions outlined in red) inhibition was only observed if the target was presented in the periphery. This "input" form of inhibition is consistent with Posner and Cohen's (1984) hypothesis that "inhibition" decreases the quality of the target's input signal. When eye movements were made, however, the evidence is consistent with Posner et al. (1985)'s hypothesis that "inhibition," thereafter called inhibition of return, biases responses against locations to which eye movements were made or primed (viz the "output" form).

From this pattern, Taylor and Klein (2000) inferred that which form of IOR would be observed depended on whether eye movements were made (an inference later endorsed with converging evidence by, among others: Fischer et al., 2003; Hunt and Kingstone, 2003; Pratt and Neggers, 2008). Based, in part, on the input pattern being observed when anti-saccades are made, Klein and Hilchey (2011) later proposed that it is not response modality *per se* that is the "switch" determining which form of IOR would be generated; rather it is the activation state of the reflexive oculomotor system. The input form is generated when this system is suppressed; otherwise, the output form is generated. Along with some of the studies presented in this section, a series of empirical papers confirmed this proposal (Satel et al., 2013; Hilchey et al., 2014a,b, 2016; Eng et al., 2017).

Measuring aftereffects in speed-accuracy space using a non-spatial discrimination task

When participants report a non-spatial property of a target stimulus (e.g., color or shape) with keypress responses, response times are faster when there is congruence between the target's location and the location of the responding effector. Discovered by Dick Simon (Simon, 1969; for a review, see Simon, 1990) this compatibility effect was later labeled the Simon effect (Hedge and Marsh, 1975). With a focus on the Simon effect and without connecting their ideas to Taylor and Klein's (2000) two forms, Ivanoff et al. (2002) proposed that the RT delay that ubiquitously characterizes IOR could arise for two quite different reasons as illustrated by the arrows and speed-accuracy tradeoff (SAT) functions in Figure 5: IOR delays or slows the

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[2] Prior to this study, IOR had been reported or inferred to be present in 10 of the 24 conditions tested by Taylor. It is noteworthy, particularly in the context of the replication crisis, that Taylor's dissertation found IOR in each of these 10 conditions. Although there are no published replications of Taylor's design, studies published since 2000 on individual or sets of conditions represented in Figure 4, largely confirm what is reported here.

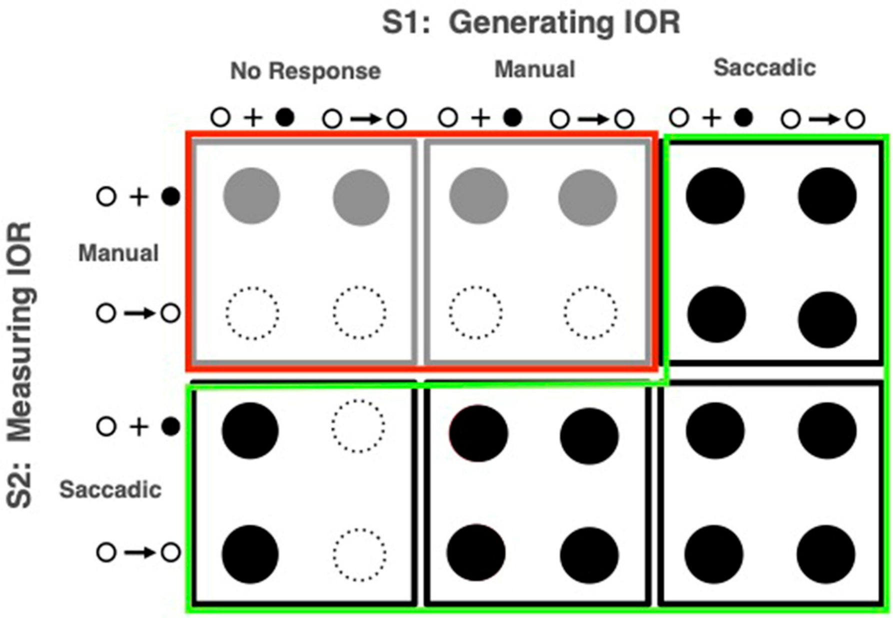

FIGURE 4
Methods and results from Taylor and Klein (2000). In a counterbalanced order each of 18 participants experienced, in sessions on different days, the conditions represented in the six boxes which differed according to the task used to generate IOR (S1) and the task used to measure it (S2). The iconograms that mark the small rows and columns within each box represent the two signal types: peripheral onset and central arrow. Solid circles depict statistically significant inhibitory effects. The conditions purported to elicit output-based effects are identified by the green highlighting (solid black circles) and input-based effects are identified by the red highlighting (solid gray circles).

accumulation of information from the inhibited location or IOR is simply a response bias against the inhibited location. Although their meta-analysis of the literature revealed a significant interaction between IOR and the Simon effect (with a larger Simon effect at the inhibited location) this interaction was consistent with either explanation of IOR's slowing of RTs (as illustrated above in Figures 5A, B) depending on one's view of how the inhibition might affect the task-irrelevant location code activated by the target.

Importantly, the direction of the cuing effect in SAT space (as in Figure 5C) can be used as a diagnostic for the form of IOR one has generated in an experiment with a non-spatial discrimination task. When RT is delayed by the inhibition and accuracy is either unaffected or decreases, the input form was generated. Otherwise, the output form was generated. Using and supporting this framework, Chica et al. (2010) reported IOR findings from a non-spatial discrimination task: when observers were instructed to ignore the cue the input form was generated, whereas when observers were instructed to make a pro-saccade to the cue and back to fixation, the output form was generated. Redden et al. (2016) built upon this result, and, using an anti-saccade condition rather than "ignore" condition in Chica et al. (2010), explicitly tested the hypothesis posited by Klein and Hilchey (2011) that the nature of IOR is contingent not on whether an overt orienting response is or is not made, but rather on the activation state of the reflexive oculomotor system. If any overt orienting response was to generate the output form of IOR, then both the anti-saccade and pro saccade conditions ought to generate an SAT. However, if the state of the reflexive oculomotor system is the determining factor, then the anti-saccade condition ought to generate an input effect akin to the "ignore" condition. This is based on the proposal (Forbes and Klein, 1996) that an observer must suppress the reflexive oculomotor system in order to correctly perform an anti-saccade (Everling et al., 1999; Ignashchenkova et al., 2004). The striking results are presented in Figure 6 where it can be seen, using the SAT diagnostic, that the input form of IOR was generated by the cue when an anti-saccade was executed (Figure 6A) while the output form was generated by in the pro-saccade condition (Figure 6B). It is noteworthy that when diffusion modeling was applied to these data (Redden et al., 2021) the parameters that best explained these findings were a slower accumulation of evidence in the anti-saccade condition and an increase in the threshold evidence for triggering a response in the pro-saccade condition.

Linking back to the Simon effect, Redden et al. (2016) also found that IOR and Simon interact in opposite directions depending on which form was generated: when the input form was generated, the Simon effect was enhanced at the cued relative to the uncued location—a pattern consistent with an increased tendency toward the prepotent response when target signal quality is reduced, whereas when the output form was generated, the Simon effect was reduced at the cued relative to the uncued location—a pattern consistent with a reluctance to make responses in the direction of the cue.

Recently, using pro-saccades to elicit the output form of IOR, Redden et al. (2023) combined the central arrow diagnostic with the SAT diagnostic by intermixing peripheral and central arrow targets.

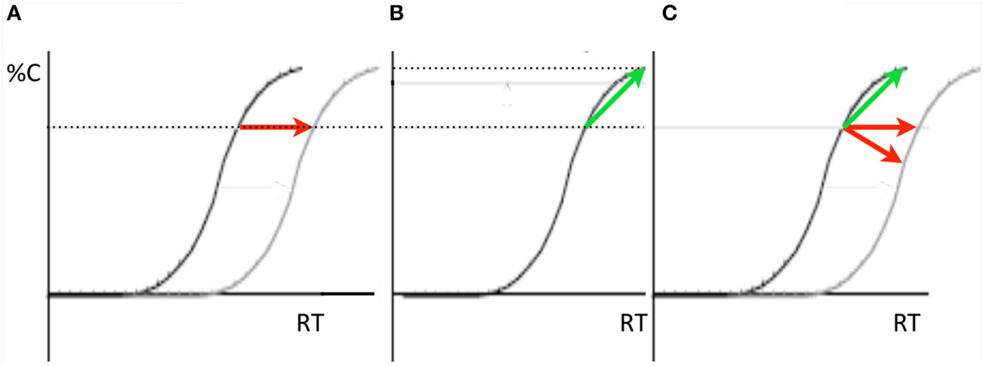

FIGURE 5
In all three panels in which performance is represented in SAT-space, accuracy of responses is represented on the Y-axis and response time is represented on the X-axis. **(A, B)** Two ways IOR might slow reaction times when a non-spatial discrimination is required to a target. **(A)** IOR might delay (as illustrated here) or slow the accumulation of task-relevant evidence about the target's identity (this would be represented as a shallower slope of the accumulation function). **(B)** IOR might have no effect on the accumulation of evidence about the target but instead is a bias against responding to targets at the inhibited location (represented here as in increase in the amount of evidence required to make a response to inhibited targets). **(C)** The slope of the IOR effect in SAT-space can be used as a diagnostic for determining whether the effect is of the input form (red arrows) or likely not (green arrow).

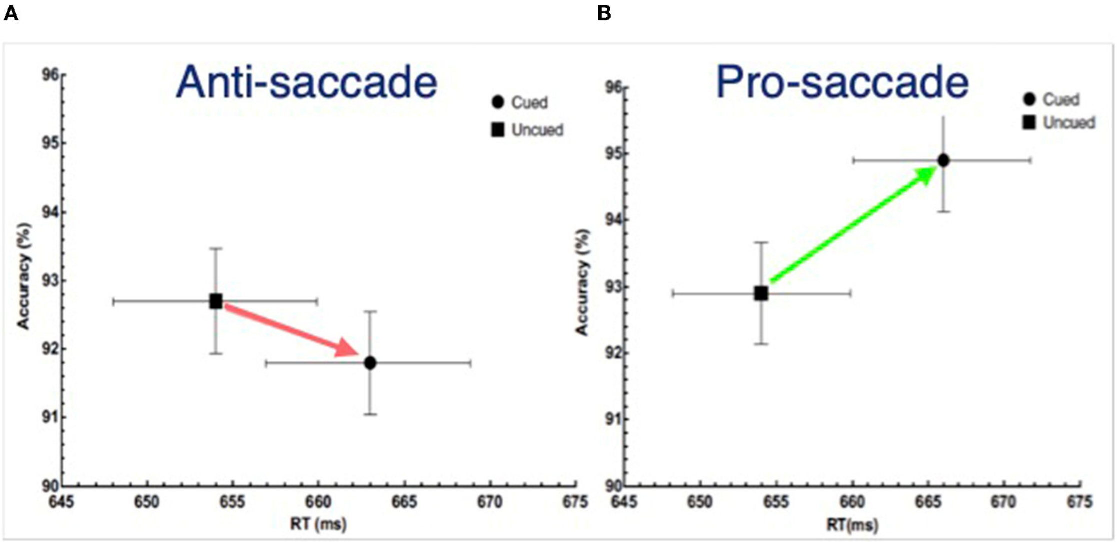

FIGURE 6
Reaction time (X-axis) and accuracy (Y-axis) as a function of whether the target requiring a non-spatial discrimination was presented at the previously cue or uncued location following either **(A)** an anti-saccade or **(B)** a pro-saccade made in response to the cue. Redrawn from Redden et al. (2016).

Performance following peripheral targets replicated the pattern found by Redden et al. (2016), with slower but more accurate responses to targets presented at the location of the original cue and a reduced Simon effect for these targets. Responses to central arrows calling for responses toward the cued location were also delayed. These three findings converge on the conclusion that the output form of IOR is a bias against responses in the direction of the cue.

Measuring aftereffects using the psychological refractory period paradigm

When two targets requiring speeded responses by different responding effectors are presented in close succession to different sensory modalities, the response to the second target suffers a delay that typically decreases linearly with increases in the interval separating the targets. The delay, which is thought to reflect a relatively "central" bottleneck stage of processing that can only be allocated to one task at a time, has come to be called the "Psychological Refractory Period."

When the task associated with the second target is delayed by some factor, such as IOR, the "locus of slack" logic (as described in Pashler, 1998, p. 275–287) can be used to determine whether the effect is operating relatively early in the processing sequence (before the bottleneck) or relatively late (at the bottleneck stage or after the bottleneck). At short intervals between the two targets, when the IOR effect is operating early, no effect is seen in reaction time because the extended processing during a pre-bottleneck stage is absorbed into the period of slack. In contrast, if the effect is

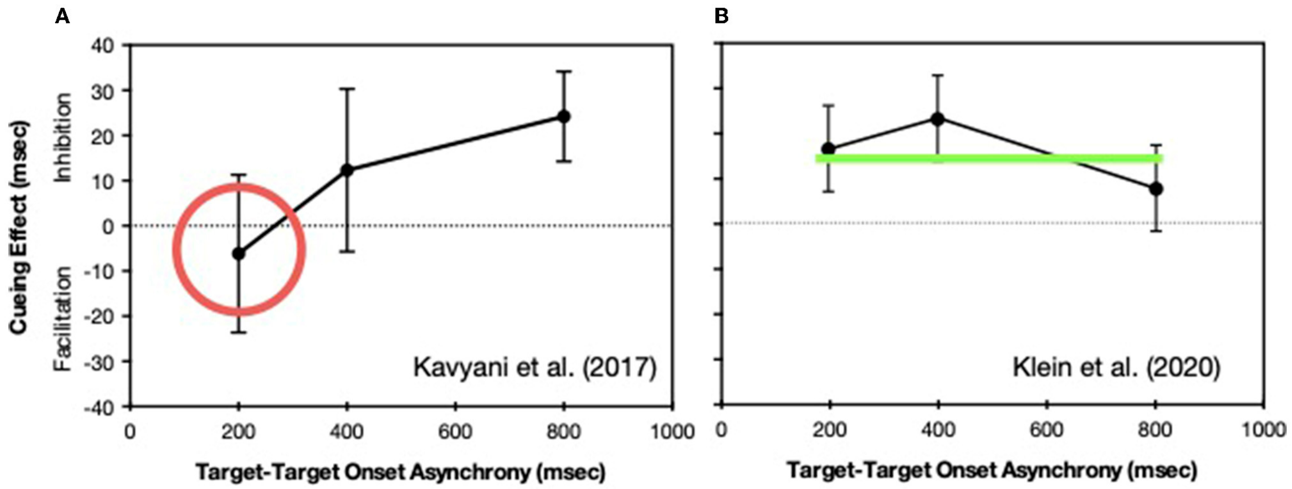

FIGURE 7
Cuing effects (positive values represent inhibition of return) in reaction time as a function of the intervals between the onsets of the targets for Task 1 and Task 2. In two experiments Task 1 required an auditory discrimination whereas Task 2 required indicating whether a visual target appeared to the left or right of the current fixation. **(A)** Eye movements were forbidden and IOR was generated by an uninformative peripheral cue presented (before both targets) at a position to the left or right of a central fixation stimulus (from Kavyani et al., 2017). **(B)** The initial fixation (drift-correction) stimulus (cue) was presented either to the left or right of the central stimulus and IOR was generated at the initial fixation location when an eye movement to the central stimulus was made (from Klein et al., 2020). The two tasks were presented after this saccade.

operating at or after the bottleneck, it will be the same regardless of the interval between targets. As can be seen in Figure 7 we found that IOR when generated while the reflexive oculomotor system was suppressed was operating early (Kavyani et al., 2017) whereas when IOR was generated while the reflexive oculomotor system was active (generated by a saccade to center after the drift-correction in the periphery) the effect was operating later (Klein et al., 2020).

Summary

We believe that the evidence described above converges inexorably on the conclusion that there are at least two different inhibitory effects and that what determines which effect will be generated depends on the state of the reflexive oculomotor system around the time the effect is generated. As originally proposed and demonstrated by Posner et al. (1985) we also believe that the cause of the output effect is activation of the oculomotor system. For a variety of reasons, we are less confident about the cause of the input form, a question to which we will return later.

From the simple model task to visual search

Several paradigms have been used to converge on the conclusion that IOR operates during search by encouraging orienting to novel items or discouraging inspections of already inspected items. In the sections that follow some of these paradigms and the evidence they have provided will be described.

IOR after a covert-search episode

To test Posner's proposal that the inhibitory tags left in the aftermath of orienting might encourage novelty-seeking and play a constructive role in some visual search tasks, Klein (1988) combined search arrays inspired by Treisman's work (see Figure 8, top panel) with post-search probes inspired by Posner's cuing paradigm (see Figure 9). The search arrays were designed to generate pop-out or serial search (as was obtained, see bottom panel of Figure 8) and the post-search probes called for a simple, speeded detection response. Klein reasoned that if inhibitory tags were left behind whenever attention visits a location (display item), then (particularly on target absent trials) there should be inhibition at the locations of distractors (relative to empty locations) following serial but not popout search which is precisely what was found. After some early failures to replicate (Wolfe and Pokorny, 1990; Klein and Taylor, 1994) it was demonstrated (Müller and von Mühlenen, 2000; Takeda and Yagi, 2000) that the inhibitory tags are likely to be removed if the search array is removed before the probe is presented—as if the tags are in the brain's mental representation of the scene (for a review see, Wang and Klein, 2010).

One limitation of this paradigm is that the shifts of attention are not directly observable. They are, instead, hypothetically implied by the notion of the covert (see Klein and Farrell, 1989) sequential inspection by attention of the items or groups of items in a display when search is difficult. Relatedly, the notion of serial self-terminating search makes a focus on target absent trials particularly useful, because after such trials it is assumed that attention had examined each of the non-targets (distractors) in the search array.

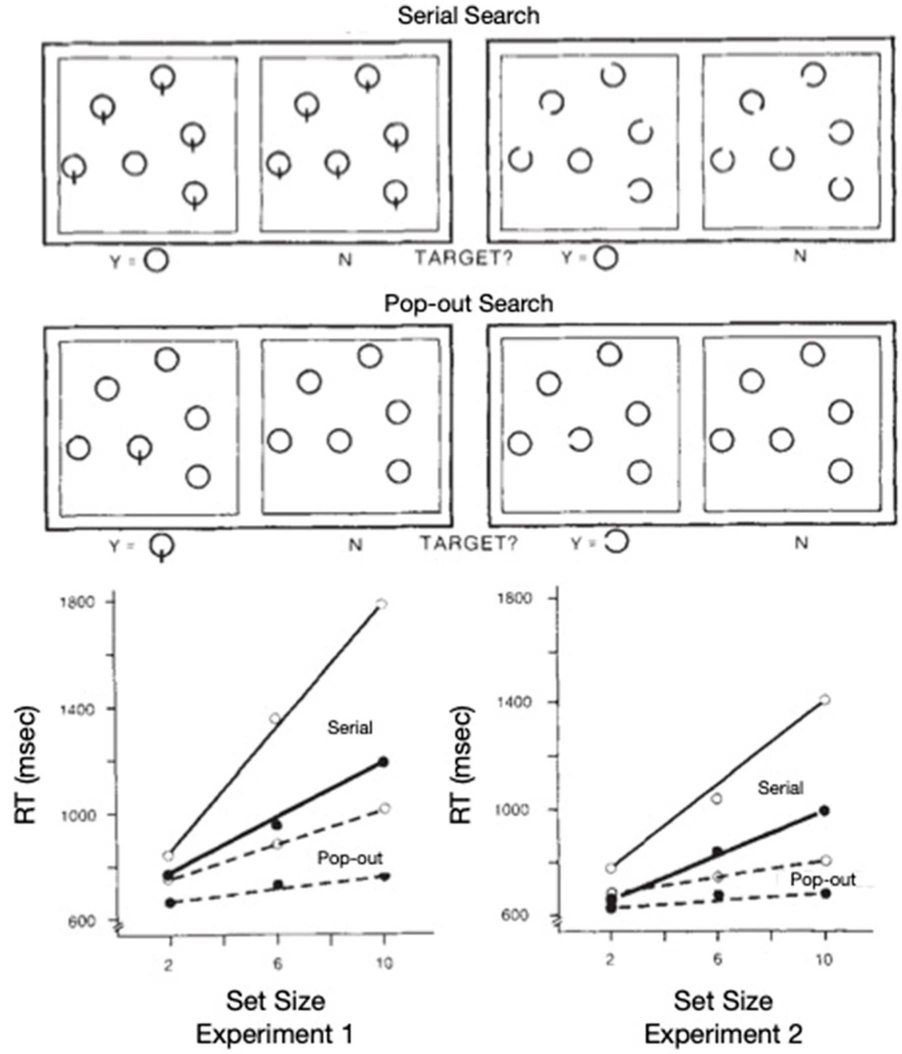

FIGURE 8
Top: Search displays used in the two experiments from Klein (1988) are exemplified (not to scale) using set size of 6. **Bottom**: Search performance (combined across the gap and line stimuli) is shown as reaction time as a function of set size. Popout search is plotted with dashed lines; serial search with solid lines. Open symbols are for target absent trials; solid symbols for target present trials.

IOR during or after an overt search episode

Klein and MacInnes (1999) sought to evaluate the manifestation of IOR under more ecologically valid conditions by exploring it using a search task with images from Martin Handford's, Where's Wally (or Waldo—in North America) series of books. While not really real-world searching, looking for Wally is considerably more real than looking for the absence of a feature in a display of circles with lines (one of the conditions in Klein, 1988). Importantly, when the eyes are allowed to move, as they must to find Wally, their path can be recorded and thus an objective indication of which locations had been fixated—overtly attended—can be generated.

By monitoring participant's eye movements it was possible to probe locations that were the same distance from the current fixation but were either at an old (previously fixated) location or at a new location at varying angular distances from the old one. If IOR were present saccades to an old location (0 deg) would be slow and if there were a gradient of inhibition, there would be a monotonic effect of distance from this location. This is what was found when the search array was maintained; but (in agreement with Müller and von Mühlenen, 2000; Takeda and Yagi, 2000) there was no inhibitory gradient when the probe was presented after removal of the search array (see Figure 10A).

Why–despite being unaffected by the distance from the previous fixation–were participants so slow in the removed condition? Here targets would not be embedded in the complex Wally scene but instead would be single objects in an otherwise empty field. Perhaps the sudden removal of the scene at the same time the next search saccade was being planned may have disrupted saccade programming to the unexpected probes. This idea was tested (MacInnes and Klein, 2003) by changing the search task while keeping the stimuli the same: Instead of asking participants to find Wally, they were asked to find something interesting and

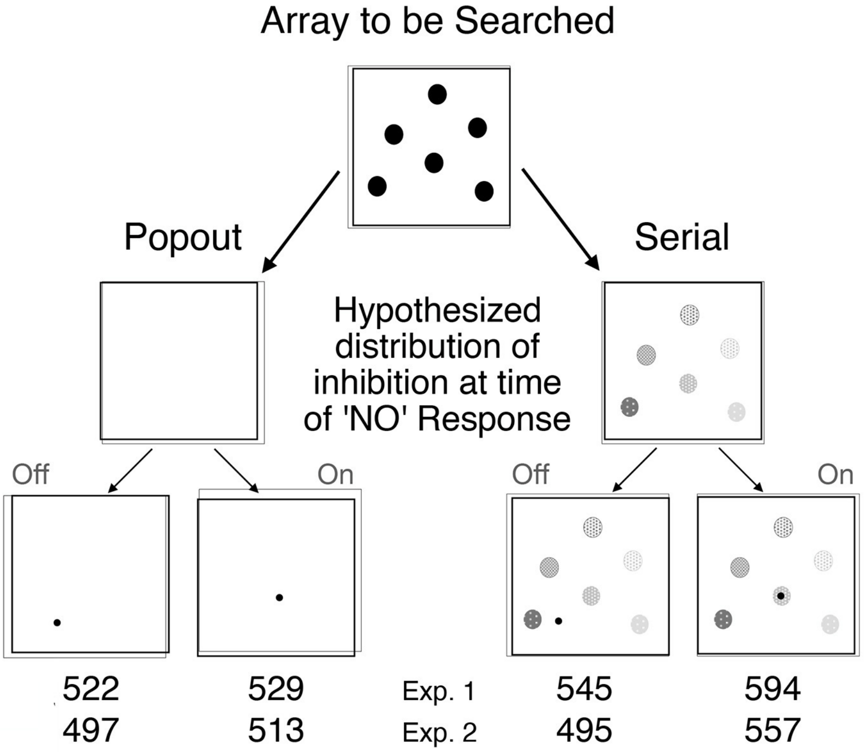

FIGURE 9
Methods and probe results from Klein (1988). A search array designed to generate either popout or serial search was presented. Klein reasoned that if inhibitory tags are left behind whenever attention visits a location, then there should be such tags at the locations of distractors on target absent trials following serial but not popout search. The different shades of gray are meant to reflect differential decay (older tags being weaker). Occasionally a probe (calling for a simple detection response) was presented AFTER the search response and removal of the search display. According to the proposal, the time to detect ON probes following serial search should be delayed by IOR. RTs to probes presented after search responses on target absent trials are presented for each of the four key conditions.

then stop there. When a pause in eye movements of 500 ms was detected it was assumed that participants had stopped at something interesting and would not be planning their next saccade. At this point the probing and scene removal methods described above (Klein and MacInnes, 1999) were applied. As before, a gradient of IOR was observed when the scene remained present; but now saccades in the removed condition were, as expected, faster and unaffected by distance from the previously fixated locations (See Figure 10B).

A substantial literature looking for IOR during and after visual search was stimulated by these two seminal studies. The studies (about 15) published in the 20 years or so after the publication of Klein (1988) were reviewed in 2010 by Wang and Klein (2010) (see also Klein and Hilchey, 2011). IOR during search has since been explored in the monkey (e.g., Torbaghan et al., 2012; Westerberg et al., 2020) and using ERPs (Pierce et al., 2017). The relative ubiquity of oculomotor IOR was recently demonstrated by Murziakova et al. (2022) who explored five tasks: static and dynamic visual search, foraging (akin to search but with multiple targets), memorization and change detection. Their use of the probe paradigm revealed IOR in all of these tasks except for change detection and, consistent with the novelty-seeking proposal for IOR, across the five tasks there was a relatively strong negative relation between these IOR scores and the probability of return saccades.

Several interesting findings about IOR during and after search have been reported by Höfler and colleagues. Höfler et al. (2011) had their participants search the same array twice in succession for different targets. Using the probe during search method they found that IOR was present in each of the successive searches. Interestingly, when they tested for IOR after the first target was found and immediately after the presentation of the second target, there was no evidence for IOR at the locations previously fixated during the first search. This resetting of the inhibitory tags suggests a flexibility that is consistent with IOR's sensitivity to task (e.g., Dodd et al., 2009). When the second target was presented before the first target was found, IOR from the first search was maintained (that is resetting was abolished). When considered together with the studies reviewed earlier in which IOR was probed after the search response, it was suggested that the resetting of IOR might depend on both the completion of the first search and the start of a new one. Regardless, the resetting discovered by Höfler et al. (2011) likely ensures that the inhibition from a prior search will not interfere with a subsequent one. Recently, Höfler and Kieslinger (2022) demonstrated that the number of items being held in spatial working memory had no effect on IOR when measured using a

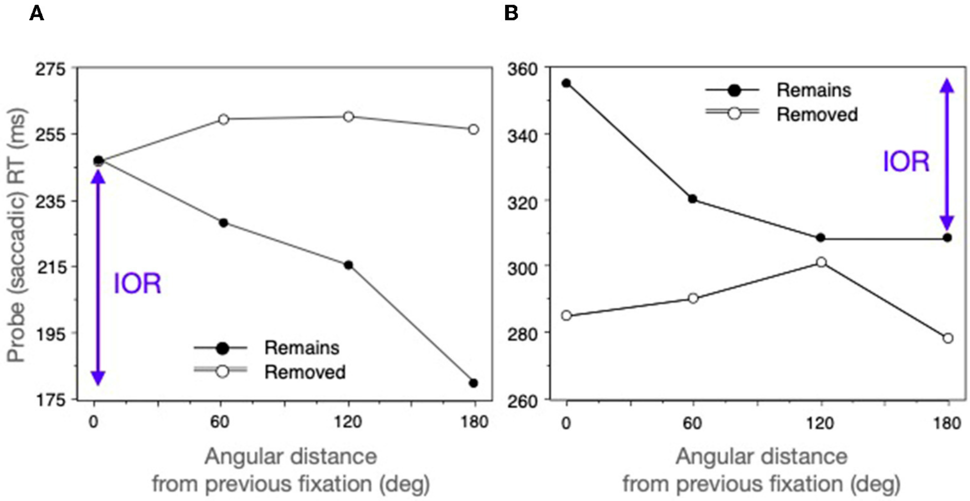

FIGURE 10
Saccadic probe reaction time as a function of distance from a previous fixation. **(A)** From the 2-back condition of Klein and MacInnes (1999). **(B)** From MacInnes and Klein (2003) who only tested 1-back. See text for further explanation.

probe during search method similar to that of Klein and MacInnes (1999).

Summary

In the context of visual search, the need for and benefit of inhibitory tags at previously inspected locations in a scene was highlighted in Itti and Koch's (2001) computational model of attention. Their model hypothesizes that the sequence of inspections is guided by a "winner-take-all" algorithm that operates on a saliency map. This map is generated primarily by bottom-up processing of the features in the visual scene, processing which can be biased by top down mechanisms and prior experience. Importantly, inhibition of return is hypothesized as necessary to suppress "the last attended location from the saliency map, so that attention can focus onto the next most salient location" (p. 196). A similar role for IOR was previously proposed by Klein (1988) who asked: "How does the serial search mechanism keep track of where attention has been, so that it does not return there again?" and answered: "Inhibition of return might help perform this function" (p. 430).

It is our view that either of the two forms of IOR described in the earlier sections of this paper can perform the novelty-seeking, search-facilitating function of minimizing unnecessary return inspections. The output form does so by operating on a priority map which represents the likelihood (perhaps likely value) of different possible next behaviors. The input form does so, as described by Itti and Koch, by operating on a saliency map.

Some outstanding puzzles/questions

There are many puzzles/questions in the literature about IOR and visual search and it is not our intention to be comprehensive in this section. What we have covered below are some of the puzzles/questions that have intrigued us and for which we believe the solutions/answers will be illuminating for scholars interested in attention and visual search. We believe readers will find here suggestions for exciting research.

The measurement of IOR is not straightforward

In their seminal paper Posner and Cohen (1984) suggested that "a peripheral visual stimulus both summons attention and serves to inhibit the processing of further information at that position in space. These two effects appear to be independent and may cancel each other out." That facilitation due to attention (whether under endogenous or exogenous control) and inhibition can be present simultaneously is now well-established[3] (see e.g., Berlucchi et al., 2000; Berger et al., 2005; Chica et al., 2006; Farrell et al., 2010). Thus obtaining precise measurement of IOR is challenged by the possibility that these two factors are potentially contributing whenever we are measuring RT in the cue-target paradigm. Relatedly, the time course of the transition from cue-elicited facilitation to IOR can be highly variable, and is influenced by task demands (Lupiáñez et al., 1997) and attentional control settings (Klein, 2000). Behavioral IOR manifests sooner (i.e., at shorter CTOA) when the target calls for a simple detection response than when it requires a non-spatial discrimination. Moreover, other non-IOR negative effects, such as sensory adaptation, may be contributing to an amalgam of context-dependent inhibitory consequences (Hilchey et al., 2014b).

Individual differences have been shown to influence the measurement of IOR (for a review of some of these, see Klein et al.,

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[3] Indeed, inhibition can even be present at fixation following a stimulus there (Ivanoff and Klein, 2001; Rafal et al., 2006).

2005). IOR has been shown to be quite unreliable within individuals across multiple sessions (Berger et al., 2005), however the use of a central cue-back prior to target onset seemed to partially correct this lack of consistency. Practiced individuals have shown less IOR (Weaver et al., 1998; but see also, Pratt and McAuliffe, 1999), but it is unknown whether the cognitive mechanism(s) underlying IOR attenuate due to practice, or whether observers simply become more efficient at ignoring the task-irrelevant cues.

Finally, in some paradigms IOR might be present but contaminated by binding effects which are covered in the next section.

IOR may be masked by "binding" effects

Whereas, input- and output-based forms of IOR are robustly observed in model tasks that require easily-formed localization and detection responses (Huffman et al., 2018, for review), IOR may be conspicuously absent in tasks that require participants to engage deeply with one or more stimulus dimensions (e.g., space, color, shape, etc.) in order to form correct responses (Hilchey et al., 2017, for review). This is at least partly because tasks that require greater engagement with stimuli in order to form correct responses often give rise to so-called integration or binding effects (e.g., Hommel, 1998).

In action control theories (Hommel and Colzato, 2004; Hommel, 2009; Frings et al., 2020), integration effects occur when one property of a stimulus (say color) becomes transiently associated with another property of a stimulus (say shape, or the response that was made to it) in episodic memory. Once this binary event representation is formed, stimuli that resemble it will trigger its retrieval if the task demands processing of a stimulus property that is in common with the event representation. When there is a partial mismatch between the retrieved event representation and the imperative stimulus, response times tend to be slowest. When there is a full match between the retrieved event representation and the imperative stimulus or if retrieval does not occur because there is no match, response times tend to be fastest. Notably, in some situations, binding effects co-occur with IOR, which can make it enormously difficult to derive pure estimates of IOR and binding effects. In other situations, binding effects are not a concern because they simply do not occur or occur very weakly.

More specifically, binding effects are rarely a concern in simple model tasks without distractors and with localization responses to each stimulus (Huffman et al., 2018). This is because it is usually not necessary to process non-spatial information in order to form correct responses and each stimulus location is usually associated with a unique response. Binding effects are also usually absent in simple model tasks without distractors and with detection responses to each stimulus, but they can be induced by increasing the amount of spatial processing that is needed to form correct responses (Hilchey et al., 2020). Furthermore, whereas some form of binding may occur in cue-target visual discrimination tasks (Klein et al., 2015), the findings from such tasks appear inconsistent with action control theories. Habituation theories (e.g., Dukewich, 2009) and Lupiáñez's (2010) 3-process account provide better fits.

Binding effects are most robust in tasks that impose significant non-spatial or spatial processing burdens in order to form responses and in which responses are required to successively presented stimuli (i.e., target-target paradigms). They are so robust in standard target-target, 2-alternative forced visual discrimination tasks that there is usually no behavioral trace of IOR in these tasks (e.g., Terry et al., 1994; Hilchey et al., 2017, for review). The same can be said for the visual search tasks used by the spatial negative priming (Frings et al., 2015) and priming-of-popout literatures (Hilchey et al., 2018a, 2019b). Nevertheless, our current view is that the evidence strongly suggests that some form of IOR is in effect in all such cases and is simply overshadowed by episodic retrieval processes that occur after a stimulus has been oriented to (Milliken et al., 2000; Christie and Klein, 2001; Hilchey et al., 2018b, 2019a). However, whether IOR is absent or merely overshadowed by other effects in all cases where processing requirements of a task increase or change (e.g., Smith and Henderson, 2011; Talcott and Gaspelin, 2020; Talcott et al., 2022) remains unclear.

On the relation between measured IOR and search efficiency

In 2006, one of the authors (Klein and Dukewich, 2006) suggested a positive relation between IOR and search efficiency: "If IOR plays a role in serial search by discouraging reinspections, then factors that interfere with IOR should result in decreased serial search efficiency; and, conversely, factors that enhance IOR should increase serial search efficiency" (p. 663). Whereas, these predictions still seem valid consideration of the converse (what happens to IOR when search efficiency is manipulated?) demonstrates that the relation is not so simple. If search is improved by making search less dependent on the inspection of individual items (less serial), then measured IOR during search might be reduced (for a recent example, see Li et al., 2022).

Are the inhibitions encoded retinotopically, environmentally, on objects and/or in scenes?

Posner and Cohen (1984) and Maylor and Hockey (1985) demonstrated that IOR was coded at locations in the environment rather than retinotopically (or, oculocentrically). When ERPs were collected in this paradigm by Satel et al. (2012), targets presented at the retinotopically cued location showed substantial reductions of the sensory-related P1 ERP component and significant, but very little inhibition (∼6 ms) while targets presented at the environmental location of the original cue show substantial IOR (∼19 ms) and no reduction of the P1 component. This pattern provides converging evidence for what the phenomenon itself demonstrates: That repeated stimulation of an input pathway is not necessary for generating the inhibition. Importantly, in all of these studies the retino/spatio dissociation was effected by interposing one eye movement (Maylor and Hockey, 1985; Satel et al., 2012) or several (Posner and Cohen, 1984) between the cue and target. Because in these experiments it is unclear what would have been the state of the reflexive oculomotor system, we can't be sure whether this environmental coding characterizes the input or the output form of IOR.

Later, Tipper et al. (1991) discovered that inhibition of return could be tagged to an object by cueing an object before it moved predictably in space (see also: Weaver et al., 1998; Tipper et al., 1999; Theeuwes et al., 2014; Smith et al., 2016). Moreover, object-based inhibition of return survived occlusion of the cued object (Yi et al., 2003) and was observed when the objects in the scene moved in random and unpredictable directions (Ogawa et al., 2002). While these studies measured performance with a manual response, Abrams and Dobkin (1994) found object-based IOR effects when measured with a saccadic response in the moving boxes paradigm, as did Tas et al. (2012). However, several others have failed to replicate this object-based saccadic IOR finding, either conceptually (Souto and Kerzel, 2009; Sentürk et al., 2016) or directly (Redden et al., 2018). If the output form of IOR is not tagged to moving objects, then could it possibly be useful in real-world situations such a looking for your child in a busy daycare or at a soccer game?

As noted earlier, studies exploring inhibition of return in the aftermath of a visual search task have demonstrated that the inhibitory tags depend on the persistence of the scene. The importance of the scene (or its removal) has also been observed in the model Posner cue-target paradigm (Redden et al., 2017). These findings led to the idea that removal of the scene would inexorably lead to removal of the inhibitory tags. Importantly, however, even when the scene is removed the inhibition may remain so long as the observer expects the search to continue (Thomas and Lleras, 2009).

What is the cause of the input form?

As established by Posner et al. (1985) when they named the inhibition: inhibition of return, there is little doubt that the cause of the output form of IOR is activation of the oculomotor system. An unresolved question is, "what is the cause of the input form?" Posner and Cohen (1984) identified a form of inhibition that slowed responses to targets which were "handled less efficiently" than targets at uninhibited locations. In all but two of the experiments in that seminal paper eye movements were discouraged and EOG and experimenter feedback were used to reinforce this instruction. Therefore, with the exception of the studies with intentional eye movements, we surmise that the form of IOR generated here was the input form. We also agree with Posner and Cohen's inference that the processing of visual information at the previously cued location in these experiments was made less efficient by the inhibition there. Converging evidence for this belief comes from Smith et al. (2012) who demonstrated that IOR decreased the probability of TMS-induced phosphenes in a paradigm wherein the authors proposed that participants were spontaneously suppress eye movements.

Posner and Cohen proposed that the cause of this effect could be due to the "… inhibitory effect on individual neural cells that occurs with the presentation of a second signal in the visual field occupied by a target" and that the "...inhibition effect is sensory, not attentional, in origin…" (p. 522). Two primary observations led to these conclusions. First, when generated by simultaneous cues at the two possible target locations they observed that the magnitude of the inhibition was as great as compared to following just one cue. This double cue finding was critical because Posner and Cohen thought that the attentional spotlight could not be split between two non-contiguous regions, and therefore the putative inhibition could not have been caused by attentional orienting. Second, there was no inhibition at a location that had previously been attended endogenously and covertly by way of an arrow at fixation.

With the benefit of hindsight, we know now that stimulation of the visual periphery is neither necessary nor sufficient to generate some forms of IOR (e.g., see Figure 4) and the allocation of attention might still be important so long as it is controlled exogenously (see Klein, 2009, for a review of evidence that the forms of covert orienting when controlled endogenously and exogenously are not the same). Consider, for example, that the "inhibition" in Posner and Cohen's double cue condition was estimated from a flawed baseline that confounded the mental state of the participant before the target was presented (i.e., following either a single or double cue). Klein et al. (2005) overcame this flaw by cueing 1–4 of eight equi-eccentric locations prior to presenting a target at one of the eight locations. They found little to no inhibition when the cue array (of 2 or 4 elements) was balanced around fixation and would thus be unlikely to activate a shift of attention or an eye movement. Moreover, following multiple cues whose center of gravity was some distance from fixation, IOR was maximal at unstimulated locations that were in the general direction of this center of gravity (see also, Langley et al., 2011, who replicated many patterns from Klein et al., 2005).

Others believe that repeat stimulation of an input pathway is at least partly responsible for slower responses to targets at cued as compared to uncued locations. For example, habituation theories (e.g., Dukewich, 2009; Klein et al., 2015) and the detection cost theory of IOR (e.g., Lupiáñez, 2010; Lupiáñez et al., 2013) suggest that the magnitude of IOR should be positively related to the physical resemblance between stimuli (cue and target) that are separated in time but not in space. At the crux of habituation theories is the belief that stimulus features that are irrelevant to a response become less likely to generate an orienting response upon repeated presentation because the neurons representing them cease to fire as vigorously. At the crux of the detection cost theory is the belief that the nervous system strives to integrate information that looks like other information over time and space, with increased resemblance between the cue and target increasing the probability that a cognitively demanding integration process will even occur in tasks for which integration is not useful (e.g., stimulus detection tasks).

The behavioral evidence supporting the prediction that the theories have in common is mixed. There is virtually no evidence to suggest that the magnitude of IOR gets bigger with feature matching when detection or localization responses are required to sequentially presented signals in the visual periphery and their onsets are separated by >500 ms (e.g., Hilchey et al., 2018a; Huffman et al., 2018, for review). In the cue-target analogs, where a response is required to the second but not to the first of two signals, earlier studies showed that the magnitude of IOR was similar or only weakly affected by whether cue features repeated as target features (e.g., Fox and de Fockert, 2001; Pratt et al., 2001; Riggio et al., 2004; Taylor and Ivanoff, 2005). More recent studies have shown that IOR may be significantly reduced, if not abolished, when cue features do not repeat as the target (e.g., Hu et al., 2011; Klein et al., 2015). Significant reductions in the magnitude of IOR when non-spatial features switch is a sign that repeat stimulation of input pathways can matter in critical ways, but it remains unclear when or why this occurs and thus what the boundaries are on these theories. Moreover, it is important to keep in mind that even if there were an input-based form in the visual periphery that depended critically on feature matching, this finding alone would not be sufficient to rule out whether a covert, exogenous shift of attention were also necessary to generate the effect.

Whereas the cause of the input form remains undetermined, we suggest that it could be the same as the cause of the output form: activation of the oculomotor system. Critically, when this activation occurs while the reflexive oculomotor system is suppressed, the form generated is the input form. Although we do not think there are any data that directly contradict this proposal Sumner et al. (2004) might provide an indirect challenge. They were able to generate IOR with manual responding using S-cone stimuli as cues while with saccades these same cues did not generate IOR. Because S-cone stimuli were thought to be invisible to the superior colliculus (but see, Hall and Colby, 2014) this finding could be consistent with a cortical locus for the cause of the input form. Regardless, it would be exciting if some readers would devise a direct test of our suggestion.

How are the two forms of IOR implemented neurally?

It was first proposed by Posner et al. (1985) that subcortical circuits played an important role in the generation of IOR. This proposal is supported by the presence of IOR in newborns (Valenza et al., 1994) for whom subcortical but not cortical circuits are relatively mature and by larger IOR in the temporal hemifield field (e.g., Rafal et al., 1989) which is more richly represented (than the nasal hemifield) in the superior colliculus. More directly confirming this proposal, subsequent studies of individual patients with damage to the superior colliculus (Sapir et al., 1999; Sereno et al., 2006) demonstrated that IOR was absent in the direction(s) affected by the lesions. Converging evidence for the importance of sub-cortical circuitry was provided by Gabay et al. (2013) who discovered IOR in the archer fish, a species with limited cortical circuitry.

In two split-brain patients, Tipper et al. (1997) demonstrated that an intact corpus callosum was required for inter-hemispheric transfer of object-based inhibition of return when a cued object crossed the vertical meridian. In patients with damage to the right intra-parietal sulcus, Sapir et al. (2004) demonstrated that IOR following an eye movement was present at the retinotopic location but not at the environmental location (where, as noted above, it was found in normal controls). Converging evidence for the importance of right parietal cortex was later provided by Van Koningsbruggen et al. (2010) who used dual-pulse TMS to disrupt neural circuits in the right and left anterior intraparietal cortices. Spatiotopic coding of IOR was totally disrupted when TMS was delivered to the right but not the left parietal lobe. Thus, whereas the generation of IOR requires an intact superior colliculus, once generated its preservation in the context of object motion and its coding in environmental coordinates seems to depend on cortical circuits.

Single unit recording can be a particularly revealing neuroscientific tool for understanding how a behavioral phenomenon like IOR, might be implemented neurally. By demonstrating IOR in the rhesus monkey Dorris et al. (1999) opened the door to exploring IOR using this tool and later Dorris et al. (2002) discovered a strong relation between the sensory responses of neurons in the superior colliculus and delayed saccades to targets presented at a previously cue location. Importantly, prior to presentation of the target the firing rate of neurons in the receptive field of the cue was higher than baseline and electrical micro-stimulation of these neurons generated faster not slower saccades. These finding suggest that these neurons were not inhibited but rather were receiving signals that were already reduced by some form of inhibition. Whereas, further studies from Munoz's lab (Fecteau et al., 2004; Fecteau and Munoz, 2005) have identified input-based inhibitory effects in the visual and visuomotor neurons of the superior colliculus, we believe that these effects do not last long enough to represent IOR and cannot explain instances of IOR that do not depend on repeat stimulation of an input-pathway as described earlier.

From the point of view of the role of IOR in visual search, studies from Bisley's lab are particularly pertinent. Mirpour et al. (2009) explored the responses of neurons in the lateral intraparietal (LIP) cortex while the monkey performed a visual foraging task. It was found that the responses of LIP neurons were reduced when a previously fixated (as compared to a new) distractor entered the neuron's receptive field. Later, Mirpour et al. (2019) discovered neurons in the frontal eye fields (FEF) whose activity was maintained throughout a trial once the location that these neurons represented had been fixated. It was proposed that these neurons keep track of fixated stimuli and transmit this information to priority maps in parietal cortex. In a recent review of the neuroscience of IOR, Satel et al. (2019) proposed that "such priority maps in parietal cortex, driven by FEF signals, are a likely locus for the inhibitory tags leading to the output form of IOR."

With only a few exceptions (e.g., Bourgeois et al., 2012, 2013; Satel et al., 2013, 2014) neuroscientific studies of IOR have generally ignored the possibility of the two forms of IOR that we have so clearly distinguished by their different effects. Indeed, despite reflecting a broad range of behavioral and neuroscientific findings, two relatively comprehensive theories presented by Malkinson and Bartolomeo (2018) and by Tian et al. (2011) suffer, in our view, from a similar ignoration. Finally, to our knowledge, no single unit recording studies have been set up to dissociate different forms of IOR. That is, all of these studies measured behavioral IOR using eye movements and in none was an effort made to suppress the reflexive oculomotor machinery. Monkeys can certainly be trained to make manual responses and encouraged to refrain from making unwanted eye movements (e.g., Bowman et al., 1993). Important, missing pieces of the puzzle about the neural implementation of IOR, might be provided if an effort were made to generate neuroscientific data about the input form of IOR, particularly about its cause.

Which form of IOR is operating during visual search?

We believe this is perhaps the most important and interesting puzzle that arises from our review. When search is not explicitly overt, eye movements may still be executed. As such, it is heretofore inconclusive as to whether the form of IOR that is generated in many (all?) search studies is input-based or output-based. Moreover, to our knowledge, with the exception of several experiments reported in Klein and Taylor (1994) (which suffer from the fact that the search scene was removed before the probes were delivered) none of these studies employed eye monitoring to determine (or control) the extent to which eye movements were made. Perhaps the design from Klein (1988) should be repeated with the "keep the display on" caveat while eye movements are monitored and observers are alerted when they make any (as in Klein and Farrell, 1989).

Moreover, even when search is mediated by eye movements (as in the Waldo studies), it is inconclusive as to whether the input or output form is generated and left behind to influence subsequent orienting. Logic would suggest it could be the output form because eye movements are being made and it would seem the reflexive oculomotor system might not be suppressed; but also there is reason to believe it could be the input form because eye movements are not generated so much by peripheral stimuli as by a(n endogenous) plan to find the target.

Ultimately, we believe it is likely that the activation state of the reflexive oculomotor system fluctuates during complex real-world search. When the target/goal for the search episode is robustly encoded, or when there is prior intuition regarding probable target location(s), the reflexive oculomotor system might be tonically suppressed to help avoid unwanted distractions from scene components that are either salient or share properties with the target.

Author contributions

This review is based on a presentation at the August 2022 meeting of the European Conference on Visual Perception made by RK at a symposium entitled Inhibition of Return and Visual Search. The authors of that presentation are the authors of this review. The first draft of the review was generated by RK and missing materials and many editorial improvements were made by RR and MH.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

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Attentional development can help us understand the inattentional blindness effect in visual search

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Introduction: Inattentional Blindness (IB) is the failure to notice an unexpected, usually salient stimulus while immersed in a different, often demanding attentional task. More than just a laboratory curiosity, IB is an important phenomenon to understand because it may be related to real-world errors such as missed "incidental findings" in medical image or security searches. Interest in individual differences in susceptibility to IB has produced a number of studies showing inconclusive results.

Methods: Here, we tested IB in a sample of 277 participants, 4-25 years old performing a visual search task. On two critical trials, an unexpected letter and an unexpected word were presented among photorealistic objects.

Results: There was a clear age effect with younger individuals showing higher IB levels. IB correlated with attentional control in visual search and with Continuous Performance Test-CPT for d-prime, response times and attentional shifting measures. These effects disappeared if age was controlled. There were no general effects of intelligence (IQ; RIST) or gender. Younger observers showed a negative correlation of IB for the word with the verbal components of the RIST IQ-proxy (no effect for the letter).

