Attentional control over masked visual processing

A thesis submitted during the year 2020 to the University of Hyderabad in partial fulfilment of the award of a Ph.D. degree in Cognitive Science

by

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1. Prasad, S., & Mishra, R. K. (2020). Reward Influences Masked Free-Choice Priming. ***Frontiers in Psychology***, *11*, 2938.
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List of abbreviations

ANOVA	Analysis of Variance
ACS	Attentional Control Settings
DPS	Direct Parameter Specification
FA	False Alarm
GLMM	Generalized Linear Mixed-effects Modelling
ISL	Indian Sign Language
IOR	Inhibition of Return
IR	Irrelevant
M	Mean
MAD	Median Absolute Deviation
NCE	Negative Compatibility Effect
NH	Normal-hearing
PAS	Perceptual Awareness Scale
PCE	Positive Compatibility Effect
RT	Response Time
SD	Standard Deviation
SE	Standard Error
SOA	Stimulus Onset Asynchrony

Abstract

We are constantly bombarded by a range of stimuli out of which only some cross the threshold of conscious awareness. It is now well-understood even those stimuli that we are not consciously aware of can nevertheless influence our behaviour. One of the enduring debates related to this topic is on the depths and limits of such unconscious processing. While traditional theories of cognitive control have assumed a tight link between consciousness and control, more recent theories have proposed a role for executive control over unconscious cognition. The thesis aims to address the question: to what extent is masked visual processing susceptible to different control mechanisms? Control settings can be manipulated in several ways. This thesis will focus only on attentional control. I take both a cross-sectional and an individual differences approach to examine this issue. Two different paradigms tapping into different mechanisms will be used: the spatial orienting paradigm which measures attention capture and the response priming paradigm which measures the influence of masked primes on motor responses. I will show that associating reward points with brief primes leads to enhanced priming effects. Deaf individuals are also shown to display greater masked priming effects owing to their plasticity-induced attentional advantage in visual processing. In another series of studies, I show that brief peripheral cues presented in task-irrelevant locations in a spatial orienting paradigm can be ignored to some extent. Further, deaf individuals seem to be worse at ignoring such irrelevant cues. Finally, I also address the issue of baseline in studies on attention capture and conclude that the inappropriate use of baseline can lead to misleading results. Taken together, these studies suggest that masked visual processing is not “automatic” in the traditional sense. I conclude that a more nuanced approach to the link between consciousness and control, in line with recent theories on unconscious processing, is required.

1. Introduction

> *"There are hundreds of indications leading us to conclude that at every moment there is in us an infinity of perceptions, unaccompanied by awareness or reflection; that is, of alterations in the soul itself, of which we are unaware because these impressions are either too minute and too numerous, or else too unvarying, so that they are not sufficiently distinctive on their own. But when they are combined with others they do nevertheless have their effect and make themselves felt, at least confusedly, within the whole"* (Leibniz, 1704/1981, p.53).

This quote exemplifies one of the most fascinating aspects of the human mind. Information that is not consciously detectable can nevertheless influence cognitive functioning. This idea is in equal measures perplexing and counter-intuitive. As a reflection of the extreme reaction this idea triggers in people, the research on unconscious processing has been through several alternating phases of enthusiasm and severe criticism (Kouider & Dehaene, 2007). It is important to clarify at the outset in what sense the word "unconscious" is being used henceforth. The word "unconscious" has been adopted by several branches within and outside of psychology. Freud, for example, using the famous iceberg metaphor claimed that much of the mental processing in the human mind is unconscious. The things we do become conscious of are a fraction of all the things that mind processes - the tip of an iceberg. In the late 1900's, several social psychological studies were conducted which claimed to show subliminal influences on people's behaviour in the real world (Brannon & Brock, 1994). All of these scientific traditions are fraught with controversy and I will not be getting into any of them. The approach to unconscious processing in this thesis is from the perspective of experimental/cognitive psychology. It is impossible to appreciate the current debates related to this topic without understanding the historical background because many of the questions facing the field today have been around for several decades. With this aim, I will first trace the history of experimental research on unconscious perception and then arrive at the current conceptualisation of “unconscious”. I will also briefly discuss the methodological aspects involved in the study of unconscious stimuli from multiple perspectives. Finally, I will address the main aim of this thesis: to examine the extent to which unconscious processing is susceptible to control processes and describe the relevant theories and their predictions. The chapter will end with an overview of the contents of this thesis.

1.1 Earliest studies on unconscious perception

The German philosopher Johann Herbart (1776–1841) was the first to introduce the term “subliminal”. He said that at any given point in time, the mind is occupied by a tiny fraction of all the possible ideas and thoughts. The rest is below the threshold of consciousness or limen (Kim, 2015). The earliest experimental investigations on subliminal perception can be traced back to the late 1800s (Peirce & Jastrow, 1884; Sidis, 1898). In what is arguably one of the first experimental studies to be performed, Pierce and Jastrow (1884) used two weights which differed by less than the just noticeable difference. They found that the participants (Pierce and Jastrow themselves) could make better than chance-level correct judgements of which was heavier even though they had not much confidence in their judgement. For instance, a rating of 0 was given often where 0 "denoted absence of any preference for one answer over its opposite, so that it seemed non-sensical to answer it at all" even when participants made the right judgement. As a side note, it is worth knowing that one of the authors, Charles Peirce Sanders, a contemporary of William James was the philosopher who introduced the term “quale” (Pierce, 1886/1982) which then popularly became known as qualia. Several such studies were published in the early part of the 20th century where participants were found to detect or discriminate stimuli at better than chance level while verbally reporting that they were not conscious of the stimuli (Baker, 1937; Williams, 1938, see Miller, 1939 for a review of these early studies)

Despite this early enthusiasm to investigate behaviour in the absence of awareness, there was a slump in the research on this topic in the latter half of the 20th century. Some of the credit for this goes to the paper published in 1960 by Charles Eriksen (who is now popularly known for the flanker task) where he carefully pointed out the flaws in the methodology of subliminal perception (Eriksen, 1960). His primary critique was against the use of verbal reports to assess awareness. He argued that subjective biases of the participants, and not the true extent of awareness, determine participants’ verbal reports regarding their conscious awareness. Hence, they are not to be relied upon. This critical article is said to have “effectively stopped research on unconscious perception and learning for the next 20 years (p. 1031)