Discussion: These results support a relationship between IB and cognitive-developmental changes, showing that maturation of attention and executive processes can help us understand the intriguing phenomenon of (sometimes) missing what is in front of our eyes.

KEYWORDS

inattentional blindness, visual search, development, attention, individual differences, Intelligence Quotient, gender

Introduction

In spite of our introspective impression that we see a world filled with recognizable objects, psychologists have long known that there are severe capacity limits on human perception and attention (Noë et al., 2000). The phenomenon of Inattentional Blindness (IB) is a striking example in which observers fail to detect a salient, but unexpected stimulus while engaged in a primary, attention-demanding task (Mack and Rock, 1998). The Simons and Chabris (1999) gorilla experiment is probably the most famous example. Observers were monitoring a ball game in which they had to count the number of times that a given group passed the ball to each other. About half of these observers failed to notice a person in a gorilla costume walking into the midst of the game. Subsequent studies have found that even when our eyes fixate the unexpected event/stimulus, our cognitive system can still fail to bring it into awareness (e.g. Drew et al., 2013). Indeed, IB has been studied using a variety of experimental paradigms. These include dynamic tasks with more conspicuous, moving IB stimuli (like the "gorilla-task" from Simons and Chabris, 1999), static tasks with less prominent IB-stimuli (e.g., Buetti et al., 2014, which used a version of the flanker task or Cartwright-Finch and Lavie, 2006, using a visual search task), and eye-movement recording (e.g., Drew et al., 2013). The IB magnitude in these paradigms ranges from 30–40 to 80% of people failing to notice the IB-stimulus, even when eyes fixate on the IB stimulus. IB may represent a failure of normal attentional capture when observers perform an orthogonal, demanding attentional task, even though the IB stimulus can be quite salient (see Simons, 2000, for a review). Alternatively, the IB stimulus might capture attention in the moment but might fail to leave a memory trace that can be retrieved when the IB stimulus is to be reported (*inattentional amnesia* as named by Wolfe, 1999, but see Most et al., 2005). The present work aims to shed more light on the sources of variation in the IB effect by examining the effects of age, attentional performance, intellectual capacity, and gender in a large sample ranging from 4 to 25 years old.

Several studies have used individual differences to better understand the IB phenomenon. Factors including effects of age, cognitive, and intellectual capacity have been tested (e.g., Cartwright-Finch and Lavie, 2006; Drew et al., 2013; Memmert, 2014). However, the results of these studies have been inconclusive to date, so some studies have pointed toward a purely stochastic explanation of an IB phenomenon common to everybody, arguing that, with just one or two critical trials per observer, any individual differences in IB may be nothing more than random variations, rather than reflecting any underlying stable individual differences in cognitive abilities (Kreitz et al., 2015). Surprisingly, although attentional processes seem to be critical to understand the inattentional blindness phenomenon (e.g. Simons, 2000), there are not many studies looking to determine whether differences in attentional skills/performance are correlated with differences in the IB effect.

In prior work on the effects of age, several studies have shown larger IB effects in older adults both in static and dynamic IB-tasks (O'Shea and Fieo, 2015; Horwood and Beanland, 2016), and in more applied tasks, like driving simulations (Saryazdi et al., 2019). At the other end of the lifespan continuum, using the gorilla paradigm, Memmert (2014) found that younger children were more likely to show IB effects in a large sample of 480 participants from 8 to 15 years-old. However, Zhang et al. (2018) failed to find that effect in their sample of 210 observers from 7 to 14 years-old, using a T among L's dynamic IB-task based on Most et al. (2001). Zhang et al. (2019) also tested 3 to 5 years-old observers in Mack and Rock's (1998) original "cross judging" IB paradigm. They found that IB decreased with age.

Why is there such a range of variation in the developmental studies? As with the effects of other variables, purely stochastic noise must play a role in paradigms that typically get one or very few trials per observer. In addition, there is considerable variation in the nature of the IB tasks, so there might be other factors explaining IB variability. The "gorilla-task", the "T among L's" task, and the "cross judgement" IB tasks may make different demands on attentional/executive processes. These processes, in turn, develop at different speeds in development (Anderson, 2002; Gil-Gómez de Liaño et al., 2020), potentially producing a variety of age differences in the IB effect. In the present study, we will use a visual search task as the primary attentional task, as it has shown to be useful in the study of different attentional processes during development (Gil-Gómez de Liaño et al., 2020) to test IB in a large sample from 4 to 25 years old.

As development of attentional processes could explain IB variability in age, another question raises: How might differences in "attentional performance" or "attentional demands" account for IB effects? Lavie's work suggested that perceptual load (as defined in Lavie's Perceptual Load Theory; Lavie and Tsal, 1994) might modulate IB, with higher perceptual loads producing higher levels of IB (see Lavie et al., 2014, for a review). One way to operationalize perceptual load in Lavie's theory is by changing the complexity and/or number of the distractor stimuli surrounding the target in a given display: The higher that complexity and/or number of distractors, the higher the perceptual load; and in the present case, the higher the hypothesized IB effect. Cartwright-Finch and Lavie (2006) showed data supporting this hypothesis. However, Wright et al. (2018) failed to find any modulation of attentional capture propensity with IB, although they did find that speed of processing was related to IB: Observers who showed more efficient encoding and recognition of the main task stimuli were less likely to show IB. Putting the results of Lavie et al. (2014) and Wright et al. (2018) together, we could hypothesize that those individuals with higher levels of attentional skills (with better performance in the attentional task) should show less IB. However, not all attentional tasks seem to produce data supporting this idea. Richards et al. (2010) tried and failed to modulate IB using a Stroop task and a global/local flicker task. Kreitz et al. (2015) failed to find effects of IB in spatial attention either and suggested that IB effects were driven more by stochastic processes, rather than by any stable individual differences in cognitive abilities.

Other variables have been studied in combination with IB and we will also consider some of these in the present study. We test for an effect of gender. Prior work did not find such an effect (e.g. Hannon and Richards, 2010) and we replicate that lack of an impact of gender on IB. We also look for a relationship between cognitive capacity (Intelligence Quotient -IQ- and working memory capacity) and IB. Prior work has produced somewhat unsettled results on IQ-IB relationships. Although several results show a small-to-moderate correlation, with people having higher capacity showing lower levels of IB, this result is in need of more empirical support. O'Shea and Fieo (2015) found lower levels of IQ for individuals not noticing the IB stimulus, but the sample size in this study was probably underpowered, especially for lower-IQ individuals (9 individuals noticing the IB stimulus compared to 25 that did not notice the IB stimulus), making it difficult to establish the strength of these conclusions. Zhang et al. (2016) reported a similar relationship between IQ and IB effects studying gifted children. Intellectually gifted children showed significantly lower levels of IB (18%) compared to a group of IQ-average children (46%). Though different from IQ, working memory capacity is clearly related to IQ (e.g. Colom et al., 2007).

Thus, Hannon and Richards (2010) found similar correlations when measuring working memory capacity, as measured by the *Operation Span* (OSPAN), though not when using a simple visual working memory task. In contrast, Kreitz et al. (2016) failed to find a relationship between working memory capacity and susceptibility to cross-modal IB and *inattentional deafness* in a sample of almost 100 adult participants. Taken together, the results on IQ and/or working memory capacity show that there may be a small-to-modest relationship between IB susceptibility and lower levels of capacity. As noted, the data are not strong, and our results, reported below, do not support a relationship of IQ to IB.

To summarize, we study how individual differences in age, attention, intellectual capacity and gender modulate the IB effect. Our sample of 277 participants from 4 to 25 years old is well-suited to the examination of effects of age on IB, in part because we have data on other measures of attentional and executive functions, developmental indexes that may vary with age (e.g. slopes of search functions, intercepts, misses, etc.; see Gil-Gómez de Liaño et al., 2020). To anticipate our results, effects on IB of variation in visual search in standardized measures like the Continuous Performance Test (CPT) essentially vanish if age is controlled.

Methods

Participants

An initial sample of 293 observers participated in the study. Previous studies of age effects in visual search showed that with alpha set to 0.05 and 1-beta (power) over 0.9, we can detect significant effects (partial eta-square $\eta^2 = 0.01$), if we run between 21–33 participants per age group. We maintained those numbers in each age group. Participants were excluded from the sample if they had an estimated IQ below 70 (based on the Reynolds Intellectual Screening Test—RIST—score, see materials below), sensory or neurological pathology, motor impairments, learning disabilities, a diagnosis of schizophrenia, or a generalized developmental disorder (based on family interviews and standardized questionnaires). In addition, there were two sessions for the experimental procedure (see procedure for details) and the second session is critical to this study. Thus, observers who did not show up for the second session could not be included in the final sample. Sixteen participants were excluded on those bases, leaving the final sample of 277 observers from 4 to 25 years old. We attempted to divide those 277 observers into age groups consisting of at least 21 participants. As noted above, this should yield power over 0.9. The exceptions are the 11–12 year-old group with a final sample of 18 participants after losing several to the aforementioned exclusion criteria, and the 13–14 year-old group with a final sample of 20, for the same reasons. Fortunately, the main developmental changes in visual search occur at younger ages (Gil-Gómez de Liaño et al., 2020) so the modest loss of power in the 11–14 range should have little effect on the conclusions of the study.

All participants performed the Continuous Performance Test (CPT or K-CPT- Kids Continuous Performance Test depending on the age) and the RIST test as a proxy for IQ (see materials below). For minors (observers below 18 years-old), the BASC (Behavior Assessment System for Children) and BRIEF (Behavior Rating Inventory of Executive Functions) family versions tests were administered to the caregivers/relatives as a way to control and dismiss all children with clinical or generalized development disorders, as previously mentioned. The present sample is a subset of the one reported in Gil-Gómez de Liaño et al. (2020). In this final sample, there were 138 identified as females and 139 as males, and the mean IQ as measured by the RIST was 106 (*sd* = 13.6).

All participants were drawn from public schools and universities in Madrid, Spain. All had normal or corrected-to-normal vision. The Institutional Review Board (IRB) at the Universidad Autónoma de Madrid (UAM-Ethical Committee) approved the study before any testing (Code: CEI67-1193). A parent or guardian gave written informed consent for every minor, and each child gave oral/written assent. Regular informed consent forms were given to adult participants as well.

Materials

The experiments were run using E-prime 3.0 (Psychology Software Tools, Pittsburgh, PA). All images in the visual search were taken from a heterogeneous set of 3,000 unique photorealistic objects provided by Brady et al. (2008) following the same procedure as in Gil-Gómez de Liaño et al. (2020). For the targets, we selected a pool of 190 child-friendly images (toys, animals, arts-craft images; see Figure 1). Target and distractors came from a separate pool of images, so target images would never appear as distractor. Monitor resolution was 800 x 600 pixels. Each image fit inside an invisible box that subtended a visual angle of 2.3° x 2.3° at an approximate 57 cm viewing distance. The IB targets (discussed below) were the letter "N" and the word "COLOR". These alphanumeric stimuli are perceptually and categorically different from all other images shown in the task (see Figure 1). Similar stimuli have been used likewise in other IB studies (e.g. Buetti et al., 2014). These subtended 1.3° x 1.5° and 2.3° x 0.5°, respectively. Children responded via touch-screen (Microsoft Surface pro i5).

As previously mentioned, we applied several standardized tests: The Conners Kiddie Continuous Performance Test 2nd Edition^{TM} (K-CPT) assessed attention capacity and deficits in children up to 7 years old, and the Conners Continuous Performance-3 (CPT-3) was used for observers 8+ years old. Both the K-CPT and the CPT are useful tests to measure performance in areas of inattentiveness, impulsivity, sustained attention and vigilance, being usually used in clinical contexts in the process of diagnosing Attention Deficit/Hyperactivity Disorder (ADHD), as well as other psychological and/or neurological deficits in attention. Both are based on a go/no go task in which observers must respond only to one target, avoiding responses to any other distractor. In the K-CPT the target is a soccer ball in a stream of other images. In the CPT-3, it is the letter X among other letters. To assess IQ, we used the Reynolds Intellectual Screening Test (RIST; Reynolds and Kamphaus, 2003). This short test takes around 20–30 min to be administered and shows high reliability with other measures of intelligence (Reynolds and Kamphaus, 2003). Finally, for minors, we asked parents to fill out the parent report form of the Behavioral Assessment Scale for Children

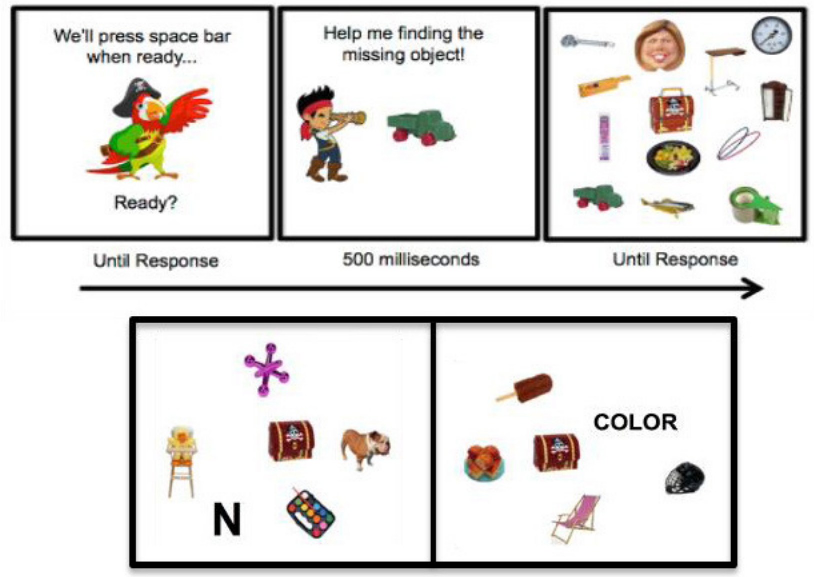

FIGURE 1
Upper row shows an example of the procedure, with presentation times written below each frame. **Lower row** images are examples of IB trials for Letter and Word conditions.

(BASC; Reynolds and Kamphaus, 2004), and the Behavior Rating Inventory of Executive Function (BRIEF) (Gioia et al., 2000). The BASC measures potential behavioral problems, assessing adaptive and problem behaviors in the community and home setting. The BRIEF measures potential problems with executive functions. The parents also provided information about the development of their children and their medical history using a short questionnaire developed by the researchers. These questionnaires were used to assess potential developmental disorders to dismiss those individuals under such circumstances to focus our study on typically developing persons.

Design and procedure

In a first phase, observers performed a visual search (VS) task (see Gil-Gómez de Liaño et al., 2020). Since different executive functions involved in VS have shown different rates of development using this task (Gil-Gómez de Liaño et al., 2020), we embed our IB stimuli into the ongoing VS task. That way, we will be able to study how those cognitive processes tested in VS can (or cannot) be related to the potential IB effect at different ages. For instance, approximately adult levels of attentional control seem to be achieved by 8–9 years old, as measured by speed-accuracy measures in the VS task, while intercepts and slopes reach adult levels later in development, potentially related to development of information processing capabilities and/or cognitive flexibility (see Gil-Gómez de Liaño et al., 2020). Thus, by relating IB to the VS indexes, we can propose potential relationships between IB and other aspects of cognitive development.

In the VS task, observers searched for a different target on each trial among a variable number of distractors. Trials were divided evenly among three set sizes (4, 12 and 32) as in classical VS tasks, allowing us to measure the standard effects of set size on search performance. A new target was identified in isolation at the beginning of each trial (see Figure 1). Set size and target presence/absence were randomized across a block of trials. Thus, each set size appeared on 33% of trials and targets were present on 50% of trials. Observers were asked to look for target items that “had been stolen” from the pirate chest (see Figure 1). They were encouraged to tap on the given target, that is, on the “stolen” items, as quickly and accurately as possible. If the target did not appear in the search display, they were told to tap on the pirate chest in the center of the screen as fast as possible in order to proceed to the next “treasure” (the next target). Nine practice trials were followed by 180 test trials (30 trials in each cell of the 3 set size by 2 target presence/absence design). The time needed to do the task varied for each participant, from about 15 min to 25 min, with younger more likely to require more time to finish, and stopping for resting times as needed by every observer. The results from this first phase are reported in Gil-Gómez de Liaño et al. (2020).

In a second phase (the IB phase), carried out on a different day from this first phase, observers performed the same VS task. This time there were only 24 trials that took between 5–8 min to be performed. Two trials were IB trials. For 50% of observers, on the 7th trial the letter, N, was shown among the distractors and on the 21st trial the word, COLOR was shown. For the other 50% of observers, the order was reversed; word trial first, letter second. Both of these IB trials were set size 4 displays and were target-present trials. According to Load Theory (Lavie and Tsal, 1994) there should be less IB in low load conditions (Cartwright-Finch and Lavie, 2006). Our aim was to create a typical IB situation in which the IB stimuli were visible and salient enough to be easily detected. We chose to use target-present trials in order to be able to determine whether finding the VS target is related to reporting the IB stimulus. Note also that the word and letter IB stimuli were visible throughout the critical trial. That is, they did not disappear until the observer ended the trial with a target present or target absent response to the search task. Unlike the classic gorilla-stimulus, the IB letter stimuli did not move. Thus, this is a static IB task. However, the instructions to the participants stressed that the observers should respond quickly because "*there was a pirate following them to steal the treasure items again*".

At the end of the 24 search trials, IB was assessed by asking observers to respond to the following questions:

A) Free-Recall:

    1) *Was there something unexpected/different in this Treasure game? If so, please tell us what,*
    2) *Did you notice there were letters or words among the images shown in the game? If so, please tell us what letter/s and/or word/s you saw,*

B) Recognition:

    3) *Did you see any of these letters or words among all the images seen during the game? S, L, N, O, P, E and SILLA, AMOR, CUENTO, AMIGO, COLOR, PUERTA.*

Statistical analyses

To assess the IB task, we calculated the proportion of individuals giving responses consistent with IB separately for the letter and word IB conditions. For some analyses, we also differentiated those individuals who showed IB in both the letter and the word conditions. Since the pattern of results is very similar across the two free recall questions and the recognition question, we report the results for the free recall. Free recall data have the advantage of not requiring a correction for guessing. All analyses reported below only included individuals who correctly selected the target in the VS trial that included the IB. These observers can be assumed to have been paying attention to the primary task, especially since they had to tap on the target to perform the task. Under these conditions, for the letter-IB condition, there were 257 participants included in the analysis, for the word-IB they were 256, while for the joint IB condition, both letter and word, there were 241 observers[1].

Both for the RIST and the CPT we used the T scores calculated in the standardized tests. The RIST screening test is composed of two scales: "*Guess What*", a verbal scale measuring crystallized intelligence; and "*Odd-Item Out*", a nonverbal scale focused on fluid intelligence. Thus, we included in the analyses the three potential scores: the verbal T-score in the first sub-scale, the nonverbal T-score in the second sub-scale, and the general IQ score, as the quotient between both. On the other hand, the K-CPT and CPT tests produce an assessment report for each participant using the T-scores for the different variables measured in the test: *Response Style* (related to the trade-off between response time and accuracy, with more *liberal* or faster over accurate, and *conservative* being more accurate over faster), *Detectability (d')* of target-distractor discriminability, errors (*misses, commissions, and perseverations*), and *response times* (for each trial and for changes between blocks, as well as for its variability between trials). In the CPT, those measures are considered to be related to different aspects of attention. For the following analyses, we will use all these T-scores both for the RIST and CPT tests measuring IQ and different attentional aspects, respectively. Finally, for the VS task, we analyzed those measures related to those executive functions reported in Gil-Gómez de Liaño et al. (2020), that is, performance (proportion of hits, response times, misses, and false alarms), the slopes of the search functions and intercepts.

We run binary logistic regressions to determine the contribution of all factors to understand how they might modulate IB effects within a unified model. Inattentional blindness results for letter and word were included as the dependent variables in the analyses, and age (in months), CPT performance (with all the T-scores previously mentioned), RIST (including general IQ and both sub-scales described), gender, and VS performance—slopes, intercepts, misses, and inefficiency scores (Townsend and Ashby, 1983), as the covariables. We did not include false alarm measures in the analyses since their levels were at or near zero for most of the observers. We ran several versions of hierarchical logistic regressions, including all factors, or only those that seemed to better contribute to explaining IB effects, both for Letter and Word conditions. However, the regressions included too many factors with very high multicollinearity among them (even after reducing them in the VS task, by calculating the inefficiency scores), making it difficult to produce an understandable, comprehensive, unified model. The results of those regressions are shown in Annex A of Supplementary material 1, for the interested reader. Those results essentially show that Age, VS and CPT factors (not gender nor IQ in any RIST factor) might contribute to explain IB variability. Thus, we decided to analyze data separated for Age, VS and CPT, to better understand their contributions to IB.

For Age, we run again binary logistic regressions, but also ANOVAs to deeply study age effects on IB using the following age-bins: 4 yr old (36 observers), 5 (25), 6 (28), 7 (25), 8 (21), 9 (27), 10 (21), 11–12 (18), 13–14 (20), 15–17 (24), and 18–25 (32). We use finer age groupings at the younger ages because studies of attentional and executive functions in visual search (Gil-Gómez de Liaño et al., 2020) and clinical neuropsychological development (Anderson, 2002) show that the changes at younger ages from 4 to 10 can be more rapid than the changes in adolescents, who we consider in 2 year-bins. All observers from 18 to 25 are grouped

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[1] As we had two IB measures per observer, we could also compute a dependent variable to test IB propensity by coding observers not showing IB as "0", those showing IB for one of the IB stimuli (letter or word) as "1", and those showing IB in both IB trials as "2". Since the results are essentially the same as those shown in the manuscript for the classic binary analyses with IB dependent variables as "IB/no-IB", the outcomes of this analysis are shown in Annex B of Supplementary material 1 for the interested reader. We have maintained the classic analysis in the manuscript, though, for two reasons: first, because it allows splitting results into IB for letter and IB for word. It is interesting to test IB in development for literacy reasons that we will explain in detail in the final discussion. Second, because it allows comparisons with other IB studies.

into a single, "adult" bin. Given the nature of the variables analyzed here, a binary logistic regression could be considered to be a better option than the ANOVA. However, the regression does not allow us to look for differences among different age bins. Since we wanted to compare age bins between each other, especially at those initial ages from 4–5 years to 11–12 years, the ANOVA is a good option to do so. Moreover, the binary logistic regressions show very high levels of collinearity (as just mentioned), so looking at the ANOVA results could help us better understand those relationships among all variables. Indeed, the results using the ANOVAs and the regressions show similar patterns for the main effects, strengthening our conclusions, and the ANOVA allowed us to study finer differences among age-bin groups.

Following the same rationale, we also performed ANOVAs and logistic regressions for the VS and CPT measures after splitting the sample as follows. For some analyses, we split the sample into 4–8 years old observers and those equal to or above 9 years. As we will see in the results, the biggest changes in IB occur at the first stages of development, so looking carefully at those ages can help in understanding IB. For other analyses, we split every age bin into those with higher or lower skills in the VS and CPT tasks, to more deeply understand the relationship between IB and attentional performance. Although splitting the sample for each age bin group will result in some loss of power, it can still give us some hints as to how the attentional variables are related to IB levels. Finally, we compared IB effects between the two IB conditions (letter and word), using the McNemar test.

Results

Inattentional blindness by condition and age

Figure 2 shows the rates of IB for letters and words as a function of age group. The impression is of a clear age effect with, perhaps, a modest interaction of Age and Letter/Word Condition. The McNemar test showed no significant differences between Letter and Word IB conditions [$\chi^2_{\text{McNemar}}(1, N = 241) = 2.58; p = 0.11; \varphi = 0.004$]. When looking at those differences by age using an ANOVA with Letter/Word Condition as the within-subjects factor and Age-Group as the between-subjects factor, the Letter/Word condition effect does reach significance [$F(1,230) = 3.71; p = 0.05; \eta^2_p = 0.02$], although the effect size is very small[2]. In general, if anything, it seems a little easier to see the word (41% reporting the word) than the letter (33% reporting the letter). The main effect of age was significant and large [$F(10,230) = 5.92; p < 0.001; \eta^2_p = 0.20$]. As we can see in Figure 2, as age increases, IB decreases both for Letter and Word conditions. Logistic regression for age confirmed these effects: An age predictor showed a significant effect both for Letter [$\chi^2(1, N = 257) = 5.12; p = 0.02; OR = 0.99$] and Word [$\chi^2(1, N = 256) = 26.5; p < 0.001; OR = 0.98$] conditions. The predicted change odds ratio was equal to 0.99 for Letter, and 0.98 for Word, showing that increases in age correspond to decreases in IB (see again Figure 2). Finally, returning to the ANOVA, the interaction between Age-Group and Letter/Word condition did not reach significance [$F(10,230) = 1.11; p = 0.35; \eta^2_p = 0.05$]. As can be seen in Figure 2, age effects are not linear. Changes in IB are obvious at early developmental stages, and by ages 8–9, those IB effects have roughly stabilized (c.f. Anderson, 2002; Gil-Gómez de Liaño et al., 2020). Indeed, if the analysis is split into observers less than or greater/equal to 9 years, the results change. For older observers (9+ years) there is no effect of Age-Group, ($F < 1$), while the Letter/Word Condition effect is clearer now [$F(1,125) = 8.09; p = 0.005; \eta^2_p = 0.06$], although still small. On the contrary, the effect of age is clear for younger observers (4–8 years) [$F(4,105) = 6.83; p < 0.001; \eta^2_p = 0.21$], with no effects of Letter/Word Condition ($F<1$). Interaction is not significant for any split sample ($F < 1$ for both ANOVAs).

To have a more robust measure (two, instead of just one data point per observer), we also calculated the proportion of observers showing IB in both Letter and Word trials compared to those showing IB just once or not at all. As we can see in Figure 2 (triangle line), there is again an evident age effect, both in the logistic regression [$\chi^2(1, N = 241) = 26.8; p < 0.001; OR = 0.98$], and in the ANOVA with Age-bin as the factor [$F(10,230) = 6.14; p < 0.001; \eta^2_p = 0.21$]. Although the tendency to show IB in both conditions is a bit lower than the Word and Letter alone conditions (especially for participants 6 years old and older), the pattern is quite similar to that found for the Word condition: The main age changes occur from 4 to 8–9 years (steeper age function), after which performance stabilizes, showing similar IB levels from 8 to 9 and above ages ($p > 0.05$ for those older ages).

Inattentional blindness and the visual search task

We performed binary logistic regression with IB as the dependent measure, using VS variables separately for Letter and Word conditions[3]. Although the omnibus test was significant for Letter [χ^2 $(7) = 16.32; p = 0.02; r^2_{\text{Nagelkerke}} = 0.08$], the effect was not as big as for Word [χ^2 $(7) = 57.66; p < 0.001; r^2_{\text{Nagelkerke}} = 0.27$]. When we split the sample at 9 years, we find no effects for Letter ($p > 0.14$ in both samples), while for Word, all the variability comes from the younger observers (below 9 years) [χ^2 $(7) = 23.7; p < 0.001; r^2_{\text{Nagelkerke}} = 0.28$], with no significant effects for the older observers of +9 years [χ^2 $(7) = 9.12; p = 0.25; r^2_{\text{Nagelkerke}} = 0.09$]. For the younger group, there are marginally significant effects of absent trials in inefficiency scores ($p = 0.06$). The more efficient the search, the less IB is shown. This may be related to development of attentional control since the factors from VS that are related to IB modulations (essentially, efficiency measures in the VS) are those overlapping attentional control processes (see Figure 5 in Gil-Gómez de Liaño et al., 2020, and reproduced also in Figure 5 in the discussion below).

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[2] All interpretations of effect sizes were based on Cohen (1988) and Lakens (2013).

[3] We will not include further analyses for those observers presenting IB both for Letter and Word from now on, as we have shown for the Age analysis. The reason is that the results of those analyses replicate those found for the Word condition, both for the VS and the CPT analyses. So, we do not want to overload the manuscript with too many analyses.

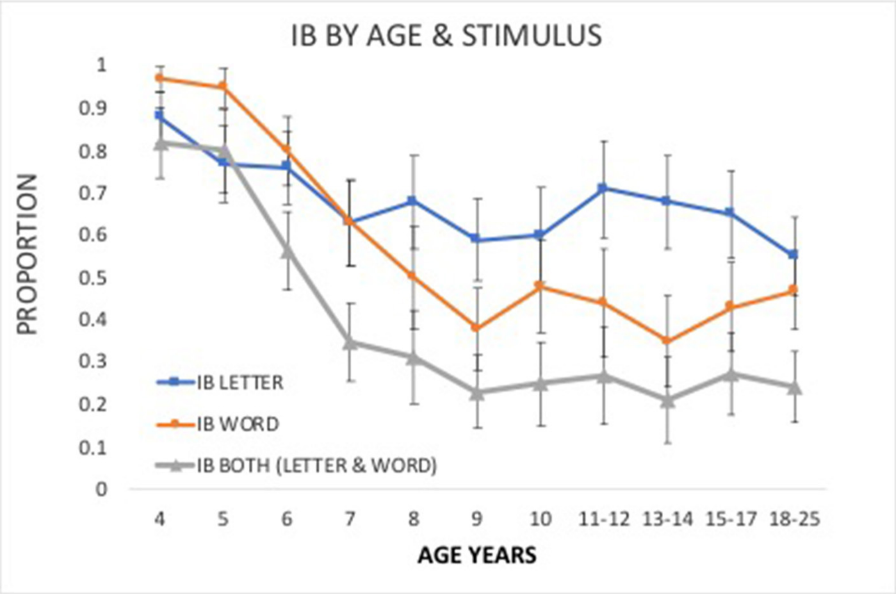

FIGURE 2
Proportion of inattentional blindness for letter, word, and both for letter and word by age.

As another way to examine the effects of Age together with those of the VS measures, we performed ANOVAs with the VS measures (inefficiency scores, slopes, intercepts, and misses) split by the median into two levels as shown in Figure 3 (High-Low), and used Age-Group levels as the factors and IB results as the dependent measure. Figure 3 shows the effects of those VS variables as a function of age for the Word condition. The Letter condition produced a similar pattern and the small effects were not informative. There are hints that *more* IB may be associated with *worse* VS performance but the effects are modest.

Indeed, the ANOVAs showed a pattern of results pointing to effects of Age in the Word IB condition[4] for the VS measures (age main effect for inefficiency scores [$F(10,234) = 6.05$; $p < 0.001$; $\eta^2_{\text{partial}} = 0.21$], for slopes [$F(10,234) = 6.20$; $p < 0.001$; $\eta^2_{\text{partial}} = 0.21$], for intercepts [$F(10,234) = 6.17$; $p < 0.001$; $\eta^2_{\text{partial}} = 0.21$], and for misses [$F(10,234) = 6.16$; $p < 0.001$; $\eta^2_{\text{partial}} = 0.21$]), but no modulation by VS-skills ($p > 0.20$, for all cases), nor for the interactions ($F<1$, for all cases). Therefore, although splitting age bin samples for every group of age could reduce statistical power, these results support the impression that there are no main effects of other variables besides age development in those attentional processes immersed in VS related to attentional control (as we have seen, misses and inefficiency scores). If effects were present, they appear to be not as big as those found for age changes. That is, the changes in IB are essentially produced by age, and/or changes in cognitive function (particularly, attentional control) that accompany age.

Inattentional blindness and the CPT test

Using the same binary logistic regression with IB as the dependent measure and CPT variables as factors, the omnibus test showed that the model was not significant for Letter [$\chi^2(10) = 7.89$; $p = 0.64$; $r^2_{\text{Nagelkerke}} = 0.04$]. When the sample was split, no differences showed up either for younger or +9 observers.

For the Word condition, as before, the omnibus test is significant [$\chi^2\ (10) = 25.41$; $p < 0.001$; $r^2_{\text{Nagelkerke}} = 0.13$]. The significant factors from the CPT that contribute to the model are D-prime ($p = 0.04$), Mean RT ($p = 0.01$), and RT block change ($p = 0.04$). However, these effects do not survive if the data are split by age at 9 years. No significant effects are found in younger or older groups, assessed separately. If age is added as a factor in the main analysis, the model is significant [$\chi^2\ (11) = 39.05$; $p < 0.001$; $r^2_{\text{Nagelkerke}} = 0.19$], but, again, the CPT variables are no longer significant ($p > 0.05$ for all). Age is clearly significant ($p < 0.001$) though. Again, as age increases, IB decreases. Thus, the results of this analysis indicate that the CPT measures only modulate the IB effects for the Word condition when age is not controlled. This is illustrated in Figure 4, where the age functions are split into high- and low-performing groups, based on the median scores in each age group. As with the similar analysis for VS variables (Figure 3), it is clear that there are substantial effects of age before age 9 and no very systematic effects of the CPT variables. This is born out in ANOVAs on the Word condition with CPT (high/low) and Age Group as factors. Although again, the high/low split of age bins could result in some lack of power, the main effects of CPT variables are not significant: mean RT($F<1$), D-prime ($F<1$), and Block-change RT [$F(1,234) = 2.76$; $p = 0.10$; $\eta^2_{\text{partial}} = 0.01$]. On the contrary, the effect for Age Group is significant for the three ANOVAs: For mean RT [$F(10,234) = 6.29$; $p < 0.001$; $\eta^2_{\text{partial}} = 0.21$], D-prime [$F(10,234) = 6.53$; $p < 0.001$; η^2_{partial}

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[4] We also made the same analyses for Letter conditions, and, essentially, no significant effects show up in these ANOVAs, replicating previous results both for VS and Age modulation of IB effects.

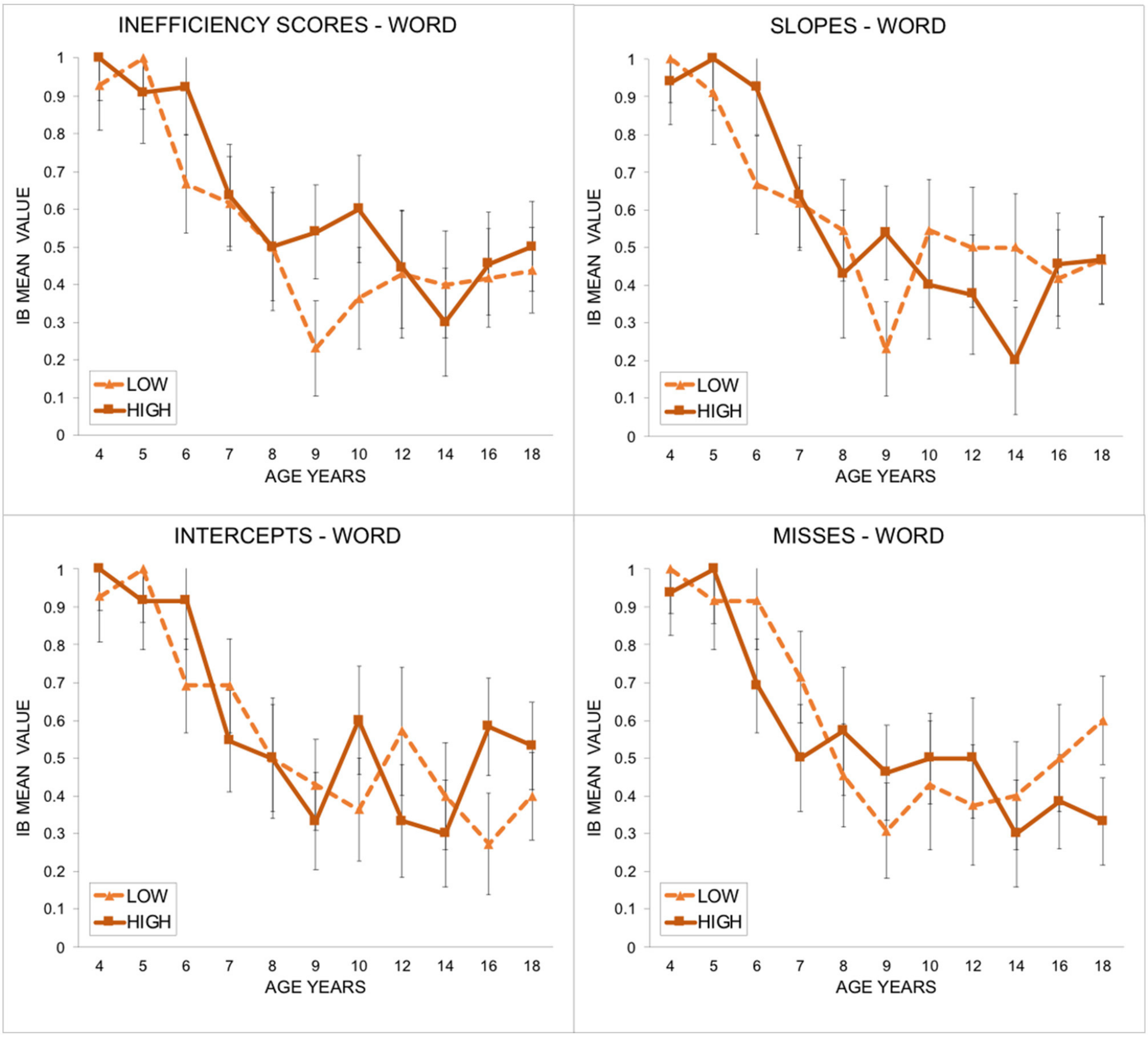

FIGURE 3
IB effects for different levels in the VS (high/low) for inefficiency scores, slopes, intercepts, and misses by age. They are examples to show the randomness of the distribution for IB, and the decrease by age-group.

= 0.22], and Block-change [F(10,234) = 6.15; $p < 0.001$; η^2_{partial} = 0.21].

Inattentional blindness and other individual differences: gender, IQ, and prior-IB

For gender, there are no significant differences for Letter [$\chi^2(1, N = 257) = 0.08$; $p = 0.78$; $\varphi = 0.017$] or for Word [$\chi^2(1, N = 256) = 0.11$; $p = 0.74$; $\varphi = 0.021$]. For Letter, there were 65% of women and 62% of men showing IB effects. For word, there were 50% of women and 48% for men.

For intelligence, we used the RIST and its subscales. We conducted logistic regressions with IQ as measured by the RIST (RIST T-scores) and its sub-scales as the factors and mean IB as the dependent measure. The results show no main effects of IQ on IB values both either letter and word conditions (see Table 1).

As in the previous analyses, we split the sample into two groups at age 9. In this case, although there seems to be no modulation of IQ in IB for our sample, the differences between letter and word conditions at different ages (particularly above or below 9 years) could be related to some sort of reading-like or language capacity that could be directly related to the verbal subscale of the RIST ("Guess What"). For letter, as expected there were no significant models for the regressions. But for word, we found a modulation for the non-verbal subscale of the RIST for the younger (<9 years) observers [χ^2 (1) = 8.56; $p = 0.003$; $r^2_{\text{Nagelkerke}} = 0.11$]. The effect showed that the higher the verbal capacity on the "Guess What" RIST sub-scale, the lower the propensity to show IB. Maybe, those

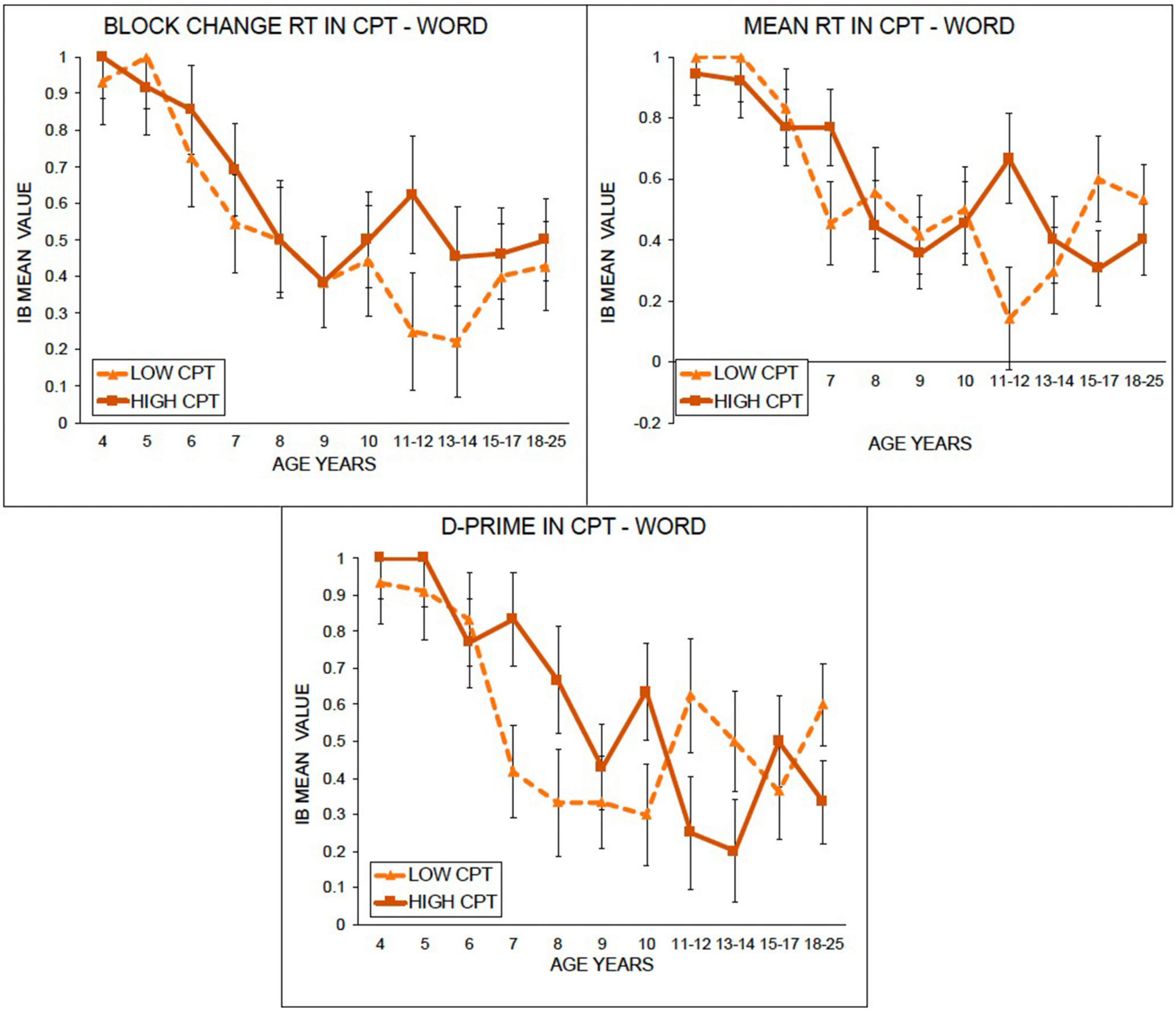

FIGURE 4
IB effects for different levels in the CPT (high/low) by age. We show here examples for D-prime, mean RT, and RT-block change, the factors that were significant for word IB conditions, when age was not controlled in the model.

TABLE 1 Statistics of the logistic regressions for the RIST screening test predicting IB values.

Predictor	*Wald's* χ^2	*df*	n	p	*OR*
RIST T Score	L(.12) W(.25)	1	L(257) W(256)	L(.74) W(.62)	L(.99) W(.99)
"Guess What" T Score	L(1.12) W(.001)	1	L(257) W(256)	L(.29) W(.98)	L(.98) W(1)
"Odd-Item Out" T Score	L(.48) W(.93)	1	L(257) W(256)	L(.49) W(.33)	L(1) W(.98)

L, for Letter condition; W, for Word condition.

younger observers who are acquiring reading skills (4–8 years) and thus have lower verbal-IQ results are less likely to identify and/or report out a word in the IB test. Similar results arise when testing the dependent measure of showing IB both for the letter and word conditions. For this measure, there is a significant effect for younger observers (<9 years) for the verbal sub-scale of the RIST, and also for the general IQ measure [χ^2 (1) $= 5.59$; $p = 0.018$; $r^2_{\text{Nagelkerke}} = 0.07$]; although as can be seen, the effects are small.

Finally, we also correlated IB between the two letter and word conditions. That is, is it more likely to show IB for the second IB stimulus, if you have previously shown IB for the first stimulus? The answer seems to be no, as the correlation between IB-letter and IB-word is very close to zero [$r(n = 241) = 0.004$; $p = 0.96$]. Similar results arise when splitting the sample (±9 years) or for any correlation at any Age-Group.

Discussion and conclusions

The results presented here are consistent with the failure to find conclusive evidence for individual differences that predict inattentional blindness effects (IB). With a sample of 277 observers, and using a visual search task as the primary attentional task, our results only show clear effects of age as a relevant variable to explain IB modulations. We find a strong age effect: Younger children from 4 to about 8–9 years old were less able to detect unexpected stimuli in our static-typical visual search task, with IB varying from 60% to over 90%. From about 9–10 years old to young adulthood, they significantly increased their capacity to detect these unexpected stimuli in visual search to adult levels (ranging from 40 to 50% of IB). One possibility is that general cognitive development is associated with an increasing capacity to detect unexpected stimuli during a modestly demanding visual search task. Individual differences in visual search performance, CPT attentional skills, IQ, or gender did not have significant effects on IB once age was controlled for. Actually, showing IB for the letter stimulus is not even correlated with showing IB for the word stimulus at any age group. An alternative possibility is that even the age effect might be less dramatic than it appears. Because we used letters and words as the IB stimuli, it is possible that at least some of the additional IB effect in younger children arises because they do not consider a word or a letter as particularly odd (or "unexpected") as an addition to a search array filled with other potential "treasure-images". Indeed, this idea might be associated with the correlation found between verbal-IQ skills and IB for younger children when detecting the word-IB stimulus. Those with lower verbal-IQ showed more propensity to present IB compared to those with higher verbal-IQ, somehow showing the IB may be associated with the nature of the IB stimulus itself, but not as a general propensity to show IB. If Simons and Chabris (1999) had a gorilla walking through a chimpanzee exhibit, it might be less surprising if that "unexpected" primate was not reported (because it seems not to be as unexpected as other type of stimulus). In a new study, we are looking for age effects in IB using non-linguistic stimuli, also manipulating the "un-expectancy" of the IB-stimuli. The effect on the youngest children might also be influenced by the use of letters/words. Although children of 4–5 years old are able to distinguish letters and words from images (Evans et al., 2009), it would be useful to replicate the IB results with non-linguistic stimuli. For the present, the development of reading skills at those ages may have complicated the detection of IB, particularly in the word IB task.

It seems unlikely that *all* of the age effects are due to the development of literacy though. There is a clear age effect up to about age 9, by which time children are very familiar with letters and words. How should we interpret the fact that IB performance seems to plateau around the age of 8–9 years old? This age seems to be a critical age when other important selective attention processes, particularly attentional control processes, approach their fully developed state. This is true for aspects of visual search performance (Gil-Gómez de Liaño et al., 2020), as well as for other attentional tasks like the Posner task (e.g. Rueda et al., 2005), and for applied neuropsychological assessments during childhood (Anderson, 2002). Indeed, our results support this idea, since those VS factors related to IB when age was not controlled were those shown in previous studies to correlate with attentional control. In Figure 5, reproduced with permission from Gil-Gómez de Liaño et al. (2020), we can see that those factors related to search efficiency were directly overlapping attentional control development described in previous neuropsychological child-development models (Anderson, 2002). It may be that IB declines because children become more competent at attentional control, which could be related to the propensity to show IB.

In sum, it seems that, before selective attentional processes mature around 8–9 years old, IB rates are in general higher, at least in our visual search task. Once selective attention processes reach nearly full development (8–9 years old), IB plateaus. The adult rate of IB of between 40–60% does not seem to be correlated with attentional capacity/performance, IQ, gender, or previous IB propensity. As is almost always the case, more research would be helpful. Cognitive differences between 4 to 8 year-old children with those ranging 9–25 years old are probably larger than the differences between people with IQs of 90 or 110, or with different scores in visual search or CPT variables. As noted above, it would be useful to see if the same IB x Age functions are seen with a non-linguistic IB stimulus too, or explore those slight effects found for IQ, especially those associated with verbal components of intelligence. It could also be helpful to investigate different IB manipulations that have shown to be critical to understand IB modulations outside the individual differences field. For instance, target present conditions and low load (4 items) for IB trials could have caused some sort of trade-off effect on IB levels. As we have seen, we expect to see less IB in low load conditions (Cartwright-Finch and Lavie, 2006), but at the same time, target guidance on target present trials would presumably increase IB too. More research is needed to determine how load and guidance could interact in IB effects in visual search.

The inattentional blindness effect to study attentional processes

As we have seen in the introduction, the Inattentional Blindness effect (IB) has been widely studied from the first Mack and Rock (1998) experiment and the famous Simon and Chabris's gorilla study (1999). Here, we are using a similar, very standard IB paradigm in which only one (or two in our case) trials are studied as the critical trials to determine if observers can detect and/or report an unexpected/rare stimulus shown within an ongoing task that demands some attention. In our case, that is a visual search task. As we noted in the introduction, using a one-trial test causes obvious statistical power problems. Unfortunately, the problem is that once the observer is alerted to the phenomenon ("Did you see the gorilla?"), the effect goes away. The next time there is a gorilla or, in our case, a word or a letter, observers will report it. By using this paradigm, we can connect our work to the previous IB papers that have used this final one-trial measure of reporting seeing the IB

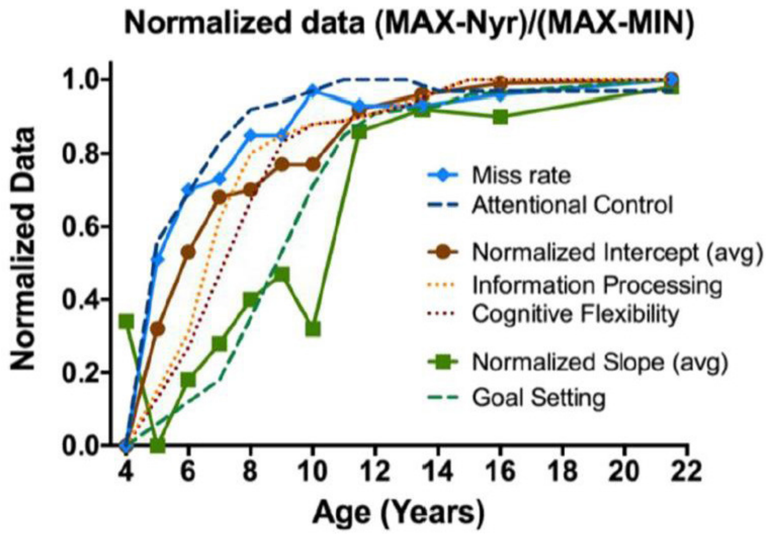

FIGURE 5
Normalized developmental curves for misses, slopes, and intercepts, reproduced with permission from Gil-Gómez de Liaño et al. (2020).

stimuli. However, this comes with the statistical problems inherent in a task that yields only one or two critical trials per participant. More research is needed to develop new IB paradigms that permit multiple trials, but within the classic IB paradigm, one trial is all you can get for analyses.

Other methodological factors can also have an impact on these types of IB paradigms. For instance, the dynamics of the task could be critical given that some tasks, like the "gorilla-task" of Simons and Chabris (1999), involve motion and very distinctive IB stimuli while other tasks are static and use less prominent IB-stimuli (e.g., Buetti et al., 2014). Indeed, some contradictory results have been found, using different dynamic/static tasks (Memmert, 2014; Zhang et al., 2018). Dynamic stimuli do not necessarily generate IB in the same manner as the static stimuli used here, especially in children (for instance, a word can attract more attention than a letter, as we have seen in the results for our older 9+ observers). Tasks with moving stimuli have shown interesting results in developmental studies, perhaps because they are easier and/or more attractive for younger observers. For instance, observers as young as 4-year-olds show a pop-out "attentional-capture" effect for chasing stimuli (Hofrichter and Rutherford, 2019). With these effects in mind, it might be that a dynamic attentional task, together with a moving IB stimulus could produce lower levels of IB in the younger children, at least compared to the high IB levels we have found using our static visual search paradigm with letter/word stimuli. A recent study has found results supporting this possibility, showing that young children (4–6 years-old) are more able to detect unexpected stimuli under dynamic conditions (Fang et al., 2021). We are also testing this motion-static difference in IB in our new study with non-linguistic stimuli. So far, these results may be telling us that IB is not a single phenomenon but something more like a term that covers a variety of situations that cause observers to miss seemingly obvious stimuli. Indeed, as we have seen, the IB effect can vary from 30–40% to 80% of people failing to notice the IB-stimulus upon some of these factors.

However, it is important to note that if attentional control development can help us understanding IB, thus supporting theories of attention failures are more likely to be in the base of the IB effect (Simons, 2000), and against the *inattentional amnesia* hypothesis (Wolfe, 1999). Forgetting is thought to be governed more by storage processes (and, potentially, by access to awareness), than by retrieval processes in childhood (see Howe and O'Sullivan, 1997, for a review). Looking at the time course of these processes, Drummey and Newcombe (2002) showed that older children do show more effective retrieval processes than do younger ones. Their 4-year-old children showed higher levels of amnesia in a source memory task. However, their 6–8 year-old children showed very few errors and, therefore, little amnesia in their task. Since in our study, those 6–8 year children still show elevated levels of IB compared to the older ones (8–9 years old and over), it appears that the time course of this aspect of memory development does not match the time course of IB development. Again, we need more developmental research using a wide-ranging battery of IB tasks across the lifespan, to rule out the *amnesia* hypothesis. Perhaps our search task (or other's) was too easy to reveal meaningful differences between observers over the age of 8–9 years old, and the use of linguistic stimuli might have affected the results.

Final conclusions

What seems relevant is that the present results constitute new evidence that the study of developmental changes in IB can be critical to understanding IB, and stress the fact that more research on IB across the lifespan will help to understand the phenomenon. More lifespan studies (not only developmental) are needed to test these hypotheses, as are new ways to improve IB paradigms to allow for more than one or two critical trials per observer. Those studies should also include older adults who have been found to show increasing IB levels at older ages quite consistently over several works (e.g., O'Shea and Fieo, 2015; Horwood and Beanland, 2016; Saryazdi et al., 2019). Lifespan studies could be a source of information to understand how these task-related variation and cognitive process maturation might interact to understand why we sometimes miss what is right in front of our eyes.

Data availability statement

The datasets presented in this article are not readily available because of the ethical restrictions as the study involved minors. Requests to access the datasets should be directed to the corresponding author.

Ethics statement

The studies involving human participants were reviewed and approved by UAM Ethical Committee Code: CEI67-1193. Written informed consent to participate in this study was provided by the participants' legal guardian/next of kin.

Author contributions

MQ-G programmed the experiments, and CC carried out the data collection and codification with help from MQ-G, BG-G, and EP-H. BG-G and JW performed the data analysis and wrote the paper. All authors conceived and planned the design and review the final version of the manuscript. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fcogn.2023.1134505/full#supplementary-material

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Cognitive inhibition abilities explain inter-individual variability in gender-space associations

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There is a great deal of research describing the close association that exists between numerical and spatial representations, illustrating the SNARC (Spatial-Numerical Association of Response Code) effect. This effect signals the spatial mental representation of small numbers to the left and larger numbers to the right, coinciding with the direction of reading and writing. Subsequent research has found a similar spatial representation for other stimuli (e.g., size of objects and animals, and words associated with time). Some of these spatially represented stimuli are social in nature, even suggesting a spatial mental organization of stimuli based on gender (e.g., the upper part of a vertical axis for males and the lower part for females). The aim of the present study was threefold (1) to replicate and extend results on the existence of a mental gender line (as a function of response hand: female-left hand and male-right hand) when responding simply to gender of stimuli; (2) to explore the influence of inhibitory control; and, (3) to determine whether gender-space associations depend on the explicit or implicit nature of a gender task. Three experiments were designed to pursue these objectives. In Experiment 1, female, male and neutral faces and names were displayed, and the participants were asked to identify their gender. Experiment 2, which also included a Stroop task, followed the same procedure as Experiment 1, but displayed objects that could be designated as female or male and others not related to any gender. Finally, in Experiment 3, in which participants were asked to respond to the direction of an arrow, object gender was not relevant to the task. Consistent with previous research and confirming our hypotheses, the results showed a spatial mental representation of the stimuli based on gender in all three experiments, regardless of whether the stimulus was consciously perceived. Moreover, inhibitory ability showed a relationship with the gender-space line effect. The contributions and implications of this study are discussed, as are possible limitations and future lines of research.

KEYWORDS

mental representation of gender, spatial bias of gender, individual differences, cognitive inhibition, conscious/non-conscious perception

Introduction

The close relationship between certain abstract or concrete concepts, such as time, valence or social power, number or size, or spatial representations has been widely explored in recent decades (Schubert, 2005; Pitt and Casasanto, 2020). This research has shown a motor prime response depending on the mental representation of the concepts. For instance, positive valence is associated with rightward space, and negative valence with leftward space in right-handers, while left-handers show the opposite pattern (Casasanto, 2009; Kong, 2013). However, the mental representation of other concepts, such as number, size or time are not influenced by handedness, but rather the direction of reading and writing. Santiago et al. (2007) asked western participants to classify words presented to the left or right of the central fixation point, according to whether they referred to the past (e.g., yesterday) or the future (e.g., tomorrow). Participants responded faster with the left hand for words with a past meaning and with the right hand for words with a future meaning, regardless of spatial location, suggesting a spatial representation of time. Fuhrman and Boroditsky (2010) compared the performance of English and Hebrew-speaking participants using a task consisting of ordering pictures representing different phases of a temporal sequence. The English-speaking participants, who read from left to right, ordered the images according to reading direction, while Hebrew speakers did it in a right-to-left direction. Other aspects, such as size, also seem to be represented spatially, as shown in the study by Sellaro et al. (2015), in which smaller objects and animals were placed on the left and larger ones on the right. What is more, even though participants were not explicitly instructed to process the stimuli by size, this perceptual feature influenced how they responded. This pattern was observed even when the task consisted of classifying stimuli as "living" or "non-living".

Other studies also provide information on the possible influence of these spatial mental representations, biased by the direction of reading and writing, on social aspects. Presaghi and Rullo (2018) found that participants, influenced by the feeling of group belonging, responded faster with the left hand to the image of a person from the same social group (ingroup), and with the right hand to the image of a person from a different social group (outgroup). This effect was called Spatial Organization of Social Categories (SOSC). Moreover, Maass et al. (2009) (see also Suitner and Maass, 2016) found that participants with strong sexist stereotypes tended to draw men to the left of women in an action scene (e.g., a volleyball match). The authors argue that this response pattern is due to a spatial bias in the representation of social groups consistent with writing direction. In another experiment, they observed that Italian-speaking participants showed a tendency to place agentic groups (men and young people) to the left of less agentic groups (women and older people), whereas Arabic-speakers tended to represent agentic groups to the right. In the context of their theoretical model, Spatial Agency Bias (SAB), this biased representation would be the result of the joint function of two interrelated asymmetries, one from writing direction and the other from subject-object order. Other works reported a spatial representation of information related to gender on a vertical axis. For instance, Zhang et al. (2014) and Zarzeczna et al. (2020) observed that participants placed the male gender at the upper end, and the female gender at the lower end.

An effect that has received much attention is the serial representation of numbers over a mental line from left to right, which is known as the SNARC effect (Spatial-Numerical Association of Response Codes). One of the pioneering works in this field was Dehaene et al. (1993), which, through a series of experiments in which participants were asked to classify numbers as odd or even, observed an interaction between the response hand and the magnitude of the number. Participants showed shorter latency time to respond with the left hand to small numbers, and with the right hand to large ones, than when the response-hand/numerical-magnitude association was left/large, right/small. Subsequent studies showed that the direction of this mental number line representation depended on cultural factors, such as the direction of mother tongue reading and writing (Shaki et al., 2009). In addition to cultural factors, several authors deem it essential to consider other aspects to explain this effect, such as the nature of the numerical task, age or interference control (Wood et al., 2008; Hoffmann et al., 2014; Wu et al., 2020).

Hoffmann et al. (2014), for instance, examined the roles played by age, processing speed, working memory, and cognitive inhibition (as a source of inter-individual variability) to demonstrate the SNARC effect. For this purpose, participants performed several tasks which included: a speed task to assess general processing speed, a pencil and paper version of the Stroop test, a computerized version of the Simon task to measure cognitive interference and inhibition, a digit span task to examine short-term and working memory, and an odd-even number sorting task to measure the SNARC effect. The results showed a relationship between response times to sort numbers and Stroop interference, age, and processing speed. The magnitude of the SNARC effect, in terms of difference between the response time with the left hand to a number and the response time with the right hand to the same number, was greater in older participants among both those with slower processing speed and those who showed lower inhibition ability in the Stroop task. In contrast, no relationship was observed between the occurrence of the effect and Simon-type interference, working memory and short-term memory.

Similarly, Georges et al. (2018) confirmed the role of inhibition ability in the inter-individual variability observed in the SNARC effect, and, furthermore, extended these results by considering that this relationship depends on explicit or implicit processing of number magnitude. The authors included the same Stroop-like task and the General Processing Task described in Hoffmann et al. (2014), in addition to a flanker task (Eriksen and Eriksen, 1974) to measure the control of distracting information interference. Two versions of the number task were employed: a task in which participants were instructed to classify numbers as "> or < 5", and another in which they were asked to classify them as "odd or even". In the first case, the perception of the magnitude of the number would be intentional or explicit, while in the second case an implicit or unintentional perception of magnitude would occur because magnitude is an irrelevant dimension for that task. The results showed an interesting pattern: participants with weaker interference control in the Stroop task presented greater SNARC effect, but only when the odd-even number task was employed. In contrast, when magnitude becomes the relevant dimension, as it is in the numerical task, a stronger SNARC effect is associated with better interference control in the flanker task.

The authors theorize that these results are due to the nature of the interference involved in the paradigms for assessing cognitive inhibition. These paradigms are verbal in the Stroop task and spatial in the Flanker task, coinciding with the cognitive processes responsible for the appearance of the SNARC effect in each numerical task, which are verbal in nature when implicit and spatial in nature when explicit or intentional. Thus, in the flanker task, which involves responding to a target "flanked" by irrelevant stimuli, a greater ability to inhibit the irrelevant stimuli could also mean a better ability to inhibit the numbers (and their magnitudes) flanking the target number represented on the mental number line. However, when the criterion for sorting numbers is different from their magnitude (e.g., an odd-even task), the observation of the SNARC effect could be due to a worse (or less effective) inhibition of the activated magnitude-related information.

The studies by Hoffmann et al. (2014), and Georges et al. (2018) bring to light the importance of inter-individual variability in the strength of number-space associations, highlighting individual differences in the ability to resist interference from irrelevant information, and the explicit or implicit nature of the numerical task. Indeed, it would be interesting to know if these effects can extend to other types of spatial representations like the gender-space association cited above (Maass et al., 2009; Suitner and Maass, 2016; Presaghi and Rullo, 2018). To our knowledge, it remains unclear whether this type of spatial organization also includes objects typically considered to be masculine or feminine, and whether the gender-space association effect can be modulated by the inhibitory abilities of participants.

Current study

The aim of the present study was threefold: (i) to replicate and extend results on the existence of a mental gender line; (ii) to explore the influence of inhibitory control, assessed by using the classic Stroop task, on gender-space association; and (iii) to determine whether gender-space associations depend on the explicit or implicit nature of a gender task. To achieve these goals, three novel experiments were designed. In Experiment 1, faces and names were displayed in the center of the screen, and participants were asked to identify the gender (masculine vs. feminine) of each item (explicit gender task). Experiment 2 included an explicit object classification task and a Stroop task to extend the possible occurrence of a gender-space association when classifying objects with gender implication (e.g., a lipstick) and to examine the relationship between the ability to inhibit distracting information and the strength of the gender-space association. Experiment 3 was conducted to explore whether the gender spatial representation effect could be observed when gender perception was not relevant to the task, and whether individual differences in inhibitory capacity influence the strength of occurrence of the gender spatial effect, as suggested by Hoffmann et al. (2014), and Georges et al. (2018). This experiment included two tasks, the same Stroop task employed in Experiment 2, and the Arrow task. In the latter, gender object images (e.g., lipstick) were followed by arrows pointing to the left or right. Participants were asked to respond to the direction of the arrow. All stimuli were presented under two perceptual processing conditions, conscious (delayed masking condition) and non-conscious (immediate masking condition). The masking condition was manipulated to explore whether the gender-space mental representation could be activated in both conditions of awareness.

To the extent that writing direction influences our social cognition (Maass et al., 2009; Suitner and Maass, 2016), an interaction between the response hand and stimulus gender was expected. In contrast to previous studies on social cognition-space associations, in the gender task, the stimuli were presented individually in the center of the screen, in the absence of context or an action scene, so the response pattern could be partially biased by the direction of writing, but not by the subject-object order, as established by the SAB model (Maass et al., 2009; Suitner and Maass, 2016). Furthermore, the stimuli were presented in a novel way, that is, under conditions of conscious and non-conscious perception, which made it possible to explore whether the spatial representation of gender could be observed even when information was processed without awareness.

The results obtained in the context of the SNARC effect establish a relationship between the capacity to inhibit distracting information and the strength of the number-space association. This relationship seems to depend on the nature (explicit vs. implicit) of the numeric task, and on the type of paradigm used to assess inhibition (Hoffmann et al., 2014; Georges et al., 2018). Taking this into account and employing a Stroop-like task to measure individual differences in inhibition, we expected to find a larger gender-space effect for those participants with lower inhibition ability, in both conscious and non-conscious perceptual conditions. In other words, the presentation of a gender-stereotyped object would activate the gender-space representation and, thus, the congruent response hand schema. However, this activated schema could conflict with the (hand of) response to the target arrows. Those individuals with less ability (or efficiency) to inhibit the gender-activated schema would take longer to settle said conflict and, therefore, would show a greater difference of response latency between congruent and incongruent gender-space/arrow-direction trials.

Materials and methods

Participants

Participants were recruited from the Psychology Degree program, receiving credit toward course requirements as compensation for their collaboration. All participants signed a written consent form after receiving an explanation about the nature of the research, but without disclosing the hypotheses. All of them had normal or correct to normal vision. The study was approved by the Ethical Committee of the University of Almería and conducted in accordance with the Helsinki Declaration.

Experiment 1 involved 44 undergraduates (22 women) with a mean age of 24.6 years (*SD* = 8.86), 36 right-handers, 6 left-handers and 2 ambidextrous individuals. In Experiment 2 the sample was comprised of 30 college students (sixteen women; *M* = 22.63, *SD* = 4.03), 31 right-handers, 6 left-handers and 1 ambidextrous individual. Another sample of 20 volunteers was selected to participate in Experiment 3 (nine men; *M* = 21.81, *SD* = 2.34), consisting of 19 right-handers and 1 left-hander.

Sensitivity power analyses, given the alpha and power values, were performed with G*Power software, version 3.1.9.2 (Faul et al., 2007) to determine the minimum effect size that could reliably be detected from the sample size in each experiment. In Experiment 1, with an alpha = 0.05, a large effect size (d = 0.81) and total sample size = 44, the analysis revealed statistical power >0.99, and a minimum effect size (d = 0.48). In Experiment 2, with an alpha = 0.05, a medium effect size (d = 0.53) and total sample size = 30, the analysis revealed statistical power >0.99; and a minimum effect size of 0.46. Finally, in Experiment 3, with an alpha = 0.05, a medium effect size (d = 0.60) and total sample size = 20, the analysis showed statistical power >0.90, and a minimum effect size of 0.56. The effect size was performed from the minimum partial eta square of main effect in each experiment.

Regarding the *T*-test, the sensitivity analyses showed the following results: in Experiment 1, a medium effect size of 0.62 was revealed, with statistical power higher than 0.80 and a minimum effect size of 0.57; in Experiment 2, a large effect size of 0.99 was revealed, with statistical power higher than 0.99 and a minimum effect size of 0.74; and in Experiment 3, a medium effect size of 0.56 was exhibited, with statistical power higher than 0.90 and a minimum effect size of 0.53.

All of these results demonstrate that the design of our study has sufficient statistical sensitivity.

At the initial phase of the study participants were asked to perform the gender stereotypes questionnaire (Castillo and Montes, 2007). All participants assigned positive adjectives to the female gender and negative ones to the masculine gender. Also, 82% considered themselves feminists.

Stimuli and apparatus

A set of 45 faces was selected from The Chicago Face database (Ma et al., 2015), while the set of 45 names were randomly selected from different lists of the most common names (Statistical National Institute database of Spain, INE). Two forms, elaborated by using Google Forms, were administered to 50 participants to rate how masculine or feminine they perceived these faces and names on a 1–10 numeric rating scale (1 = most feminine to 10 = most masculine). Thus, a total of 20 faces and 20 names valued as the most stereotypic items were selected. Those with values between 4 and 6 were identified as neutral. For the object classification task, a set of 60 CC0-licensed images of objects was used. As before, all of them were included in a form to be rated on a scale of 1–7 by a sample of 36 volunteers. The 10 objects perceived as most feminine (e.g., a lipstick), and the 10 as most masculine (e.g., an electric shaver) were used. Another set of 10 objects close to a score of four were classified as neutral (e.g., a clip), since they can be used interchangeably by men and women. None of the participants of this phase (stimuli classification and selection) performed any experimental task. Selected stimuli are included as Supplementary material.

The size of the images was 3 × 3° of visual angle for faces, objects and masks. The length of names ranged from five to seven characters, and they were displayed with 2° of visual angle, in Times New Roman, 18, black font. The length of arrows was 2° of visual angle, and they were displayed 2° above or below the fixation point in black font. All stimuli were displayed on a 19-inch screen with white background. The masks were made by a random combination of colored pixels, using the free d-Code application. The E-prime v3.0 software (Psychology Software Tools, Pittsburgh, PA) was used to design all the tasks, and record the responses.

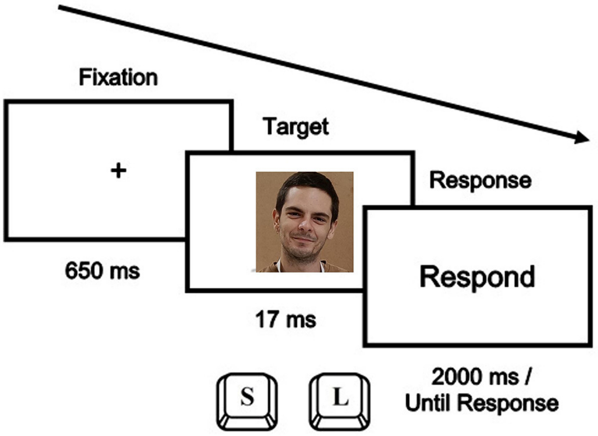

FIGURE 1
Temporal sequence of events in the gender-face identification task (the face shown is of one of the authors, AC, who gives his permission to the image to be published).

Experiments tasks and procedure

Experiment 1 (face and name classification by gender tasks)

Face classification by gender task

Participants were instructed to classify faces as male or female by pressing the "S" key (located on the left side of the keyboard) with the left hand, or the "L" key (located on the right side) with the right hand. The task was divided into two experimental blocks so that, in one block the "S" key was associated with one gender (e.g., male), and the "L" key with the other (e.g., female). In the other block, the key-gender association was the opposite. The order of presentation of the blocks was counterbalanced.

Each block started with six practice trials, in which only male and female faces appeared, and participants received feedback on the accuracy of their responses. Then, an experimental block of 90 trials began with the same 30 faces (10 male, 10 female, and 10 neutral) appearing three times each throughout the block. Figure 1 illustrates the procedure of a single trial. In each experimental trial, a central fixation cross was presented for a randomly varied duration of 650 ms to 1000 ms, which was replaced by the target stimulus for 17 ms. A new display containing the word "respond" appeared in the center of the screen for 2000 ms, or until the participant's response.

Name classification by gender task

The design and procedure for the name classification task were very similar to those for face identification, except that now the stimuli were names, and the target presentation time was increased to 33 ms. Likewise, the distribution of trials across the

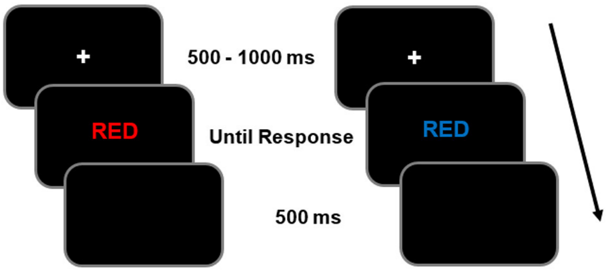

FIGURE 2
Temporal sequence of events of a congruent trial **(left)** and incongruent **(right)** in the Stroop task.

two experimental blocks was identical to the previous task. Both face and name classification tasks comprised Experiment 1, with the order in which they were performed being counterbalanced across participants.

Experiment 2 (object-gender classification task and stroop task)

Object-gender classification task

This task was included in Experiment 2, and participants were asked to classify objects based on whether they were perceived as stereotypically feminine or masculine. A procedure similar to that used in the name classification task was followed, in terms of the temporal sequence of events, distribution of trials and arrangement of response keys.

Stroop task

A computerized version of the Stroop task was used in Experiments 2 and 3. Figure 2 illustrates an example of a trial that began with a central cross as a fixation point with a variable duration of 500–1000 ms. Then, a color word (red, green, blue or yellow) appeared in the center of the screen, and participants had to identify the color of the font with which it was written. For example, if the word “RED” appeared written in a blue font, the correct response would be “blue”. A video game controller with four buttons, each associated with one of the colors, was used to respond. Firstly, participants performed a block of 8 trials to familiarize themselves with the response keys associated with each color. If the error rate of this block exceeded 30%, then these trials were repeated. Otherwise, the task continued with 16 practice trials followed by the experimental block of 48 trials, of which 70% were congruent (i.e., the meaning and color of the word matched, e.g., RED written in red font), while the remaining 30% were incongruent, as both meaning and font color did not match (e.g., RED written in blue font).

Experiment 3 (arrow task and stroop task)

Arrow task

Participants were asked to respond to the direction of a target arrow, which pointed to the left in 50% of the trials and to the right in the remaining 50%. They had to press the “S” key with their left hand when the arrow pointed to the left, and the “L” key with their right hand when it pointed to the right. The target was preceded by the image of an object that acted as a prime stimulus. The set of stimuli used as primes was the same as that used in the task of classifying objects by gender.

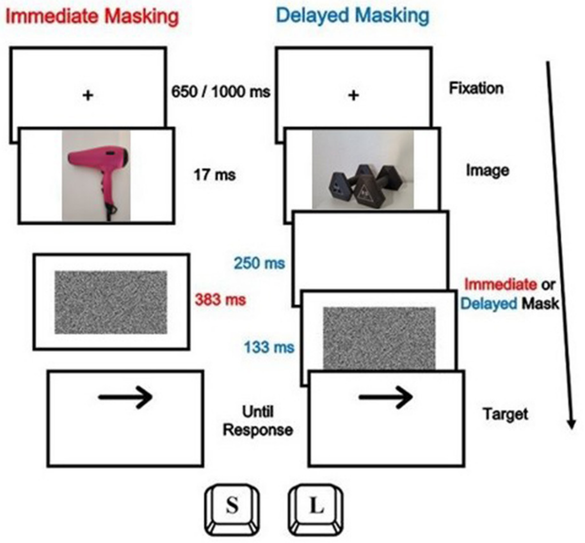

FIGURE 3
Temporal sequence of events of a trial for both masking conditions in the Arrow Task. On the left incongruent gender-space condition (female- right direction of the arrow), and, on the right, congruent gender-space condition (male-right direction of the arrow).

As can be seen in Figure 3, each trial began with a variable fixation point of 650 or 1000 ms, followed by the image of an object for 17 ms. Participants were not given explicit instructions on how to process the prime stimulus, only to focus on responding to the direction of the arrow. The prime stimuli were either masked for 383 ms (immediate masking condition in 50% of the trials) or were followed by a delay of 250 ms and a mask for 133 ms (delayed masking condition in the remaining 50%). In the first masking condition the immediate presence of a mask prevents them from being consciously perceived, while in the second condition they are clearly visible. Two blocks of 120 trials were administered. In half of each block (60 trials) the arrow was above the fixation, and in the other half below. Each object was presented once in each masking condition. The order of the blocks was counterbalanced, and the trials were randomly displayed. Finally, a recognition test was administered to measure the objective threshold of visibility of the objects preceding the target arrow presentation. Participants were instructed to identify the objects as “feminine” or “masculine” by pressing the B key or the N key, respectively. There was a total of 10 trials for each masking condition.

Statistical analysis

The Kolmogorov-Smirnov test and Levene’s test were conducted to assess normality of data and homogeneity of variance, respectively. The results showed a normal distribution

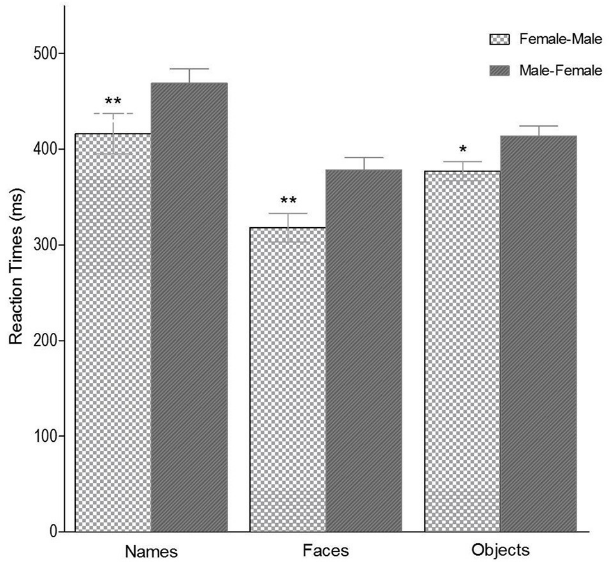

FIGURE 4
Gender-Space Line effects, Congruent (Female-Male) and Incongruent trials (Male-Female), in the names, faces and objects gender identification task. Error bars represent the standard error of the mean. Simple asterisk means $p < 0.01$. Double asterisk means $p < 0.001$.

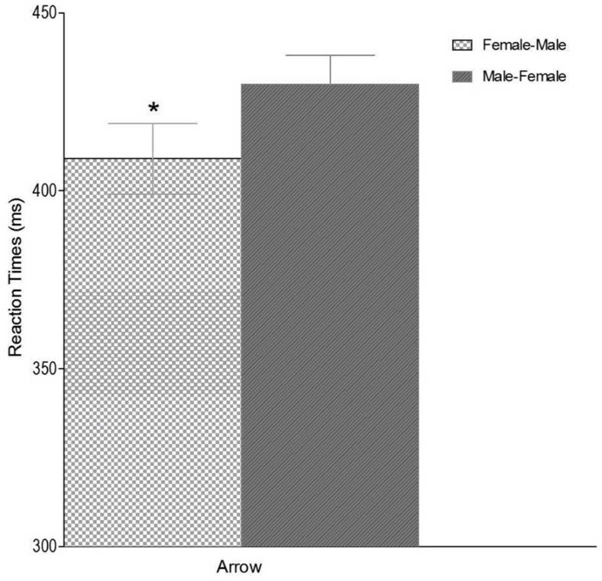

FIGURE 5
Gender-Space Line effect, Congruent (Female-Male) and Incongruent trials (Male-Female), in Arrow task. Error bars represent the standard error of the mean. Simple asterisk means $p < 0.05$.

of data and homogeneity of variance in all variables. Latencies larger than 2.5 standard deviations above the means were excluded from the analyses. An alpha of 0.05 was used for tests of statistical significance.

Reaction times (RTs) of correct responses were analyzed in Experiments 1 and 2 as a function of 4 factors in a repeated measures ANOVA: Stimulus Type (names, faces) × Gender-Space Line (congruent block [Female-Male] and incongruent block [Male-Female] × Stimulus Gender (male, female, neutral), as the within-subject factors; and the Gender of participants (male and female) as the between subject factor. The effect of the Gender-Space Line was based on response hand and gender association, such that the left-female/right-male (female-male) will be considered as a congruent schema, and the left-male/right-female (male-female) association as incongruent.

Regarding Experiment 2, an analysis of covariance (ANCOVA) was also conducted, treating Stroop interference (calculated as the difference between the mean RT of incongruent trials and the mean RT of congruent trials) as a continuous covariate variable.

An ANOVA was performed in Experiment 3 with two within-subject factors: Mask (immediate, delayed) × Arrow direction (congruent with Left-Female object/Right-Male object], incongruent with Left-Male object/Right-Female object, and Gender of participants (male and female) as the between subject factor. Preliminary analyses showed that the location of the arrow (above or below the fixation point) did not affect the results, which is why this variable was excluded from the analyses. The Stroop interference results were employed in a linear regression analysis.

Follow-up paired t-test comparisons were performed to examine the significant interactions.

The discriminability index was calculated using data from the recognition task in Experiment 3 for the two masking conditions, according to the equation d' = ZHits – ZFA (Macmillan and Creelman, 1991; Russo et al., 2017). Chance-level discrimination when d' = 0.

Results and discussion

Experiment 1 (faces and names gender classification tasks)

The ANOVA showed a main effect of Stimulus Type [$F_{(1,42)} = 84.20$, $p < 0.001$, $\eta^2 = 0.671$], with shorter response times for faces than for names, 359.40 ms ($SD = 15.39$) and 491.75 ms ($SD = 12.36$), respectively. Also, a main effect of Stimulus Gender [$F_{(2,42)} = 101.21$, $p < 0.001$, $\eta^2 = 0.770$] was found, with longer response times to classify neutral stimuli (*Mean* = 505.52 ms, *SD* = *15.75*) than for male (*Mean* = 383.67 ms, *SD* = 10.60) and female (*Mean* = 387.26 ms, *SD* = 11.25). The paired comparison t-test confirmed the significant differences between the RTs for neutral and male stimuli [$t_{(43)} = 7.2$, $p = 0.0007$] and between neutral and female stimuli [$t(43) = 6.2$, $p = 0.0008$]. No significant differences were observed between male and female stimuli ($p > 0.05$). In addition, there was a significant effect for the Gender-Space Line [$F_{(1,42)} = 27.66$, $p < 0.001$, $\eta^2 = 0.400$]. Participants showed shorter response times with the left hand to the female and with the right hand to the male stimuli (*Mean* = 404, 80 ms, *SD* = 11.92) than the opposite pattern, left hand-male/right hand-female (*Mean* = 446,17 ms, *SD* = 13.22). These data are graphically represented in Figure 4. No further main effects or interactions were found (*ps* > 0.1).

Experiment 2 (object gender classification task)

The ANOVA showed a main effect for the Gender-Space Line [$F_{(1,28)} = 7.940$; $p = 0.009$; $\eta^2 = 0.221$] due to lower response times with the left hand to female objects and with the right hand to male objects (*Mean* = 377.5 ms, *SD* = 14.6), compared to the male-left and female-right association (*Mean* = 404 ms, *SD* = 16.4). This finding is shown in Figure 4. A main effect for Object Gender also emerged [$F_{(2,56)} = 72.140$, $p < 0.001$, $\eta^2 = 0.720$]. A paired samples *t*-test showed significant differences between the mean response times of the three levels: Female and Male [$t_{(29)} = -2.66$, $p = 0.012$]; Female and Neutral [$t_{(29)} = -2.66$, $p = 0.012$]; Female and Neutral [$t_{(29)} = -9.99$, $p = 0.012$], and Male and Neutral [$t_{(29)} = -8.36$, $p < 0.01$]. Specifically, shorter response times were obtained for Female objects (*Mean* = 352 ms, *SD* = 84.6), than for Male objects (*Mean* = 369.3 ms, *SD* = 84.6), while the longest latency was found for Neutral objects (*Mean* = 447.2 ms, *SD* = 93.6). No other main effects nor interactions were found (*ps* > 0.1), with $F_{(2,27)} = 2.79$; $p = 0.079$, $\eta^2 = 0.172$ in the interaction Object Gender x Participant Gender. Regarding Stroop task performance, ANCOVA analysis showed no modulating effect of Inhibition Capacity on the Gender-Space Line effect ($p > 0.1$) in the object classification task. No significant correlations were found (*ps* > 0.1).

The results of Experiments 1 and 2 showed, firstly, that there was a spatial mental representation of faces, names and objects based on their gender. This is consistent with previous research in which a similar representation based on gender was also found (Maass et al., 2009; Zhang et al., 2014; Zarzeczna et al., 2020). Secondly, no relationship was found between this Gender-Space Line effect, measured with a task where the gender of the stimulus was explicitly asked, and the inhibition of distracting information measured by using a Stroop task. These data are in line with those obtained by several investigations (e.g., Hoffmann et al., 2014; Georges et al., 2018), to the extent that the results of the Stroop task did not explain the strength of the SNARC effect, when explicitly asked for the magnitude of the number. Based on these results, a new experiment was conducted to examine the relationship between cognitive inhibition and spatial mental representation of gender, but in this case, participants were instructed to respond to the direction an arrow pointed and not to gender.

Experiment 3 (arrow task)

The analysis of RTs showed, again, a significant main effect for arrow-direction [$F_{(1,18)} = 6.80$, $p = 0.018$, $\eta^2 = 0.270$]. As shown in Figure 5, the mean RTs were shorter for the female object/left direction and male/right direction associations or congruent trials (*Mean* = 419.94 ms, *SD* = 10.45), than for the male/left direction and female/right associations or incongruent trials (*Mean* = 428.11 ms, *SD* = 10.37).

The two way interaction Arrow Direction x Mask was not statistically significant [$F_{(1,18)} = 0.090$, $p = 0.77$, $\eta^2 = 0.005$]. The Arrow Direction effect was significant in the Delayed masking [$t_{(19)} = 2.0$, $p = 0.05$] and in the Immediate masking condition [$t_{(19)} = 2.1$, $p = 0.05$].

No other main effects nor interactions were found (*ps*>0.1).

Further correlation analysis showed a high rate of positive correlation between the difference in the average latency in the congruent and incongruent conditions of the Stroop task (the Stroop interference effect), and the average response times in the incongruent male/left direction and female/right direction of arrow conditions, in both delayed and immediate masking conditions, Immediate [$r_{(20)} = 0.457$, $p = 0.025$, critical r value $= 0.441$], and Delayed [$r_{(20)} = 0.403$, $p = 0.039$, critical r value $= 0.398$]. By contrast, the correlation analyses did not show any significant relationship between the Stroop interference and the average response times in the congruent female/left and male/right direction, neither in the Delayed [$r_{(20)} = 0.31$, $p = 0.178$] nor the Immediate masking conditions [$r_{(20)} = 0.31$, $p = 0.188$].

This pattern is graphically represented in Figure 6. Moreover, a linear regression analysis revealed that the Inhibition Capacity was a predictor of response times in incongruent trials, in the immediate masking condition [$R^2 = 0.209$, $F_{(1,18)} = 4.75$, $p = 0.043$]. Although it was not found in the delayed masking condition [$R^2 = 0.13$, $F_{(1,18)} = 3.85$, $p = 0.078$].

Recognition task

The analyses showed a high discriminability index in the delayed masking condition (d' = 0.74), which was significantly higher than chance level [$t\ (20) = 7.2$, $p < 0.001$], and a low index around chance level [$t\ (20) = 0.81$, $p > 0.05$] in the immediate masking condition (d' = 0.01).

The results of Experiment 3 again confirmed the occurrence of the spatial representation effect of gender, even when the gender of the stimulus was not task relevant. Moreover, this effect also emerged in the immediate mask condition, when the participant was not aware of the stimulus. These results would indicate that the activation of the spatial mental schema of gender takes place automatically, as would occur in the SNARC effect.

Moreover, coinciding with the findings of Hoffmann et al. (2014) and Georges et al. (2018) in relation to the magnitude of the number and the SNARC effect, a relationship was found between inhibition capacity and the appearance of this effect when the task did not explicitly ask for the identification of the gender of the stimulus.

General discussion

Based on the SNARC effect and on subsequent studies on the spatial representation of different stimuli, including more social aspects of cognition (Maass et al., 2009; Zhang et al., 2014; Zarzeczna et al., 2020), we raised the possibility that there was also a spatial mental representation of gender. Indeed, in Experiments 1, 2 and 3, a gender-space association effect was observed, in terms of faster responses to the left-female/right-male than the left-male/right-female order. This gender-space association effect was obtained for all three types of stereotyped stimuli (faces, names and objects), extending the effect beyond the mental number line, and the spatial representation of non-numerical stimuli (Santiago et al., 2007; Fuhrman and Boroditsky, 2010; Sellaro et al., 2015). In addition, the spatial representation of gender was clearly biased by

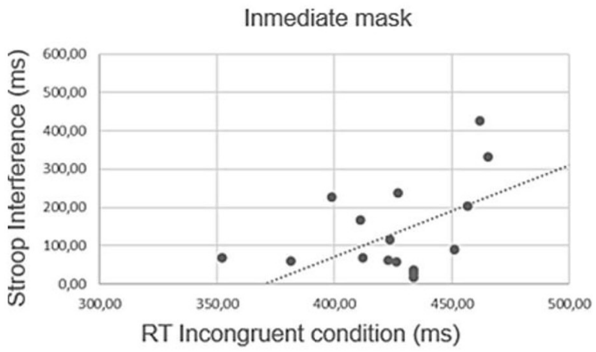

FIGURE 6
Correlations between Stroop Interference (in ms) and the reaction times (RT) in Incongruent condition (male-left direction of arrow, female-right direction of arrow) as a function of making condition (Immediate and Delayed).

writing direction in Western cultures, replicating the same pattern obtained in other studies (Maass et al., 2009; Shaki et al., 2009; Suitner and Maass, 2016).

Although Maass and colleagues had already explored the spatial mental representation of gender (SAB effect), it should be noted that they examined this effect by means of action scenes, which involve a subject-object order. They observed an opposite pattern to the one found in the present study, with males on the left and females on the right (Maass et al., 2009). It is not, therefore, a matter of mentally representing a single object in space, but a scene that includes actions that require the active participation of a subject. Thus, it is possible that the mental schema activated for action scenes differs from that activated when single stimuli are presented.

The fact that the SNARC effect was not observed in all participants by Maass et al. (2009) led some authors to consider whether individual differences in aspects, such as working memory capacity, age or the ability to inhibit irrelevant information, could somehow influence the strength of number-space associations. Hoffmann et al. (2014) reported that age, Stroop interference and processing speed conditioned the magnitude of the SNARC effect. The role played by inhibition capacity in the inter-individual variability was later confirmed by Georges et al. (2018), who also emphasized the importance of the nature (explicit vs. implicit) of the main task.

To the extent that these issues have yet to be addressed in the context of gender-space association representation, the present study aims to explore whether the gender-space line effect could also be modulated by the inhibitory ability of participants, and/or by the nature of the gender task. The data from the third experiment showed that the gender-space line also emerges when identifying the gender of an object was not relevant to the task (but rather indicating the direction of an arrow). In this case, variability in cognitive inhibition, as measured through the Stroop task, would explain part of the appearance of the effect, but not when participants were instructed to classify an object as masculine or feminine (as in Experiment 2). In other terms, the (greater or lesser) capacity for inhibition did not influence the strength of occurrence of the gender-space line effect when the gender task was of an explicit nature, defined in terms of responding by attending to gender. However, in the arrow task, the presentation of an object before the target arrow appeared would automatically induce the activation of the female-male schema, even though one did not respond to the gender, but to the arrow. In incongruent trials, the response hand based on this mental schema would interfere with the target response hand (e.g., female object-left hand/rightward arrow-right hand). Presumably, participants with a greater ability to inhibit the gender-space line (and associated responses) will be more efficient to respond to the actual demands of the task, compared to those with lower cognitive inhibition ability (or efficiency), who showed a stronger gender-space association effect. This response pattern would be in line with the above-mentioned works (see also Hoffmann et al., 2014; Georges et al., 2018; Xiang et al., 2022).

It is important to note that the results showed in the gender stereotypes questionnaire (Castillo and Montes, 2007) suggest positive valence of the female gender in our sample. Hence, the pattern positive valence-right, negative valence-left seems to be weaker than the female-left, male-right pattern showed in our study. The present study also provides evidence on the activation of a spatial mental representation for gender under subliminal perceptual conditions, that is, when the participant is not aware of the prime object (immediate masking condition). This fact reinforces the idea of the automatic nature of this process.

Although all the similarities and differences between the spatial mental representation of number and gender remain unclear, it must be recognized that the variable used in this study (gender) cannot be measured objectively, as is the magnitude of a number. This fact limits the comparison of studies, both in the design and in the theoretical conclusions of the experiments. The use of stimuli in which gender can be conceived in a more objective and uniform way (e.g., grammatical gender of Spanish words) would fix the limitations and help to compare both effects. The task of classifying words according to grammatical gender will also allow us to explore the extent to which the spatial association of gender is related to subjective and cultural aspects. By manipulating the relationship between word meaning and gender stereotypes and roles, we could examine, for example, whether it takes longer to respond with the right hand to a grammatically masculine word but belonging to the feminine gender, than to a grammatically masculine word and belonging to the masculine gender. The results of this experiment would help to better understand the effect found in the Object Classification Task, in which participants identify feminine (vs. masculine) objects significantly faster, an effect that was not observed when classifying names and faces.

Further research is needed to explain the cognitive processes that underlie the gender-space line effect. It would be interesting to observe through neuroimaging techniques the cortical areas involved, both in a face, name and object gender classification task. This would not only provide information about which cortical areas are activated during the occurrence of the effect but would also provide clues about the cognitive mechanisms behind the gender-spatial association and its strength of occurrence. In addition, it will be necessary to explore the implicit self-categorization of gender to identify any possible relationships between the gender-space association and the feeling of group belonging (ingroup vs. outgroup) in line with what happens in the SOSC effect.

Finally, in future research these results could be extended to include different stimuli to determine, for instance: what would occur if what was classified were the faces of both young and old men and women; whether this would have any relationship with agentic traits as occurred with Maass et al. (2009); and, whether there would be a relationship between the spatial mental representation of gender and time. Trying to answer these questions would provide us with more information about the basis of stereotypes, their relationship with prejudices, with implicit associations that we are not aware of and that may be modulating our patterns or actions. By designing activities similar to the tasks described in this study, values, negative stereotypes or prejudices could probably be addressed in early childhood, and/or in people with antisocial behavior.

Data availability statement

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

Ethics statement

The studies involving human participants were reviewed and approved by the University of Almería Human Research Ethics Committee and conducted in accordance with the Declaration of Helsinki. The patients/participants provided their written informed consent to participate in this study.

Author contributions

AC, IC, and CN developed the concept and the design of the experimental work and were responsible for writing the manuscript. AC, IC, DÁ, SF, and CN participated in the implementation of the experimental tasks, data collection, and data analyses. All authors supervised the processes of accomplishing the study, substantially contributed to the interpretation of data, to writing and reviewing the manuscript, as well as to approving the final version of the manuscript.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fpsyg.2023.1130105/full#supplementary-material

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Is the reversed congruency effect unique to the eye-gaze? Investigating the effects of finger pointing, eye-gaze and arrows stimuli on spatial interference

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Introduction: Spatial interference tasks have been recently used to investigate the supposed uniqueness of gaze processing and attention. For instance, it has been observed that gaze stimuli elicited faster responses when their direction was incongruent with their position ("reversed spatial congruency effect", RCE), whereas arrows produced faster reaction times (RT) when it was congruent ("standard spatial congruency effect", SCE). In the present study, we tested whether the RCE is unique to eye-gaze stimuli or can be observed in response to other important social stimuli such as pointing fingers.

Method: To this aim, congruency effects elicited by eye gaze, arrows, and pointing fingers were compared in a spatial interference task.

Results: The RCE was only observed in response to eye-gaze stimuli while pointing fingers and arrows elicited the SCE.

Discussion: This suggests that the RCE reversed congruency effect is specific to gaze stimuli and cannot be generalized to finger-pointing stimuli.

KEYWORDS

eye-gaze, finger pointing, congruency effect, spatial interference task, arrows

Introduction

The answer to the question "is eye-gaze a special stimulus" is often an obvious one. Routine behaviors like following the gaze direction of a stranger while walking down the street or our attention being caught by an advertisement model's gaze support the notion that eyes are a special type of stimuli. They convey unique information about others' intentions, emotional expressions, and other mental states, which is often beneficial and sometimes critically important for our social interactions and survival (Baron-Cohen et al., 1997; Emery, 2000). However, the extent to which eye gaze uniquely engages human cognition and attention remains an open question in the broader understanding of human social behavior processes (see, for example, Blakemore et al., 2004; Chacón-Candia et al., 2022). This issue has emerged recently in social neuroscience research, given that the available experimental procedures to measure social attention, such as the classical version of the gaze cueing paradigm, have failed to show quantitative differences between the abovementioned scenarios. In the gaze cueing paradigm, a face is presented unpredictably at fixation, gazing either left or right. A target is presented afterwards either in the cued or opposite locations. Participants are typically faster to detect or identify the target when the eye-gaze is directed toward the target location, as compared to when it is directed toward the opposite location (i.e., the so-called gaze cueing effect). There is debate in literature around the question of whether the gaze cueing effect is elicited by the directionality of the gaze cue (e.g., Tipples, 2002) or the social information it carries (e.g., for review, see Birmingham and Kingstone, 2009; Capozzi and Ristic, 2018).

On the one hand, a recent systematic review and meta-analysis of the literature on social attention (Chacón-Candia et al., 2022) showed that, despite generating a large amount of data and a notably increased interest in multiple fields of research, the classical version of the cueing paradigm produces the same attentional effects for social directional cues, such as gaze, and non-social directional cues, such as arrows. This challenges the vastly extended intuition that social stimuli are special in modulating human attention and questions the potential utility of the classic cueing task in revealing social-specific attentional effects.[1] On the other hand, several studies have suggested that when other specific variant of the cueing task are used, the processing of eye-gaze direction may rely, at least in part, on the computation of mental states and intentions. For example, it has been observed that attentional orienting in response to eye-gaze direction is reduced when participants believe that the gazer is not able to see a potential target (Nuku and Bekkering, 2008; Teufel et al., 2010) or when its gaze behavior is believed to be controlled by a computer programme (Wiese et al., 2012; Gobel et al., 2018). Moreover, by using a variant of the double-rectangle task, some studies (Marotta et al., 2012; Chacón-Candia et al., 2020) showed that cueing a portion of an object spreads attention across the entire object when arrows are used as cues, while it restricts attention at the specific portion of the cued object when eye-gaze cues are used. These findings are coherent with research showing that when reference objects are presented on the scene, gaze-cues trigger an attentional orienting only to the exact gaze-at object (Vuilleumier, 2002; Wiese et al., 2013). This "special" aspect of gaze attentional orienting may be mediated by theory of mind processes as a consequence of a specific intention automatically attributed to gaze but not arrows. Consistent with this view, Bayliss et al. (2006) adapted the gaze cueing task to investigate to what extent the direction of the gaze can be interpreted as a window into other's intentions. Specifically, in their study, participants had to mark how much they liked target objects after completing a cueing procedure. When eye-gaze was used as a cue, in addition to the classic cueing effect, it was found that objects that other people looked at were likelier than those that did not receive much attention from others. This affective preference for cued objects was not found when arrows were used as cues, despite observing the classic cueing attentional effect. Similarly, combining a traditional gaze cueing paradigm with a visual memory task, Dodd et al. (2012) and Gregory and Jackson (2017) have shown that, despite similar cueing attentional effects, only gaze cues but not arrow cues improved memory accuracy for cued information. This suggests that the eye-gaze stimulus -unlike arrows- is interpreted as an intentional cue that indicates interest and desire.

Taken together all these evidences are difficult to reconcile with the idea that gaze cueing exclusively reflects the operation of mechanisms that only respond to stimulus directionality and it has recently proposed by different authors that both domain-general and mentalizing processes play a crucial role in social orienting (Dalmaso et al., 2015; Capozzi and Ristic, 2020; Chacón-Candia et al., 2022).

All the above studies used variations of the cue-based paradigms where social or nonsocial stimuli were used as cues of the position of an upcoming target in combination with additional manipulations employed to measure extra processes related to target processing such as its selection, learning, memory and likeability.

Recently, target-based paradigms, such as the spatial interference task, have also been used to investigate the supposed uniqueness of gaze processing and attention (i.e., Marotta et al., 2019; Román-Caballero et al., 2021b; Aranda-Martín et al., 2022; Narganes-Pineda et al., 2022). In this task, the critical social or nonsocial stimuli are used as targets instead of as cues. They are presented either to the left or to the right of the fixation point pointing either left or right, and participants are required to respond to the location the stimuli are pointing at. With this type of task, it has been generally observed that non-social stimuli such as words or arrows elicited faster responses when their direction was congruent with their position (e.g., right pointing arrows presented to the right; typical spatial congruency effect, SCE), whereas eye-gaze produced faster reaction times (RT) when it was incongruent (e.g., left looking eye-gaze presented to the right; the "reversed spatial congruency effect", RCE). This dissociation has been studied and replicated by our and other different research groups and supports the intuition of a unique attentional mechanism for eye gaze stimuli (Torres-Marín et al., 2017; Marotta et al., 2019, 2022; Edwards et al., 2020; Ishikawa et al., 2021; Román-Caballero et al., 2021a,b; Aranda-Martín et al., 2022; Hemmerich et al., 2022; Narganes-Pineda et al., 2022; Tanaka et al., 2022). Moreover, the fact that the RCE is modulated by the emotional expression of the target face (Jones, 2015; Torres-Marín et al., 2017; Marotta et al., 2022) and the finding that only the RCE elicited by eye gaze but not the congruency effect elicited by arrows or words is negatively correlated with social anxiety scores (Ishikawa et al., 2021) emphasize the social nature of this effect. However, which social mechanisms are responsible for the RCE are still under debate. In particular, according to the eye-contact hypothesis, the RCE has been interpreted as resulting from the incongruent gaze trials being misattributed by participants as direct gaze (Cañadas and Lupiáñez, 2012; Marotta et al., 2018). This bias would accelerate reaction times in this condition. On the other hand, according to the joint attention hypothesis (Edwards et al., 2020), participants interpret gaze direction in incongruent trials as directed toward the fixation cross to which they are also looking, facilitating performance in this condition. Conversely, gaze discrimination is not facilitated when the eyes look away from where the participant is looking because joint attention cannot be established. Finally, according to the joint distraction hypothesis (Hemmerich et al., 2022), in congruent trials eyes are directed

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[1] Using the so-called counterpredictive cueing paradigm (the target was more likely to appear in the location opposite the one indicated by the cue), Friesen et al. (2004) showed that better performance at the indicated location was only observed when eye gaze was used as cue, but not when the indicated location was cued by an arrow. However, using the same paradigm, Tipples (2008) found that both eye and arrow cues produce similar reflexive shifts of attention, while Guzzon et al. (2010; Experiment 1) observed an early (i.e., from 100 ms) advantage for the predicted, although spatially not signaled, positions for both eye gaze and arrow cues. A recent meta-analysis showed that in counterpredictive paradigms eye gaze and arrow cues produce similar reflexive shifts of attention since a significant and early cueing effect (around 100–200 ms) was observed with both types of cues.

away from where the participants are looking toward withdrawing attention from the relevant task area, consequently leading to the observed increase in RT in this condition.

Both joint attention and joint distraction hypotheses underline the importance of the sharing attention and theory of mind mechanisms on the emergence of RCE. This raises the critical issue of whether the RCE observed with eye-gaze stimuli might be generalized to other socio-biological stimuli, such as pointing fingers. Developmental research has shown that young infants display evidence of interpreting pointing fingers and gaze direction in referential terms and are very sensitive to the communicative situations in which these actions occur (Csibra, 2003). Moreover, both stimuli are crucial to developing language understanding (Tomasello et al., 2007). Other studies have shown that in healthy participants finger pointing cues elicit attentional orienting effects similar to those generally produced by eye-gaze cues (Langton and Bruce, 2000; Belopolsky et al., 2008; Sato et al., 2009). Interestingly, some authors have found that gaze but not finger-pointing cues influence how objects are later valued (Ulloa et al., 2015). Specifically, in Ulloa et al. (2015) study, participants had to mark how much they liked target objects after completing a cueing procedure. When eye-gaze was used as a cue, in addition to the classic attentional orienting effect, it was found that objects that other people looked at were likelier than those that did not receive much attention from others (liking effect; see also Tipples and Pecchinenda, 2019 for different results). This affective preference for cued objects was not found when pointing fingers were used as cues, despite the observation of the classic cueing attentional effect. Of relevance, the presence of a typical attentional orienting but the absence of a liking effect has also been observed in response to arrow cues (Bayliss et al., 2006). This may suggest that only eye-gaze stimuli - unlike finger-pointing or arrows - are interpreted as an intentional cue that indicates interest and desire. The effects of finger-pointing and eye gaze on attention have also been compared in several studies with clinical populations. For example, reduced early attentional orienting has been observed in patients with anorexia nervosa in response to gaze and arrow cues but not pointing gestures (Dalmaso et al., 2015). On the other hand, impairment in attentional orienting was observed in patients with schizophrenia only in response to gaze but not in response to finger-pointing and arrows (Dalmaso et al., 2013). However, it is noteworthy that these cues show similar patterns of orienting attention on simple cueing tasks measured by reaction times in healthy controls (Sato et al., 2010; Dalmaso et al., 2013, 2015). Therefore, pointing gestures represent a crucial comparison tool to evaluate the nature of the RCE elicited by eye-gaze. Indeed, it is a powerful referential cue that we use to draw attention to objects or persons, like gaze direction (Langton and Bruce, 2000; Belopolsky et al., 2008; Sato et al., 2009). However, it does not reflect the same higher cognitive systems, such as the theory of mind mechanisms (Ulloa et al., 2015). For this reason, this study examines how spatial interference effects triggered by eye-gaze stimuli differ from those elicited by finger-pointing gestures and typical non-social stimuli such as arrows. As mentioned above, previous studies have shown that in spatial interference tasks, the RCE is observed when a face with averted eyes or eye gaze alone is used as target stimuli, while the SCE is observed when non-social stimuli such as words or arrows are used. However, it remains unclear whether the RCE is unique to the eye-gaze stimuli or generalizes to other socio-biological stimuli, including the pointing finger. If the RCE is mediated by a common foundation that processes several socio-biological cues (e.g., orienting attention and signaling objects in the environment), then finger-pointing stimuli, like eye gaze, should elicit the RCE. In contrast, if RCE is mediated by mechanisms only elicited by eye-gaze stimuli (e.g., theory of mind mechanisms), finger-pointing should produce, as arrows, the SCE.

Experiment 1

Participants

The study included 24 participants (17 women, 7 men) with a mean age of 23.13 years; they were all students from the University of Granada and received partial course credit for participating. All of them had self-reported normal or corrected-to-normal vision, and they were naïve as to the purpose of the experiment. We estimated the required sample size assuming a significance level of .05 and a power of .9, taking as a reference the effect size obtained in Narganes-Pineda et al. (2022, Experiment 1).

Apparatus and stimuli

Stimuli presentation, timing, and data collection were controlled by a program written using E-prime 2.0 (Schneider et al., 2002) run on a standard Pentium 4 PC. Stimuli were presented on a 17″ widescreen monitor with a 1024 × 768 pixel resolution. They consisted of two black arrows display, two full color cropped eyes, or two fingers subtending a 1° × 4° degrees of visual angle at a viewing distance of 57 cm. Cropped eyes were obtained by manipulating an original face (taken from the MacBrain Face Stimulus Set; https://www.macbrain.org/resources.htm)[2] with Adobe Photoshop CS.

Procedure

After expressing their consent to participate in the study, participants filled in a short version of Autism-Spectrum Quotient (AQ-10) by Baron-Cohen et al. (2001). It is a 10 items self-report measure of autistic spectrum-related traits in adults with normal intelligence. For each item, participants had to respond on a 4-points Likert scale ranging from *definitely disagree* to *definitely agree*. Due to previous works showing that the degree of autistic traits is inversely related to the ability to draw mentalistic inferences from the eyes (Baron-Cohen et al., 2001), the attentional cueing from eye-gaze (Bayliss and Tipper, 2005; Bayliss et al., 2005) and the processing of eye-gaze direction in spatial interference task (Marotta et al., 2022), in the present study we used the AQ-10 to

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[2] The face stimulus was drawn from the NimStim Face Stimulus Set, developed by NimTottenham and supported by the John D. and Catherine T. MacArthur Foundation ResearchNetwork on Early Experience and Brain Development. Please contact Nim Tottenham, attott0006@tc.umn.edu, for more information concerning the stimulus set.

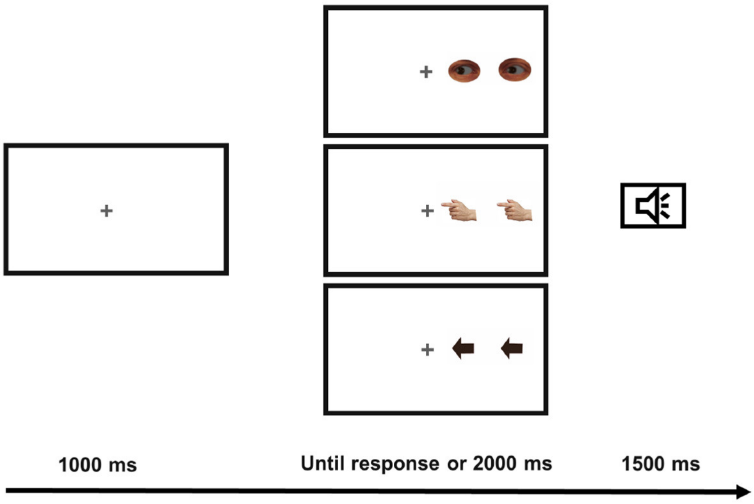

FIGURE 1
Schematic view of a trial sequence from the left to right. One of the three types of target were used: gaze, finger and arrow targets. The example represents incongruent trials. The speaker icon represents the given auditory feedback.

exclude participants with a score of 6 or higher. According to the test, this cut-off is indicative of autism or a significant number of autistic traits. No participant was excluded from both Experiments 1 and 2.

Then participants were conducted in a sufficiently lit room where they seated ∼60 cm from the computer screen; they were instructed to put on headphones. They were required to perform a discrimination task in which they had to respond as fast and accurately as possible to the direction (left or right) indicated by the eye gaze, arrows or fingers. The experiment consisted of three experimental blocks (one for each target type), each composed of 15 practice trials followed by 72 experimental trials. The order of blocks was counterbalanced across participants.

Each trial (see Figure 1) began with a fixation cross presented in the center of a white screen for 1 s. Participants were instructed to fixate on the cross. Then a pair of eyes, arrows, or fingers looking/pointing to the right or the left was presented to either the left or the right of the fixation cross until the participant's response or for 2 s.[3] The distance from the center of the lateral stimulus to the central fixation cross subtended 4.8° of visual angle. Participants were instructed to press the "Z" key in response to targets indicating the left and the "M" key in response to targets indicating the right, independent of the target's location. Feedback was provided when the participants did not respond to the trial and for incorrect responses. In the latter case, a 220 Hz tone was presented for 1,500 ms. Visual feedback of the same duration was provided in the center of the screen when no response was detected. Importantly, this design produced congruent (e.g., a right-indicating target presented on the right) or incongruent trials (e.g., a left-indicating target presented on the right). An equal number of congruent and incongruent trials were presented throughout the experiment.

Design

The experiment had a two-factor repeated measures design, with 36 observations per experimental condition. Data were submitted to a 3 (Target type) × 2 (Congruency) repeated measures ANOVA. Target type had three levels: gaze, arrow and finger. Congruency had two levels: congruent and incongruent trials. *Post-hoc* tests were conducted to analyze the interactions. For each participant, median RTs and accuracy (as mean percent errors) were calculated for each experimental condition. Original data to this study can be found online at: https://osf.io/trzbk/

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[3] Consistent with the majority of the studies investigating the reversion of the spatial congruency effect, in the present study gaze targets were presented until the behavioural response (Torres-Marín et al., 2017; Marotta et al., 2019, 2022; Edwards et al., 2020; Ishikawa et al., 2021; Hemmerich et al., 2022; Tanaka et al., 2022). However, to our knowledge, the effect of the gaze target duration on congruency effect has never been investigated in the context of a spatial interference task. When eye-gaze stimuli have been used as cues, instead of as targets, it has been shown that from an SOA of 200 ms, the cueing effect with both gaze and arrow decreased progressively as the cue-target interval increased. This reduction in the magnitude of the effect was more pronounced with short cues (≤ 300 ms of duration) compared to long cues (for a meta-analysis of the cueing literature, see Chacón-Candia et al., 2022). Moreover, some studies have shown that at very long SOAs (2400 ms) responses to targets presented at the gazed location are slower than to targets presented at the ungazed location, leading to the well-known inhibition of return (IOR) effect (Frischen and Tipper, 2004; Frischen et al., 2007; Marotta et al., 2013). However, is important to know that in our task gaze stimuli were used as targets not as cues. Further studies are surely necessary to investigate the effect of target duration in a context of a spatial interference task.

TABLE 1 Medians (RT in milliseconds), standard deviation (SD) and percentage of incorrect responses (%IR) for each experimental condition.

Target type	Congruency	RT	SD	%IR
Fingers	Congruent	463.87	105.91	0.25
	Incongruent	496.27	104.99	3.33
Arrow	Congruent	465.12	68.23	0.62
	Incongruent	504.27	72.27	4.54
Gaze	Congruent	610.14	73.02	1.62
	Incongruent	574.85	79.52	2

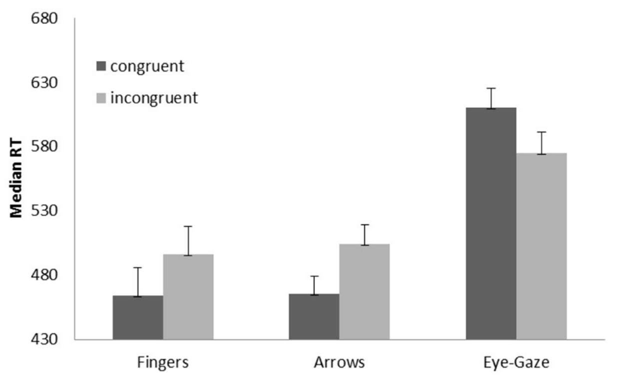

FIGURE 2
Medians (RT in milliseconds) for each type of target and experimental condition. Error bars represent standard errors of the means.

Results

Incorrect responses (2% of the trials) were excluded from the RT analysis. Table 1 shows the median (± SDs) of the RTs and percentages of errors for each experimental condition.

Reaction times

The ANOVA performed on RTs showed a main effect of target type, $F_{(2,46)} = 50.87$, $p < 0.001$, $\eta^2{}_p = 0.69$, with slower RTs for the gaze targets (592 ms) compared to both arrows (485 ms; $\mathrm{F}_{1,23} = 118.15$, $p < 0.001$) and finger pointing targets (480 ms; $\mathrm{F}_{1,23} = 67.05$, $p < 0.001$); RTs were not significantly different between arrow targets and fingers pointing targets ($\mathrm{F}_{1,23} < 1$). The main effect of congruency was also significant, $F_{(1,23)} = 8.76$, $p = 0.007$, $\eta^2{}_p = 0.27$, with slower RTs for incongruent than congruent trials (524 ms vs. 513 ms). Importantly, the critical target type × congruency interaction was significant, $F_{(2,46)} = 23.77$, $p < 0.001$, $= 0.51$ (Figure 2). *Post-hoc* tests on each target type showed that RTs were significantly longer on incongruent than on congruent trials when both arrows and fingers were used as the targets, $F_{(1,23)} = 39.07$, $p < 0.001$, $= 0.63$ and $F_{(1,23)} = 67.83$, $p < 0.001$, $= 0.75$, respectively; in contrast, RTs were significantly faster on incongruent than on congruent trials when eye gaze was used as the target, $F_{(1,23)} = 9.03$, $p = 0.006$, $= 0.28$.

Errors

The analysis of errors showed a main effect of congruency, $F_{(1,23)} = 17.98$, $p = 0.001 = 0.44$, with more errors for incongruent (3.28%) than congruent trials (0.83%). The main effect was target type was not significant, $F_{(2,46)} = 1.15$, $p = 0.326$. The target type × congruency interaction was also significant, $F_{(2,46)} = 5.27$, $p = 0.009 = 0.19$. *Post hoc* tests on each target type showed that participants made more errors on incongruent than on congruent trials when both arrows and fingers were used as the targets, $F_{(1,23)} = 24.20$, $p < 0.001$, $= 0.51$ and $F_{(1,23)} = 8.3$, $p = 0.008$, $= 0.26$, in contrast, no difference between incongruent and congruent trials were observed when eye gaze was used as the target, $F < 1$.

Experiment 2

The primary aim of this experiment was to replicate and extend the findings of Experiment 1. Importantly we wanted to investigate whether the different congruency effects elicited by the three types of stimuli (eye gaze, arrows, and finger pointing) would also be observed even when they are presented within the same block of trials in a random sequence. The type of target stimuli was manipulated between experimental blocks in the previous experiment. Thus, participants might have adopted different strategies for the different target conditions. Consequently, the different findings observed in the three conditions might not be related to different attentional mechanisms elicited by each stimulus type; instead, they might be determined by the different attentional strategies adopted by participants. In order to control for this possibility and to replicate the main findings obtained in the previous studies, we will conduct the present study using a within-block design. It will replicate Experiment 1, except that the type of stimulus will vary randomly across trials. Such a within-block design will prevent participants from adopting a specific "task set" according to the type of stimuli used as targets.

Method

Stimuli and procedure were nearly identical to those used in Experiment 1, except for the order of targets: trials with eye-gaze, arrows and finger pointing were randomly interspersed in each of the three blocks of trials. A different group of 22 participants (17 women, 5 men) participated in this experiment, with the same characteristics as those of Experiment 1. We estimated the required sample size assuming a significance level of .05 and a power of .9, taking as a reference the effect size obtained in Experiment 1.

Design

Data were submitted to a 3 (Target type) × 2 (Congruency) repeated measures ANOVA. Target type had three levels: gaze, arrow and finger. Congruency had two levels: congruent and incongruent trials. *Post hoc* tests were conducted to analyze the interactions.

TABLE 2 Medians (RT in milliseconds), standard deviation (SD), and percentage of incorrect responses (%IR) for each experimental condition.

Target type	Congruency	RT	SD	%IR
Fingers	Congruent	499.48	55.16	0.82
	Incongruent	519.93	58.9	2
Arrow	Congruent	475.25	53.22	0.68
	Incongruent	508.73	54.98	1.54
Gaze	Congruent	640.11	79.88	4.77
	Incongruent	611.93	87.87	5.86

Results

Trials with incorrect responses (3%) were excluded from the RT analysis. Table 2 shows the medians (± SDs) of the RTs and percentages of errors for each experimental condition.[4]

Reaction times

The ANOVA performed on RTs showed a main effect of target type, $F_{(2,42)} = 188.84$, $p < 0.001$, $\eta^2_p = 0.89$, with slower RTs for the gaze targets (626 ms) compared to both arrows (492 ms; $\mathrm{F}_{1,21} = 228.40$. $p < 0.001$) and finger pointing targets (510 ms; $\mathrm{F}_{1,21} = 171.65$, $p < 0.001$); RTs were significantly slower for finger pointing than for arrows targets ($\mathrm{F}_{1,21} = 28.61$, $p < 0.001$). The main effect of congruency was not significant, $F_{(1,21)} = 4.06$, $p = 0.057$, $\eta^2_p = 0.16$. Importantly, the critical target type × congruency interaction was significant, $F_{(2,42)} = 18.09$, $p < 0.001$, $= 0.046$ (Figure 3). *Post hoc* tests on each target type showed that RTs were significantly longer on incongruent than on congruent trials when both arrows and fingers were used as the targets, $F_{(1,21)} = 37.64$, $p < 0.001$, $= 0.64$ and $F_{(1,21)} = 19.51$, $p < 0.001$, $= 0.48$, respectively; in contrast, RTs were significantly faster on incongruent than on congruent trials when eye gaze was used as the target, $F_{(1,21)} = 6.62$, $p = 0.018$, $= 0.24$.

Errors

Only the main effect of target type was significant, $F_{(2,42)} = 27.02$, $p < 0.001$, $= 0.56$, with more errors for the eye-gaze targets compared to both arrows ($\mathrm{F}_{1,21} = 31.65$, $p < 0.001$) and finger pointing targets ($\mathrm{F}_{1,21} = 36.05$, $p < 0.001$); no differences were observed between finger and arrow targets ($F < 0.1$). Neither the main effect of congruency, $F_{(1,21)} = 2.39$, $p = 0.137$, nor the target type × congruency interaction was significant, $F < 1$.

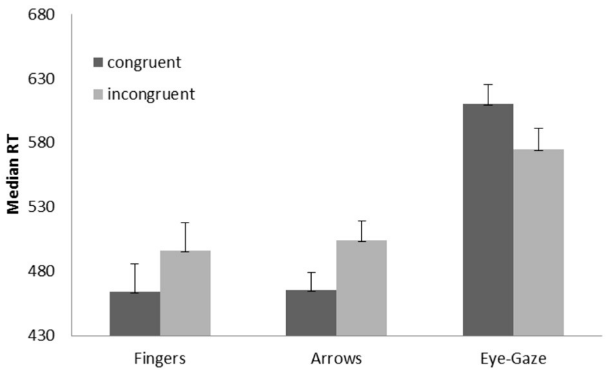

FIGURE 3
Medians (RT in milliseconds) for each type of target and experimental condition. Error bars represent standard errors of the means.

Discussion

The present study aimed to explore if the RCE elicited by eye-gaze stimuli can be generalized to another powerful, social, referential, and attention-orienting stimulus, such as finger-pointing. To this aim, congruency effects elicited by eye gaze, finger-point, and arrows were compared in a context of a spatial interference task. Consistent with previous studies, we observed that the eye gaze and arrow stimuli led to opposite spatial interference effects, with arrows producing the SCE (e.g., faster RTs when the arrow direction was congruent with its position) and eye gaze producing the RCE (faster RTs when eyes direction was incongruent with its position). Of relevance for the present study, we also showed that finger-pointing did not elicit the RCE instead of a robust SCE similar to that produced by arrows. These results indicate that RCE elicited by eye-gaze stimuli is not generalizable to finger-pointing stimuli. This may suggest that the joint attention explanation of the RCE is not the correct one, since pointing with the index finger has been generally considered a crucial tool for referring to the intentions and actions of others. However, the debate about whether the influence of pointing gestures on visual attention reflects higher cognitive systems, such as the theory of mind mechanisms, is still open. Therefore, before acknowledging the possible limitations of this study, we will mention which important aspects could differentiate eye-gaze direction and finger-pointing as referential stimuli. As suggested by Ulloa et al. (2015), although both gaze and finger-pointing are useful stimuli for signaling objects of interest in the environment, an important difference between these two types of stimuli is that eye gaze is intrinsically bearing on other's preferences and intentions (Baron-Cohen et al., 1997; Ulloa and George, 2013). Consistent with this view, they showed that when eye-gaze stimuli were used as cues, a robust attentional orienting effect was observed, and participants liked the objects looked at by others more than non-looked-at objects (liking effect). However, when finger-pointing was used as a cue, only attentional orienting was observed, while the liking effect was absent. Thus, finger-pointing, like arrows, may not communicate information about others' preferences *per se*. This

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[4] To check if there was a speed-accuracy trade-off only for some of the 3 types of stimuli, a Pearson correlation was conducted between reaction times and error for each type of stimuli. In Experiment 1 a negative correlation was observed for arrows ($r = -0.4502$ $p = 0.027$) and eye-gaze ($r = -0.4239$ $p = 0.039$), but not for fingers ($r = 0.3458$ $p = 0.098$). In Experiment 2, none of the correlations was significant (arrows: $r = -0.0476$ $p = 0.833$; eye-gaze: $r = -0.1077$ $p = 0.633$; fingers: $r = -0.0512$ $p = 0.821$).

is a fundamental property of the joint attention and mentalizing processes that may underlie the RCE and would explain why only eye-gaze stimuli elicit it.

Recently, to explain the dissociation observed between the spatial congruency effects observed with eye-gaze and arrow stimuli, we proposed an integrated framework in which both domain-general attentional and domain-specific social processes contribute to the RCE (Chacón-Candia et al., 2022; Hemmerich et al., 2022). In particular, on the one hand, domain-general attentional processes linked to the stimulus's pointing direction and its spatial location would lead to either congruent or incongruent responses, producing a standard congruency effect. On the other hand, additional "special" processes would take place in the case of eye-gaze, reverting the nature of the spatial conflict. In the context of this framework, our results suggest that only eye-gaze stimuli elicited social-specific unique processes, while the congruency effect elicited by arrows and finger-pointing can only rely on domain-general attentional processes.

Moreover, the fact that finger-pointing produced a robust SCE similar to that observed with arrows suggests that it can function as a symbolic cue acquired through daily experience and learning rather than functioning as a socio-biologically cue, such as a gaze cue. Indeed, infants as young as 3 months attend in the same direction as the eyes of an adult face (Hood et al., 1998), while pointing is acquired at ∼12 months (Liszkowski et al., 2004). Consequently, these results reflect the earlier establishment of gaze direction as a cue than the establishment of pointing with a finger. Taken together, our results are consistent with the view that eye gaze has a special status in non-verbal communication and social cognition. However, the possibility that RCE generalizes to other types of social stimuli cannot be excluded. Indeed, from a perspective of cross-cultural study, pointing with hand gestures is not necessarily one of the most powerful, social, referential stimuli since at least some communities prefer face-related stimuli such as the nose or head orientation (Cooperrider et al., 2018). Moreover, our experiment used an image including only hands and fingers as a finger-pointing stimulus. It is possible that this type of impoverished stimulus was not able to communicate the intention and trigger mentalizing processes. Finally, whether the RCE reflects social processing or not is still open. Additional studies must examine whether mentalizing or other social processes mediate this effect. Moreover, consistent with previous studies, responses were generally slower for gaze than arrow stimuli (Vlamings et al., 2005; Hietanen et al., 2006). They were also slower for gaze than finger stimuli. In this study we chose to use realistic eye-gaze stimuli to ensure their approximation to a real social situation. Nevertheless, this may have affected the complexity of stimuli direction detection, being eye gazes direction more difficult as compared to arrows and fingers direction. From our point of view, the slowing of reaction times observed for gaze stimuli may be due to both their social significance and complexity that induces a greater exploration of it. Supporting this view, Vlamings et al. (2005) showed slower reaction times after eye-gaze than arrow stimuli only in typically developed individuals but not in individuals with autism, who are generally referred to as impaired in social attention behavior (Leekam et al., 2000; Werner et al., 2000; Marotta et al., 2012). However, this does not rule out the possibility that the complexity of the three types of stimuli may have partially affect the different congruency effects observed among the three types of stimuli in our study. A previous study showed that when eye-gaze stimuli were compared with equivalent complex non-social stimuli (e.g. inverted triangles) using the spatial interference paradigm, equivalent RTs were observed while preserving the opposite congruency effects observed between eye-gaze and no-social stimuli (Cañadas and Lupiáñez, 2012). This may suggest that differences between gaze and other stimuli are due to his social meaning rather than to his increased complexity. However, further studies manipulating the complexity of both social and non-social stimuli direction are surely necessary to shed light on this issue.

Finally, it is important to know that the interference task we used is a Simon + Spatial Stroop task, in other words, a type 7 dimensional overlap according to Kornblum et al. (1992) taxonomy. Given the compatible mapping between the stimulus direction and the response location (for example, an arrow pointing left always required a left response), two sources of spatial congruency may have contributed to our measure of the congruency effects. In particular, on incongruent trials, there was a stimulus–stimulus (S-S) source of spatial conflict between the irrelevant stimulus location and the relevant stimulus direction, as well as a stimulus–response (S-R) source of spatial conflict between the irrelevant stimulus location and the response location. This second type of spatial congruency is usually referred to as the Simon effect (see Simon and Small Jr, 1969; Simon et al., 1973; Lu and Proctor, 1995, for a review). As such, it is unclear which of these two sources of spatial conflict was reversed by eye-gaze stimuli. In a recent study using an implicit version of the spatial interference task in which participants were required to respond to the color of both directional eyes-gaze and arrow stimuli (Narganes-Pineda et al., 2022; Experiment 2), a compatible response mapping was directly compared with an incompatible response mapping, where participants respond with left keypresses to stimuli pointing right and right keypresses to stimuli pointing left. The results of this study revealed a similar Simon effect (S-R spatial conflict) with both eye-gaze and arrow stimuli. This may suggest that the Simon effect is not modulated by the type of stimuli. However, in this type of implicit task, S-S spatial conflict effects were not observed either with arrows or with eye-gaze. Therefore, it is unknown if the Simon effect can contribute to the congruency effects observed in the explicit version of the task, such as that we used in the present study. On the other hand, in another experiment of the same study (Narganes-Pineda et al., 2022; Experiment 3), it was observed that when the manual Simon effect was eliminated using a verbal task (see Experiment 3) the interaction between Target Type and Congruence (a standard congruency effect with arrows and a reversed congruency effect with eye-gaze) was still observed. This suggests that the manual generation of a spatial response is not responsible for the congruency differences observed between eye gaze and arrows. However, since the Simon effect has been reported also with vocal responses (Wühr, 2006) the possibility that it can contribute to the different congruency effects observed in our study cannot be ruled out. In any case, we humbly consider that the important point of the present study is not whether eye-gaze modulates S-R or S-S source of spatial compatibility, but that it produces opposite congruency effects as compared to other social and non-social stimuli such as directional fingers and arrows.

Conclusion

The present study is the first to examine the effect of different types of social and non-social stimuli on spatial congruency effects. Results indicate that the RCE was only elicited by eye-gaze stimuli while pointing fingers and arrows elicited the SCE. This suggests that the RCE is specific to gaze stimuli and underlie their importance for the human attentional systems.

Data availability statement

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

Ethics statement

The studies involving human participants were reviewed and approved by the University of Granada Ethical Committee. The patients/participants provided their written informed consent to participate in this study.

Author contributions

AM: conceptualization, funding acquisition, analysis and interpretation of results, and draft manuscript preparation. SB: analysis and interpretation of results and draft manuscript preparation. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The author AM declared that they were an editorial board member of Frontiers, at the time of submission. This had no impact on the peer review process and the final decision.

The remaining author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

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On the relationship between rhythm-based temporal expectations and endogenous spatial attention in simple reaction-time tasks

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Introduction: Recent evidence suggests that the temporal expectations afforded by a regular rhythmic structure operate independently from endogenous spatial attention in simple reaction-time tasks. The most common manipulation followed in previous studies consisted of presenting a target stimulus either "in-time" or "out-of-time" (earlier or later) with a preceding rhythm. However, contrary to the proposal of entrainment models predicting a behavioral advantage for in-time compared to both early and late targets, responses were still faster for late targets, according to the so-called "foreperiod effect". This finding makes it difficult to fully disentangle the impact of rhythm and the benefits afforded by the passage of time on the relationship between rhythm-based temporal and endogenous spatial attention.

Methods: To shed more light on this issue, we combined a spatial orienting task with a rhythmic manipulation, in which two placeholders flanking the fixation cross flickered at either a regular or irregular pace. Spatial orienting to the target location was deployed by symbolic color cues that were displayed independently of the rhythm (independent cues) or were integrated with the temporal rhythmic cues (integrated cues). Crucially, for both independent and integrated cues, and for regular and irregular rhythms, the interval between the rhythm and the target (i.e., the foreperiod) was kept fixed in Experiments 1–3 to control for the foreperiod effect, while the effect of foreperiod was explored in Experiment 4.

Results: Results showed a more beneficial effect of rhythms with independent cues as compared to integrated cues. Additionally, the benefit of rhythms was slightly but significantly larger at valid, compared to invalid, spatial locations, regardless of the foreperiod.

Discussion: Our results extend previous studies by showing that interactive effects of rhythms and endogenous spatial attention may emerge in low-demanding detection tasks.

KEYWORDS

endogenous spatial orienting, attention, entrainment, foreperiod, rhythm, temporal expectations

1. Introduction

Selective attention is a crucial cognitive process to guarantee appropriate interactions with the surroundings. Adaptive behavior in rich and dynamic environments relies on efficient prioritization of some events amongst many others, which might be considered distractors. Selective attention operates by means of orienting mechanisms that unfold both in space and time. Directing attention to a specific region of space can enhance sensory processing at that location, producing faster response times (RTs) and more accurate responses (see Chica et al., 2014, for a review). In rapidly changing situations or dynamic contexts such as driving, temporal orienting enables us to focus on specific moments in time so as to enhance sensory processing during a brief period (see Capizzi and Correa, 2018; Nobre and van Ede, 2018, for reviews). Orienting in time can be driven endogenously, by symbolic cues (e.g., Kingstone, 1992; Coull and Nobre, 1998; Capizzi et al., 2013; Weinbach et al., 2015; Coull et al., 2016), or exogenously, by rhythmic sequences (e.g., Jones et al., 2002; Sanabria et al., 2011; De la Rosa et al., 2012; Rohenkohl et al., 2012; Breska and Ivry, 2018).

According to the Dynamic Attending Theory (DAT; Large and Jones, 1999), rhythmic information may automatically entrain periodic oscillations in the sensory systems leading to optimal moments in time, in contrast with other less appropriate moments. There is consistent evidence showing that sensory processing of an event appearing at the "right" moment is enhanced: responses are faster and more accurate (Sanabria et al., 2011; Rohenkohl et al., 2012; Morillon et al., 2016; Breska and Ivry, 2021). In most of the experimental settings employed to study rhythmic attention, the target location is fixed, eliminating any form of spatial uncertainty. However, in everyday life, or in more ecological situations, adaptive behavior not only requires the selection of the optimal time window for target appearance but also the optimal location in space. The question of whether the underlying mechanisms of spatial and temporal attention work independently or in a combined fashion to improve behavior is still debated (Seibold et al., 2020; Boettcher et al., 2022; Tal-Perry and Yuval-Greenberg, 2022).

The relation between spatial and temporal attention has been mainly investigated in protocols where symbolic cues predict both the target location and the most likely moment of target onset. The literature points to independent processes serving spatial and temporal attention in the context of detection tasks, as revealed by independent and additive effects (Olk, 2014; Weinbach et al., 2015). The pattern changes when the perceptual demand associated to the task increases. For instance, Rohenkohl et al. (2014) asked participants to discriminate the orientation of a Gabor-patch stimulus that was preceded by an arrow-like cue. The arrow direction predicted the Gabor location, whereas the arrow color informed about the likely onset of the Gabor (800 ms/2,000 ms). Temporal expectations were only beneficial at the attended location, in accordance with a previous study in which visual rhythmic cues were used to deploy spatial and temporal attention (Doherty et al., 2005). To account for the observed results, the authors proposed a neurophysiological model of spatiotemporal attention according to which temporal orienting leads to time-specific synchronization of neural populations in specific retinotopic receptive fields. The effects of temporal orienting would thus be spatially constrained, at least when spatial attention is endogenously driven by symbolic cues.

In the study by Rohenkohl et al. (2014), both temporal and spatial orienting operated through endogenous symbolic cues. A few studies have instead combined endogenous spatial cues and rhythmic expectations. In the paradigm by Kizuk and Mathewson (2017), a series of visual entrainers were flashed at the alpha frequency band (every 83.33 ms, 12 Hz) to enhance sensory processing at specific moments in time (periodic fluctuations). Targets were briefly flashed to the left or right location, either "in-time" (83.33 ms or 166.66 ms) or "out-of-time" (41.66 ms or 125 ms) with respect to the entrainers. Target location was predicted by an arrow-like cue that was presented at the beginning of each trial (before the entrainers). Interestingly, the authors observed interactive effects between spatial orienting and entrainment, which were indeed opposite to what could be predicted following the neurophysiological model of spatiotemporal attention (Rohenkohl et al., 2014): performance improved for targets appearing "in-time," compared to "out-of-time," only at invalidly attended locations.

Two further studies supply the current debate over the combination of endogenous spatial attention and rhythm-based temporal expectations. Jones (2015; 2019 Experiment 1) showed that orienting by means of endogenous spatial attention and rhythmic sequences were independent and manifested themselves in additive effects. In his work, participants detected (or discriminated) a lateralized target that was preceded by a synchronous rhythm. Spatial attention was directed to the left or right location with symbolic cues (either color, sound, or touch manipulations, depending on the experiment). The moment of target appearance was also manipulated, as targets were presented "in-time" or "out-of-time" (early or late) with the preceding rhythm. In several experiments, with different sensory modalities, Jones (2015, see also Jones, 2019) consistently demonstrated independent effects between spatial and temporal orienting elicited by rhythmic cues.

However, in the studies cited above, it is difficult to disentangle the role of rhythm and foreperiod in temporal orienting. Targets could indeed appear after variable delays (commonly called foreperiods), giving the opportunity to build expectations on the basis of elapsing time, which typically translates into better performance at longer foreperiods, i.e, the foreperiod effect (Niemi and Näätänen, 1981; Capizzi and Correa, 2018). The foreperiod effect is formally described by the hazard function (i.e., the conditional probability that an event will occur given that it has not yet occurred; Janssen and Shadlen, 2005; Herbst et al., 2018; Visalli et al., 2019, 2021). Both Kizuk and Mathewson (2017) and Jones (2015) investigated the relationship between endogenous spatial orienting and temporal expectations based on a mixture of rhythmic information and the foreperiod effect. For instance, as Kizuk and Mathewson collapsed intervals "in-time" with the entrainers (83.33 ms and 166.66 ms), and intervals "out-of-time" (41.66 ms and 125 ms), the effect of temporal orienting could have resulted from both entrainment and elapsing time. With respect to the study of Jones (2015), a pure effect of entrainment should have led to a facilitation effect for "in-time" targets as compared to both early and late targets (U-shaped performance). By contrast, there was no evidence for an advantage of "in-time" targets as compared to "late" targets, thus suggesting that temporal orienting could have been masked by the foreperiod effect.

Considering that the relation between endogenous spatial attention and rhythmic orienting remains poorly understood, the objective of the present study was to further investigate the combined effects of spatial and temporal attention. Temporal expectations were generated by rhythms, rather than guided by the hazard function. Targets were indeed preceded by a regular or irregular rhythm to enhance time-specific synchronization in the former situation. Unlike previous studies, we used a fixed foreperiod duration between the last rhythm and the target in order to obtain a "pure" effect of rhythm on target processing. The fixed duration was twice as long as the ISI used in the regular sequence in Experiments 1, 2, and 3 (see Lange, 2010; Cutanda et al., 2015, for a similar procedure) to obtain a "pure" rhythm effect and to balance the weight of the foreperiod in regular and irregular rhythm trials. In Experiment 4, the role of the foreperiod in the combination of spatial attention and temporal expectations based on rhythms was further explored (using a variable foreperiod of 400 ms and 800 ms). Cue color was used to manipulate endogenous spatial attention in all the experiments, but the type of cue changed between experiments. Besides controlling for the foreperiod effect, we also manipulated how spatial and temporal information was delivered. Previous studies on the relationship between rhythm-based temporal expectations and endogenous spatial attention have manipulated spatiotemporal information either with independent or integrated cues. However, the precise role of this manipulation has been somehow neglected in previous studies. In Experiment 1, two independent cues were used to provide spatial and temporal information, as in the study by Kizuk and Mathewson (2017). In Experiment 2, spatial and temporal information was still conveyed by independent cues but we used a central rhythmic cue to orient attention in space (as in the studies by Jones, 2015, 2019), which might facilitate the integration of spatial and temporal information. Finally, in Experiments 3 and 4, we used a peripheral rhythmic cue as the contour of the placeholder flickered in different colors to provide both spatial and temporal information (hereafter referred to as integrated cues).

2. Experiment 1: independent cues

The goal of Experiment 1 was to investigate whether target detection could be enhanced by means of endogenous spatial symbolic cues and visually-presented rhythmic sequences. Spatial information was provided by manipulating the color of the fixation cross, which predicted the target location on approximately 70% of the trials (see Methods). Additionally, targets were always preceded by a series of placeholders flickering either in a synchronous or asynchronous pace (hereafter referred to as regular and irregular rhythm, respectively). We aimed to test whether participants could combine spatial endogenous orienting with rhythmic information to enhance target detection while controlling for the foreperiod effect. In this first experiment, two independent cues provided both types of information independently.

2.1. Method

2.1.1. Participants

Thirty-eight undergraduate psychology students from the University of Paul Valéry Montpellier (France) participated in the study as part of a course requirement. All participants (in this and the following experiments) had a normal or corrected-to-normal vision, none of them was color-blind, and all gave written informed consent before their inclusion. The studies had ethical approval from the local committee (CER UPVM-n°2020-02) and were conducted in accordance with the Declaration of Helsinki.

Three participants were excluded from data analysis due to excessive responses to catch trials (> 40%). One additional participant was excluded for low performance (>20% missing responses), whereas another participant was excluded for low compliance with task instructions (only one response on invalid trials), leaving a final sample size of 33 participants (mean age = 19.80 years, age range = 18–26 years, 3 males, 5 left-handed). A posteriori sensitivity power analysis (G*Power 3 software; Faul et al., 2007) showed that the sample size was adequate to detect significant ($\alpha = 0.05$) mean differences between two dependent means (i.e., the main effect or the interaction effect of a 2-by-2 repeated measures ANOVA) with a medium effect size $d = 0.5$ (Cohen, 1977) and a statistical power of 0.80.

2.1.2. Apparatus and stimuli

The experiment was run on 22″ Intel® Core™ i5-64002 Duo personal computers in a group testing room at the University of Paul Valéry Montpellier. Stimulus presentation and data recording were controlled by E-prime v2 software (Schneider et al., 2002). The viewing distance was approximately 60 cm. All stimuli were presented against a gray background. Stimuli consisted of a fixation point, two placeholders, and a target. The fixation point was a black cross (0.38° × 0.38° visual angle) that was filled in with red or green color to signal the likely spatial location of target appearance, or with black color during the inter-trial-interval (ITI) (see below). The placeholders were two gray circles (diameter: 0.21° of visual angle; located 0.57° to the left and right of the fixation cross) whose contours were lighter than the background gray color. The target was a Gabor patch (0.17°) that could appear inside one of the two placeholders. For comparison with future studies involving discrimination requirements, the Gabor was tilted either 45° to the left or 45° to the right, with each orientation equally likely to be presented. Gabor patches were created in Matlab (version 2018a; Mathworks Inc., Natick, MA) with a maximum contrast of 1.

2.1.3. Procedure and task

Figure 1 illustrates the timing and the sequence of events in a given trial. Each trial started with the presentation of a central fixation cross for a random duration ranging from 1,000 to 1,500 ms. The color of the fixation cross (green or red) indicated the side (left or right) at which the target was more likely to occur (cue colors were counterbalanced between participants). The (red or green) fixation cross remained on the screen during the entire trial. Next, a series of placeholders (six in total) were displayed sequentially to create a rhythmic sequence. Specifically, each placeholder's presentation lasted for 50 ms and was spaced from the other one by a blank inter-stimulus interval (ISI). The duration of the ISI varied as a function of the rhythm condition used in the trial, assuming either a fixed duration of 400 ms (regular rhythm) or a random duration among the following values: 100, 250, 400, 550, and 700 ms (irregular rhythm). For both regular and irregular sequences, the ISI between the last placeholder and the target (i.e., the foreperiod) was always 800 ms (i.e., twice as long as the ISI used in the regular sequence; see Lange, 2010; Cutanda et al., 2015, for a similar procedure). Therefore, both regular and irregular rhythmic sequences comprised the same number of visual stimuli (6 placeholder repetitions) and had an identical duration before target onset, such that the only difference between the two conditions concerned the regularity of the rhythm. After the foreperiod elapsed, the Gabor stimulus appeared inside one of the two placeholders for 100 ms. Participants had to respond to the onset of the Gabor by pressing the spacebar on the computer keyboard. A maximum interval of 1,500 ms was allowed to respond. The ITI lasted for 1,000 ms. During the ITI, the color of the fixation cross changed to black.

Participants were explicitly instructed to keep their gaze on the fixation cross at all times. They were encouraged to use the color of the fixation cross to predict the likely location of target onset, whereas they were told that the preceding rhythm was task-irrelevant and could therefore ignore it. The task consisted of four blocks, for a total of 192 trials, equally divided into regular and irregular rhythm conditions (96 trials each). For each rhythm condition, 68 were spatially valid trials (in which the Gabor stimulus appeared at the location indicated by the color cue, 70.83%), 20 were spatially invalid trials (in which the Gabor stimulus appeared at the opposite location to the one indicated by the color cue, 20.83%), and 8 were catch trials (in which no Gabor stimulus was presented, 8.33%).

The experimental blocks were preceded by a short practice session comprising two sequential parts. In the first part, participants completed 10 trials to familiarize themselves with the general task structure. No instructions about the meaning of the fixation cross were provided. In the second part, comprising 10 extra practice trials, they were informed about the meaning of the fixation cross color and instructed to use it to predict the target location. After each practice trial, participants received feedback on their reaction time (RT) performance; the French translations for the expressions: “Correct!,” “Correct! But try to be faster” (for responses slower than 700 ms), “Pay attention! Target absent” (for responses to catch trials), and “Too late! Be faster” (for no responses), were displayed for 1,000 ms. No feedback was given during experimental trials. The experiment lasted about 35 min.

2.1.4. Data analysis

Catch trials and data from the practice session were discarded before any further analysis. Trials without responses, trials with responses given during the foreperiod, and trials with premature responses (i.e., RTs $<$ 100 ms) were excluded (0.6% of all the trials). For each trial type, RT values more extreme than one and a half times the interquartile range (i.e., the difference between the upper and lower quartile) above the upper quartile or below the lower quartile were identified as outliers (Borcard et al., 2011; see also Vallesi et al., 2022, for a similar approach) and removed (5.75% of the remaining trials). Mean RTs were then computed for each trial type and submitted to a repeated-measures analysis of variance (ANOVA) with Validity (valid, invalid) and Rhythm (regular, irregular) as within-participant factors (JASP Team, 2022).

2.2. Results and discussion

The ANOVA yielded significant main effects of Validity [$F_{(1,32)} = 43.06$, $p < 0.001$, $\eta^2p = 0.57$], and Rhythm [$F_{(1,32)} = 29.67$, $p < 0.001$, $\eta^2p = 0.48$], showing that participants were faster for valid compared to invalid trials (M = 318 ms and M = 332 ms, respectively), and after the regular compared to the irregular rhythm (M = 318 ms and M = 331 ms, respectively), (see Figure 2). The Validity by Rhythm interaction was not significant [$F_{(32,1)} = 0.12$, $p = 0.74$, *ns*, $\eta^2p = 0.004$].

Despite the lack of significant Validity by Rhythm interaction in our data, we were interested in testing whether the effect of rhythm was significant in both spatial conditions. Additional *post-hoc* analyses, Bonferroni corrected, confirmed that the effect of Rhythm significantly enhanced RTs both at valid and invalid target locations ($p < 0.001$ and $p = 0.04$, respectively).

In sum, Experiment 1 suggests that endogenous spatial cueing and rhythmic orienting can act independently to enhance target detection and yield additive effects. However, the combination of spatial and temporal attention might depend on how information is conveyed by the cues and whether the integration of spatial and temporal information is favored. When separate and dissociable cues convey spatial and temporal information, one could argue that spatial expectations might be generated first, allowing for a later occurrence of rhythmic expectations that would develop independently from spatial ones, thus limiting their interaction (Mattler, 2003). Experiment 2 tests the possibility that rhythmic cues could favor the interaction between spatial and temporal attention by triggering the updating of spatial information at a regular or irregular pace.

3. Experiment 2: independent rhythmic cues

Experiment 2 was very similar to Experiment 1 except that the endogenous spatial cue provided by the color of the fixation cross was not steady but rather flickered at the same pace as the placeholders (see Figure 1).

3.1. Method

3.1.1. Participants

Thirty-five new undergraduate psychology students took part in the study, but only 31 were included in the analysis (mean age = 20.26 years, age range = 18–30 years, 6 males, 6 left-handed). Data from two participants were excluded for excessive responses to catch trials ($>$ 40%), whereas two additional participants were discarded for the presence of a high proportion of missing responses ($>$ 20%). A posteriori sensitivity power analysis (G*Power 3 software; Faul et al., 2007) showed that the sample size was adequate to detect significant ($\alpha = 0.05$) mean differences between two dependent means (i.e., the main effect or the interaction effect of a 2-by-2 repeated measures ANOVA) with a medium effect size $d = 0.52$ (Cohen, 1977) and a statistical power of 0.80.

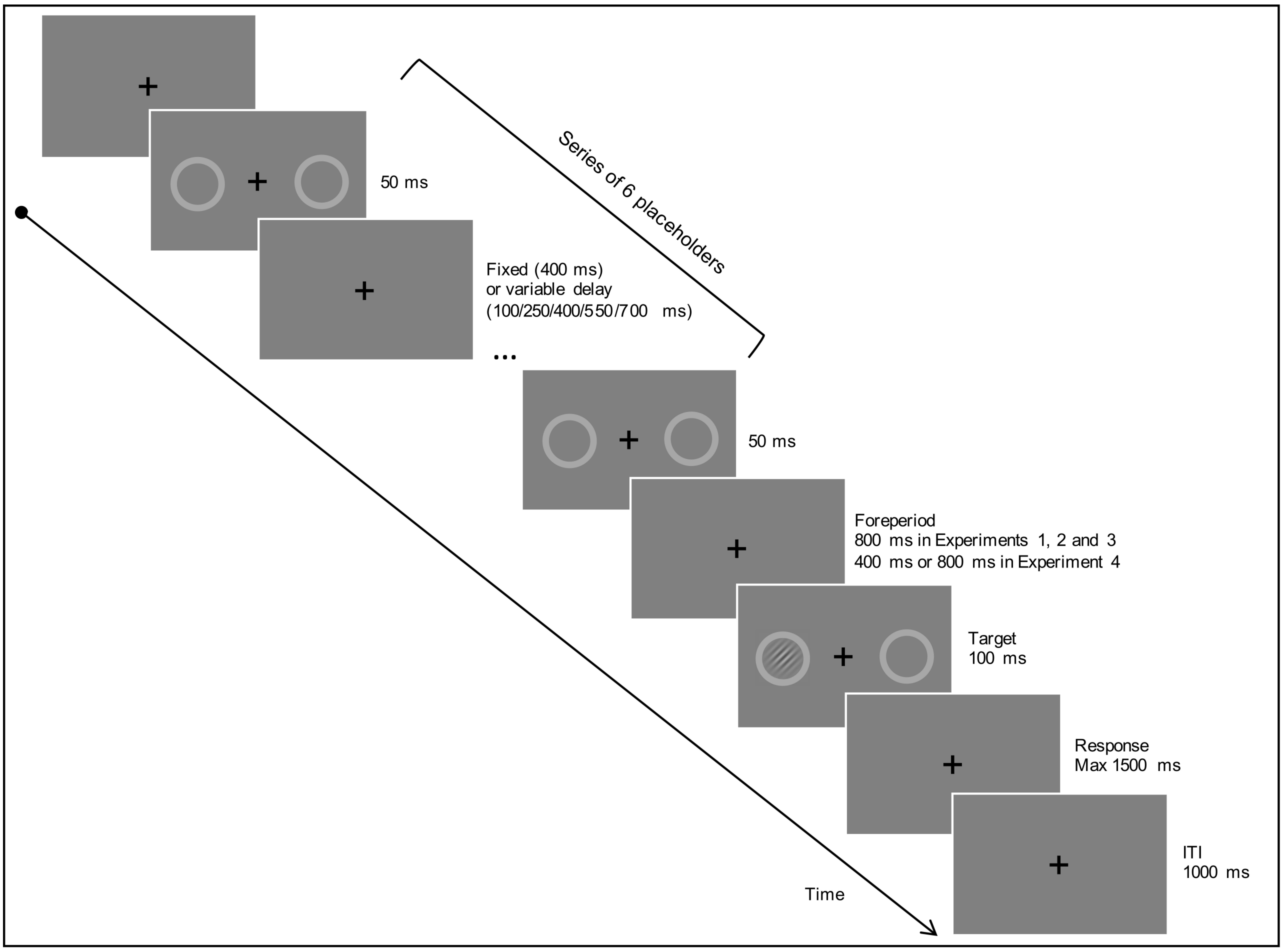

FIGURE 1
Illustrates the timing and the sequence of events in a given trial in Experiments 1–4. For Experiments 1 and 2, the color of the fixation cross oriented spatial attention to the left or right side. The fixation cross was steady in Experiment 1 and flickered at the same pace as the rhythm in Experiment 2. In Experiments 3 and 4, the fixation cross was presented in black (as in the figure) and the color of the placeholders changed to orient spatial attention. Please refer to the main text for further details on each experiment.

3.1.2. Apparatus, stimuli, and procedure

The apparatus, stimuli, and procedure were the same as in Experiment 1 with the following exception: the fixation cross flickered at the same pace (regular or irregular) of the rhythm used in a given trial. Participants were instructed that only the color of the fixation cross, but not its flickering, was predictive of the target location.

3.1.3. Data analysis

The same cleaning procedure (0.1% of rejected trials) and RT outlier removal (4.32%) were applied to the data as in Experiment 1. A 2 × 2 repeated measures ANOVA was used on mean RTs, with Validity (valid, invalid) and Rhythm (regular, irregular) as within-participant factors.

3.2. Results and discussion

Replicating Experiment 1, the ANOVA yielded significant main effects of Validity, $F_{(1,30)} = 17.39$, $p < 0.001$, $\eta^2 p = 0.37$, and Rhythm, $F_{(1,30)} = 52.59$, $p < 0.001$, $\eta^2 p = 0.63$. As expected, mean RTs were faster for valid targets (M = 318 ms) compared to invalid targets (M = 333 ms), and when targets were preceded by the regular rhythm (M = 317 ms) compared to the irregular rhythm (M = 334 ms). The Validity by Rhythm interaction was not significant, $F_{(1,30)} = 1.69$, $p = 0.20$, $\eta^2 p = 0.05$ (see Figure 2). *Post-hoc* analyses, Bonferroni corrected, confirmed that the effect of Rhythm significantly enhanced RTs at valid and invalid target locations ($p < 0.001$ and $p = 0.002$, respectively).

To summarize, Experiment 2 showed again no evidence for an interaction between spatial and temporal orienting. As expected, target detection improved at validly attended (as compared to invalidly attended) locations, and when preceded by a regular (as compared to an irregular) rhythm. Despite the use of rhythmic spatial cues, the two effects did not interact.

4. Experiment 3: integrated rhythmic cues

In Experiment 3, spatial and temporal information was conveyed by a single cue to emphasize their integration. According to the adjusted expectancy model, the type of cues influences how expectations combine (Mattler, 2003), favoring additive effects with independent and separate cues but leading to interactive effects with an integrated cue. Rhythmic information was thus provided by the flickering of the placeholders (triggering regular or irregular rhythms) as in Experiments 1 and 2, but the placeholder color was manipulated to endogenously orient spatial attention. Therefore, a single signal, a red or green placeholder flickering, could serve to anticipate both the moment in time in which the target was presented and its location. We hypothesized that interactive effects between endogenous spatial cueing and rhythmic orienting might be observed when spatial and temporal information is integrated using an integrated cue (Mattler, 2003).

4.1. Method

4.1.1. Participants

Fifty new undergraduate psychology students took part in the study. Of these, six participants were excluded for excessive responses to catch trials (> 40%), leaving a final sample size of 44 (mean age= 20.55 years, age range= 18–34 years, 7 males, 5 left-handed). Note that more participants were included in Experiment 3 because of course requirements and enrollment rates. As in the previous experiments, the sample size was adequate to detect significant ($\alpha = 0.05$) mean differences between two dependent means with a small/medium effect size $d = 0.43$ (Cohen, 1977) and a statistical power of 0.80.

4.1.2. Apparatus, stimuli, and procedure

The apparatus, stimuli, and procedure were the same as in Experiment 1 except for the following aspects. The fixation cross was always presented in black, whereas the color (red or green) of the placeholders predicted the target location. The placeholders turned gray at the target onset. Participants were instructed that only the color of the placeholders, but not their flickering, was useful to anticipate the target location.

4.1.3. Data analysis

After the cleaning procedure (2.13 % of rejected trials) and RT outlier removal (3.67 %), mean RTs were submitted to a 2 × 2 repeated measures ANOVA with Validity (valid, invalid) and Rhythm (regular, irregular) as within-participant factors.

4.2. Results and discussion

Again, the ANOVA showed significant main effects of Validity, $F_{(1,43)} = 12.71$, $p < 0.001$, $\eta^2p = 0.23$, and Rhythm, $F_{(1,43)} = 13.15$, $p < 0.001$, $\eta^2p = 0.23$. Participants were faster for valid (M = 331 ms) compared to invalid targets (M = 346 ms), and when targets were preceded by the regular rhythm (M = 334 ms) compared to the irregular rhythm (M = 343 ms). The Validity by Rhythm interaction was not significant, $F_{(1,43)} = 1.35$, $p = 0.25$, $\eta^2p = 0.03$ (see Figure 2). However, *post-hoc* analyses, Bonferroni corrected, showed that the effect of Rhythm was significant only at valid target locations ($p < 0.001$), but not at invalid locations ($p = 1$).

In Experiment 3, we expected to emphasize the integration of spatial and temporal attention by using an integrated cue that provided information about both the target location and the moment of target appearance. Although we did not observe a significant Validity by Rhythm interaction, additional explorative analyses revealed that rhythmic information only improved performance at valid locations. Overall, the present results hint at the possibility that temporal orienting can be spatially constrained when an integrated cue is used, in line with the neurophysiological model of spatiotemporal attention (Doherty et al., 2005; Nobre and Rohenkohl, 2014; Rohenkohl et al., 2014). In Experiment 4, we aimed to further test this hypothesis in a design similar to the one of Experiment 3 with a single rhythmic cue conveying both spatial and temporal information. Moreover, we were interested in controlling for another aspect common to the last three previous experiments. That is, even if we employed one foreperiod for both regular and irregular rhythms, one might wonder whether the use of a long duration doubling the ISI of the regular rhythm (i.e., 800 ms) might have somehow introduced some sort of temporal preparation for the target in both regular and irregular rhythm conditions, thus, mitigating general rhythmic effects. To strengthen the reliability of the rhythm in Experiment 4, we intermixed two foreperiod durations, one matching the regular rhythm (400 ms, “in time”) and one doubling it (800 ms, “out of time”) on a trial-by-trial basis. Our objective was to test whether the combination of spatial and temporal attention could be enhanced when targets appeared at the in-time critical foreperiod (400 ms) as compared to when they appeared at the longer foreperiod (800 ms).

5. Experiment 4: integrated rhythmic cue with variable foreperiod

In Experiment 4, we added an in-time critical foreperiod (400 ms) in opposition to Experiments 1–3 in which targets were only presented at a critical foreperiod that was a multiplication of the in-time rhythm (two steps of the regular rhythms-800 ms).

5.1. Method

5.1.1. Participants

Forty-nine new undergraduate psychology students took part in the study. Of these, six participants were excluded for excessive responses to catch trials (> 40%), while one was excluded for excessive missing responses (> 20%), leaving a final sample size of 42 (mean age= 20.21 years, age range= 18–35 years, 4 males, 4 left-handed). As in Experiment 3, the sample size was adequate to detect significant ($\alpha = 0.05$) mean differences between two dependent means with a small/medium effect size $d = 0.44$ (Cohen, 1977) and a statistical power of 0.80.

5.1.2. Apparatus, stimuli, and procedure

The apparatus, stimuli, and procedure were the same as in Experiment 3 except that two foreperiods of either 400 or 800 ms

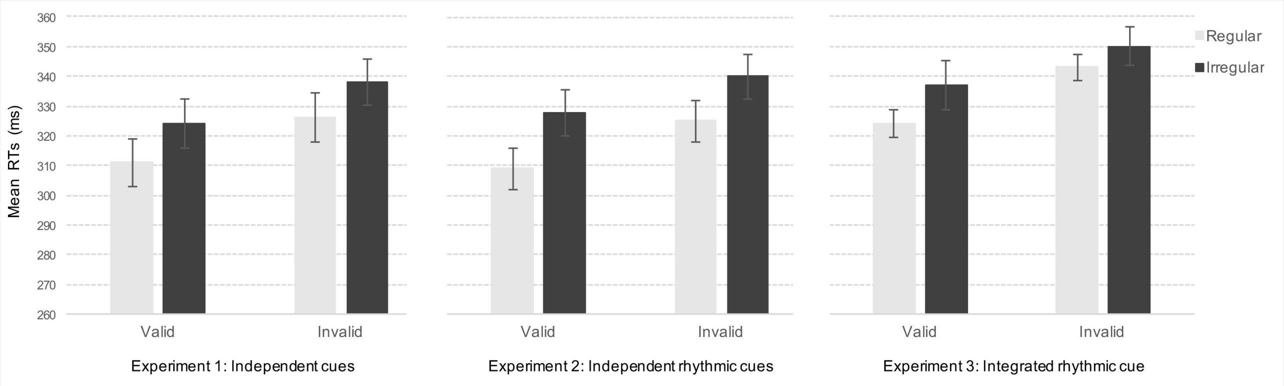

FIGURE 2
Depicts mean reaction times (RTs) in milliseconds (ms) for valid and invalid targets as a function of Rhythm (regular or irregular) in Experiments 1–3.

were used. For each rhythm condition, there were 36 valid-short foreperiod trials, 36 valid-long foreperiod trials, 12 invalid-short foreperiod trials, 12 invalid-long foreperiod trials, and 8 catch trials.

5.1.3. Data analysis

The same cleaning procedure (1.69% of rejected trials) and RT outlier removal (6.21%) were applied to the data as in Experiment 1. A 2 × 2 repeated measures ANOVA was used on mean RTs, with Validity (valid, invalid), Rhythm (regular, irregular), and Foreperiod (short, long) as within-participant factors.

5.2. Results and discussion

As predicted, the ANOVA revealed significant main effects of Validity, Rhythm, and Foreperiod [$F_{(1,41)} = 17.52$, $p < 0.001$, $\eta^2p = 0.29$, $F_{(1,41)} = 3.98$, $p = 0.053$, $\eta^2p = 0.09$, $F_{(1,41)} = 143.14$, $p < 0.001$, $\eta^2p = 0.78$, respectively]. Targets appearing at the attended location or after a regular rhythm were detected faster than targets at the unattended location or after an irregular rhythm, thus confirming efficient spatial and temporal orienting. Regarding the foreperiod, participants were faster to targets appearing after an 800 ms delay, compared to the 400 ms delay (M = 340 ms and M = 370 ms, respectively). None of the other interactions reached significance [$F_{(1,41)} = 2.06$, $p = 0.16$, $\eta^2p = 0.05$ for Rhythm x Foreperiod; Fs<1 for Validity x Foreperiod and Validity x Rhythm x Foreperiod], except for the Validity x Rhythm interaction that was marginally significant [$F_{(1,41)} = 3.97$, $p = 0.053$, $\eta^2p = 0.09$, see Figure 3]. *Post-hoc* analyses, Bonferroni corrected, revealed that participants benefited from regular rhythm only on valid trials (M = 349 ms and M = 359 ms, respectively, for regular and irregular rhythm, $p < 0.001$). At invalid locations, rhythm did not enhance target detection (M = 363 ms and M = 363 ms, respectively for regular and irregular rhythm, $p = 0.995$).

In sum, Experiment 4 showed that endogenous spatial attention and rhythm-based temporal expectations can interact under certain situations in simple detection tasks. Importantly, this interaction is independent of the foreperiod effect (no significant Rhythm x Foreperiod and Validity x Rhythm x Foreperiod interactions). Performance benefits from regular rhythms were only observed at valid locations, regardless of whether targets appeared at 400 or 800 ms after the last entrainer.

To clarify the impact of independent and integrated cues on the combination of temporal and spatial attention, we conducted a global analysis with the factor Cues manipulated between participants (Independent cues by grouping Experiments 1 and 2 *vs.* Integrated cues by grouping Experiments 3 and 4) and the factors Validity and Rhythm manipulated within participants.[1] The analysis showed a main effect of Cue [$F_{(1,148)} = 12.5$, $p < 0.001$, $\eta^2p = 0.08$], suggesting that mean RTs were slower for integrated compared to independent cues (M = 348 ms and M = 325 ms, respectively). As expected, the analysis revealed main effects of Validity and Rhythm [$F_{(1,148)} = 63.17$, $p < 0.001$, $\eta^2p = 0.29$ and $F_{(1,148)} = 74.04$, $p < 0.001$, $\eta^2p = 0.33$, respectively]. Interestingly, the Validity x Rhythm interaction was significant [$F_{(1,148)} = 5.24$, $p = 0.02$, $\eta^2p = 0.03$] and was not qualified by the Cue factor [$F_{(1,148)} = 0.98$, $p = 0.32$, $\eta^2p = 0.007$). These results revealed that the effect of Rhythm was significant at both valid and invalid locations (ps < 0.001, Bonferroni corrected *post-hoc*), but was larger at valid locations compared to invalid locations (see Figure 4). Additionally, the analysis highlighted a significant Rhythm x Cue interaction [$F_{(1,148)} = 8.37$, $p < 0.004$, $\eta^2p = 0.05$]. Bonferroni corrected *post-hoc* comparisons confirmed that although the effect of Rhythm was significant for both independent and integrated cues (ps < 0.001), the effect was larger when using independent cues. The interaction Validity x Cue was not significant [$F_{(1,148)} = 0.36$, $p = 0.55$, $\eta^2p = 0.002$]. In short, the present series of experiments reveals two main findings. First, temporal expectations based on rhythms improve target detection to a larger extent at validly attended locations (an effect that is evident when a large number of participants are analyzed in a simple detection task). Second, the use of integrated cues providing both spatial and temporal attention reduces the beneficial effect of temporal expectations based on rhythms.

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[1] As neither the interaction Validity x Foreperiod, nor the Rhythm x Foreperiod interaction reached significance (ps > 0.15), in the former analysis, we collapsed behavioral data for 400 and 800 ms in Experiment 4.

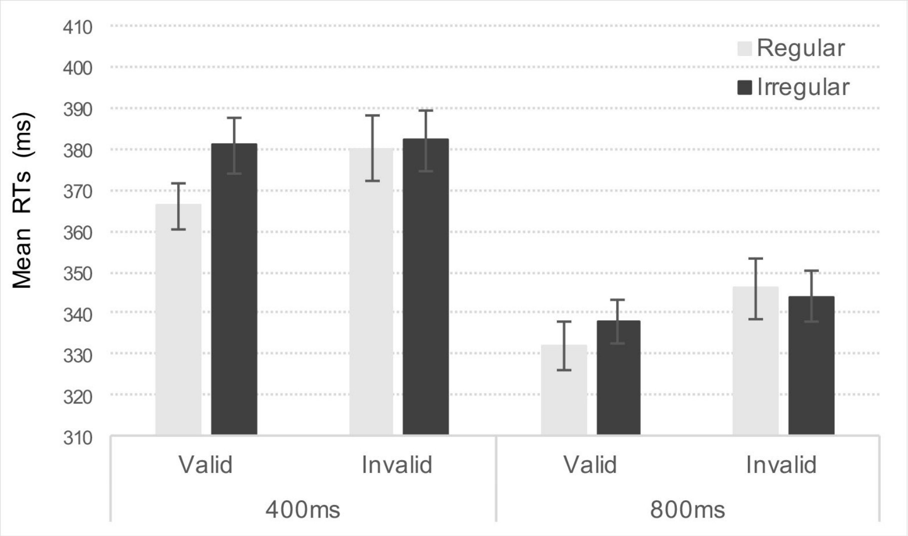

FIGURE 3
Depicts mean reaction times (RTs) in milliseconds (ms) for valid and invalid targets as a function of Rhythm (regular or irregular) for the 400 ms and 800 ms foreperiods.

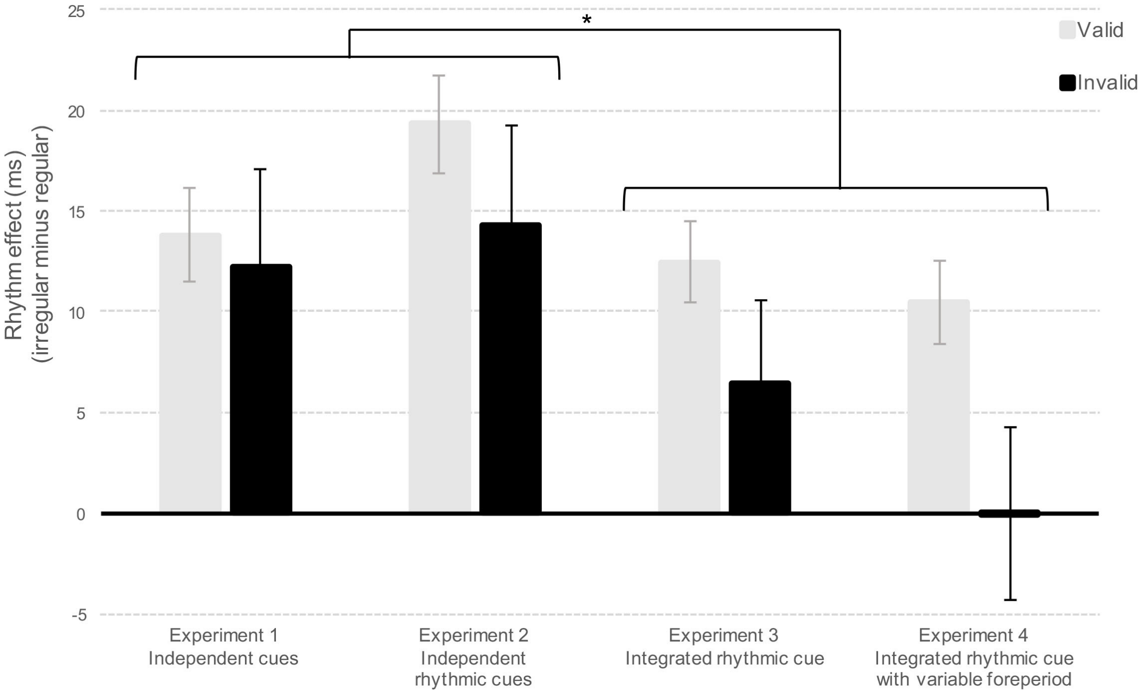

FIGURE 4
Depicts the Rhythm effect (RTs from regular minus RTs from irregular) in milliseconds (ms) for Experiments 1, 2, 3, and 4 as a function of spatially valid or invalid locations.

6. General discussion

The present study investigated the relationship between endogenous spatial attention and rhythmic temporal expectations. Across four experiments, participants detected a suprathreshold Gabor target appearing after a regular or irregular rhythm, either at the left or right side of space. In Experiments 1 and 2, attentional orienting in space and time was delivered by independent cues. The color of the fixation cross remained steady throughout the whole trial in Experiment 1, or flickered at the same pace as the rhythm in Experiment 2 (central rhythmic cue). In Experiments 3 and 4, an integrated cue (the flickering of the placeholders with colored contours, see Figure 1) was employed to orient spatial and temporal attention. Overall, target detection was enhanced when targets were preceded by the regular compared to the irregular rhythm and when presented at valid compared to invalid locations. When considering Experiments 1 and 2 separately, our data suggest independent and additive effects of endogenous spatial orienting and rhythmic temporal expectations on target detection. However, a global analysis in which the four experiments were collapsed as a function of cue type (Independent in Experiments 1 and 2; Integrated in Experiments 3 and 4) revealed two major findings. First, the data pointed to minor benefits of temporal expectations when temporal information was delivered by integrated cues (Experiments 3 and 4), as compared to independent cues (Experiments 1 and 2). Second, temporal expectations based on rhythms appeared to be stronger at valid compared to invalid locations.

6.1. Independent vs. integrated cues

Extending previous studies, our results showed that the advantage provided by regular rhythms depended on how the two expectancies were generated. When cues consisted of two clearly separable events, the effect of rhythm was maximized. However, when the same cue was used to generate both expectancies, temporal expectations were less beneficial. Our data are in line with the idea of a general phenomenon of expectancy interaction detailed in the Adjusted Expectancy model (Mattler, 2003, 2004), according to which, cues are processed in parallel when consisting of two separate pieces of information. Hence, failure to comply with one expectation does not interfere with the remaining cueing information. However, when the two pieces of information are integrated, partial noncompliance disrupts the global cueing effects. Previous studies on the impact of combined perceptual expectancies in a broader perspective demonstrated reduced or even absent spatial cueing effects on trials with an unexpected target stimulus as compared to an expected target stimulus (Klein, 1980; Lambert and Hockey, 1986; Klein and Hansen, 1987, 1990; Lambert, 1987; Kingstone, 1992). A similar result has been observed with perceptual and motor expectancies (Mattler, 2003, 2004). The Adjusted Expectancy model (Mattler, 2003, 2004) could account for the attenuated benefits afforded by rhythms with integrated, as compared to independent, cues. However, Kingstone (1992) proposed that crosstalk between expectations led to favoring the more automatic information compared to a more resource-demanding attribute. This view is hard to reconcile with our findings given that rhythm-based temporal expectations are supposed to be more automatically driven than endogenous spatial attention. One could hypothesize that the cue associated with task-relevant information, here the spatial information, is prioritized when combining expectancies. In our study, integrated cues did not impact the endogenous orienting of spatial attention which was predictive of target location, in opposition to the task-irrelevant rhythm. The integration process might depend on the task relevance of spatial and temporal information. Overall, these observations suggest a more global structural mechanism for integrating expectations, beyond spatial and temporal orienting.

6.2. Independent vs. interactive contributions of spatial and temporal attention

In the literature, there is compelling evidence that endogenous spatial orienting operates independently of temporal attention in low-demanding tasks, at least when using symbolic cues or contextual associations (i.e. distribution of foreperiods). For example, Weinbach et al. (2015) conducted a study in which participants detected targets as fast as possible. In the first two blocks, targets were preceded by a symbolic central cue that either predicted the target location or the moment of target appearance. In a third block, the cue predicted, as a function of its color and shape, both the location and the moment of target appearance. This study provided evidence for independent and additive effects of spatial and temporal endogenous forms of attention. Olk (2014) drew similar conclusions with pure symbolic cues that predicted the target location, the moment of target appearance, or both. More recently, the combination of endogenous spatial cues and temporal expectations elicited by contextual associations was also investigated (Tal-Perry and Yuval-Greenberg, 2022). In that experiment, combining endogenous spatial cues with a blocked manipulation of temporal expectations (i.e., different probability distributions of the foreperiod were used between participants) led to independent effects of spatial attention and temporal expectations (foreperiod and sequential effects). These studies demonstrate that spatial orienting is independent of temporal expectations emerging from associative or contextual information (symbolic cues, foreperiod, and sequential effects).

With respect to rhythm-based expectations, the picture is less clear-cut with studies reporting either independent or interactive effects of endogenous spatial attention and rhythmic expectations (Jones, 2015, 2019; Kizuk and Mathewson, 2017). Our results complement these observations by showing that rhythmic expectations and endogenous spatial orienting can combine interactively to foster target processing even in a simple reaction time task with low perceptual demands. The benefit afforded by regular rhythms was indeed larger in the attended region of space. Yet, this effect only emerged when temporal uncertainty was introduced by means of a variable foreperiod (Experiment 4) or when collapsing the data of the four experiments, but it still suggests that the interaction of spatial and temporal attention might occur in a low-demanding context. Temporal expectations based on rhythms facilitated target detection at both attended and unattended locations in our study, but exerted a larger effect in the attended region of space. The finding that rhythmic temporal expectations can be spatially constrained is consistent with the spatiotemporal neurophysiological model of spatiotemporal attention (Doherty et al., 2005; Nobre and Rohenkohl, 2014; Rohenkohl et al., 2014). This model assumes that temporal orienting leads to time-specific synchronization of neural populations in specific retinotopic receptive fields. The present study extends this model by showing an interactive contribution of spatiotemporal orienting to behavior, not only when temporal expectations are endogenously driven by symbolic cues but also when triggered by irrelevant synchronous rhythmic cues. Interestingly, this neurophysiological model emphasizes the role of task demands in this interaction, as temporal expectations are proposed to interact synergistically with spatial predictions to improve perceptual discrimination of visual events (Doherty et al., 2005; Rohenkohl and Nobre, 2011; Nobre and Rohenkohl, 2014; Rohenkohl et al., 2014; van Ede et al., 2020). Overall, our findings suggest that the proposal of the spatiotemporal view could likely apply to simple reaction-time tasks in well-powered experiments (see our global analysis). This observation raises questions about the role of the motor system in the combination of spatial and temporal attention. The contribution of the motor system in predictive behavior, and more specifically in temporal expectations, is not new to date and has been mainly investigated in the auditory domain (Schubotz, 2007; Morillon et al., 2014; Morillon and Baillet, 2017). Recent evidence also suggests that temporal expectations can enhance both motor preparation and perceptual discrimination as a function of task demands in the visual domain (van Ede et al., 2020). Here, we further show from a simple RT-task stressing motor preparation that the motor system could play a role in the combination of spatial and temporal attention.

Finally, with respect to our study, the interaction between spatial and temporal orienting approached significance when temporal uncertainty was introduced by using a variable foreperiod (Experiment 4). Of importance, these interactive effects were not modulated by the foreperiod, revealing that the orienting mechanisms could persist over time. Of importance, these interactive effects were not modulated by the foreperiod, thus extending previous work (Jones, 2015; Kizuk and Mathewson, 2017).

In addition to the debate of independent vs. interactive processes, further research should tackle the issue of the underlying processes responsible for interactive effects. Kizuk and Mathewson (2017) indeed reported interactive effects that are opposite to our findings and to the predictions of the neurophysiological model of spatiotemporal attention (Doherty et al., 2005; Rohenkohl and Nobre, 2011; Nobre and Rohenkohl, 2014; Rohenkohl et al., 2014). Participants localized toughly visible targets embedded in a stream of entrainers displayed at a pace of 12 Hz. By manipulating the foreperiod, targets appeared either “in-time” or “out-of-time” with the entrainers. Unexpectedly, they provided both behavioral and electrophysiological evidence for an enhancement of target processing when targets were presented at the unattended region of space (invalid with respect to a 70% validly colored cue). The study confirmed previous EEG evidence that spatial attention triggers more lateralized alpha power during the cue and target periods, and it additionally suggests that rhythmic entrainment would more easily modulate larger alpha power at the contralateral side of target expectation. As a consequence, larger benefits of temporal expectations are observed at unexpected regions of space where spatial attention is expected to inhibit alpha oscillations (Klimesch et al., 2007; Jensen and Mazaheri, 2010; Mathewson et al., 2011; Jensen et al., 2014; Kizuk and Mathewson, 2017).

Taking into consideration previous models that account for expectation combinations with various dimensions (such as space, feature, or response; Kingstone, 1992; Mattler, 2003, 2004) could help revisit these discrepancies by elaborating a more general view. Further investigation is definitely needed to draw more straightforward conclusions on the combination of spatial and temporal attention. Current theoretical frameworks stipulate that the relation between temporal orienting and spatial attention depends on perceptual task demands (van Ede et al., 2020). The present study adds further insights into this issue by revealing small but significant interactive effects in low-demanding detection tasks, thus paving the way for future investigation on the role of task demands in the relationship between spatial and rhythmic temporal attention.

Data availability statement

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

Ethics statement

The studies involving human participants were reviewed and approved by CER UPVM-n°2020-02-Université Paul Valéry Montpellier-Local Committee. The patients/participants provided their written informed consent to participate in this study.

Author contributions

PC, MC, and AC participated in the study conception and design. PC and MC programmed the experimental tasks, and PC was in charge of data collection. MC analyzed the data. PC wrote the draft manuscript. MC and AC provide substantial feedback to the draft and revised the draft critically for important intellectual content. All authors contributed to the article and approved the submitted version.

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Conflict of interest

MC and AC declared that they were editorial board members of Frontiers, at the time of submission. This had no impact on the peer review process and the final decision.

The remaining author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

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Quantifying attention span across the lifespan

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Introduction: Studies examining sustained attention abilities typically utilize metrics that quantify performance on vigilance tasks, such as response time and response time variability. However, approaches that assess the duration that an individual can maintain their attention over time are lacking.

Methods: Here we developed an objective attention span metric that quantified the maximum amount of time that a participant continuously maintained an optimal "in the zone" sustained attention state while performing a continuous performance task.

Results: In a population of 262 individuals aged 7–85, we showed that attention span was longer in young adults than in children and older adults. Furthermore, declines in attention span over time during task engagement were related to clinical symptoms of inattention in children.

Discussion: These results suggest that quantifying attention span is a unique and meaningful method of assessing sustained attention across the lifespan and in populations with inattention symptoms.

KEYWORDS

sustained attention, vigilance decrement, attention span, continuous performance task (CPT), attentional modeling

1. Introduction

The ability to maintain a stable state of attention while performing a mundane activity is often referred to as sustained attention (SA) or vigilance (Mackworth, 1948; Langner and Eickhoff, 2013; Esterman et al., 2014). SA plays a crucial role on performance in real-world situations, such as driving, academic settings, and success in the workplace (Wei et al., 2012; Divekar et al., 2013; Clayton et al., 2015). Objective metrics that quantify different aspects of SA may provide useful information for how individuals engage in daily activities (e.g., conduct on our roads, school curriculum, and workplace policy) with cognitive limitations in mind. For instance, receiving feedback about when SA wanes can help signal when a break may be beneficial.

Studies that have examined SA have historically focused on response time (RT) metrics, such as average RT and response time variability (RTV), while participants perform vigilance tasks that require continuous attention (McAvinue et al., 2012; Staub et al., 2013; Fortenbaugh et al., 2015). In addition to traditionally used RT based metrics, measures derived from signal detection theory, such as D', are commonly used to assess performance accuracy during sustained attention tasks (Fortenbaugh et al., 2015). While these metrics inform us about an individual's *overall* performance during a SA task, they do not provide information about how long one can maintain their attention *over time.* Some studies have assessed how performance in the RT metrics change over the course of a SA task by quantifying "vigilance decrements" (Parasuraman et al., 1989; Tucha et al., 2009; Langner and Eickhoff, 2013; Rosenberg et al., 2013; Wang et al., 2014). These studies have demonstrated that performance on SA tasks decline over time (Mackworth, 1948), that this worsening in task performance over time reflects cognitive fatigue (Wang et al., 2014), and that it may be exacerbated by conditions that affect attention, such as normal aging and ADHD (Parasuraman et al., 1989; Huang-Pollock et al., 2012; Langner and Eickhoff, 2013). While insightful, these types of analyses still do not quantify the amount of time that an individual is able to maintain a stable optimal attentional state, and thus do not yield a direct, objective metric of attention span (A-span)—the length of time that an individual can maintain an optimal attentional state.

Although the phrase "attention span" is commonly used by the general population to describe the ability to sustain attention, methods to objectively quantify this capacity in both research and clinical settings are largely lacking. To this end, we defined a new metric to quantify an individual's attention span (A-span): how long one is able maintain a state of optimal attention, defined as a period of high performance without response errors and consistent RTs. We specifically calculated an individual's A-span by assessing the maximum length of time that a participant was able to maintain this optimal attentional state while performing a visual continuous performance task (CPT), a commonly used vigilance task in which participants respond to frequently occurring targets and withhold responses to infrequent non-targets (Esterman et al., 2013, 2014). We also quantified vigilance decrements in A-span to examine changes in A-span over the course of the CPT ("A-span decrements").

Here, we leveraged a large dataset from children, young adults, and older adults to examine how A-span captures attention abilities. First, we compared A-span to traditional metrics of SA performance (i.e., RT and RTV) in a population of young adults. We then tested the hypothesis that A-span measures would follow an inverted-U pattern across the lifespan, such that it peaks in young adulthood and is reduced in older adults and children. Changing in a similar manner as traditional metrics would suggest that A-span metrics are sensitive to detecting age-related SA changes (McAvinue et al., 2012; Staub et al., 2013; Fortenbaugh et al., 2015). Finally, we evaluated the clinical utility of these metrics by examining if there were relationships between A-span measures and real-world symptoms of inattention in children, as indexed by the Vanderbilt ADHD Diagnostic Rating Scale (VADRS), given that SA impairments are well documented in individuals with ADHD (Huang-Pollock et al., 2006, 2012). In doing so, we assess whether A-span can serve as a unique and meaningful approach to evaluate SA abilities in separate age groups across the lifespan and in populations with attention impairments.

2. Materials and methods

2.1. Participants

We compiled CPT data from a series of studies recently performed at the UCSF Neuroscape Center by the present authors, with a total of 68 children (mean age = 9.57 +/− SD 1.62 years, range 7–13 years; 15 female, 53 male) recruited from 3 studies (Gallen et al., 2021; Mishra et al., 2021; Anguera et al., 2023), 88 young adults (mean age = 25.02 +/− SD 2.96 years, range = 19–32 years; 55 female, 33 male) recruited from 3 studies (2 of which have been published Ziegler et al., 2019; Mishra et al., 2021), and 106 older adults (mean age = 68.49 +/− SD 6.45 years, range = 56–85 years; 50 female, 56 male) recruited from 2 studies (1 of which has been published Anguera et al., 2022). See Supplementary material for more information about the studies in which the CPT data reported here were collected.

All participants had normal or corrected-to-normal vision, had no history of stroke, traumatic brain injury, or psychiatric illness (except for diagnosed ADHD), and were not taking psychotropic medication, except for 8 children who were taking stable doses of ADHD medication during their participation in the study. Additionally, older adult participants were screened for severe cognitive impairment using a Montreal Cognitive Assessment (MOCA) cutoff score of 18 (Trzepacz et al., 2015) and a composite score from a battery of neuropsychological tests (see Supplementary material for more information). All participants and their parents and/or legal guardians (for all children under the age of 16) gave informed consent to participate in the study according to procedures approved by the Committee for Human Research at the University of California San Francisco. The methods employed in this study were performed in accordance with the relevant guidelines specified in the Declaration of Helsinki.

2.2. Paradigm and stimuli

Participants from all age groups completed the same visual CPT in the same research lab at the UCSF Neuroscape Center (Figure 1A), except for 16 children who completed the same CPT using identical equipment at Cortica Healthcare's labs in Marin County. The CPT was modeled after the Test of Variables of Attention (TOVA) (Leark et al., 2007) and has been used in several previously published studies from our group (Anguera et al., 2013, 2017a,b; Ziegler et al., 2019). The CPT was programmed in Presentation (http://neurobs.com) and the stimuli consisted of light gray squares that appeared on a black background at either the top or bottom half of the computer screen (see Figure 1A). Participants were instructed to respond to target stimuli (squares at the top half of the screen) with the spacebar and to withhold responses to non-target stimuli (squares at the bottom half of the screen). Each stimulus remained on the screen for 100 milliseconds, with a 1,400 millisecond inter-trial-interval. The CPT consisted of two conditions: The first condition had infrequent target stimuli (a 1:4 target to non-target ratio), while the second condition had frequent target stimuli (a 4:1 target to non-target ratio). For our analyses here, we only analyzed the condition with frequent targets to maximize the number of trials with correct (target) RT values, which are required for a precise A-span measurement. In this CPT condition, participants completed 2 blocks that each contained 125 total trials (100 targets and 25 non-targets) per block. The blocks were separated by a brief break in the task. The break was included to maintain consistency with the TOVA. Across the entire CPT condition, there were a total of 200 targets and 50 non-targets and took 6 min and 15 seconds to complete.

2.3. Computing traditional attention metrics

We computed traditional SA metrics, average RT and RTV (the standard deviation of RTs), for all correct responses to target stimuli across the entire CPT. RTs that were faster than 150 msec were excluded from the traditional metric computations, as this is often considered too fast for accurate perceptual discrimination and thus likely reflects a more error-prone state (Leark et al., 2007). We also computed RT and RTV in each of the 2 blocks separately to examine vigilance decrements (defined as the percent change in RT and RTV from the first to the second block).

2.4. Computing A-span

We computed the novel A-span metric using custom MATLAB code that built upon an approach commonly used in the literature to quantify moment-to-moment fluctuations of attention (Esterman et al., 2013, 2014; Kucyi et al., 2017). This approach characterizes when a participant is "in the zone" or "out of the zone" (defined below) using trial wise accuracy and RT (Figure 1B). Here, we extended this approach to characterize an individual's A-span by computing the maximum amount of time that a participant was able to maintain an "in the zone" state without deviating to an "out of the zone" state.

To quantify A-span, we first *z*-scored the correct RTs at the single participant level. Any correct RT that fluctuated around the average RT and was faster than 1 *z*-score above an individual's average RT was characterized as an "in the zone" trial. RTs that were slower than 1 *z*-score were characterized as "out of the zone" trials. Trials when the participant made an error were characterized as "error trials". RTs that were faster than 150 msec were also characterized as "error trials", since this is considered to be too fast for accurate perceptual decision making (Leark et al., 2007). All "error trials" were categorized as contributing to the participant being not "in the zone", as incorrect responses in CPTs reflect a drift of attention away from the task (Robertson et al., 1997; Smallwood and Schooler, 2006; Esterman et al., 2013). Additionally, if a stretch of "in the zone" trials was punctuated by the break between blocks, we considered that as the end of the "in the zone" segment because the absence of task demands during the break meant that they were no longer in an optimal task-engaged state. We next computed the maximum amount of time (in seconds) that a participant was able to maintain an "in the zone" optimal attentional state (spanning at least 2 consecutive trials). We refer to this duration of time throughout this manuscript as "A-span". Though it was not examined in the present study, the average amount of time that a participant can stay "in the zone" (i.e., average A-span) may also be a meaningful approach of measuring A-span (see Supplementary material for more information). As with th traditional attention metrics, we computed these A-span metrics across the entire CPT. We also examined vigilance decrements in A-span (percent A-span change between the first and second task blocks). Additional details regarding the A-span calculations can be found in Supplementary material. We then examined whether this new metric was distinct from traditional SA metrics (e.g., RT and RTV). Further, we asked how these A-span metrics differed across age groups and how they were related to symptoms of inattention in children.

2.5. Characterizing inattention symptoms in children

For 44 of the 68 children, we also collected parent ratings of inattention in the real world using the Vanderbilt ADHD Diagnostic Rating Scale (VADRS-IA). ADHD symptoms were assessed using 18 questions that probed the frequency that the child displays various ADHD symptoms, with questions 1–9 assessing inattentive symptoms and questions 10–18 assessing hyperactive/impulsive symptoms. Parents rated each symptom on a scale of 0 ("Never") to 3 ("Very Often"). Given our interest in SA, we focused on relating the inattentive symptoms (questions 1–9) to A-span performance metrics. Therefore, we correlated our A-span metrics with the number of positive responses (a 2 "Often" or 3 "Very Often") on the 9 questions that probe inattention symptoms (Wolraich et al., 2003). Of the 8 children in this study who were taking ADHD medication at the time of data collection, only 1 of them provided VADRS-IA data. Therefore, we did not control for medication status during this analysis.

2.6. Statistical analysis

All statistical analyses were conducted in IBM's SPSS Statistics 20 software. First, we examined A-span metrics within each age group independently. We assessed whether there were significant A-span decrements across the CPT (i.e., if the percent change scores significantly differed from 0) using Wilcoxon signed rank tests. We chose to use this non-parametric approach to reduce the influence from potential extreme values. Since the Wilcoxon signed rank test compares our sample median against a hypothetical median, we highlighted the median percent change scores when reporting A-span decrements in each age group.

We then evaluated relationships between traditional and A-span metrics by conducting Spearman correlations between these metrics in young adults only. We chose to use Spearman correlations to reduce the influence that potential extreme values had on the correlations (Akoglu, 2018). Additionally, Bayesian non-parametric correlations were conducted to test the independence between A-span and traditional metrics.

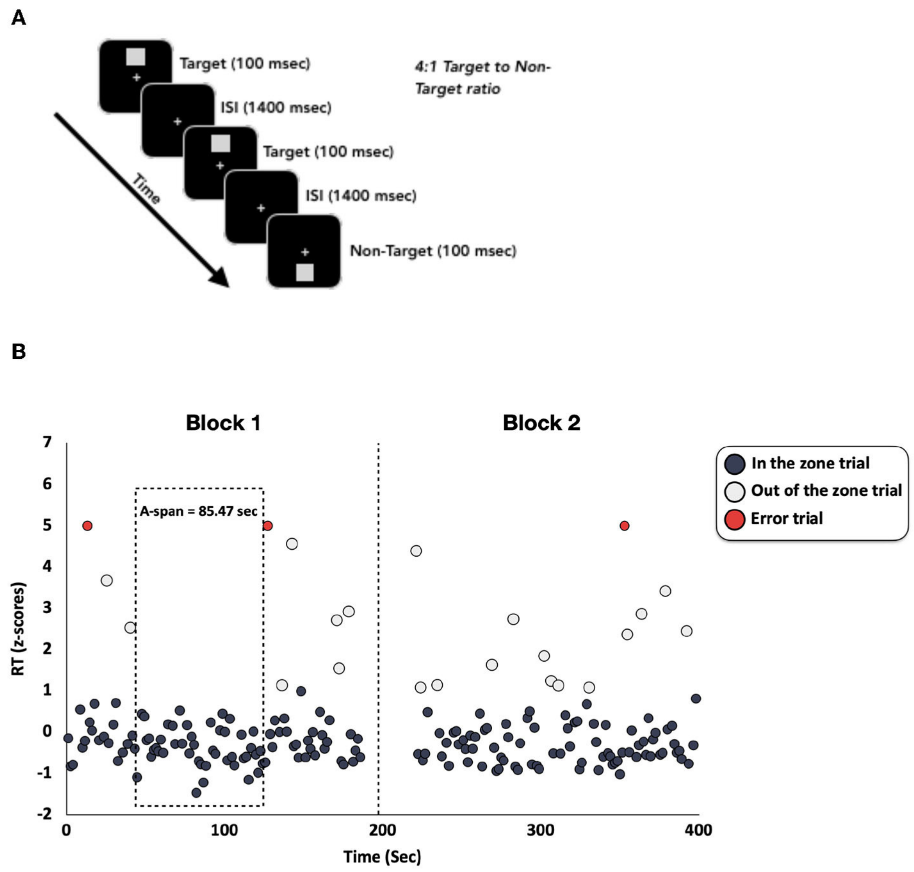

FIGURE 1
(A) Stimuli and protocol for the CPT. There were a total of 250 trials, with 80% targets and 20% randomly occurring non-targets. **(B)** Z-scored RTs from an example participant. Each RT was z-scored and plotted over time. RTs that are faster than 1 z-score above the mean are plotted in dark gray and are labeled as "in the zone" trials. RTs slower than 1 z-score above the mean are plotted in light gray and are labeled as "out of the zone" trials. Trials in which there was an error were plotted in red and were labeled as "error trials". The dashed vertical line represents the break between the first and second CPT blocks. The dotted box highlights the longest period during the CPT when this participant was able to maintain an "in the zone" state (i.e., their A-span).

To examine age group differences on A-span and traditional metrics, we conducted one-way ANOVAs on each metric with a between-subjects factor of age group (children, young adults, and older adults). We followed these analyses with an interrogation of pairwise differences between age groups with independent samples *t*-tests (see Supplementary material).

Finally, to evaluate the clinical utility of A-span metrics in children, we examined the relationship between these metrics and clinically-used inattention symptoms, as indexed by the number of positive responses to the VADRS-IA that these children displayed, using Spearman correlations. To determine if the relationships between attention span metrics and inattention symptoms were stronger than the relationships between traditional metrics and inattention symptoms, we converted Spearman correlation coefficients to Pearson correlation coefficients (Myers and Sirois, 2004), and then formally compared the correlation coefficients (Pearson and Filon, 1898; Diedenhofen and Musch, 2015). For each set of analyses where we ran multiple statistical tests (e.g., correlations between inattentive symptoms and both A-span metrics), we corrected *p*-values using an FDR correction for multiple comparisons and used a two-tailed significance threshold of $p < 0.05$.

3. Results

3.1. Characterizing A-span across the lifespan

We began by calculating and characterizing the new A-span metrics in each age group separately (Table 1). We found that children had an A-span of 29.61 seconds, which declined significantly (−27.41%) over the course of the CPT ($Z = 687.00$, $p = 0.003$). Young adults had an A-span of 76.24 seconds, which did not decline significantly (−2.54%) over the course of the CPT ($Z = 2{,}193.00$, $p = 0.328$). Finally, the older adults had an A-span of 67.01 seconds, which also did not decline significantly (−8.40%) over the course of the CPT ($Z = 2{,}672.00$, $p = 0.606$). Although the median A-span percent change was negative in each of the age groups, there were several participants who experienced very large increases in A-span (>100%) throughout the CPT. Most of these

TABLE 1 Descriptive statistics of A-span and A-span percent change for each age group.

		Children	Young adults	Older adults
A-span	Mean	29.61 sec	76.24 sec	67.01 sec
	Median	27.37 sec	72.17 sec	59.31 sec
	Stdev	13.86 sec	30.55 sec	28.28 sec
	Range	8.88–77.92 sec	27.12–189.74 sec	25.95–186.98 sec
A-span % change	Mean	−12.55%	20.02%	4.88%
	Median	**-27.41%**	**-2.54%**	**-8.40%**
	Stdev	46.73%	78.09%	61.62%
	Range	−68.45–160.25%	−76.21–346.08%	−67.20–299.38%
	p-value	0.003**	0.328	0.606

The row indicating "*p* value" reflects results from the Wilcoxon signed rank tests assessing if A-span percent change significantly differed from 0. $^{**}p < 0.01$.

participants were young adults ($n = 15$ out of 88), while fewer were older adults ($n = 7$ out of 106), and the fewest were children ($n = 2$ out of 68).

3.2. Determining the uniqueness of A-span and A-span decrements in young adults

We then assessed the relationships between A-span and traditional SA metrics in a population of young adults to determine the uniqueness of the new A-span metrics. We found that A-span was not correlated with RT or RTV [Figure 2A; RT: $rho_{(88)} = -0.13$, $p_{FDR} = 0.711$, $\text{BF}_{01} = 3.46$; Figure 2B; RTV: $rho_{(88)} = 0.06$, $p_{FDR} = 0.711$, $\text{BF}_{01} = 6.39$]. Similarly, A-span percent change was not correlated with either RT or RTV percent change [Figure 2C; RT percent change: $rho_{(88)} = 0.06$, $p_{FDR} = 0.711$, $\text{BF}_{01} = 6.32$; Figure 2D; RTV percent change: $rho_{(88)} = 0.04$, $p_{FDR} = 0.711$, $\text{BF}_{01} = 6.96$]. Together, these findings suggest that A-span and A-span decrement metrics may be distinct from traditional metrics and their vigilance decrements.

3.3. Age group effects on A-span metrics

We then examined changes in A-Span across the three age groups to assess whether A-span metrics follow similar patterns of SA change across the lifespan as reported elsewhere (McAvinue et al., 2012; Staub et al., 2013; Fortenbaugh et al., 2015). We specifically examined age group effects for all CPT metrics, as well as for vigilance decrements in each metric from the first to second block of the task.

3.3.1. A-span

First, we examined whether there were age group differences in A-span. A one-way ANOVA revealed a significant age group effect for A-span [Figure 3A; $F_{(2,259)} = 66.32$, $p < 0.001$, $\eta^2 = 0.34$], such that young adults had longer A-spans than children and older adults. See Table 2 for details on pairwise comparisons between age groups. The age group effect on A-span was nearly identical when excluding children who were taking ADHD medication at the time of data collection [$F_{(2,251)} = 66.23$, $p < 0.001$, $\eta^2 = 0.34$]. Additionally, the age group effect on A-span was similar when using an ANCOVA that used a type III sum of squares to control for differences in sample size between age groups while also setting the study in which the data were originally collected as a covariate [$\text{F}_{(2,262)} = 33.96$, $p < 0.001$, $\eta^2 = 0.21$].

3.3.2. Traditional metrics

Next, we confirmed that the traditional metrics (RT and RTV) also showed this expected pattern of changes across the lifespan (McAvinue et al., 2012; Staub et al., 2013; Fortenbaugh et al., 2015). One-way ANOVAs with a between-subjects factor of age group (children, young adults, and older adults) showed that there was a significant age group effect for RT [Supplementary Figure 3a; $F_{(2,259)} = 110.30$, $p < 0.001$, $\eta^2 = 0.46$] and RTV [Supplementary Figure 3b; $F_{(2,259)} = 264.03$, $p < 0.001$, $\eta^2 = 0.67$]. Similar to A-span, young adults had lower RT and RTV than children and older adults. See Supplementary material for statistics on pairwise comparisons between age groups. The similarities between the way that A-span and traditional metrics differ across age groups suggest that they may reflect distinct attentional processes that similarly fluctuate during development and aging.

3.3.3. Decrements in A-span

We then examined whether A-span decrements followed this pattern of age group differences. A one-way ANOVA revealed a significant age group effect for A-span decrements, as indexed by A-span percent change [Figure 3B; $F_{(2,259)} = 4.91$, $p = 0.008$, $\eta^2 = 0.04$]. Young adults experienced smaller A-span decrements than children but had similar A-span decrements as older adults. See Table 2 for details on pairwise comparisons between age groups. The age group effect on A-span percent change was similar when excluding children who were taking ADHD medication at the time of data collection [$F_{(2,251)} = 6.27$, $p = 0.002$, $\eta^2 = 0.05$]. Additionally, the age group effect on A-span percent change was similar when using an ANCOVA that used a type III sum of squares to control for differences in sample size between age groups while also setting the study in which the data were originally collected as a covariate [$\text{F}_{(2,262)} = 3.79$, $\text{p} = 0.024$, $\eta^2 = 0.03$].

3.3.4. Decrements in traditional metrics

Next, we confirmed that vigilance decrements over time in traditional metrics followed the pattern of expected changes across the lifespan as previously reported (Parasuraman et al., 1989; Langner and Eickhoff, 2013). One-way ANOVAs with a between-subjects factor of age group (children, young adults, and older adults) showed that there was a significant age group effect for RT percent change from first to second block of the task [Supplementary Figure 3c; $F_{(2,259)} = 9.38$, $p < 0.001$, $\eta^2 =$

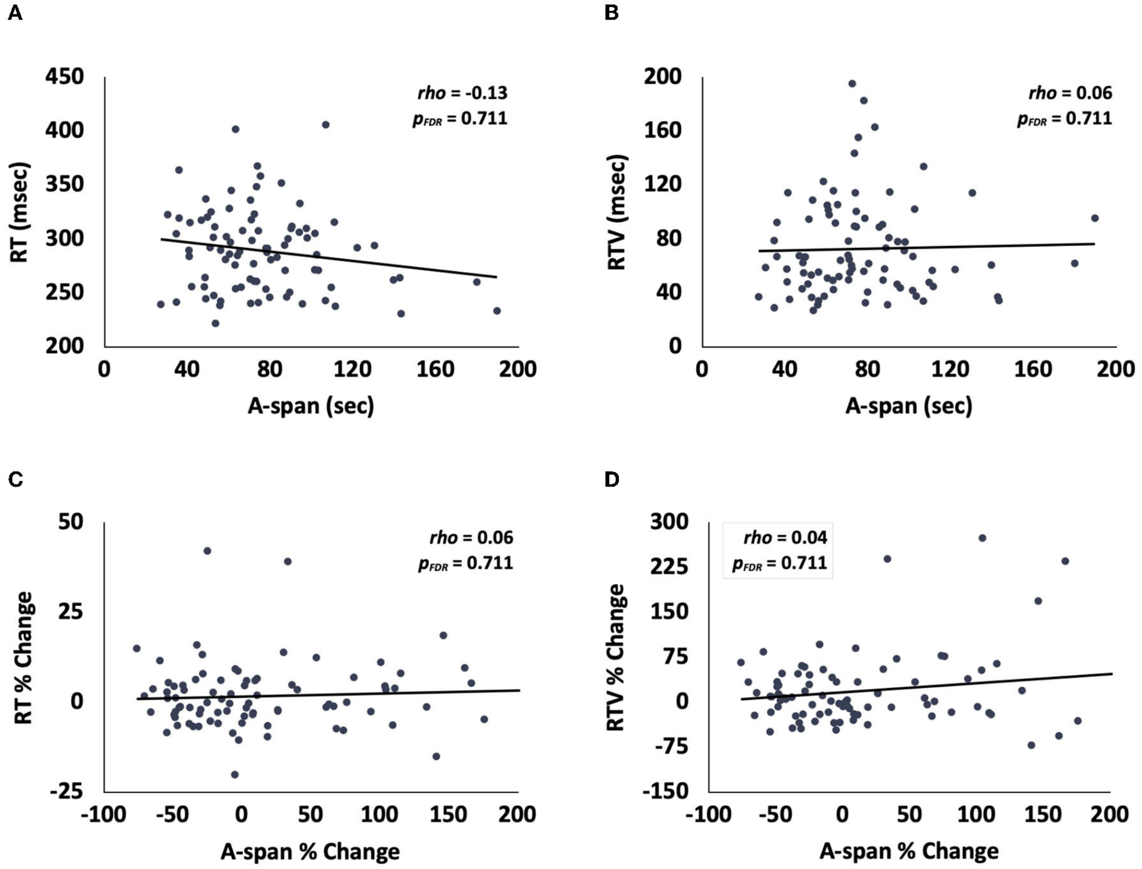

FIGURE 2
Scatterplots showing that, in young adults, **(A)** A-span was unrelated to RT and **(B)** RTV, and that A-span percent change was unrelated to **(C)** RT percent change and **(D)** RTV percent change.

0.07]. Young adults had smaller RT percent changes (i.e., more stable performance throughout the entire CPT) than children but had similar RT percent changes as older adults. Unexpectedly, however, there was no effect of age for RTV percent change [Supplementary Figure 3d; $F_{(2,259)} = 1.37$, $p = 0.257$, $\eta^2 = 0.01$]. See Supplementary material for statistics on pairwise comparisons between age groups. Like the metrics computed across the entire task, the similarities between the way that decrements in A-span and traditional metrics differ across age groups suggest that they may reflect distinct attentional processes that similarly fluctuate during development and aging.

3.4. Relationship between inattention symptoms and A-span decrements in children

We then assessed the potential clinical utility of A-span measurements by examining whether A-span metrics were related to real-world symptoms of inattention in children. We subsequently followed these analyses by testing for similar relationships between traditional metrics and inattention symptoms, to determine if the children included here exhibit similar SA deficits as reported elsewhere (Huang-Pollock et al., 2006, 2012).

3.4.1. A-span metrics

We interrogated the relationships between each A-span metric and the number of inattention symptoms reported on the VADRS questionnaire. We found that the vigilance decrement in A-span was negatively related to ADHD-inattentive symptoms in children (i.e., a more negative A-span percent change was related to having more inattention symptoms) (Wolraich et al., 2003) [Figure 4B; $rho_{(44)} = -0.34$, $p_{FDR} = 0.044$]. However, there was no relationship between A-span (i.e., across the entire task) and inattention symptoms [Figure 4A; $rho_{(44)} = 0.15$, $p_{FDR} = 0.317$].

3.4.2. Traditional metrics

Next, we sought to confirm that the traditional metrics showed similar relationships with inattention symptoms as documented elsewhere (McAvinue et al., 2012; Staub et al., 2013; Fortenbaugh et al., 2015). Interestingly, there was no relationship between any of the traditional metrics and inattention symptoms [Supplementary Figure 4a; RT: $rho_{(44)} = 0.19$, $p_{FDR} = 0.603$; Supplementary Figure 4b; RTV: $rho_{(44)} = 0.05$, $p_{FDR} = 0.766$;

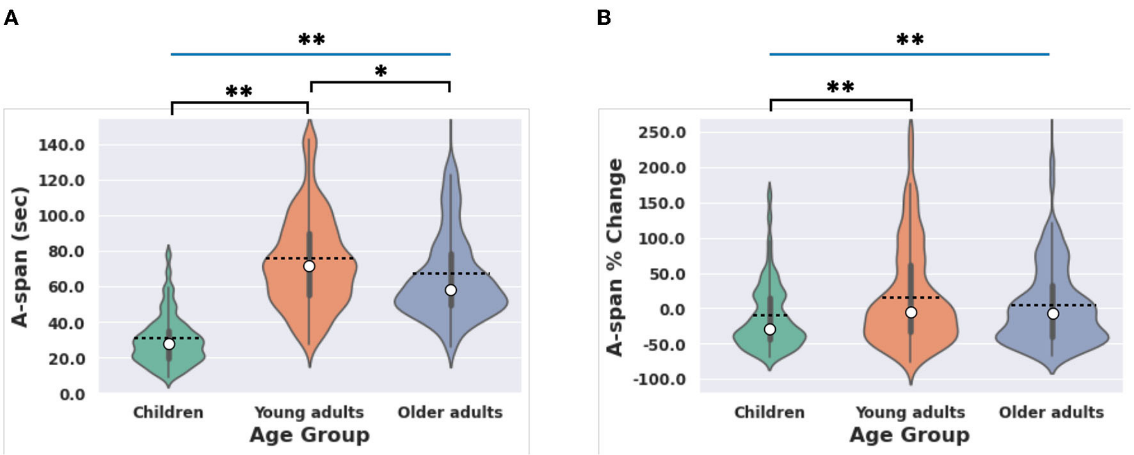

FIGURE 3
Age group effects on A-span metrics. **(A)** Age effects on A-span were driven by children and older adults having shorter A-spans than young adults. **(B)** Age effects on A-span percent change were driven by children having greater A-span decrements (i.e., a more negative A-span percent change) than young adults. Box and whisker plots represent the bounds of each quartile. Dashed lines represent the group average. White dots represent the group median. Blue significance bars indicate significant interactions revealed from the ANOVAs and black significance bars indicate significant *t*-test results. $^*p < 0.05$, $^{**}p < 0.01$.

TABLE 2 Pairwise comparisons of A-span measures comparing young adults to children and older adults separately.

		Young adults vs. children	Young adults vs. older adults
A-span	*t*-statistic	$t_{(127.77)} = -12.72$	$t_{(192)} = 2.18$
	Cohen's *d*	$d = -1.89$	$d = 0.32$
	p-value	$p < 0.001^{**}$	$p < 0.030^{*}$
A-span % Change	*t*-statistic	$t_{(145.69)} = -3.23$	$t_{(163.92)} = 1.48$
	Cohen's *d*	$d = -0.49$	$d = 0.22$
	p-value	$p = 0.002^{**}$	$p = 0.142$

$^*p < 0.05$, $^{**}p < 0.01$.

Supplementary Figure 4c; RT percent change: $rho_{(44)} = 0.12$, $p_{FDR} = 0.603$; Supplementary Figure 4d; RTV percent change: $rho_{(44)} = 0.15$, $p_{FDR} = 0.603$].

3.5. Inattention symptoms are more closely related to A-span percent change than traditional metrics

In an exploratory analysis, we sought to determine if the relationship between A-span percent change and inattention symptoms was significantly stronger than the relationships between traditional metrics and inattention symptoms. We found that the correlation between A-span percent change and inattention symptoms was significantly stronger than that for each of the traditional metrics and inattention symptoms (RT: $z = -2.77$, $p = 0.006$; RTV: $z = -1.98$, $p = 0.047$; RT % change: $z = -2.11$, $p = 0.035$; RTV % change: $z = -2.43$, $p = 0.015$).

4. Discussion

Here, we report a method of quantifying attention span by calculating the maximum amount of time that a participant was able to maintain an "in the zone" high performance state while performing a CPT. Our approach revealed that children had an A-span of 29.61 seconds, young adults had an A-span of 76.24 seconds, and older adults had an A-span of 67.01 seconds. Furthermore, A-span decrements were most pronounced in children, who experienced an A-span decline of −27.41% over the course of the CPT, while young and older adults experienced non-significant A-span decrements (−2.54 and −8.40%, respectively). A-span decrements were also sensitive to detecting inattention symptoms in children. The results we report here suggest that our approach of quantifying A-span is a unique and meaningful method of assessing SA abilities in separate age groups across the lifespan and in clinical populations.

4.1. A-span fluctuations across the lifespan

Although A-span performance followed previously seen patterns of change across the lifespan as the traditional metrics, A-span metrics were uncorrelated with traditional metrics in young adults. Bayesian analysis also provided evidence that A-span was independent from traditional metrics, suggesting that they may reflect distinct attentional processes. These findings are likely the result of two possible scenarios. First, A-span and traditional metrics may reflect different aspects of a common, more general, set of SA processes that change with development and aging. Second, these metrics may reflect distinct, unrelated cognitive processes that both happen to increase during development and decline during aging. Future work is warranted to address this question by identifying the neural activity profiles that facilitate A-span

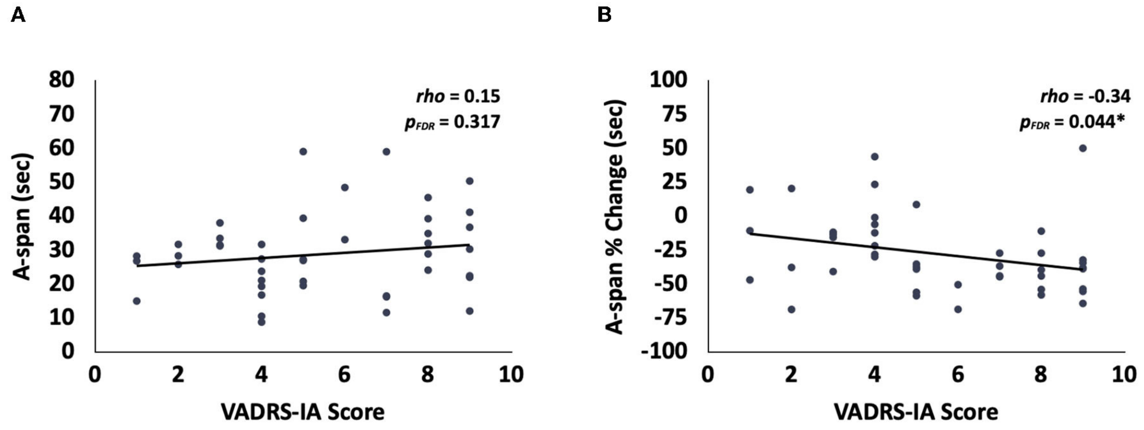

FIGURE 4
Relationships between A-span measures and inattention in children. **(A)** There was no significant relationship between the VADRS-IA score and A-span. **(B)** There was a significant relationship between the VADRS-IA score and the A-span % change. $^*p < 0.05$.

maintenance, as this type of interrogation would identify the similarities and differences between the neural correlates of A-span and traditional SA metrics, thereby enhancing our understanding of these cognitive processes.

Unexpectedly, we did not see any effects of age group on RTV vigilance decrements. Although many studies have shown that SA and vigilance decrements change across the lifespan (Parasuraman et al., 1989; McAvinue et al., 2012; Langner and Eickhoff, 2013; Staub et al., 2013; Fortenbaugh et al., 2015), there have been studies that have reported no SA changes with aging (Carriere et al., 2010). Thus, our results suggest that A-span might be more sensitive to detecting age-related vigilance decrements than RTV.

4.2. Clinical relevance of A-span

Importantly, we also observed that A-span percent change was related to inattentive symptoms in children, while traditional metrics were not. Further, the relationship with A-span percent change was significantly stronger than the correlations with traditional metrics. While declines in traditional metrics are well documented in individuals with ADHD (Huang-Pollock et al., 2006, 2012), null reports of SA deficits in ADHD populations do exist (Corkum and Siegel, 1993; Tucha et al., 2009). This inconsistency in the literature could be influenced by the heterogeneity of cognitive deficits in ADHD. Alternatively, traditional metrics may be too coarse to reveal group differences in a population with known elevated levels of performance variability (Huang-Pollock et al., 2012). It has been suggested that more granular approaches, such as vigilance decrements (Huang-Pollock et al., 2012), for assessing attention deficits in ADHD populations may be useful for better understanding how SA is impacted in ADHD. This new approach of A-span assessment may be a useful approach for assessing SA in ADHD given that it reflects how long an individual can hold their attention in an optimal state, and how this changes with time on task. However, these results should be interpreted with an abundance of caution. Future work should rigorously examine the reliability of using A-span measurements to detect inattention symptoms (Hedge et al., 2020).

Although we saw effects of age on A-span decrements, only children displayed significant A-span decrements over the course of the CPT (see Table 1). This finding highlights how children are poorer at maintaining stable attention over time relative to adults, and is even more intriguing when considering that A-span decrements in this age group are associated with symptoms of inattention. Together, these results suggest that A-span stability is sensitive to development, and impairments in an individual's ability to maintain a stable A-span over time could be an important marker of attention impairments.

4.3. A-span as a new approach for assessing attention over time

Although traditional metrics that assess CPT performance are useful for detecting overall SA abilities, they do not directly quantify the ability to maintain uninterrupted attention over a sustained period of performance (Huang-Pollock et al., 2012). An individual's average RT during a CPT could be fast because their psychomotor speed was fast while they were in an attentive state, but they could have had frequent lapses in attention that were not detected when computing an average RT across the whole CPT. Our finding that RT was uncorrelated with A-span in young adults supports this notion. Contrasting the neural correlates of A-span with what is known about the neural processes that underlie SA could further highlight how A-span differs from traditional metrics (Rosenberg et al., 2016; Helfrich et al., 2018). Many researchers have leveraged vigilance decrements to assess the extent of attentional decline over time (Parasuraman et al., 1989; Tucha et al., 2009; Langner and Eickhoff, 2013; Rosenberg et al., 2013; Wang et al., 2014). While this work has illuminated how performance in traditional metrics change over the course of a task, it has not helped researchers understand how the amount of time that an individual is able to maintain a stable optimal attentional state is relevant. Our new

A-span metric achieves this while also providing an approach to quantify an ability that is seemingly intuitively understood amongst the general public.

When considering A-span as a measure of interest, researchers should consider the type of tasks that are aligned with its use. In general, CPTs, such as the SART, TOVA, and gradCPT (Leark et al., 2007; Carriere et al., 2010; Esterman et al., 2013, 2014), which have been used to assess metrics of SA, are likely to yield meaningful A-span measurements. These types of paradigms that sample a participant's focus frequently (i.e., ones that require frequent responses) are more likely to capture brief fluctuations in attention, and thus will yield more precise A-span metrics. However, these tasks may index SA differently. Further research is necessary for determining which SA tasks are best suited for measuring A-span. Investigators should use caution when calculating A-span from more complex cognitive tasks (e.g., working memory, decision making, and interference resolution tasks). Longer RTs and errors in these types of tasks may not reflect attentional lapses, but instead may stem from other difficulties in cognitive processing, such as reaching working memory capacity limits or when there is uncertainty during complex decision making. Therefore, measuring A-span during a more challenging task might not purely reflect how long an individual can stay in an optimal SA state. Additionally, the task duration is an important factor to take into consideration when computing A-span. The CPT employed in this study was relatively short. A longer CPT may yield A-span measurements that reflect SA abilities differently. Utilizing CPTs that require less frequent responses may also provide meaningful, and potentially distinct, A-span calculations. However, since these types of CPTs have fewer trials, they will likely need to be longer than the task used in this study to obtain a precise A-span.

4.4. Future directions

Interrogating the similarities and differences in the neural processes underpinning A-span and traditional metrics is a potentially exciting future avenue of research. Several fMRI studies have implicated several widespread brain networks, including the default mode, salience, and dorsal attention networks, in maintaining "in the zone" attentional states (Esterman et al., 2013, 2014; Kucyi et al., 2017). Thus, these networks likely play a role in A-span maintenance. Additionally, incorporating recently developed neuroimaging analysis methods that are sensitive to detecting neural dysfunctions related to inattention into A-span studies can further illuminate how A-span is impacted by inattention (Cai et al., 2021). Ultimately, reaching a better understanding of how A-span decrements might be related to inattention could lead to better characterization of ADHD subtypes, and enhanced treatment personalization and efficacy (Leikauf et al., 2017; Griffiths et al., 2021).

Understanding how different task parameters contribute to A-span measurements is an important extension of this research. As described previously, future research should seek to identify whether longer tasks capture more meaningful A-span fluctuations than the A-span % change reported in this study. Establishing the minimum task length that can be used for calculating A-span is also an important avenue of future work. Finally, identifying the effects that taking a short break between blocks has on A-span decrements may illuminate how vigilance decrements may be mitigated or exacerbated.

4.5. Limitations

There are a few noteworthy limitations in this study. First, although we showed that a relatively short CPT (only 6 min and 15 sec in total) can yield meaningful A-span metrics, the optimal length of a CPT for measuring A-span (and decrements) remains to be determined. Computing A-span over longer periods in future work will allow us to understand more precisely how the rate and magnitude of A-span decrements might signify the presence of attention impairments. It is possible that some individuals who have short A-spans when measured on timescales of 5–10 min can maintain high task performance for several hours (or vice versa). Interestingly, some individuals experienced an increase in A-span with time on task. On the surface, this seems to contradict theoretical models of SA, such as the resources depletion theory (Esterman and Rothlein, 2019). A longer task might reveal that the amount of time it takes for an individual to reach their maximum A-span provides meaningful information regarding sustained attention abilities. Furthermore, it might reveal that the individuals who initially experienced large increases in A-span over time eventually show A-span decrements, thus capturing a "warm-up" period that has been reported in the SA literature (Kamza et al., 2019). It could also explain the disproportional distribution of these individuals across age groups that we observed here. Based on the present findings, future work examining individual differences in A-span dynamics over longer timescales is warranted to better understanding the utility of this metric in different scenarios. Ultimately, doing so could facilitate the use of A-span in real-world settings. Closed-loop systems can interpret shortening A-spans as an indication of a need to take a rest, or lengthening A-spans as a sign that an individual has yet to reach their maximum A-span.

Although we found evidence that A-span is unique from traditional measures, there are likely some individuals whose A-spans are affected by their RTV. For instance, an individual with frequent attentional lapses (i.e., slower responses) will likely have a shorter A-span than an individual with infrequent, but large lapses (i.e., several consecutive very slow responses), even though they may have similar RTV values. Understanding how the temporal distribution of variable responses impacts A-span measurements is a topic that future studies should examine more thoroughly. Moreover, the result that A-span is independent from traditional metrics should be interpreted with caution and replicated before concluding that A-span is truly measuring a unique aspect of SA that is not captured by traditional metrics.

Additionally, although we analyzed data from participants from a wide age range, we did not have any participants between the ages of 14–18 and 33–55. Therefore, it remains unknown how A-span and A-span decrements change during adolescence and middle adulthood. Finally, the present study did not examine the relative contribution of state (i.e., mood, fatigue, and stress) to A-span measurements. Future studies should seek to disentangle state vs. trait impacts on A-span.

5. Conclusion

Here, we demonstrated that A-span is a unique and meaningful index of SA abilities that differs between age groups across the lifespan, and that A-span decrements are related to clinical inattention symptoms in children. Our work suggests that A-span is a promising new approach for characterizing SA performance at the behavioral level, and should be further utilized when examining the effects of development and aging on SA abilities, and in clinical conditions that impact cognition.

Data availability statement

The data analyzed in this study is subject to the following licenses/restrictions: The data were compiled from a series of recent studies conducted by the present authors. The data used to generate A-span measurements reported in this paper is available from the corresponding authors upon reasonable request. Requests to access these datasets should be directed to adam.gazzaley@ucsf.edu.

Ethics statement

One of the studies that provided data for the current study was approved WIRB Copernicus Group (Gallen et al., 2021). The Committee for Human Research at the University of California San Francisco approved the other studies that provided data for the current study (Ziegler et al., 2019; Mishra et al., 2021; Anguera et al., 2022). Written informed consent to participate in this study was provided by the participants' legal guardian/next of kin.

Author contributions

Conceptualization and writing—original draft preparation: AS, CG, DZ, JA, and AG. Methodology and formal analysis: AS. Investigation and writing—review and editing: AS, CG, DZ, JM, EM, JA, and AG. All authors contributed to the article and approved the submitted version.

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Conflict of interest

AG is co-founder, shareholder, BOD member, and advisor for Akili Interactive Labs, a company that produces therapeutic video games.

The remaining authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

Publisher's note

Supplementary material

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/fcogn.2023.1207428/full#supplementary-material

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Aligning top-down and voluntary attention control across individuals

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Previous research has suggested that standard manipulations of top-down information in the spatial cueing paradigm do not elicit voluntary attention control across all participants. Instead, this research showed that only about half of the individuals exhibited the expected pattern in which they reported feeling more agency when they performed visual search with the aid of an informative (arrow or onset) cue than when they performed this task with an uninformative cue or without any cue at all. The present study replicated these previous findings under conditions in which the standard manipulation of top-down information was conveyed by a number cue (as opposed to an arrow or onset cue). But more importantly, the present study also found that the proportion of individuals who aligned top-down and voluntary attention control could be increased to approximately 90% by combining the standard manipulation of top-down information with a novel manipulation of volition in a separate condition in which participants were given the opportunity to freely choose (or not) the direction of the spatial cue on each trial. Despite conceding experimental control of cued direction (but not cue validity) to participants in this latter condition, most participants (85%) nevertheless distributed their direction choices equally across the four directions. These findings suggest that providing participants active control of stimulus parameters may be required to elicit a strong sense of agency (and voluntary control) in the laboratory.

KEYWORDS

visual search, top-down attention control, voluntary attention control, agency, individual differences, agency during visual search

Introduction

Top-down attention control occurs when individuals can guide their attention in accordance with internal sources of information such as knowledge, memories, intentions, expectations, and goals. Likewise, voluntary attention control occurs when individuals can guide their attention "at will" in accordance with their internal goals and intentions. At first glance, these two forms of attention control seem very similar. Indeed, many attention control researchers use the terms "top-down" and "voluntary" interchangeably (see Theeuwes, 2018 for a clear example). However, the extent to which top-down information about a task-relevant target gives rise to voluntary attention control has recently been questioned on several different fronts (Davis and Gibson, 2012; Pauszek and Gibson, 2016, 2018; Gaspelin and Luck, 2018; Wolfe, 2018; Gibson et al., 2023).

For instance, Gibson et al. (2023) recently argued that the similarity between top-down and voluntary attention control could not be adequately determined by previous attention control studies because: (i) the experience of volition is typically defined as a conscious *feeling* that prior intentions led to a subsequent action (Wegner et al., 2017); and, (ii) that aspect of an individual's conscious experience had not been assessed in previous studies of top-down attention control. Accordingly, Gibson et al. attempted to shed light on this relation by measuring individuals' self-reported sense of "agency" (using a seven-point scale) as they were offered different levels of top-down information in a spatial cueing task. Agency was measured because it has been posited as one of the defining features of voluntary behavior (Firth, 2013), and individuals tend to rate themselves as having a strong sense of agency when they consciously experience their intentions as being the primary cause of their actions (Synofzik et al., 2013; Tapal et al., 2017).

In addition, Gibson et al. (2023) also distinguished this "control felt" aspect of agency from the "control used" aspect of agency in their Experiment 2. This distinction is potentially important because these two aspects of agency have been shown to be inversely related to one another at the group level in any given task context (Potts and Carlson, 2019). That is, task contexts that elicit the *use* of only a small amount of control tend to elicit the *feeling* of a larger amount of control whereas task contexts that elicit the *use* of a larger amount of control tend to elicit the *feeling* of a smaller amount of control. For the purposes of this study, we will focus exclusively on the "control felt" aspect of agency (see also, Craig, 2015; for a broad discussion of how bodily feelings guide human actions), and we will simply use the term "agency" to refer to this aspect of agency.

In Gibson et al.'s (2023) study, top-down information about the location of a task-relevant target letter (E vs. U) was conveyed by arrow and onset cues that indicated one of four possible fixed directions (*above* vs. *below* vs. *left* vs. *right*) on each trial. Gibson et al. used the widest range of top-down information that was possible in this context. This range included 100%-valid cues at one extreme and 25%-valid cue (i.e., chance) at the other extreme. In addition, 70%-valid cues were also included as an intermediate value so that linear functions could be fit across the three levels of cue validity.

Although it is commonplace for attention control researchers to describe manipulations of top-down information in terms of cue validity (Jonides, 1980, 1983; Posner, 1980; Posner et al., 1980; Eriksen and Yeh, 1985; Madden, 1992; Riggio and Kirsner, 1997; Vossel et al., 2006; Chica et al., 2014), the cue-validity scale may not be the most appropriate scale upon which to quantify how much information has been conveyed by the cues in this paradigm (Gibson et al., 2021). Rather, information-theoretic quantities such as "mutual information" and "conditional target entropy" are more transparent about how much information has been conveyed because these terms explicitly reflect the entropy associated with locating and identifying the target in this spatial cueing paradigm. For instance, in Gibson et al.'s (2023) task context, there was a total of three bits of information associated with locating and identifying the target, where one bit is equal to the amount of information that is gained by resolving two equally likely outcomes. In particular, one bit was associated with resolving the orientation of the target (which was equally likely to appear on the horizontal or vertical axis); one bit was associated with resolving the direction of the target (which was equally likely to appear at one endpoint or the other of an axis); and one bit was associated resolving the identity of the target (which was equally likely to be the letter E or U).

Mutual information reflects the average reduction in uncertainty about the target's identity and location that is provided by the cue (or *vice-versa*), and conditional target entropy reflects the average uncertainty in target identity and location that remains after processing the spatial information conveyed by the cue. These two quantities are complementary in the sense that they must sum to the total entropy (i.e., 3.00 bits) associated with locating and identifying the target in this context. When the cue was 100% valid, mutual information was equal to 2.00 bits and conditional target entropy was equal to 1.00 bit because the cue provided perfectly accurate information about the target's location (orientation and direction) but no information about its identity. When the cue was 70% valid, mutual information was equal to 0.64 bits and conditional target entropy was equal to 2.36 bits because the cue provided partially accurate information about the target's location (orientation and direction) but no information about its identity. And, when the cue was 25% valid, mutual information was equal to 0.00 bits and conditional target entropy was equal to 3.00 bits because the cue provided no information about the target's location or identity. Although the magnitude of the agency-slope would be equivalent regardless of whether it was fit across the three levels of mutual information or across the three levels of conditional target entropy, Gibson et al. (2023) chose to analyze changes in agency ratings as function of conditional target entropy because that scale is unique to target entropy whereas mutual information equally reflects the reduction in entropy associated with knowing either the target or the cue.

If top-down and voluntary attention control are interchangeable forms of attention control, then Gibson et al. (2023) reasoned that the sense of agency should decrease in a linear fashion as conditional target entropy increased across the 1.00-bit (100%-valid), 2.36-bit (70%-valid), and 3.00-bit (25%-valid) cue conditions. In other words, they reasoned that individuals should feel most in control when they had the least uncertainty about the location of the target, and they should feel the least in control when they had the most uncertainty about the location of the target. Gibson et al. used a linear growth-curve model to analyze the results of their study which allowed them to distinguish between a fixed slope factor that reflected the average slope of all the participants, and random factors that allowed each participant to have a unique slope and intercept.

The results of two experiments consistently showed that the average (fixed) slope was only slightly negative and did not differ significantly from zero. On the face of it, this finding suggests that, on average, feelings of agency did not change as a function of how much top-down information was provided about the task-relevant target, even though the range of cue validity values used was maximal. However, a much different conclusion was warranted by the analysis of the random factors: namely, the variance associated with both the random slope and intercept factors was consistently found to be significant across the two experiments. Of critical importance, the variance associated with the random slope factor showed variation in both the magnitude and direction of the individual slopes values which shed light on the near-zero fixed slope factor (see Miller and Schwarz, 2018, for a more general discussion of how individual differences can lead to null average effects). Their findings suggested that only 59% of the area under the theoretical population distribution of random slope values corresponded to negative slope values whereas the remaining 41% of the area under this curve corresponded to positive slope values.

In addition, the results of these two experiments also consistently showed significant covariation between the random slope and intercept factors suggesting that the direction of the random slope values varied inversely with the random intercept values. Those individuals who generated a negatively sloped growth curve exhibited the expected trajectory in which the 1.00-bit (100%-valid) cues elicited the highest ratings, and the 3.00-bit (25%-valid) cues elicited the lowest ratings. However, those individuals who generated a positively sloped growth curve exhibited an unexpected trajectory in which the 1.00-bit (100%-valid) cues elicited the lowest ratings, and the 3.00-bit (25%-valid) cues elicited the highest ratings.

The findings reported by Gibson et al. (2023) suggested that individuals can differ dramatically in how much agency they feel in response to standard manipulations of top-down information in the spatial cueing paradigm. But, why did approximately 40% of the individuals fail to experience the expected pattern of agency in their study? As Gibson et al. noted, the spatial cues used in the spatial cueing paradigm are external to the individual and the top-down information that is conveyed by these cues about the target is delivered in a random fashion by the experimental software. As such, the shifts of attention that are elicited by those cues may not be experienced as voluntary by some individuals because their attention is being guided by an extrinsic source of spatial information that they played no role in choosing (see e.g., Bargh, 1994; Bargh and Ferguson, 2000). As a result, the sense of agency these individuals experience while searching for the target may have increased only when the cues became less informative because only then did these individuals feel that they were searching for the target on their own. Indeed, Gibson et al. (2023) also showed that the agency ratings of all participants were significantly higher in a "no-cue" condition relative to the 3.00-bit (25%-valid) cue condition, suggesting that individuals generally felt more agency when searching without a cue at all.

In the present study, we attempted to actively increase the sense of agency elicited by external spatial cues by allowing individuals in one condition to voluntarily choose the direction indicated by the spatial cue on each trial, though the actual validity of the cue (and resulting conditional target entropy) remained out of their control. We considered this "controllability" manipulation to be like other attention control studies that have recently attempted to manipulate agency more directly (Wen and Haggard, 2018; Huffman and Brockmole, 2020). For instance, Huffman and Brockmole manipulated the role of agency in attention control by varying the extent to which participants were able to voluntarily choose the direction of a visual search display of moving circles. Participants controlled the directional movement of one of four display circles by pressing the directional arrows keys on a computer keyboard. Participants were considered to have more voluntary control when they were allowed to press the arrow keys in whatever manner they chose whereas they were considered to have less voluntary control when they were required to press the arrow keys in accordance with a computer-generated pattern of key presses. Unfortunately, Huffman and Brockmole (2020) did not consistently obtain agency ratings across their three experiments, and thus they could not explicitly compare how agency ratings might have changed when participants were free to exercise volitional control over the direction of the controlled circle vs. when they were not free to do so. Furthermore, they focused on average performance and did not examine individual differences.

In the present study, we explicitly compared the agency ratings obtained across two separate volition conditions that were encountered by two independent samples of participants. In the "direction selected" condition, the cued direction was voluntarily selected by the participant on each trial whereas, in the "direction assigned" condition, the cued direction was randomly assigned by the experimental software on each trial (as in Gibson et al., 2023 original experiments). We expected that a greater proportion of individuals would generate the negatively sloped growth curves when cued direction was selected relative to when it was assigned randomly on each trial.

In addition to directly comparing the reports of agency across the direction selected and assigned conditions, the present study also included another potentially important modification. Namely, the use of arrow and onset cues may have been problematic for Gibson et al.'s (2023) purposes because others (Ristic and Kingstone, 2012; Ristic et al., 2012) have argued that both of these cues may routinely elicit involuntary (or automatized) shifts of attention which may in turn temper conclusions about the ability of these stimuli to elicit voluntary shifts of attention (see also, Hommel et al., 2001; Gibson and Bryant, 2005; Gibson and Kingstone, 2006). Accordingly, in the present study, we used the numbers "1," "2," "3," and "4" as cues to arbitrarily refer to the *above*, *right*, *below*, and *left* directions, respectively. Our intention was to use cues that were not strongly associated *a priori* with these directions, and therefore would be less likely to elicit involuntary (or automatized) shifts of attention.

In summary, previous research reported by Gibson et al. (2023) has shown that top-down and voluntary attention control are aligned for only about half of the participants in the standard spatial cueing paradigm. The present study attempted to replicate these previous findings under conditions in which the standard manipulation of top-down information was conveyed by a number cue (as opposed to an arrow or onset cue). But more importantly, the present study was also designed to examine if the proportion of individuals who aligned top-down and voluntary attention control could be increased by combining the standard manipulation of top-down information with a novel manipulation of volition in a separate condition in which participants were given the opportunity to freely choose (or not) the direction of the spatial cue on each trial.

Method

Participants

A total of 160 participants were recruited through Prolific (www.prolific.co) in exchange for monetary payment ($6.00 USD). The two volition conditions were run consecutively. The first group of 80 participants was run in the direction assigned condition, and the second group of 80 participants was run in the direction selected condition. These sample sizes were chosen to match the sample size used in Experiment 2 of Gibson et al. (2023) study. Note that power analyses are not well developed for mixed-effects models (Maxwell et al., 2018); this is especially true for estimating the power of random factors because the null value (population variance equal to zero) is also the minimum possible value of this parameter, and the sampling distributions of such "boundary values" are not well understood by statisticians (Verbeke and Molenberghs, 1997). To be included in the experiment, participants were required to (1) self-report that they were a fluent English speaker; (2) self-report normal or corrected-to-normal visual acuity; and, (3) finish the experiment with an overall percent error rate on the visual search task that was ≤30%. The Institutional Review Board at the University of Notre Dame approved all procedures reported in this manuscript. These data were collected in the fall of 2021.

Stimuli and apparatus

Both volition conditions were programmed using PsychoPy Experiment Builder (Peirce et al., 2019), and virtual data collection was hosted through PsychoPy's open science website Pavlovia. The sizing of stimuli in PsychoPy are specified in 'height units' which are *relative* to the height of participants' computer screen while the ratio of the height to width of the stimuli remain *absolute*. The use of these units in PsychoPy ensure that stimuli are presented consistently without restricting participation based on screen-size or OS requirements. In the following description, we report the size of the stimuli in terms of height units, but for the sake of clarity, we also report their size in terms of centimeters (cm) based on a 13-inch widescreen display.

As shown in Figure 1 Top, each trial in the direction assigned condition consisted of three displays which were presented against the black background of the screen: a fixation display, a cue display, and a target display. The fixation display contained a small white fixation dot in the center of the display; the fixation dot measured 0.015 units (0.3 cm) in diameter. Four boxes were presented 0.16 units (2.80 cm) *above*, *below*, *left of*, or *right of* central fixation. Each box appeared as a square, 0.07 units (1.30 cm) tall and 0.07 units (1.30 cm) wide, and had a black fill and gray outline. The fixation dot was replaced by a white number between 1 and 4 that indicated the direction of one of the four peripheral boxes. The "1" cue referred to the *above* location; the "2" cue referred to the *right* location; the "3" cue referred to the *below* location; and the "4" cue referred to the *left* location. The number cues were 0.04 units (0.50 cm) at their widest point and 0.06 units (1.10 cm) tall. The target display contained a single white target letter (E or U) along with three non-target letters (A, P, and S). Each letter was 0.04 units (0.50 cm) tall and 0.04 units (0.50 cm) wide and appeared in one of the four gray boxes; the target was equally likely to appear in any of the four directions (*above*, *below*, *left*, or *right*).

As shown in Figure 1 Bottom, the sequence of displays in the direction selected condition was identical to the sequence of displays in the direction assigned condition with the sole exception being the insertion of a selection display that appeared at the start of each trial. In the selection display, one of the four number cues was randomly assigned (as in the direction assigned condition); however, participants in the direction selected condition were allowed to change the direction of the cue on each trial by pressing the *right* arrow key on the keyboard. This arrow key advanced through a random sequence of the number cues, and the cycle could be repeated until a cue was selected. Participants locked in their choice by pressing the space bar which then triggered the appearance of the fixation display followed by the cue display (which contained the selected cue) and then the target display.

Experimental design

The design of the direction assigned condition was identical to the design of the two experiments reported in Gibson et al. (2023) with the sole exception being that number cues were used to convey direction in the present study. Four levels of a conditional target entropy (or cue validity) factor [1.00-bit (100% valid), 2.36-bit (70% valid), 3.00-bit (25% valid), and no cue] were presented within the context of a repeated measures design. Note that although the cue was absent from the display in the no cue context, we treated this context as a separate level of the conditional target entropy factor for the purposes of balancing the design. Each of the four conditional target entropy conditions was presented in a separate block of 40 trials, and this group of four blocks was repeated four times for a total of 16 blocks (640 total experimental trials). The order of the four conditional target entropy conditions was randomized separately within each repetition group for each participant.

The design of the direction selected condition was identical to the design of the direction assigned condition with the sole exception being the elimination of the no-cue condition, which was omitted out of necessity because there is no cue to be selected in the no-cue condition. Each of the remaining three conditional target entropy conditions was presented in a separate block of 40 trials, and this group of three blocks was repeated four times for a total of 12 blocks (480 total experimental trials). The order of the three conditional target entropy conditions was randomized separately within each repetition group for each participant.

Procedure

At the beginning of each block in the direction assigned condition, participants were informed of the presence and validity of the cues; the direction of the cues was described as "always accurate" in the 1.00-bit (100%-valid) cue condition, "mostly accurate" in the 2.36-bit (70%-valid) cue condition, and "rarely accurate" in the 3.00-bit (25%-valid)

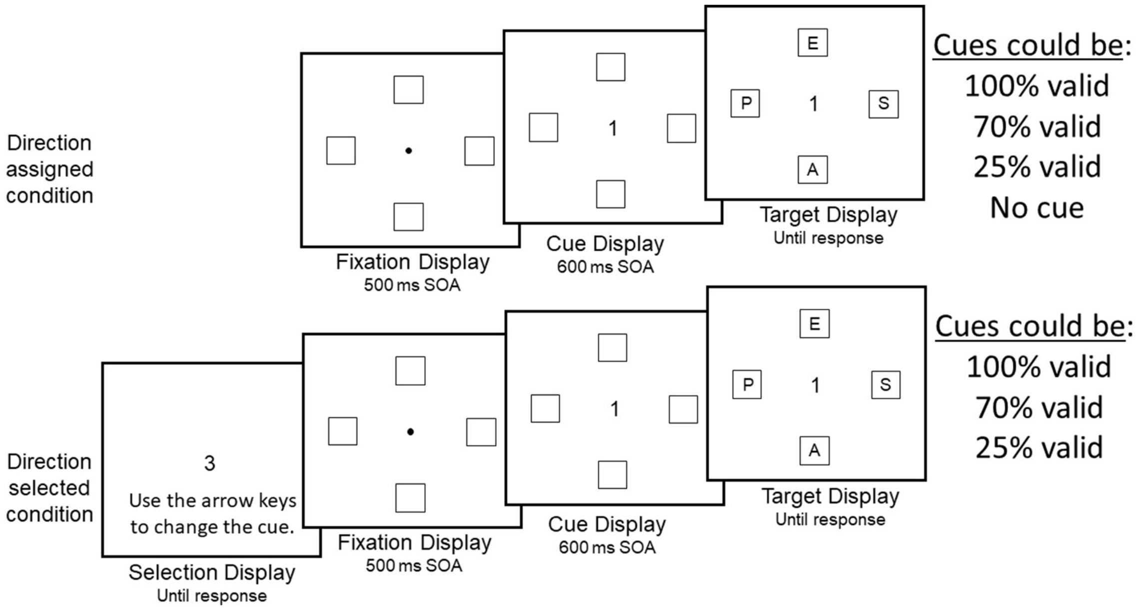

FIGURE 1
Example display sequences used in the direction assigned **(Top)** and direction selected **(Bottom)** conditions of the present experiment.

cue condition. Note that when the cue was invalid, the target was equally likely to appear at each of the three uncued locations. In the 2.36-bit (70%-valid) cue condition, the target appeared at each of the uncued locations 10% of the time; and, in the 3.00-bit (25%-valid) cue condition, the target appeared at each of the uncued locations 25% of the time. On each trial within a block, a fixation display appeared first for 500 ms followed by the cue display. After 600 ms, the target display was added to the cue display and the two displays remained on screen until a response was made. On each trial, the target letter was equally likely to be an E or U. Observers always pressed the "E" key with their *left* hand to discriminate the identity of the E target and the "U" key with their *right* hand to discriminate the identity of the U target.

At the end of each block, participants were instructed to rate the level of agency they felt. Participants were told that individuals are thought to have a positive sense of agency when they consider themselves to be the initiator of their actions, along with the following instructions, provided at the beginning of the experiment:

"We are interested in how much agency you feel in these different visual search contexts. Please always use the cue to try to find the target, regardless of how useful or accurate it is. At the end of each block, a rating scale will appear on the screen, and you will be asked to rate the extent to which you considered yourself to be in control of finding the target. A rating of '1' will correspond to 'no control' whereas a rating of '7' will correspond to 'full control.' You will respond by using the corresponding number keys on your keyboard to report the magnitude of your positive sense of agency in each block."

The procedure in the direction selected condition was identical to the procedure in the direction assigned condition with two exceptions. First, cues were always present in each block. Second, a selection display appeared first on each trial; the selection display remained on the screen until participants made their direction choice, at which point they were instructed to press the space bar and then the trial proceeded as in the direction assigned condition.

Results and discussion

Analysis of valid and invalid response times

We began by examining valid and invalid response times (RTs) in each of the two volition conditions to ensure that standard spatial cueing effects were obtained in this study. The top panel of Figure 2 shows mean correct valid and invalid RTs as a function of conditional target entropy in each of the direction assigned and selected conditions; the bottom panel of Figure 2 shows the corresponding percent error rates. First, a 2 × 3 repeated measures analysis was conducted on mean correct valid RTs, with volition condition (direction assigned vs. direction selected) and conditional target entropy (1.00 bit vs. 2.36 bits vs. 3.00 bits) as the two within-subjects factors. These analyses were conducted using the multivariate approach to avoid violating the sphericity assumption. As can be seen in Figure 2, mean correct valid RTs were significantly slower overall (by 176 ms) in the direction selected condition than in the direction assigned condition, $F_{(1,158)} = 28.26$, $p < 0.001$, $\eta_p{}^2 = 0.15$, for the main effect of volition condition. Although this main effect of volition condition was not explicitly predicted, it may reflect a greater contribution of intentional processes, which are known to be slower (Wolfe et al., 2000), in the direction selected condition. In addition, as expected, mean correct valid RTs also increased significantly as a function of conditional target entropy, $F_{(2,78)} = 213.86$, $p < 0.001$, $\eta_p{}^2 = 0.85$, for the main effect of conditional target entropy. However,

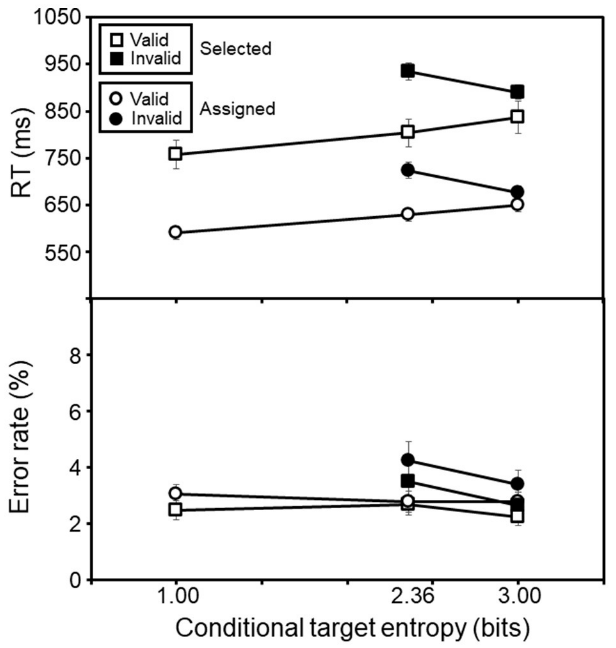

FIGURE 2
Mean correct overall RTs and error rates obtained in each of the direction assigned and direction selected conditions as a function of conditional target entropy. Error bars reflect standard error of the mean.

the effect of conditional target entropy was very similar across the two volition conditions resulting in a non-significant interaction between volition condition and conditional target entropy, $F_{(2,78)} = 1.49$, $p = 0.23$, $\eta_p{}^2 = 0.04$. Likewise, an identical analysis of percent error rates revealed no significant main effects or interaction (all p's > 0.26 or more).

Second, we also examined the relation between valid and invalid trials in the 2.36-bit (70%-valid) and 3.00-bit (25%-valid) cue conditions (recall that the 1.00-bit (100%-valid) cue condition did not include any invalid trials). A 2 × 2 × 2 Split-Plot ANOVA was conducted on mean correct RTs, with volition condition as the sole between-subjects factor, and with conditional target entropy (2.36 bit vs. 3.00 bit) and cued location (valid vs. invalid) as the two within-subjects factors. This analysis was conducted using the univariate approach because the sphericity assumption could not be violated with only two levels of each factor. Most importantly, as expected, there was a significant two-way interaction between conditional target entropy and cued location, $F_{(1,158)} = 64.72$, $p < 0.001$, $\eta_p{}^2 = 0.29$, indicating that the 113-ms spatial cueing effect observed in the 2.36-bit (70%-valid) cue condition was larger than the 40-ms spatial cueing effect observed in the 3.00-bit (25%-valid) cue condition. In addition, there was also a significant two-way interaction between volition condition and cued location, $F_{(1,158)} = 3.86$, $p = 0.051$, $\eta_p{}^2 = 0.02$, indicating that the 93-ms spatial cueing effect observed in the direction selected condition was larger than the 61-ms spatial cueing effect observed in the direction assigned condition. This interaction may reflect the manifestation of a greater effect of agency on RTs in the direction selected condition. The three-way interaction between volition condition, conditional target entropy and cued location did not attain significance, $F_{(1,158)} = 0.31$, $p = 0.58$, $\eta_p{}^2 = 0.002$. An identical analysis was also conducted on percent error rates. Although the pattern of error rates mirrored the pattern of RTs, the two-way interaction between conditional target entropy and cued location was only marginally significant, $F_{(1,158)} = 2.92$, $p = 0.089$, $\eta_p{}^2 = 0.02$. Furthermore, the interaction between volition condition and cued location did not attain significance, $F_{(1,158)} = 0.42$, $p = 0.52$, $\eta_p{}^2 = 0.003$.

Analyses of agency ratings

A mixed-effects (growth-curve) modeling approach was used in the present study to examine potential individual differences in voluntary attention control. Mixed-effects models explicitly distinguish between fixed factors and random factors in the analysis of repeated measures designs (see Singer and Willett, 2003, for an introduction to these methods), where a fixed factor refers to an independent variable whose levels have been pre-selected by the researcher and a random factor refers to an independent variable whose levels have been chosen randomly. In this way, each participant in a repeated measures design is a random level of an independent variable labeled "subject;" in this context, a significant effect of the random "subject" factor is reflected by significant variation across the different levels of the subject factor (constituting individual differences). One of the main advantages to using mixed-effects models is that they afford greater flexibility in how the random subject factor is allowed to interact with the fixed factor (see, Kliegl et al., 2011; Barr et al., 2013; Matuschek et al., 2017; Oberaurer, 2022 for recent discussion).

To help understand the advantage of the mixed-effects model approach, consider the standard univariate ANOVA approach to analyzing repeated measures designs. In this standard approach, the fixed factor—conditional target entropy—must be treated as a categorical variable whereas it can be treated as a continuous variable in the mixed-effects model approach. Figures 3A, B show both the individual (light gray lines) and average (dark black symbols and lines) agency ratings plotted as a function of conditional target entropy in each of the direction assigned and direction selected conditions, respectively. In these designs, the subject X conditional target entropy interaction serves as the error term (i.e., the denominator) in the F-test that evaluates the main effect of conditional target entropy within each volition condition. However, this interaction is assumed to be zero in the population within the context of the univariate ANOVA approach; this assumption is called the "sphericity assumption."

From the perspective of mixed-effects models, sphericity corresponds to a model that includes conditional target entropy as the fixed factor along with a random intercept factor that allows each participant to have a unique intercept. However, this model assumes that the individual difference in intercepts is the only difference between individuals' true trajectories which are all assumed to be parallel. For instance, Figures 3C, D show the individual trajectories that would be predicted by a random intercept model in each of the direction assigned and direction selected conditions. The discrepancy that is apparent between the actual and predicted individual trajectories reflects nothing more than error according to the univariate approach.

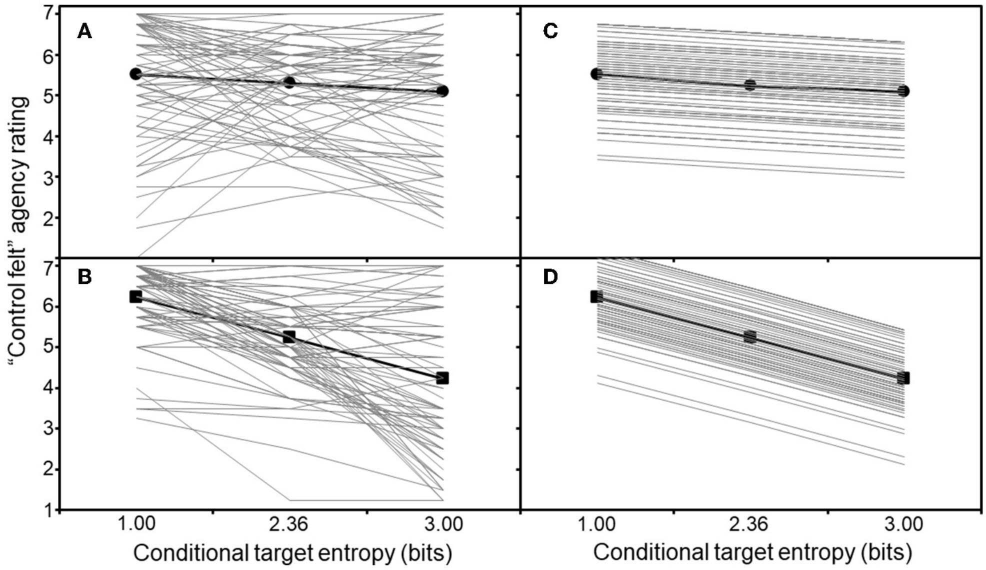

FIGURE 3
The univariate approach to analyzing "control felt" agency ratings as a function of conditional target entropy. Note the three levels of conditional target entropy are treated as a categorical variable in the univariate approach. **(A)** Actual individual (light gray lines) and average (dark black symbols and lines) "control felt" agency ratings depicted as function of conditional target entropy in the direction assigned condition. **(B)** Actual individual (light gray lines) and average (dark black symbols and lines) "control felt" agency ratings depicted as function of conditional target entropy in the direction selected condition. **(C)** Predicted individual (light gray lines) and average (dark black symbols and lines) "control felt" agency ratings depicted as function of conditional target entropy in the direction assigned condition when sphericity is assumed. **(D)** Predicted individual (light gray lines) and average (dark black symbols and lines) "control felt" agency ratings depicted as function of conditional target entropy in the direction selected condition when sphericity is assumed.

However, if the actual individual trajectories depicted on the left-hand side of Figure 3 reflect the existence of slopes (and intercepts) that are truly different for each participant, then this would represent a violation of the sphericity assumption, and the univariate approach to analyzing repeated measures designs would no longer be statistically appropriate because the type I error rate could be drastically higher than the nominal alpha value (see Maxwell et al., 2018). Fortunately, there are three potential solutions to a violation of the sphericity assumption: (1) use various correction factors (such as Greenhouse-Geisser) that adjust the degrees of freedom and thus the critical values associated with the univariate F-tests; (2) use the multivariate approach to analyzing repeated measures designs; and (3) use the mixed-effects approach to analyzing repeated measure designs. Options 2 and 3 have been deemed the most appropriate because they take into consideration all the error that results when the subject X fixed factor interaction is non-zero (Maxwell et al., 2018). The main difference between options 2 and 3 is that the multivariate approach does not attempt to explicitly model the random factors whereas the mixed-effects model does.

Accordingly, we used a growth-curve modeling approach in the present study that treated the two levels of volition condition (direction assigned vs. direction selected) as a categorical variable and the three levels of conditional target entropy as a continuous variable. The statistical model included fixed main effects of volition condition and conditional target entropy as well as the interaction between these two fixed factors. In addition, the statistical model also included both random slope and intercept factors that were fit across the three levels of conditional target entropy. Note that the conditional target entropy scale was shifted by -1.00 bit in the analysis so that the value of the intercept estimate corresponded with the value predicted in the 1.00-bit (100%-valid) cue condition. These analyses were conducted using SAS PROC MIXED. Model parameters were estimated using restricted maximum likelihood (REML), and accompanying p-values were calculated based on a Wald test, both of which are the default setting in SAS PROC MIXED.

Figure 4 depicts the individual (light gray lines) and average (dark black symbols and lines) agency slopes (and intercepts) that were fit across the three levels of conditional target entropy in each of the direction assigned (panel A) and direction selected (panel B) conditions. There were significant fixed main effects of volition condition, $F_{(1,158)} = 15.04$, $p = 0.0002$, $\eta^2 = 0.09$, and conditional target entropy, $F_{(1,158)} = 64.19$, $p < 0.0001$, $\eta^2 = 0.29$. More importantly, as expected, there was also a significant volition condition X conditional target entropy interaction, $F_{(1,158)} = 26.23$, $p < 0.0001$, $\eta^2 = 0.14$, suggesting that conditional target entropy had a larger effect on agency ratings in the direction selected condition than in the direction assigned condition.

The two-way interaction between volition condition and conditional target entropy was examined further by analyzing the fixed effect of conditional target entropy, along with the random intercept and slope factors, within each volition condition separately. With respect to the direction assigned condition, we expected to replicate the main findings reported by Gibson et al. (2023). Consistent with their findings, the average slope was found to be slightly negative (-0.21 units of agency/bit), and it was also found to be significant in this experiment, $F_{(1,79)} = 3.98$, $p = 0.049$,

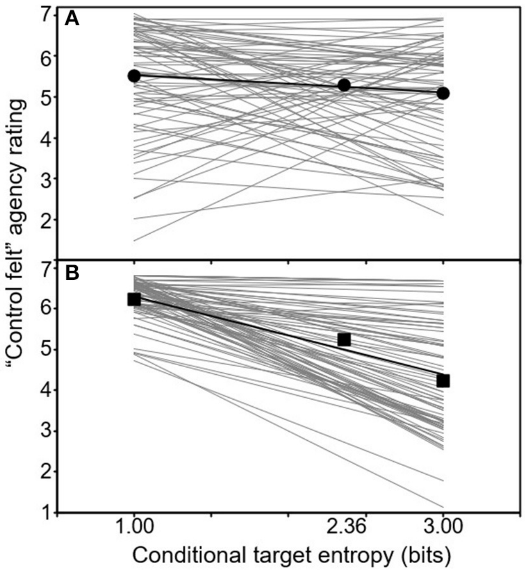

FIGURE 4
The linear growth curve model approach to analyzing "control felt" agency ratings as a function of conditional target entropy. Note the three levels of conditional target entropy are treated as a continuous variable in the linear growth curve approach. **(A)** Predicted individual (light gray lines) and average (dark black symbols and lines) "control felt" agency ratings depicted as function of conditional target entropy in the direction assigned condition when both random slope and intercept factors are included in the growth curve model. **(B)** Predicted individual (light gray lines) and average (dark black symbols and lines) "control felt" agency ratings depicted as function of conditional target entropy in the direction selected condition when both random slope and intercept factors are included in the growth curve model.

$\eta^2 = 0.05$, 95% CI $[-0.42, -0.0006]$. In addition, the variance associated with the random slope factor was found to be significant, $Var = 0.79$, $z = 5.55$, $p < 0.0001$, 95% CI [0.57, 1.16], as was the variance associated with the random intercept factor, $Var = 1.96$, $z = 5.70$, $p < 0.0001$, 95% CI [1.43, 2.86]. The fact that the variance associated with the random slope factor was significant suggests that the individual differences in the slope estimates represented true slope differences; therefore, contrary to the sphericity assumption underlying the standard univariate ANOVA approach to analyzing repeated measures designs, these findings suggest that a statistical model which only allows individuals to differ in their intercepts should be rejected. In addition, there was also significant covariation between these two random factors, $Cov = -0.79$, $z = -4.22$, $p < 0.0001$, 95% CI $[-1.15, -0.42]$, which corresponds to a correlation (r) of -0.632. This finding suggests that the slope estimates tended to vary inversely with the intercept estimates.

With respect to the direction selected condition, the average slope was strongly negative (-0.95 units of agency/bit), and it was also found to be significant in this experiment, $F_{(1,79)} = 90.65$, $p < 0.0001$, $\eta^2 = 0.53$, 95% CI $[-1.15, -0.76]$. In addition, the variance associated with the random slope factor was found to be significant, $Var = 0.55$, $z = 4.10$, $p < 0.0001$, 95% CI [0.36, 0.95], as was the variance associated with the random intercept factor, $Var = 0.47$, $z = 2.72$, $p = 0.003$, 95% CI [0.26, 1.13]. As in the direction assigned condition, the fact that the variance associated with the random slope factor was significant suggests that a statistical model which only allows individuals to differ in their intercepts should be rejected. However, the covariation between these two random factors did not attain significance, $Cov = -0.10$, $z = -0.86$, $p = 0.39$, 95% CI $[-0.40, 0.13]$, which corresponds to a correlation (r) of -0.198.

One way to help visualize the treatment effects associated with the random slope and intercept factors is to depict the corresponding theoretical population distributions that were extracted by the growth-curve models in this experiment. The distributions associated with the direction assigned and direction selected conditions are depicted in Figure 5; as required by the mixed-effects modeling approach, these distributions are assumed to have a normal shape.

With respect to the direction assigned condition (solid lines), the mean value of the slope distribution was -0.21 units of agency/bit, and it had a standard deviation of 0.89 units of agency/bit (see Figure 5, Left); the mean value of the intercept distribution was 5.53 units of agency, and it had a standard deviation of 1.40 units of agency (see Figure 5, Right). Based on the random slope distribution, it was estimated that a slight majority (60%) of individuals would be expected to exhibit the expected pattern in which they felt the greatest agency in the 1.00-bit (100%-value) cue context and the least agency in the 3.00-bit (25%-valid) cue context—i.e., a slope <0, though this also means that a substantial percentage of individuals (40%) would also be expected to exhibit the opposite pattern. These findings corroborate the findings reported by Gibson et al. (2023) using a number cue that should not have elicited involuntary (or automatized) shifts of attention.

With respect to the direction selected condition (dotted lines), the mean value of the slope distribution was -0.95 units of agency/bit, and it had a standard deviation of 0.74 units of agency/bit (see Figure 5, Left); the mean value of the intercept distribution was 6.30 units of agency, and it had a standard deviation of 0.68 units of agency (see Figure 5, Right). Based on the random slope distribution, it was estimated that a substantial majority (90%) of individuals would be expected to exhibit the expected pattern in which they felt the greatest agency in the 1.00-bit (100%-value) cue context and the least agency in the 3.00-bit (25%-valid) cue context—i.e., a slope <0. These findings support the conclusion that allowing participants to voluntarily choose (or not) the direction of the number cue on each trial increased the proportion of participants who generated negatively sloped growth curves.

The percentage of positive and negative slopes estimated by the mixed-effects modeling approach requires an assumption of normality, which can be difficult to prove, especially when relatively small samples are used. To allay these concerns, we also used the Ordinary Least Squares (OLS) method to estimate the observed agency slopes for each participant in order to count the relative number of positive and negative slopes observed in our sample, as well as to help visualize how the direction of the slope estimates varied inversely with the intercept estimates, at least in the direction assigned condition.

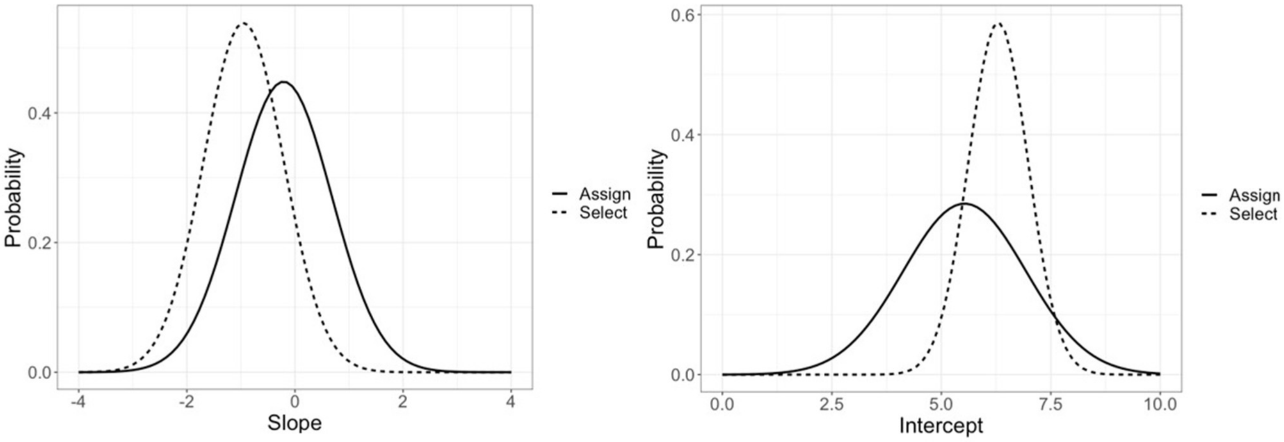

FIGURE 5
Theoretical population distributions of agency ratings that were extracted by the growth-curve models in the direction assigned (solid line) and direction selected (dotted line) conditions. The distribution of slopes is shown in the **Left** and the distribution of intercepts is shown in the **Right**.

With respect to the direction assigned condition, there were 48 individuals (60%) with negative slopes, and 24 individuals (30%) with positive slopes; eight individuals (10%) provided the same rating across all three conditional target entropy conditions and were assigned a slope of zero. These percentages are very similar to the percentages estimated by the corresponding growth curve model. Likewise, with respect to the direction selected condition, there were 65 individuals (81.25%) with negative slopes, and 10 individuals (12.5%) with positive slopes; five individuals (6.25%) provided the same rating across all three conditional target entropy conditions and were assigned a slope of zero. These percentages were also very similar to the percentages estimated by the corresponding growth curve model. Thus, the percentage of positive and negative slopes observed in the empirical frequency distribution of OLS slopes was similar to the percentage inferred from the theoretical distributions suggesting that the assumption of normality was justified.

It is also worth pointing out that, within the context of classical test theory, reliability is conceptualized as the ratio of true variance to observed variance. In the present study, the estimated variance of the random slope and intercept factors can be interpreted as the true variance and the estimated variance of the individual OLS slopes and intercepts can be interpreted as the observed variance. In the direction assigned condition, the ratio of these two variances was found to be 0.89 for slopes and 0.91 for intercepts; in the direction selected condition, the ratio of these two variances was found to be 0.68 for slopes and 0.49 for intercepts.

Figures 6A, B show the relation between the direction of the OLS slope estimates and the magnitude of the OLS intercept estimates in the direction assigned and direction selected conditions, respectively. With respect to the direction assigned condition (see Figure 6A), in the 1.00-bit (100%-valid) cue condition, the agency ratings reported by the group of individuals with negatively sloped growth curve estimates ($M = 5.97$, $SE = 0.18$) were significantly higher than the agency ratings reported by the group with positively sloped growth curve estimates ($M = 4.19$, $SE = 0.25$), $F_{(1,70)} = 33.85$, $p < 0.001$, $\eta^2 = 0.33$. In contrast, in the 3.00-bit (25%-valid) cue condition, the agency ratings reported by the group of individuals with negatively sloped growth curve estimates ($M = 4.40$, $SE = 0.19$) were now significantly lower than the agency ratings reported by the group with positively sloped growth curve estimates ($M = 5.86$, $SE = 0.27$), $F_{(1,70)} = 20.08$, $p < 0.001$, $\eta^2 = 0.22$. In the 2.36-bit (70%-valid) cue condition, the agency ratings reported by the group of individuals with negatively sloped growth curve estimates ($M = 4.99$, $SE = 0.16$) were more similar to the agency ratings reported by the group with positively sloped growth curve estimates ($M = 5.41$, $SE = 0.22$) and the two groups did not differ significantly in this condition, $F_{(1,70)} = 2.41$, $p = 0.12$, $\eta^2 = 0.03$.

In addition, we also conducted a repeated measures analysis on agency ratings in the direction assigned condition with cue presence (no cue vs. 3.00 bits) as the sole within-subjects factor in both the negatively sloped and positively sloped growth curve groups separately (see the black and white triangles in Figure 6A, respectively). This analysis was conducted using the univariate approach because the sphericity assumption could not be violated with only two levels of the factor. As expected, agency ratings were significantly higher in the no cue condition ($M = 5.30$, $SE = 0.21$) than in the 3.00-bit (25%-valid) cue condition ($M = 4.40$, $SE = 0.21$), for those individuals with negatively sloped growth curve estimates, $F_{(1,47)} = 20.33$, $p < 0.001$, $d = 0.92$. Likewise, agency ratings were also significantly higher in the no cue condition ($M = 6.55$, $SE = 0.11$) than in the 3.00-bit (25%-valid) cue condition ($M = 5.86$, $SE = 0.18$), for those individuals with positively sloped growth curve estimates, $F(1,23) = 12.33$, $p = 0.002$, $d = 1.01$. Thus, these findings also replicate the findings reported by Gibson et al. (2023), and suggest that individuals had a stronger sense of agency when they searched for the target without a cue, even though neither context provided any top-down information about the location (or identity) of the target.

Turning now to the direction selected condition (see Figure 6B), in the 1.00-bit (100%-valid) cue condition, the agency ratings reported by the group of individuals with negatively sloped growth curve estimates ($M = 6.22$, $SE = 0.11$) were now similar

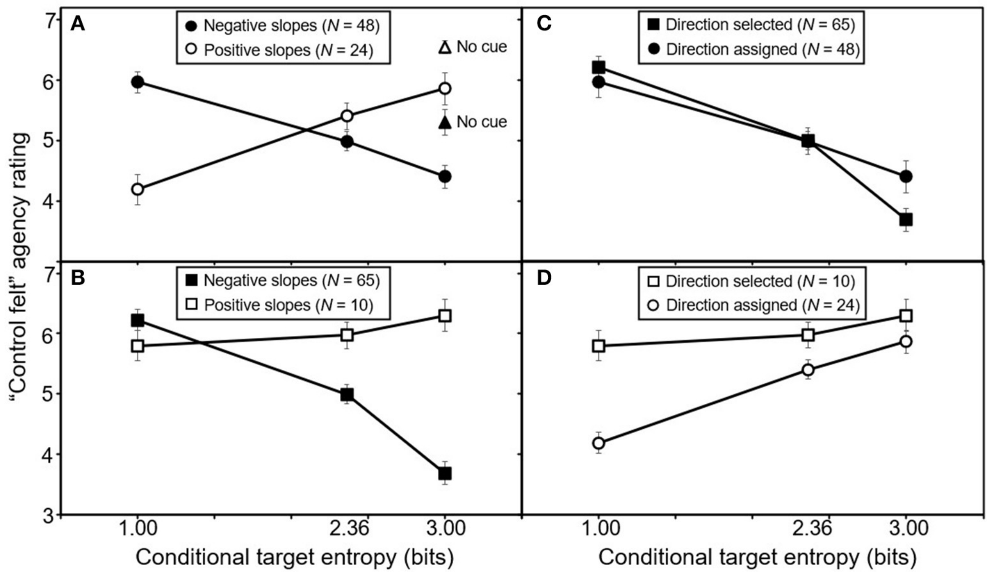

FIGURE 6
Average "control felt" agency ratings depicted as function of conditional target entropy. **(A)** Average agency ratings shown separately for those individuals who generated negatively sloped and positively sloped growth curves in the direction assigned condition. **(B)** Average agency ratings shown separately for those individuals who generated negatively sloped and positively sloped growth curves in the direction selected condition. **(C)** Average agency ratings shown separately for those individuals who generated negatively sloped growth curves in the direction assigned and direction selected conditions. **(D)** Average agency ratings shown separately for those individuals who generated positively sloped growth curves in the direction assigned and direction selected conditions. Error bars reflect standard error of the mean.

to the agency ratings reported by the group with positively sloped growth curve estimates ($M = 5.88$, $SE = 0.29$), and the two groups did not differ significantly in this condition, $F_{(1,73)} = 1.82$, $p = 0.18$, $\eta^2 = 0.02$. In contrast, in the 2.36-bit (70%-valid) cue condition, the agency ratings reported by the group of individuals with negatively sloped growth curve estimates ($M = 4.97$, $SE = 0.12$) were now significantly lower than the agency ratings reported by the group with positively sloped growth curve estimates ($M = 5.98$, $SE = 0.31$), $F_{(1,73)} = 8.71$, $p = 0.004$, $\eta^2 = 0.11$. Likewise, in the 3.00-bit (25%-valid) cue condition, the agency ratings reported by the group of individuals with negatively sloped growth curve estimates ($M = 3.69$, $SE = 0.16$ units of agency/bit) were also significantly lower than the agency ratings reported by the group with positively sloped growth curve estimates ($M = 6.30$, $SE = 0.48$), $F_{(1,73)} = 25.54$, $p < 0.001$, $\eta^2 = 0.26$.

For the sake of comparison, Figure 6C shows the growth curves of those individuals who generated negatively sloped growth curves from each of the two volition conditions, and Figure 6D shows the growth curves of those individuals who generated positively sloped growth curves from each of the two volition conditions. As can be seen, those who generated negatively sloped growth curves were very similar across the two volition conditions whereas those who generated positively sloped growth curves tended to have higher agency ratings in the direction selected condition than in the direction assigned condition across the three levels of conditional target entropy, and especially in the 1.00-bit (100%-valid) cue conditions.

We also examined the extent to which participants exercised their freedom to choose the direction of the cue in the direction selected condition. Figure 7 shows the proportion of trials that participants chose the randomly assigned direction in the direction selected condition as function of conditional target entropy. The results of the linear growth curve analysis revealed that the average slope was close to zero (0.007 units of agency/bit), and non-significant, $F_{(1,79)} = 1.26$, $p = 0.27$, $\eta^2 = 0.02$. In addition, only the variance associated with the random intercept factor was found to be significant, $Var = 0.10$, $z = 5.99$, $p < 0.0001$, 95% CI [0.08, 0.15]. The variance associated with the random slope factor was found to be non-significant, $Var = 0.0004$, $z = 0.71$, $p = 0.24$, 95% CI [0.00009, 0.40], as was the covariation between these two random factors, $Cov = -0.001$, $z = -0.53$, $p = 0.60$, 95% CI [-0.005, 0.003], which corresponds to a correlation (r) of -0.17.

Given that the average slope did not differ from zero and given that individual slopes did not differ significantly around this average, we averaged the proportion of trials that participants chose the randomly assigned direction across the three levels of conditional target entropy. The modal proportion was 1.00 ($N = 18$) and a total of 41 participants (51.25%) had proportions that were 0.90 or above. Thus, a substantial number of participants chose *not* to choose the direction of the cue in the direction selected condition. Of course, this finding may not be completely unexpected given that a similar number (48) of individuals were able to align their experience of agency with the top-down information that was conveyed by the cues in the direction assigned condition. However, upon closer inspection, those participants who chose not to choose the direction of the cue in the direction selected condition did not always align their experience of agency with the top-down information conveyed by the cues. Specifically, seven of these participants generated positively sloped growth curves and three of these participants generated a zero slope. Thus, a small

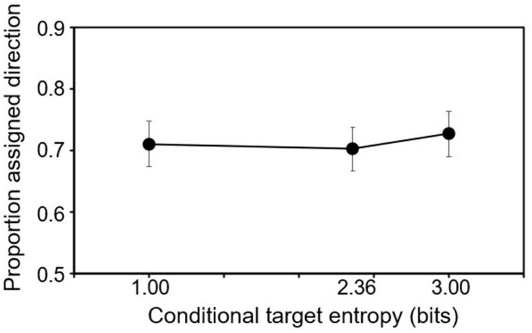

FIGURE 7
Average proportion of trials that participants chose not to change the randomly assigned direction cue in the direction selected condition as a function of conditional target entropy. Error bars reflect standard error of the mean.

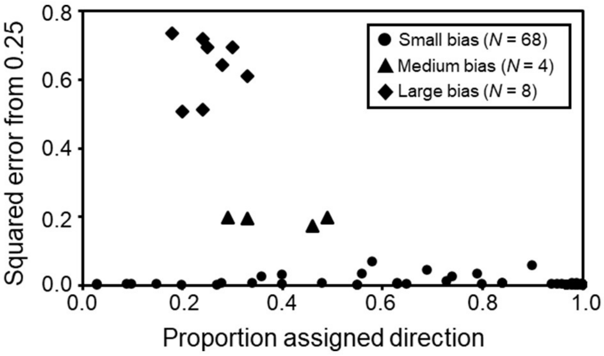

FIGURE 8
Scatterplot showing the average proportion of trials that each participant chose not to change the randomly assigned direction of the cue in the direction selected condition on the x-axis and the sum of squared error from a model that predicted equal distribution of cue choice across the four directions on the y-axis.

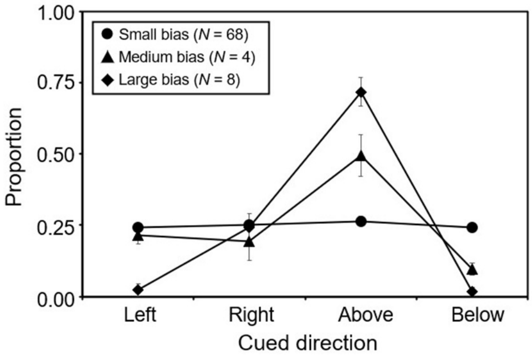

FIGURE 9
Average proportion that each of the four directions was chosen in the direction selected condition for each of the small, medium, and large bias groups identified in Figure 8. Error bars reflect the standard error of the mean.

number of participants seemed to resist exercising their freedom to select the direction of the cues as well as the opportunity to align their agency with the top-down information conveyed by the cues.

In addition, we also examined how participants distributed their choices across the four possible directions in the direction selected condition. We measured the distribution of direction choices by comparing the observed proportion of times each participant chose each direction with a model that predicted an equal distribution (0.25) of choices across the four directions. The magnitude of the deviation of the observed proportion from the predicted proportion was then squared, and we summed these squared deviations across the four directions to get a measure of error.

Figure 8 is a scatterplot that relates this error to the average proportion of trials that each participant chose the randomly assigned direction. As expected, the error was essentially zero on y-axis when the average proportion of accepting the randomly assigned direction was near 1.00 on the x-axis because these participants chose *not* to change the cued direction, and the experiment was programmed to randomly assign the cued direction across the four directions. However, most individuals (N = 68) managed to distribute their choices equally across the four directions even when they exercised their freedom to choose the cued direction on nearly all the trials. We identified this large group of participants as the "small bias" group (see the circle symbols clustered on or near the x-axis) in Figure 8. However, we also identified two smaller groups of participants who exercised their freedom to choose the cued direction on nearly all the trials, but who also seemed to consistently chose only one or two directions throughout the duration of the experiment. For instance, we identified one group of four participants as the "medium bias" group (see the triangle symbols), and another group of eight participants as the "large bias" group (see the diamond symbols) in Figure 8 based on the magnitude of their error from the random distribution model.

Figure 9 shows more clearly how these three groups of participants distributed their direction choices (or not) across the four cued directions. As can be seen in Figure 9, when there was a direction bias (as in the "medium bias" and "large bias" groups), participants tended to prefer the *above* direction. Note that there is nothing inherently wrong with this pattern of choices, as participants in the direction selected condition were allowed to choose any direction they desired. In fact, it is perhaps surprising that only a total of 12 participants (15%) opted to cue the same direction on every (or nearly every) trial.

General discussion

The present study attempted to bring top-down attention control into alignment with voluntary attention control by combining a standard manipulation of top-down information with a novel manipulation of volition within the context of the spatial cueing paradigm. In the present study, top-down information about the location of a task-relevant target letter was conveyed by number cues that indicated one of four possible fixed directions (*above* vs. *below* vs. *left* vs. *right*) on each trial. The amount of top-down information was manipulated in the standard way by varying the accuracy (or validity) of the cue from 100% (perfect accuracy) to 25% (chance accuracy); 70%-valid cues were also included as an intermediate value so that linear functions could be fit across the three levels of cue validity. The amount of volition was manipulated in a novel way by manipulating the extent to which participants could choose the direction of the number cue. Participants were considered to have more voluntary control when they were allowed to freely choose (or not) the direction conveyed by the number cue on each trial (i.e., the direction selected condition); whereas participants were considered to have less voluntary control when they were *not* allowed to change the direction conveyed by the number cue (i.e., the direction assigned condition).

Although experimental manipulations of top-down information are usually reported in terms of the accuracy (or validity) of the cue, here we translated cue validity into conditional target entropy values—expressed in terms of bits of uncertainty—to improve the linear fit of our statistical model (see also, Gibson et al., 2021). In general, we considered top-down and voluntary attention control to be in alignment when the "control felt" sense of agency decreased in a linear fashion across the 1.00-bit (100%-valid), 2.36-bit (70%-valid), and 3.00-bit (25%-valid) cue conditions. In other words, we expected that individuals should feel most in control when they had the greatest amount of top-down information (or the least amount of uncertainty) about the location of the target; whereas individuals should feel the least in control when they had the least amount of top-down information (or the greatest amount of uncertainty) about the location of the target. However, based on previous findings (Gibson et al., 2023), we also expected that the effect of top-down information on agency would interact with the volition manipulation in that we expected that the fixed effect of conditional target entropy on agency would be weaker in the direction assigned condition than in the direction selected condition due to the fact that a smaller proportion of individuals would have negative slopes in the former condition than in the latter condition.

The main results of our study were consistent with these expectations. As expected, there was a significant two-way interaction between volition condition and conditional target entropy indicating that the average (fixed) slope was less negative in the direction assigned condition (−0.21 units of agency/bit) than in the direction selected condition (-0.95 units of agency/bit), though both slopes were found to be significantly <0. Furthermore, an analysis of random factors indicated that the variance associated with both the random slope and intercept factors was consistently found to be significant across the two volition conditions. Of critical importance, as can be seen in Figure 5, the present findings suggested that only 60% of the area under the theoretical population distribution of random slope values corresponded to negative slope values in the direction assigned condition; whereas 90% of the area under the theoretical population distribution of random slope values corresponded to negative slope values in the direction selected condition.

In addition, the results also showed significant covariation between the random slope and intercept factors in the direction assigned condition, suggesting that the direction of the random slope values varied inversely with the random intercept values in the direction assigned condition, but this covariation was not found to be significant in the direction selected condition. For instance, Figure 6A shows agency ratings plotted separately for those individuals who generated negatively sloped growth curves vs. those who generated positively sloped growth curves in the direction assigned condition. The most striking aspect of these two groups is that those individuals who generated negatively sloped growth curves exhibited the expected trajectory in which the 1.00-bit (100%-valid) cues elicited the highest ratings, and the 3.00-bit (25%-valid) cues elicited the lowest ratings. However, those individuals who generated positively sloped growth curves exhibited an unexpected trajectory in which the 1.00-bit (100%-valid) cues elicited the lowest ratings, and the 3.00-bit (25%-valid) cues elicited the highest ratings. Thus, the findings obtained in the direction assigned condition corroborate the main findings reported by Gibson et al. (2023), and they suggest that individuals can differ dramatically in how much agency they feel in response to standard manipulations of top-down information in the spatial cueing paradigm, even when that information is conveyed by arbitrary number cues.

In contrast, Figure 6B shows agency ratings plotted separately for those individuals who generated negatively sloped growth curves vs. those who generated positively sloped growth curves in the direction selected condition. Unlike in the direction assigned condition, both groups reported high levels of agency in response to the 1.00-bit (100%-valid) cues in the direction selected condition. However, those individuals who generated negatively sloped growth curves tended to report the lowest ratings in response to the 3.00-bit (25%-valid) cues; whereas those individuals who generated positively sloped growth curves tended to report slightly higher ratings in response to the 3.00-bit (25%-valid) cues than in response to the 1.00-bit (100%-valid) cues. Thus, the findings obtained in the direction selected condition are important because they suggest that top-down attention control can be brought into greater alignment with voluntary attention control when standard manipulations of top-down information are combined with novel manipulations of volition.

In the present study, volition and corresponding feelings of agency were manipulated by allowing participants to change the direction of the number cue on each trial. Of course, by relinquishing the choice of cue direction to the participants, the experimenter has conceded loss of experimental control over cued direction; consequently, some directions might have been cued more than others. However, somewhat surprisingly, our analysis of how participants distributed their choices over the four directions suggested that the vast majority (85%) of participants continued to distribute their choices more or less equally across the four directions even when they made a voluntary choice on most trials (see Figures 7–9). Thus, the significant gain in voluntary attention control that was observed in the direction selected condition appears to come with relatively little cost to experimental control (though see the discussion on selection history below).

The present study has focused on the extent to which self-reported ratings of agency can be influenced by combining the standard manipulation of top-down information with a novel manipulation of volition. As has been discussed, the present study has found that the proportion of negatively sloped growth curves can be increased when participants are allowed to freely choose (or not) the direction of the spatial cue (i.e., the direction selected condition) relative to the standard paradigm in which participants are only given top-down information without the option of controlling the direction of the spatial cue (i.e., the direction assigned condition). One issue that has not been addressed in the present study concerns the extent to which other manipulations of volition might decrease the proportion of negatively sloped growth curves when combined with top-down information relative to the standard paradigm. Although Gibson et al. (2023) noted that those individuals who generated positively sloped growth curves in the standard paradigm might have been reacting to the extrinsic nature of the spatial cues, this interpretation was only adopted by approximately 40% of the participants. One manipulation of volition that might emphasize the lack of intrinsic control more strongly is to provide participants with a direction selection display at the start of each trial, but unlike the direction selected condition in the present study, this volition manipulation would require participants to change the direction of the cue in compliance with an externally controlled instruction (see e.g., Experiment 3 in Huffman and Brockmole, 2020). Demonstrating the ability to increase as well as decrease the sense of agency in this task would strengthen our understanding of voluntary attention control in this task.

Existing theories of attention control have typically assumed that top-down and voluntary forms of attention control are largely synonymous, and they have not considered the possibility that voluntary control processes might have consequences for behavior that are distinct from top-down control processes. We believe that such consequences could be explored by examining the relation between agency ratings and performance measures such as RT. Notice that in such an analysis agency has gone from being an outcome variable (as in the present study) to being a predictor variable. In addition, agency would be a "time-varying" predictor variable because each participant has contributed four agency ratings—one rating after each of four blocks—for each of the three levels of conditional target entropy. As discussed below, each individual's average agency rating, as well as the block-by-block fluctuations around this average, can be useful for predicting different aspects of the RT effect.

In one preliminary analysis, Gibson et al. (2023) showed that the agency growth curves were not significantly related to the corresponding RT growth curves across individuals. However, there are other, potentially more sensitive analytical techniques that might be better able to reveal the nature of this relation. We have decided to treat this issue in a separate article because we believe that successful adjudication of this issue will require the introduction of analytical techniques, such as those that enable the disaggregation of between-person (BP) and within-person (WP) effects (Curran and Bauer, 2011; Wang and Maxwell, 2015), that are well established in developmental psychology, but which are still unfamiliar to those researchers working in the cognitive sciences (Note: BP and WP effects should not be confused with between-subjects and within-subjects experimental designs).

The distinction between BP and WP effects reflects a potentially important distinction because these effects often vary not only in magnitude, but also in direction. In fact, these two effects have been shown to be equivalent only under very restricted conditions that are rarely met in psychology (Molenaar, 2004; Molenaar and Campbell, 2009). Moreover, both of these BP and WP effects could be different from group-level effects. Indeed, group-level RTs in the direction selected condition were found to be significantly slower than group-level RTs in the direction assigned condition (see Figure 2), suggesting that increases in agency might slow RTs. However, contrary to this conclusion, it is likely that increases in agency would actually result in faster RTs.

For example, consider the 1.00-bit (100%-valid) cue condition. In general, BP effects would reflect the extent to which persons who report higher average agency ratings in this conditional target entropy condition also have faster RTs than persons who report lower average agency ratings; whereas WP effects would reflect the extent to which any given person has faster RTs when they report higher levels of agency than when they report lower levels of agency. Moreover, we also expect greater alignment between top-down information and the sense of agency to have a greater influence on BP effects than on WP effects. That is, when top-down information is aligned with the sense of agency (as in the direction selected condition), then most participants will also report their strongest sense of agency in the 1.00-bit (100%-valid) cue condition. However, when top-down information is not aligned with the sense of agency (as in the direction assigned condition), then only approximately half the participants will report their strongest sense of agency in this condition whereas the other half will report their weakest sense of agency in this condition. Thus, although participants in the direction selected condition might have overall slower RTs than participants in the direction assigned condition, we expect larger decreases in RT as a function of increasing average agency ratings in the direction selected condition than in the direction assigned condition as a result of this greater between person alignment. In contrast, because WP effects are relative to fluctuations in agency around each individual's average rating, such effects may not vary across the two volition conditions.

The extent to which voluntary attention control processes might have unique effects on performance will require isolating this form of control from other forms of control such as top-down and experience-dependent attention control processes. Fortunately, isolating voluntary attention control from top-down attention control can be easily accomplished by examining the BP and WP effects of agency across the two volition conditions for each level of conditional target entropy (cue validity) separately. As such, any observed BP and/or WP effects could be uniquely attributed to voluntary attention control processes because the amount of top-down attention control would be equated across the two volition conditions.

In addition, isolating voluntary attention control processes from experience-dependent attention control processes should also be easily accomplished so long as selection-based experiences can be equated across the two volition conditions. For instance, one type of selection-based experience that is known to "prime" or speed RTs in various attention tasks is the repetition of target location on successive trials (for reviews, see Awh et al., 2012; Anderson et al., 2021). As was noted above, a small number of individuals ($N = 12$) in the direction selected condition did tend to choose the same cued direction across trials, which could facilitate the selection of information from that location and thus lead to faster RTs for these individuals. Indeed, a preliminary analysis indicated that overall mean correct RTs were approximately 120 ms faster for those 12 individuals in the direction selected condition who tended to choose the same cued direction across trials relative to the majority of participants who tended to distribute cued direction (either by choice or by default) across the four directions more equally, though it should be noted that this RT difference did not attain significance ($p = 0.19$) in the present study. Thus, examination of the BP and WP effects of agency on RTs must be confined to trials in which the location of the target is not repeated on successive trials to ensure that those effects are distinct from experience-based control processes. In addition, the design of the direction selection condition could also be easily modified to prohibit participants from choosing the same cued direction on successive trials.

In conclusion, previous research reported by Gibson et al. (2023) has suggested that top-down and voluntary attention control are aligned for only about half of the participants in the standard spatial cueing paradigm. The present study replicated the previous findings reported by Gibson et al. (2023) under conditions in which the standard manipulation of top-down information was conveyed by a number cue (as opposed to an arrow or onset cue). But more importantly, the present study also showed that the proportion of individuals who aligned top-down and voluntary attention control could be increased (up to approximately 90%) by combining the standard manipulation of top-down information with a novel manipulation of volition in a separate condition in which participants were given the opportunity to freely choose (or not) the direction of the spatial cue on each trial. Moreover, despite conceding control of cued direction (but not cue validity) to participants in this latter condition, most participants (85%) nevertheless distributed their direction choices equally across the four directions. These findings suggest that providing participants active control of stimulus parameters may be required to elicit a strong sense of agency (and voluntary control) in the laboratory.

Data availability statement

The datasets presented in this study can be found in online repositories. The names of the repository/repositories and accession number(s) can be found below: https://osf.io/rvd32/.

Ethics statement

The studies involving human participants were reviewed and approved by University of Notre Dame Institutional Review Board. The patients/participants provided their written informed consent to participate in this study.

Author contributions

BG conceived of and designed the study, performed the statistical analyses, interpreted the data, and wrote the manuscript. JT wrote the experimental software, acquired and organized the database, and read and approved of the submitted manuscript. SM contributed to the conception and interpretation of the statistical analyses, including how to communicate the relation between newer and older forms of statistical analyses. All authors made substantial contributions to the conception or design of the work, or the acquisition, analysis, or interpretation of data for the work, drafting the work or revising it critically for important intellectual content, provide approval for publication of the content, and agree to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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Placing willed attention in context: a review of attention and free will

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Attention is the ability to focus one's awareness on relevant events and objects while ignoring distracting ones. Laboratory studies of top-down voluntary attention commonly use predictive or instructional cues to direct attention. However, in real world scenarios, voluntary attention is not necessarily externally cued, but may be focused by internal, self-generated processes. The voluntary focusing of attention in the absence of external guidance has been referred to as "willed attention," a term borrowed from the literature on willed motor actions. In a fashion similar to studies of willed (self-initiated) actions, during willed attention, participants are given the freedom to deploy attention based on their own free choices. Electrophysiological studies have shown that during willed attention, ongoing neural activity biases willed attention decisions on a moment-to-moment basis as reflected in transient patterns of brain electrical activity that predict where participants will later choose to focus their attention. Brain imaging studies have revealed that compared to cued attention, willed attention involves additional frontal cortical structures, which interact with the classic attentional control networks of the human brain to produce a modified network organization for willed attention control. In this introduction to willed attention, we briefly review the fields of voluntary attention and self-initiated motor actions, in order to describe willed attention and its neural correlates as they relate to the broader concepts of attention and volition.

KEYWORDS

attention, volition, willed action, willed attention, free will

> "You can take a road that takes you to the stars now, I can take a road that will see me through."
>
> Nick Drake, 1972
> Lyrics from *Road*
> Pink Moon (Island Records, U.K.)

Introduction

To navigate the world, one must select *relevant* information from the senses based on behavioral goals, weigh options given prior experience, make decisions, and take actions. Selective attention has an essential role in this cascade of mental events by supporting the selection of salient and/or behaviorally-relevant inputs and outputs. Voluntary attention allows one to focus their mental effort on some sensory stimuli, while suppressing irrelevant or distracting information. In vision, this influence on sensory processing is manifested as improved detection, discrimination and perception of attended visual target stimuli

(Posner et al., 1980; Luck et al., 1994; Liu et al., 2009; Carrasco, 2018; Jigo and Carrasco, 2018; Carrasco and Barbot, 2019)—for a review, see Carrasco (2011)—and has been shown to result from neural changes early in visual cortical processing (Van Voorhis and Hillyard, 1977; Moran and Desimone, 1985; Corbetta et al., 1990; Mangun and Hillyard, 1991; Motter, 1993; Heinze et al., 1994; Kastner and Ungerleider, 2001; Briggs et al., 2013; Ghosh and Maunsell, 2021); for a review, see Battistoni et al. (2017).

Voluntary attention is a conscious process that is generally considered to be self-generated or "volitional." In laboratory studies of voluntary attention, it is common, however, to provide incentives or instructions that lead observers to voluntarily focus their attention. For example, in studies of spatial attention, it is common to use an arrow cue, or other symbolic cue, to indicate a to-be-attended location in space where behaviorally relevant stimuli might be presented, requiring a response (Posner, 1980, 2016). Such a cue carries meaning in the context of the task, and therefore the observer makes a voluntary decision in using the cue information to perform the task. But it has been appreciated since at least the 19th century writings of Helmholtz (1867) and James (1890) that attention can be allocated voluntarily without being externally cued, such as in real world settings where internal goals may be pursued volitionally without explicit external instruction.

In recent years, new experimental paradigms have been developed to investigate how voluntary attention can be self-generated by observers in the absence of external cues provided by investigators. These new paradigms permit free choices that hand over the decision about where to attend to the observers rather than via investigator provided attention-directing cues (Taylor et al., 2008; Hopfinger et al., 2010; Bengson et al., 2014). We termed this self-generated form of voluntary attention, *willed attention*, a terminology proposed in our 2015 paper (Bengson et al., 2015), and which is derived in part from prior work on intention and action that used the term "willed action" to describe self-generated motor acts (Lau et al., 2004b).

The goal of this review is to describe the current state of research on willed attention and to place it in context with the extant literature on voluntary attention and willed action. First, we will set the stage by briefly introducing current models of attention. Then, we will focus on voluntary attention, describing the experimental methods that have been developed to investigate voluntary attention. The body of the review will explain the innovations that have led to the investigation of willed attention, and how cued (instructed) attention and willed attention are both related and different from each other. We will also contrast willed attention to the literature on willed actions, a field which has dominated studies of intention, volition and free will (Passingham et al., 2010; Haggard, 2019; Seghezzi and Haggard, 2022; Uithol et al., 2023). Finally, we will offer some new directions for studies to further understand the mechanisms of willed attention.

Top-down vs. bottom-up attention

Attentional control processes are commonly categorized as either voluntary, implying top-down (internally-generated or *endogenous*) cognitive control, or involuntary or reflexive, indicating they are driven by bottom-up (external or *exogenous*) sensory inputs (Jonides, 1981; Muller and Rabbitt, 1989; Posner and Petersen, 1990; Corbetta and Shulman, 2002; Petersen and Posner, 2012; Katsuki and Constantinidis, 2014). Voluntary attention is said to be goal driven (Behrmann and Haimson, 1999; Fecteau et al., 2004), while reflexive attention is thought of as automatic (Jonides, 1981; Yantis and Jonides, 1984; Koch and Ullman, 1985; Muller and Rabbitt, 1989; Hopfinger and Mangun, 1998; Lupianez et al., 2004; Hopfinger and Maxwell, 2005; Hopfinger and Ries, 2005; Wyble et al., 2020). These processes, the top-down and the bottom-up, interact in everyday vision to provide humans with sophisticated abilities to pursue behavioral goals while also retaining the ability to orient and respond to novel events (Gaspelin and Luck, 2018; Theeuwes, 2018a).

Behavioral research has shown that voluntary and reflexive attention differ in important ways. Reflexive attention results in faster shifts of attention, which are shorter lived, and often followed by a period of inhibition, known as *inhibition of return*, whereas voluntary attention takes slightly longer to engage, and is more resistant to decay, interference and inhibition (Jonides, 1981; Tassinari et al., 1987; Muller and Rabbitt, 1989; Lupianez et al., 2001). Electrophysiological studies have shown that the effects of both voluntary and reflexive visual attention result in spatial-selective modulations of visual inputs that occur early in visual cortical processing (Mangun and Hillyard, 1991; Eimer, 1994; Hopfinger and Mangun, 1998, 2001; Hopfinger and Maxwell, 2005; Hopfinger and Ries, 2005; Hopfinger and West, 2006), but as the result of different neural control mechanisms.

Neuroimaging research has shown that voluntary and reflexive attention rely on distinguishable brain control systems (Mesulam, 1981; Corbetta and Shulman, 2002; Chica et al., 2013; Vossel et al., 2014). Corbetta and Shulman (2002) articulated the concept of two distinct attentional systems for voluntary and reflexive attention, the dorsal and ventral attention networks, respectively. The dorsal attention network (DAN) controls top-down spatial, feature, and object attention (Corbetta et al., 2000, 2005; Hopfinger et al., 2000; Giesbrecht et al., 2003; Liu and Hou, 2013), while the ventral attention network (VAN) is involved in bottom-up attention, detects salient events, and supports shifting attention from currently attended stimuli to potentially relevant rare events (Fox et al., 2006; Indovina and Macaluso, 2007; Corbetta et al., 2008; Geng and Mangun, 2011; Allan et al., 2020). How precisely the interplay between these two systems occurs from moment-to-moment remains to be fully understood, but most models argue for a close association in support of everyday behavior (Vossel et al., 2014).

The overarching framework of most voluntary attention models is the idea that voluntary attention is controlled by the DAN (Hopfinger et al., 2000; Wager et al., 2004; Woldorff et al., 2004; Slagter et al., 2006; Armstrong and Moore, 2007; Corbetta et al., 2008; Sylvester et al., 2009; Weissman et al., 2009; Asplund et al., 2010; Szczepanski et al., 2010, 2013; Gazzaley and Nobre, 2011; Tamber-Rosenau et al., 2011; Gregoriou et al., 2014; Battistoni et al., 2017; Zink et al., 2021); for reviews see Corbetta and Shulman (2002) and Miller and Buschman (2013). The DAN issues control signals (Corbetta and Shulman, 2002; Rajan et al., 2021), transmitted via top-down neural pathways, that bias activity in visual cortex (Luck et al., 1997; Chawla et al., 1999; Kastner et al., 1999; Hopfinger et al., 2000; McMains et al., 2007). This biasing leads to modulations of sensory inputs based on their task relevance, which ultimately results in selective perception (Moran and Desimone, 1985; Mangun and Hillyard, 1991; Treue and Maunsell, 1996; Buschman and Kastner, 2015; Battistoni et al., 2017).

The strong dichotomy of top-down control vs. bottom-up as reflecting volitional and non-volitional/automatic processes, respectively, is a useful heuristic, but we hasten to point out that current models are more complex, and consider situations where the context, past reward and other factors influence attentional selection independent of immediate goals, and sometimes in contradiction to them (Awh et al., 2012; King et al., 2012; Egner, 2014); for a review, see Baluch and Itti (2011). In recent years, the concept of *selection history* in attentional control and selection has been quite influential in this regard (Theeuwes, 2018a), and has generated much discussion in the literature (Egeth, 2018; Failing and Theeuwes, 2018; Gaspelin and Luck, 2018; Theeuwes, 2018b). We wish to simply highlight this point here and will return to it later.

Dissecting voluntary attentional control from selective perception

Voluntary attention has been studied using various experimental paradigms, including visual search (Treisman and Gelade, 1980; Luck and Hillyard, 1995; Lamy et al., 2004), flanker interference methods (Eriksen and Eriksen, 1974; Lavie, 2011; Gaspelin et al., 2014), Stroop interference tasks (Treisman and Fearnley, 1969; Aine and Harter, 1984; Egner and Hirsch, 2005), and cuing paradigms (Posner, 1978; Klein, 1980; Donovan et al., 2020). In particular, trial-by-trial attention cuing has proven to be a powerful method for investigating voluntary attention (Posner, 2016), because it enables the experimental and theoretical dissection of attentional control from attention selection of task relevant target stimuli (Hopfinger et al., 2001).

In typical cuing studies, an observer is presented with an attention-directing cue, which instructs or biases the person to prepare to process an upcoming task-relevant stimulus. In theory, cognitive-neural events following the attention-directing cue but occurring prior to the appearance of task-relevant target stimuli (and/or distractors), can be related to the various mental processes necessary for the top-down voluntary control of attention. In contrast, following the appearance of the target stimuli, processes related to sensory-perceptual processing, and any potential modulation of them by top-down attention, are expected. Thus, because the cue and target are separated in time by hundreds or thousands of milliseconds, so are the temporally-associated cognitive, sensory, and motor processes. In spite of some important constraints about the measures employed, especially for functional imaging studies (e.g., Das et al., 2023), this temporal segregation allows one to measure the cognitive-neural events for top-down attentional control separately from subsequent stimulus selection (and task-related motor processes).

In experimental psychology studies, for the most part, the activity of the cue-induced top-down attentional control activity must be inferred from the performance differences observed in the behavioral measures obtained in the task. These behavioral measures are the responses (speed, accuracy, etc.) to the target stimuli as a function of attention (Posner et al., 1980). Cognitive neuroscience methods, however, provide an additional opportunity to measure attentional control separately from stimulus selection because the brain activity to the attention-directing cues can be measured even though there are typically no behavioral responses to the cues themselves.

Using event-related potentials (ERPs) to capitalize on the temporal separation of cues and targets in an attentional cuing study, Harter et al. (1989) derived measures of brain electrical activity following attention-directing cues separately from the subsequent target stimuli. They found characteristic ERP waveforms over the scalp following the cue and preceding the target. The cue-related responses included the sensory-evoked activity evoked by the physical features of the cues themselves, of course, but also included non-sensory cognitive responses that were related to the attention instructions. They interpreted the non-sensory cognitive ERPs as being related to the top-down (voluntary) control of attention, which included both the top-down signals themselves, and the differential biasing of cortical excitability in sensory-specific cortex. This work was replicated and extended in many studies (Hopf and Mangun, 2000; Eimer et al., 2004; Kelly et al., 2009; Seiss et al., 2009; Green and McDonald, 2010; Hong et al., 2015). Following this logic, fMRI studies have been able to isolate cue-related from target-evoked activity. In a pair of papers published side-by-side in the journal *Nature Neuroscience*, Corbetta et al. (2000) and Hopfinger et al. (2000) investigated the neural correlates of top-down voluntary visual spatial attention, showing distinct brain activity for voluntary control and stimulus selection and motor action (see also, Kastner et al., 1999). These ERP and fMRI studies, and the many to follow using this logic, punctuate the theoretical distinction between voluntary control and stimulus selection and how they can be separately measured and studied. Studies using cuing methods to investigate voluntary attentional control have flourished over the past 25 years, and the paradigm has become a gold standard methodology (Corbetta and Shulman, 2011; Fiebelkorn and Kastner, 2020; Posner and Rothbart, 2023).

Cued attention vs. free choices: willed attention

The use of attention-directing cues to study voluntary attention is a widely accepted method that is rarely questioned. Indeed, it is clear that within the context of these types of laboratory studies, subjects do exert voluntary attention in response to the cue, because such cues alone do not necessarily direct attention reflexively, but instead require the participant to focus attention as specified in the instructions provided by the investigator with respect to the cues and task goals (see, however, Ristic et al., 2007). Nonetheless, some have asked what it means to be voluntary if the act to be undertaken is somehow signaled (cued) from outside the individual observer/actor. We take the position that there is a spectrum of volition, such that depending on the circumstances, an individual may rely fully on self-generated decisions and action, or may biased by external circumstances, such as cues. So, being instructed to pay attention to a stimulus by an external cue certainly requires volition, but if it does not involve a free choice by the observer, should it be considered *fully* voluntary? This question prompted the first studies of what we now refer to as willed attention.

Taylor et al. (2008) investigated willed spatial attention by using fMRI to reveal the underlying brain activity when observers were either cued or were permitted to choose where to attend. The study was otherwise a standard visual-spatial cued attention task, but on some trials the subjects were simply prompted to make a free choice between attending a left visual field location or a right one in order to discriminate a target stimulus at the attended (chosen) location. Hopfinger et al. (2010) used a similar experimental design, including a condition which eliminated attention directing cues, allowing free attention choices by the observers. Both studies found key differences between the brain regions activated for free choice and cued attention. While there was significant overlap in the brain activity engaged for cued vs. willed attention, crucial differences, largely in frontal cortex, made clear that during willed attention there were additional brain regions engaged. These seminal studies are highly relevant to our understanding of purely voluntary (willed) attention, which is arguably critical in natural settings where internally-generated cognitive and motor acts are fundamental to goal-driven behavior (Pezzulo et al., 2018; Turner et al., 2019); for a review, see Tsotsos et al. (2021).

The foregoing fMRI studies focused on the brain activity patterns related to attention decisions and attentional control by investigating activity related to the attention-directing cues or prompts to choose where to attend; that is, time locked to the cues/prompts. However, because fMRI relies on measures of brain hemodynamics, which are sluggish and indirect measures of neural activity (Burock et al., 1998; Glover, 2011; Marxen et al., 2023), it is difficult to know the precise time course of the observed activity, and therefore, when it occurs with respect to decisions about how to focus attention. In order to measure the rapidly unfolding neural activity associated with willed attention, we entered the fray by using EEG, ERPs, and behavioral measures (Bengson et al., 2014).

In the EEG study of Bengson et al. (2014), we used a paradigm similar to those by Taylor et al. (2008) and Hopfinger et al. (2010), where we contrasted the brain signals recorded between conditions in which cues instructed subjects where to selectively attend on a trial-by-trial basis (left or right), or in which a *prompt* signaled that the participant was to spontaneously choose where (left or right) to attend on that trial (i.e., choose trials). That is, on the choose trials, although the time point of the focusing of attention was specified by the appearance of the prompt, whether to focus attention selectively on the left or right visual hemifield location was determined by the participants' free choices. The paradigm is illustrated in Figure 1: there were three stimuli used as instructive cues or prompts: one stimulus (cue) instructed the participant to attend left, another instructed then to attend right, and a third (the prompt) signaled the participant to choose whether to attend left or right on that trial. Following the cue or prompt, a sine wave grating stimulus would appear in the left or right hemifield location, and subjects would have to discriminate the spatial frequency of the grating. The task was a 100% attentional allocation task, meaning that participants would attend to and respond only to the target if it was in the cued (or chosen) hemifield; otherwise, they ignored the grating stimulus.

Before turning to the EEG results, we must take a quick detour to describe a behavioral control study critical for this work. Because the task required subjects to only attend and respond to the cued or chosen-location targets, no standard behavioral measures of selective attention were available in this design; that is, there were no reaction time measures for the uncued/unchosen-location targets (e.g., Posner et al., 1980). This was deliberate because we wanted to avoid inducing the subjects from dividing their attention in the task. Thus, in order to validate the paradigm and determine whether the behavioral attention effects for cued and willed attention were similar or different, we ran a behavioral-only version of this study where participants were told to respond to targets in both the cued/chosen location and in the uncued/unchosen location. To avoid this being simply a divided attention task, they were told to focus covert attention fully on the cued/chosen location on each trial, but to quickly switch to and respond to the uncued/unchosen targets when they appeared. Note, that there was no probability manipulation of cue-target expectancy—a common method in such designs (Posner, 1978; Klein, 1994; Chen et al., 2023)—because such a manipulation was not logically possible in the choose condition. We observed highly significant attention effects in this task, where overall, the cued/instructed and chosen location targets were responded to significantly faster than were targets at the uncued/unchosen location ($p < 0.01$; Figure 2). This reaction time spatial attention effect was not statistically different

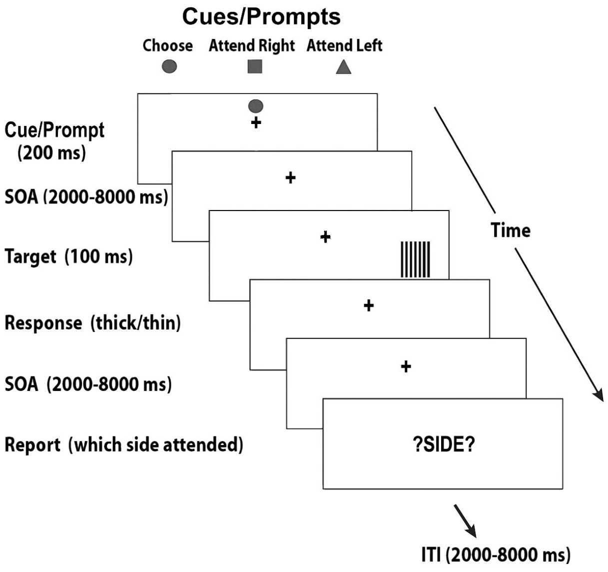

FIGURE 1
Example trial from the willed attention study of Bengson et al. (2014). Three different cues/prompts were used, and these are shown at the top of the figure. In this example, the trial is a willed attention trial, where the subject had to choose whether to attend right or attend left upon receiving the somewhat unexpected prompt (circle). Shown is a target presented to the right visual hemifield, but targets could be in either visual hemifield with 50/50 probability. If the subject would have chosen to covertly attend right on this trial, then they would have been required to discriminate the spatial frequency of the target grating (thick vs. thin bars) and to report that with the appropriate button press. At the end of the trial, the Report query appeared (?SIDE?), and if they had chosen to attend right, then they would push a button indicating their choice as "attended right." If on this trial they had chosen to attend left, then they would have ignored the right visual hemifield target entirely and would only press a button to report "attended left" to the query (adapted from Bengson et al., 2014 with permission from MIT Press).

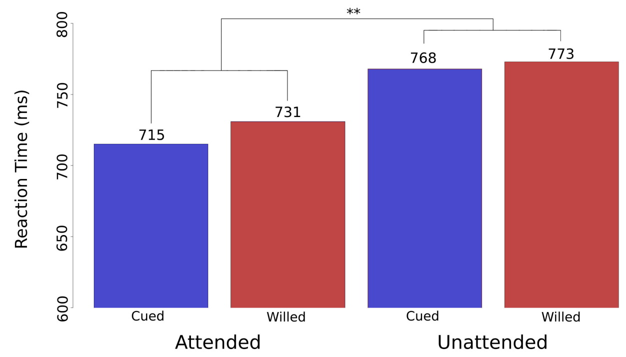

FIGURE 2
Reaction time attention effects for cued vs. willed spatial attention. Reaction time data plotted as bar graphs from the behavioral validation experiment of Bengson et al. (2014). The bar graphs show RTs for cued trials (blue) and willed trials (red). The effect of spatial attention (attended vs. unattended) was significant overall at $p < 0.01$ (as represented by '**' on the figure). There was no significant interaction between attention type (cued vs. willed) and the attention effect (attended vs. unattended), which demonstrated that at the level of behavioral performance, willed attention and cued attention produce the same performance outcomes for the target stimuli.

for cued and willed attention. This is similar to the result of Taylor et al. (2008), who compared cued/chosen-location target performance with performance in a separate divided attention condition, finding no differences in the benefits of focal attention between cued and willed attention conditions. These effects are in line with the large literature on attention's effects on reaction times (for a review, see Posner, 2016), and demonstrate that at the level of behavioral performance, cued and willed attention both result in selective processing of target stimuli, which imparts an advantage to attended-location events.

Now, we return to describe the novel EEG findings in Bengson et al. (2014) study. Using electrophysiology, we had a unique opportunity to investigate not only the post-cue/prompt brain activity during cued vs. willed attention, but also the brain activity patterns prior to the appearance of the prompts. This permitted us to investigate the antecedent brain states associated with different free choices made by the participants in response to the prompts, and to reveal the time course of any activity related specifically to willed attention; this analysis was inspired by the literature on motor intention and self-initiated movement that we will review in a later section.

Our analyses focused on EEG oscillatory activity, especially in the alpha band (8–13 Hz), because these signals had previously been shown to be related to focused spatial attention (e.g., Worden et al., 2000). Quantifying EEG alpha backwards in time from the onset of the prompts, we looked for differences in alpha power as a function of whether the subjects later chose to attend left or attend right. We found that willed attention was associated with a unique predictive pattern of alpha-band power immediately preceding the unpredictable onset of a choose prompt. This alpha-band activity was localized primarily over left parietal-occipital scalp, and predicted the upcoming decision to attend before the subject could be aware that they had to make a decision (see more on this point in the next paragraph). The predictive EEG alpha pattern was restricted to approximately 1 second prior to the unexpected appearance of the prompts (Figure 3A, left). These results indicated that ongoing neural variability in the alpha-band was correlated with attentional decision making on a trial-by-trial basis. The interpretation we offered in the Bengson et al. (2014) paper was that there were stochastic fluctuations in the patterns of brain activity that were correlated with, and therefore predicted, whether a person would choose to attend left or attend right; we wrote, "…the appearance of free will, as manifested through seemingly arbitrary cognitive decisions, may be a consequence of the role that inherent variability in brain activity plays in biasing momentary behavior." This pre-prompt neural activity is reminiscent of that observed in studies of free choice decision making in motor actions (Libet et al., 1983b; Haggard and Eimer, 1999; Lau et al., 2006; Soon et al., 2008), but differs in that it is preceding decisions regarding covert cognitive functions; i.e., the decision about how to selectively focus covert visual spatial attention. The relationship of our willed attention work to motor intention studies will be discussed in a later section.

Important design characteristics need to be acknowledged in the Bengson et al. (2014) study in order to make clear why we asserted that the pre-prompt predictive EEG alpha activity was not a reflection of a predetermined decision by the subject, but rather a stochastic brain state. That is, we argue that the subjects made a spontaneous decision about where to attend at the time they perceived the choose prompt. First, there was only a 33% chance that the prompt would appear as opposed to an instructive cue, and thus the subjects would not have strong motivation to adopt a strategy of preparing for a choose trial and picking a side to attend in advance. Put another way, two-thirds of the time they were going to receive an instructive cue, which would render any predetermined decision about where to attend moot; we verified this by post-experimental debriefing of the subjects who reported that they did not decide in advance where to attend because most of the time they received an explicit instructional cue. Second, and more critically, the experimental design also made it impossible for the participants to estimate the time of arrival of the cue or prompt with any precision because of the highly variable inter-trial interval, which varied randomly over the range from 2 to 8 seconds. Essentially, the subjects would have had to have been clairvoyant in order to make an advance decision that was restricted to the 1,000 msec just prior to the unexpected prompts.

In addition to the *pre*-prompt predictive EEG activity, we also observed *post*-prompt ERP and EEG activity that differed from that during cued attention (reported in Bengson et al., 2015). Immediately following the prompts (compared to the instructive cues) we observed two unique ERPs associated with willed attention. The first had a frontal scalp distribution in the time period of 250 to 350 msec post-prompt; we called this the Early Willed Attention Component (EWAC). A second ERP sign of willed attention occurred between 400 and 800 msec post-prompt over central scalp sites, which we called simply the Willed Attention Component (WAC). Thus, willed attention was associated with unique signs of brain activity both before choose prompts (the predictive EEG alpha) and after (the EWAC and WAC), as subjects made decisions about where to attend. Note that the pre-prompt activity is the predictive EEG alpha with respect to the subsequent decision, not a contrast of cued and willed attention, while the post cue/prompt

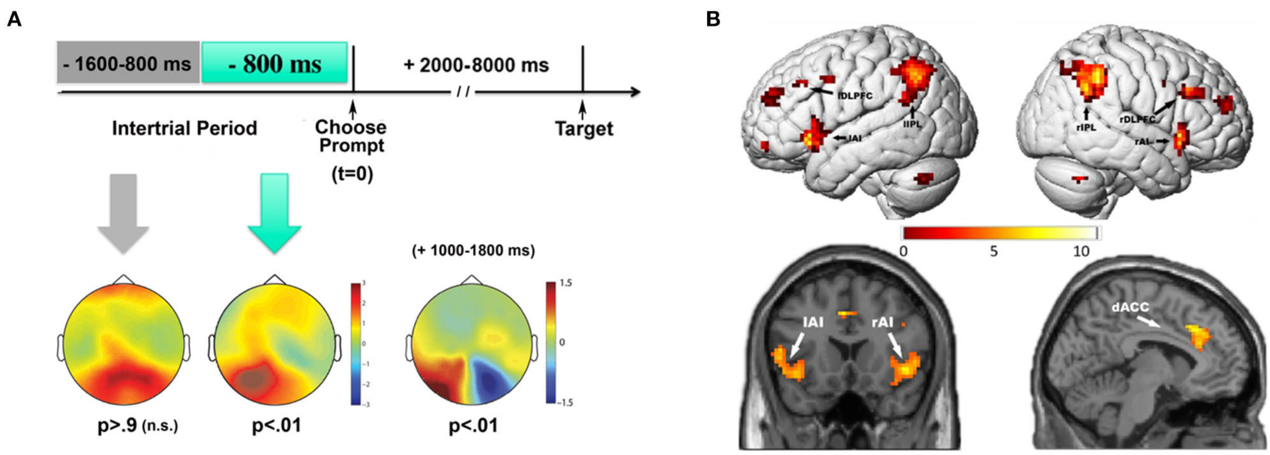

FIGURE 3
EEG and fMRI measures of willed attention. **(A)** Pre-prompt and post-prompt alpha power topographic maps for choose left vs. choose right trials (adapted from Bengson et al., 2014 with permission from MIT Press). Time = 0 msec is the onset of the choose prompt, indicating that the participants should choose whether to attend left or attend right for that trial. Predictive patterns of EEG alpha power were only observed during the 800 msec prior to the onset of the prompt, while earlier in time there was no significant EEG signals predicting the subsequent choice. *After* the prompt, in the prompt-to-target interval, the decision to attend left vs. right led to the well-known post attentional orienting occipital alpha lateralization (+1,000–1,800 msec). **(B)** Contrast of willed to cued (instructed attention) (adapted from Bengson et al., 2015, dataset; see text).

ERPs are revealed by the difference between willed and cued attention trials.

In addition to these unique electrophysiological signs of willed attention, we also observed EEG and ERP effects that were the same for cued and willed attention. Several 100 msec after the cues and prompts, the well-known occipital alpha asymmetry with spatial attention was observed (Figure 3A, right); i.e., there was a reduction of alpha power over the occipital scalp contralateral to the direction of spatial attention. Finally, the subsequent target-evoked visual ERPs showed standard spatial attention modulations in the amplitudes of early sensory ERP components (Bengson et al., 2014). We will not discuss these EEG/ERP findings further here, and instead refer the reader to the published studies (Bengson et al., 2014, 2015); however, the bottom line is that cued and willed attention result in significantly different attention-related neural activity in and around the time of cue/prompt appearance, while later in time, the brain activity for cued and willed attention are virtually identical, suggesting that the final neural outcome of cued and willed attention on brain activity and behavior (as described above) are the same.

Neural correlates of willed attention

In the foregoing, our aim was to establish the experimental and conceptual framework for the concept of willed attention, and to describe the electrophysiological evidence for the similarities and differences between cued and willed attention. In this section we will review what is known about the underlying neural mechanisms of willed attention and how they may differ from cued (instructed) attention. Several reports have helped to elucidate the underlying neural correlates of willed vs. cued attention. The literature on willed attention remains rather small, and much of the work comes from our group, which we will make clear as we survey the available evidence. In the following, we will focus primarily on the activity following the prompts and cues that elicited attentional orienting, rather than the antecedent brain states described in the study by Bengson et al. (2014). The primary reason for this is because, unlike the EEG/ERP measures, fMRI data has poor temporal resolution, making it challenging to know whether cue/prompt-related activity occurs immediately prior to or after the cues or prompts. Although we believe there are methods that may be applied to imaging data to help mitigate this limitation, and other methods such as magnetoencephalography (MEG) could be helpful, the studies to date have not applied such tools to investigate willed attention.

In an fMRI version of the study of Bengson et al. (2014), using the same experimental paradigm in the same volunteers, we investigated the brain networks supporting willed attention (Bengson et al., 2015). This multimodal method permitted us to relate the EEG/ERP data described above to the underlying brain functional anatomy (e.g., Mangun et al., 2000). By contrasting instructed and willed attention, this work showed that during willed attention, additional neural activity could be detected in the frontal cortex in the dorsal anterior cingulate cortex (dACC), middle frontal gyrus (MFG), and the anterior insula (AI) (c.f., Taylor et al., 2008). These were in addition to activity in the DAN, which were common to willed and instructed attention (Taylor et al., 2008; Hopfinger et al., 2010). In related work from the lab of the late Steve Yantis (Gmeindl et al., 2016), frontal activations, especially in dACC and MFG were again shown for willed attention in a different task (rapid serial visual search paradigm), and using multivoxel pattern analysis (MVPA) decoding methods. All these studies point to key frontal brain regions that are activated when subjects orient attention in a self-generated manner, and these include the dACC, MFG and anterior insula.

We subsequently replicated the fMRI study of Bengson et al. (2015) with our colleagues at the University of Florida. Using both datasets, we investigated new questions about the mechanisms of willed attention using graph-theoretic analysis (Liu et al., 2017). In this work we investigated possible functional network differences between instructed and willed attention. Three well known cortical networks were identified as supporting willed attention: the cingulo-opercular network, the dorsal attention network (DAN) and the frontoparietal network (He et al., 2007; Dosenbach et al., 2008). By comparing the network architecture for willed and instructed attention, we discovered that the dorsal anterior cingulate cortex (dACC) changed its network allegiance from being clustered with the dorsal attention network during instructed attention to becoming associated with the cingulo-opercular network during willed attention. This shift in dACC alignment for instructed and willed attention demonstrates its central role in both forms of attention, but via different functional network associations. That is, during willed attention, the cingulo-opercular network acts to mediate communications between the frontoparietal network and the DAN. This rather remarkable finding of a change in the dACC network allegiance for willed attention suggests a key neurobiological difference between instructed and willed attention. What does this network organization suggest about willed attention?

The dACC has long been implicated in attention (Mesulam, 1981; Bench et al., 1993; Nobre et al., 1997; Posner and Rothbart, 1998; Weissman et al., 2005; Walsh et al., 2011), cognitive control (Botvinick et al., 2001; van Veen et al., 2001; Kerns et al., 2004; Schulz et al., 2011), action initiation (Srinivasan et al., 2013), motivational value (Yee et al., 2021), and volition (Winterer et al., 2002; Nitschke and Mackiewicz, 2005). While many have bemoaned the fact that the dACC activates in many tasks, its role in attention, cognitive control, action initiation, motivational value, and volition form a common thread that suggests the dACC is relevant when an organism must make task-relevant decisions and actions. Willed attention fits well within this framework, as the subjects in willed attention tasks are asked to make free choice decisions and then act on them by selectively focusing their attention. The graph-theoretic network analysis demonstrates how the dACC may play a role in self-generated acts like our willed attention task by serving as a mediating influence on executive functions of the frontoparietal network and the top-down control ultimately exerted on sensory processes by the DAN (Meehan et al., 2017; Ray et al., 2020).

Further evidence for this network-level organization comes from analysis of the EEG data gathered in our willed attention design, which again included two datasets (UC Davis and University of Florida). Analyzing the spectral context of the EEG signals following the instructive cues vs. the choose prompts revealed significant difference in frontal theta signals (3–7 Hz) such that there was increased theta power during willed attention, arising 500 msec after the free choice about where to attend (Rajan et al., 2018); see also Bengson et al. (2020) for a replication of the increase frontal theta with willed attention. Using Granger causality analyses (Ding et al., 2006; Dhamala et al., 2008a,b)—a method to quantify the strength and directionality of brain signals—we showed that the increase in frontal theta power was paired with increased frontal-to-parietal scalp theta-band coherence, and bidirectional Granger causality. We interpret this enhanced bidirectional influence reflected in theta EEG to mean that during willed attention, frontal cortical structures and associated networks transmit information regarding the decision about where to attend to key nodes in the attentional control system (DAN), but also receive information back, perhaps to update representations of the current state of focused attention (Heekeren et al., 2008).

In addition to the increased frontal theta following free choice decisions during willed attention, as noted earlier (see Figure 3, right), we have also observed the well-known occipital EEG alpha (8–13 Hz) asymmetry with lateralized spatial attention (Worden et al., 2000). In standard cuing studies, this alpha asymmetry takes the form of reduced alpha power over the occipital scalp contralateral to attended visual hemifield, and a relative increase in the hemisphere ipsilateral to the attended field (Thut et al., 2006; Rihs et al., 2007; Romei et al., 2010; Popov et al., 2019), and is hypothesized to reflect inhibition of upcoming unattended inputs in the *Gating by Inhibition Model* of alpha (Jensen and Mazaheri, 2010). We demonstrated that the occipital attention-related alpha asymmetry was also present following willed attention decisions (Bengson et al., 2014), indicating similarities in the downstream effects of orienting and focusing spatial attention, which might be expected theoretically (see also, Trachel et al., 2015). That is, once a voluntary decision to focus spatial attention has been taken and initiated, the chain of cognitive-brain processes for focused spatial attention are largely the same following instructive cues and prompts to make free choices; as described earlier, this involves activity in the DAN (reflecting the top-down issuing of control signals for selective attention), biasing of sensory cortex by this control (reflected in baseline shifts of neuronal activity, and the occipital EEG alpha asymmetry), and subsequent selective sensory processing of attended inputs (seen as increased firing rates and ERP amplitudes for attended stimuli, we well as facilitated behavioral performance). However, as might be expected, the post-prompt occipital alpha asymmetry is delayed for willed attention compared to cued attention hundreds of milliseconds, and it appears after the frontal theta power increase that occurs post prompt (Rajan et al., 2018; Bengson et al., 2020). So, during willed attention, volitional free choice decisions about where to attend (willed attention)—arising (presumably) from frontal cortex and correlated with increased frontal theta and increased theta coherence and connectivity—result in top-down control signals from the DAN that bias the visual cortex, as reflected by the occipital alpha asymmetry, leading to selective visual processing. This cascade takes longer than for cued (instructed) attention, where the free choice decision component is largely eliminated, thus allowing for more expeditious implementation of attention-related biasing of visual cortex. Or to put it more simply, the free choice decision takes more time than acting in response to an attention-directing cue.

Finally, one may ask whether the prompts to make free choice decisions themselves might also introduce artificial or confounding circumstances, further concealing or distorting the operations of purely voluntary (self-generated) attention. Indeed, as an aside, this may be a more general concern when using cues or prompts. For example, some cuing studies on voluntary attention have come to different results when different types of cues were used (Antonov et al., 2020; Gundlach et al., 2020). Moreover, even rather simple arrow cues have been shown to engage attention in a fashion more akin to reflexive attention, due to their overlearned nature in humans (e.g., Ristic et al., 2007). So, in a recent study of willed attention, we took a further step by completely eliminating any cues or prompts (Nadra et al., 2023). The idea was that if willed attention reflects purely volitional processes, then its properties and mechanisms should be similar even when allocating attention without any cues or prompt, as may sometimes occur in natural vision (c.f., Gmeindl et al., 2016). Thence, with the goal of developing a laboratory-based experimental paradigm that was a step in the direction of natural vision, we left the decision about both when and where to focus attention to the participants. The experimental display contained two dot motion arrays (one on each side of fixation) consisting of red dots in the center and blue dots in the surround (Figure 4). The red dots gradually increased and decreased over time, with both dot motion fields uncorrelated with each other. Participants were instructed to keep their eyes on a central fixation cross for the duration of each trial. On each trial, the dot motion arrays would appear, and the subjects' task was to wait a moment, and then at a time of their own choosing, select a side to attend and then immediately voluntarily shift and focus covert attention on the left or right stimulus patch. They were then to covertly monitor the attended patch (ignoring the opposite field patch) in order to detect when the proportion of red dots in that patch reached its maximum, pushing a button in response. As in all our studies of willed attention, they were told not to use any explicit strategy or develop any pattern for choosing when or which side to deploy covert attention (such as alternating sides on each trial), and to not decide prior to trial onset which hemifield patch to attend. In other words, once the bilateral array appeared, the subjects were requested to make a spontaneous decision about when and to which side to focus covert spatial attention. Responses were made with their right hands, pushing the left arrow on a keyboard with their index finger if they had chosen to attend left on that trial, or pushing the right arrow with their middle finger if they had chosen to attend right. Thus, the button responses not only signaled the time of their detection of the target, but also provided evidence as to where they attended on that trial. Using machine learning MVPA decoding of EEG alpha power, we could identify the attended hemifield, using the button response as a time stamp and indication of the side attended. We found that the occipital attention-related alpha power asymmetry observed in cued and willed attention was also present in this uncued study, and that it preceded the motor responses by about 1 second. By eliminating explicit cues or prompts that influence the allocation of voluntary attention, this study helps to advance our understanding of willed attention, and more generally of voluntary attentional control.

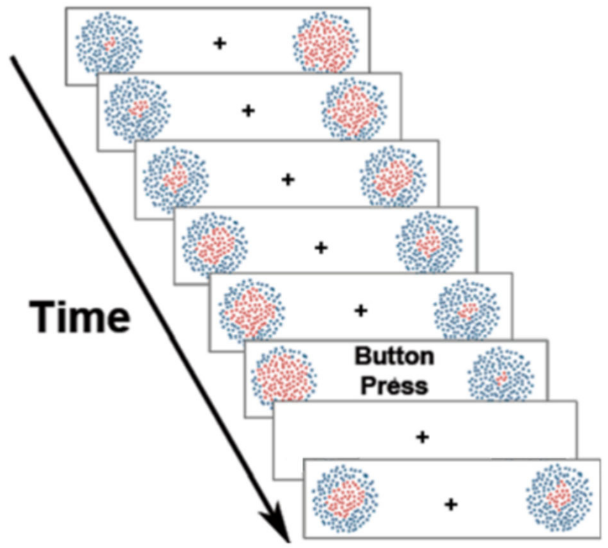

FIGURE 4
Uncued/unprompted willed attention paradigm. Participants were told to fixate the central cross, and then to spontaneously choose whether to covertly attend left or right at a time of their choosing (adapted from Nadra et al., 2023). See text for description.

Intention, volition, and free will in willed action

Intention, volition and free will are concepts with long histories, sometimes involving contentious debate. So, before using these terms in interpreting willed attention in a larger framework, we will begin with some simple, well-accepted definitions in order to clarify how we will use the terms. *Intention* can be defined as the mental state in which a future action (cognitive and/or motor) is represented in the mind and brain. Here, we consider only immediate intentions, not distant intentions (e.g., such as planning to earn a Ph.D.), and with this in mind, turn to some helpful thoughts by Pacherie and Haggard (2011). They suggested that there are two characteristics of immediate intentions, and that they are: "…accessible to consciousness," and "…they bear some relation to subsequent action." Essentially, intentions are the mental representations of a behavioral goal that are activated to execute the goal about which we are aware. *Volition* can be operationalized as a process that: (i) is internally generated (as opposed to stimulus driven), (ii) involves decisional control (as opposed to being reflexive or habitual actions), (iii) is goal directed, and (iv) is accompanied by the conscious experience of intending to take the action (Haggard, 2019; Seghezzi and Haggard, 2022). *Free will* is often used synonymously with the term volition, and therefore the preceding characteristics of volition would apply to free will as well (Lavazza, 2016). Although the literature on free will is expansive, we will stop here, and thus, in the context of our review of willed attention, we will use the terms volition and free will synonymously. One might ask why use the term free will at all (Gazzaniga, 2012; Shadlen and Roskies, 2012), but given that in our tasks, subjects are asked to make free choices (and we have used the term *willed* attention), it would seem natural to refer to free will. However, in doing so, we are most decidedly not suggesting that volition or free will are brain-independent mental functions. Rather, we hope to make clear the opposite, that willed attention relies on brain mechanisms, mechanisms that can be studied and understood. Our goal, therefore, in the following is to relate willed attention to the larger psychology and neuroscience literature on willed actions, where considerations of intention, volition and free will are commonplace. We will focus on prominent studies that have identified neural correlates of free choices, beginning with the work of Benjamin Libet.

Libet approached the concepts of intention, volition and free will by recording brain electrical activity preceding voluntary motor actions. He found that motor readiness potentials (ERPs) preceded conscious intentions to make a self-generated movement by up to half of a second (Libet et al., 1983a; Libet, 1985); how did he accomplish this? Libet's highly influential studies employed a novel paradigm where subjects were facing a clock which moved once every 2.56 seconds. The experimenters collected both EEG data as a measures of brain activity, and EMG data to track their muscle activity as well as self-reports about the time of their intentions to move (from the clock face). Subjects were told to make a spontaneous movement while looking at the clock, flexing their wrist at a time of their own choosing. The subjects were also instructed to report the position of the clock when they first became aware of their decision to move, and this reported time was used as a measure of the *onset of conscious intention.* The *onset of the willed action* was signaled by the onset of electromyographic (EMG) activity in the wrist flexor muscles (as measured by electrodes on the forearm), and the *onset of the intention to act* was pinpointed by the onset of the ERP known as the readiness potential (RP) (Kornhuber and Deecke, 1964, 2016). Libet's key finding was that the RP preceded conscious awareness of the intention to move (indexed by the clock time reported by the subjects) by 500 to 1,000 msec, which suggested that the conscious decision to move one's hand had a neural antecedent that preceded the conscious *intention* to move. Libet's breakthrough work opened the floodgates on free will and agency as a research topic in neuroscience (David et al., 2008; Moore and Obhi, 2012; Wolpe and Rowe, 2014; Braun et al., 2021).

Libet's studies, were however, often challenged and criticized. For example, the nature of the RP itself, and therefore what it tells us about volition in Libet's paradigm has been questioned. There is evidence that the RP may not be the driving force behind the decision to act, and therefore whether it is involved in volitional decision making, or only related to the act of simply being presented with a decision, is at issue (Balaguer, 2014). Haggard and Eimer (1999), for example, investigated the RP and a derived ERP measure known as the *lateralized* readiness potential (LRP), which is computed by subtracting the RP recorded at electrodes contralateral and ipsilateral to the moving hand. They concluded that it is the LRP, which develops later in time, rather than RP, that is an index of conscious motor intention. Subsequently, in a joint article, Haggard and Libet (2001) proposed that the RP was an index of the general preparation to act, whereas the LRP reflected the onset of an intention to make a specific action in the brain. Thus, the original model of Libet was refined, but the key underlying patterns of brain activity in the Libet model were nonetheless replicated, supporting his original interpretation that brain activity preceded the conscious intention to act.

There also arose a notable hypothesis that perhaps free will does not exist in the way humans intuitively believe, but that it is more related to a veto mechanism that can override an action that was initiated unconsciously in the brain. Libet referred to this mechanism as "free won't," postulating that the brain acts automatically in the generation of actions, but our will acts as an inhibitory process that guides our decisions based on what not to do, rather than by initiating a voluntary action (Libet, 1985). Our power to inhibit processes that may be underway can be implemented up until a point of no return, where actions are unable to be vetoed. Decisions to act, therefore, are hypothesized to have three separable components, *what* action to take, *when* to make the action, and *whether* to make the action—the concept of "free won't" affects whether a decision is made or not, and acts as Libet's primary argument against *determinism*: the idea that neural events, rather than free will, determine our actions (Lavazza, 2019). However, there has also been some evidence against this idea. EEG activity immediately (−150 msec) preceding decisions to act or not to act have similar patterns in the EEG, thus suggesting that similar antecedent brain states are involved in both cases (Filevich et al., 2013). This argues against the idea offered by Libet and others that free will may better be conceived of as the voluntary inhibition of motor intentions that arise by unconscious brain activity. Filevich et al. (2013) wrote: "the cause of our 'free decisions' may at least in part, be simply the background stochastic fluctuations of cortical excitability… free won't may be no more free than free will" (see also Shadlen and Roskies, 2012).

Measures of the unconscious determinants of free decisions in humans is not limited to EEG. In fMRI work, brain activity in the anterior medial prefrontal and medial posterior parietal cortex were shown to encode a decision to make a button press (left or right) up to 10 seconds before the subjects were aware of the intention to move (Soon et al., 2008). Their paradigm consisted of a stream of letters presented on a computer monitor (one every 500 msec), and the subject's task was to report which letter was on the screen at the time they made the decision to move. As in Libet's original studies, the time of the self-report of awareness to move was taken as the onset of motor intention, and significant brain activity built up in the time prior to this awareness of motor intention. Thus, there is converging neuroscience evidence for predictive brain activity during self-generated motor actions.

Additional evidence also comes from studies of free choices in both humans and animals (for a review, see Funahashi, 2017). Free choice decision making studies in humans commonly cite the prefrontal cortex as the primary driver in intentional actions, but not instructed actions (Lau et al., 2004a). Primate studies have consistently shown that prefrontal neurons can predict an upcoming decision to act when the animals are presented with a free choice decision (Marcos and Genovesio, 2016; Mione et al., 2019). This neural activity that is selectively appearing before free choice decisions is termed "choice-predictive activity," and is hypothesized to be a transient active state preceding a decision that is caused by spontaneous fluctuations in the baseline activity of prefrontal neurons (Funahashi, 2017).

Although tasks involving motor actions are the primary method of studying intention (Haggard, 2005), free choices for abstract intentions have also been investigated in human (Soon et al., 2013). Soon et al. (2013) gave participants the freedom to add or subtract numbers under the constraints of an arithmetic task in an fMRI study. They trained independent classifiers to distinguish the spatial patterns of fMRI signals related to the two choices of adding and subtracting. They found that in medial prefrontal and parietal cortex they could accurately decode the decision up to four seconds before the participant reported their conscious awareness of their decision. This study reinforces that the neuronal signals preceding a decision to attend generalize beyond merely motor signals. Thus, studying willed cognitive acts may provide additional insights into whether or not there is a unified decision network for volition that spans the domains of cognition and action.

Willed attention and willed actions

The forgoing selective review of the willed action literature provides the context in which we wish to place the work on willed attention. First, like the studies of neural activity preceding willed actions, willed attention can also be predicted by brain activity preceding the orienting of attention; i.e., as described above, Bengson et al. (2014) used EEG to demonstrate that in the approximately 1,000 msec prior to free choice decisions (but not earlier), EEG alpha power over parietal-occipital cortex predicted subsequent decisions to attend left vs. right. Thus, the emerging literature on willed attention is moving the discussion of intention, volition and free will from primarily the domain of motor actions to the broader domain of cognitive "acts" (c.f., Soon et al., 2013). But studies of cognitive acts present great challenges because there is typically no outwardly visible sign of when a covert cognitive function has occurred. Rather, as we are hoping to accomplish in our research program (e.g., Nadra et al., 2023), high temporal resolution non-invasive neural measures like EEG (and MEG) will have to be used to identify signs of the initiation of cognitive processes like attentional orienting in order to reveal the time course of willed acts of cognition. Such high-temporal resolution measures of brain activity permit the precursor brain activity and thus the antecedent brain states of willed attention to be identified separately from decision, post-decision and orienting-related brain activity. With such measures in hand, it will then be possible to understand the common and separable processes involved in willed attention and action, as well as willed cognition more generally.

While there are similarities in the investigation of willed attention and willed actions, there are also some differences. For example, in our original EEG studies of willed attention (Bengson et al., 2014, 2015), there were prompts indicating when subjects should decide where to attend. This experimental design differs from, for example, the work of Libet et al. (1983b) where subjects simply had to wait and then decide when to move; that is, they received no prompt. Because in our designs the subjects were uncertain as to whether (or when) they might be prompted to make a free choice about where to attend, the predictive EEG alpha signal during willed attention differs from the readiness potential in self-initiated motor actions. That is, our pre-prompt predictive EEG alpha pattern (Bengson et al., 2014) does not resemble the build-up of neural activity that is seen in the readiness potential in the studies of Libet, but rather is a more temporally circumscribed activity.

As noted above, one critique of Libet's research on willed action was the idea that the readiness potential is a general activator of intent, rather than being related to a specific decision vs. another (Haggard and Eimer, 1999; Haggard and Libet, 2001; Balaguer, 2014). In our research of willed attention, we distinguish the neural antecedents of a decision to shift attention to the left vs. the right before the onset of a prompt, and thus the predictive neural activity encodes a specific cognitive outcome and is not reflecting a generalized preparatory activity for simply making any decision (Bengson et al., 2014). This neural pre-prompt EEG alpha pattern may be a form of neural "noise" (stochastic neural activity) that biases free choice decisions (Shadlen and Roskies, 2012), or may reflect variations in specific attentional sets that are task and goal specific (e.g., attend left vs. attend right); the answer is not known.

Future directions for research on willed attention

The goal of the mini review was to introduce willed attention, and to place it in context by describing how it has been defined relative to the standard meaning of voluntary attention, as well as how it relates to the literature on self-initiated motor actions. Although there is now behavioral electrophysiological and neuroimaging evidence for the idea that willed attention involves (i) front-end decision processes in frontal cortex, (ii) changes in the network organization and interactions of decision and attention systems, and (iii) that these decision stages may be biased by ongoing stochastic brain states (i.e., the pre-prompt EEG alpha activity), there is much we do not understand about willed attention both theoretically and neuroscientifically.

One such area of uncertainty was presaged at the end of the last section. We do not know whether the pre-prompt predictive EEG alpha power changes observed in Bengson et al. (2014) reflect stochastic brain states (e.g., variations in general left vs. right hemisphere activation states) or are instead manifestations of task set states of the brain related to the specific task presented to the subjects. In our work on spatial attention, for example, the subjects know that they will either be cued to the left or right visual field location or be required to choose to attend to the left or right, which is a highly constrained task set. One might hypothesize that with such a task set in mind, the subjects were not in some sort of neutral brain states during the intertrial intervals, but were instead alternating or randomly varying between attending-left and attending-right brain states (unconsciously), and this might even have occurred in a systematic alternating pattern; by way of analogy, like a soccer goalie rocking back and forth prior to a penalty kick because they will only either jump left or right to defend the goal when the kick is made. If so, the predictive EEG pattern would be reflections of these varying brain states at different times, and would therefore tend to predict what the subjects would choose, even though the subjects were not consciously deciding in advance. The idea that subject invoke free will to make the decisions about whether to attend to the left or right is rather challenged by the idea that they might merely be going with a bias induced by a limited set of alternatives in a highly rarified laboratory setting. It is important to get clarity on this and related issues. So, experiments that are more naturalistic and involve fewer experimentally-induced constraints on potential free willed decisions, are needed to take this research forward. A first step might simply be to have several possible locations to choose to attend to, which would introduce more opportunities for free willed choices.

Another area of investigation that will be important in understanding willed attention will be to remove the experimenter control over when attention can be focused as well as over only where. The introduction of the prompt is an innovation over presenting instructive cues, but is still a highly constrained laboratory situation. As described above, we have taken a step in this direction by developing a task where subjects can exert a free choice over both where and when to allocation spatial attention (Nadra et al., 2023). More studies that investigate willed attention in more naturalistic contexts where free will choices are constrained less by the designs of the laboratory tasks are needed.

Investigating whether the pre-prompt alpha signals observed in willed attention reflect a mechanism that generalizes across different domains of willed attention (e.g., visual spatial, visual non-spatial, auditory, etc.) would provide insight into the nature of willed attention. Such a step will be important if willed attention is to prove useful for understanding intention, volition and free will (Norman and Shallice, 1986; Lau et al., 2004a,b; Humphreys et al., 2010).

Since it is known that selection history and unconscious influences can alter how we allocate attention (Theeuwes, 2018a), the neural antecedents of attentional decision making likely vary with different perceptual, cognitive and motor contexts (Mudrik et al., 2022). Thus, in the future, the neural activity associated with free choice decisions should be considered in the context that the decisions are being made (Bode et al., 2014), perhaps like a spectrum of volition that is engaged as needed by the presented scenario (Dresler et al., 2014). This could be investigated by considering how selection history in more naturalistic experimental designs could interact with willed attention decisions. Similarly, how reward and reinforcement influence willed attention will be important for understanding the constraints place on free will by the ongoing environmental and behavioral contexts.

Direct investigations of the interactions of willed attention and willed action in terms of both the underlying computational and neural of each will be an important step in understanding volition and free will. Put another way, willed attention and willed action should fit into a common theoretical framework and this needs to be elaborated. For example, integrating these domains as in the classic model of attention and action proposed by Norman and Shallice (1986), where controlled (willed) actions are contrasted with automatic actions (Shallice and Burgess, 1996). In this case, automatic is defined as a situation where an action is routine and does not require any explicit attention to complete, while controlled actions are those requiring deliberate conscious control to execute (Shiffrin and Schneider, 1984; Rubinstein et al., 2001; Carlisle and Woodman, 2011). For controlled actions, the model proposes a supervisory attentional system (SAS) which biases the activations of specific schemas, inhibiting those which are not relevant and activating those which are the most relevant out of all presented options (Shallice and Burgess, 1996). The necessity of attention varies based on the experience of the human executing the action. In the field of voluntary attention, most studies of attention using instructional cues engage some level of volition (allowing subjects to follow the experimenter's instructions), but have not adequately studied self-generated ("willed") shifts of attention, which require more volition than instructed attention, while involuntary shifts of attention are more akin to automatic actions in the Norman and Shallice model, where they require very little to no volition to carry out. Because willed attention has the potential to allows us to study volition in a variety of scenarios that are more akin to real life (shifting attention with or without the use of cues and prompts), future research on willed attention may provide a new view on the nature of attention, intention, volition and free will that is different from the dominant approaches in research on voluntary attention or free choice actions.

To advance our theoretical models of voluntary attention, future work should consider willed attention research. Insights from the study of willed attention may also contribute to the ongoing debate about the terminology we use as a field (e.g., Buzsaki, 2020; Poeppel and Adolfi, 2020) and allow the field to revise our collective understanding of what constitutes truly voluntary attention. Finally, the study of willed attention may also provide insights into the mechanisms of intention, volition and free will, by expanding the work beyond the dominant paradigms of willed actions to enable the study of willed cognition, which is a path toward understanding what we mean by free will and what it relates to in the brain.

Conclusion

Willed attention is a form of top-down voluntary attention that is characterized by participants having the ability to choose where, what and/or when to selectively attend. It is focused attention that is self-generated in the absence of external cues or external control, as is found in many laboratory studies where cues are used to guide what we call voluntary attention. The study of willed attention thus aims to understand fully volitional shifts of attention, with a goal of revealing the underlying neural mechanisms involved, as well as how they might be the same or different from those revealed in cued (instructed) attention studies. But much beyond these proximal goals, studies of willed attention address the very nature of intention, volition and free will by providing additional methods and insights that can help us advance understanding of agency empirically and theoretically. Such advances will be necessary for a more complete description of human cognition, but also, and importantly, to support efforts to translate basic science knowledge into interventions that can help ameliorate losses of normal functioning from damage or disease that limit humans from converting their intentions to everyday actions (Nicolelis, 2022; Metzger et al., 2023).

Author contributions

JN and GM wrote and edited the review. All authors contributed to the article and approved the submitted version.

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Conflict of interest

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

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Publisher's note

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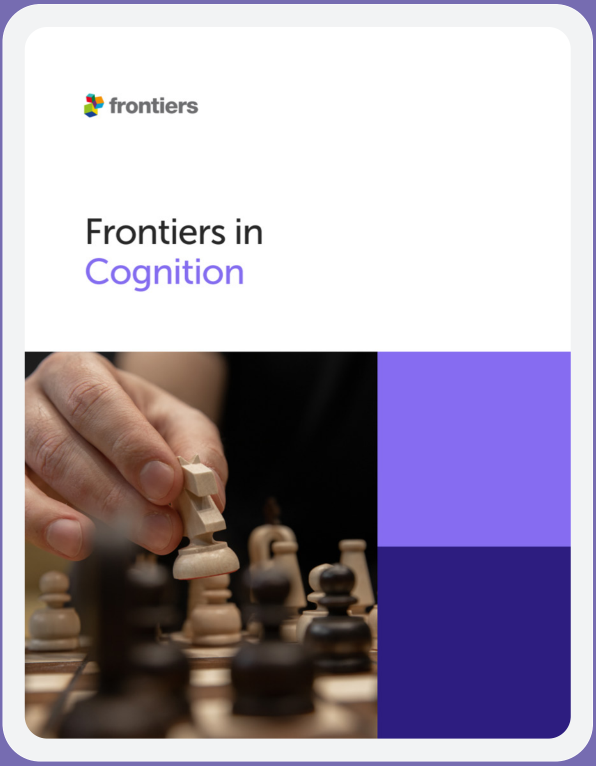

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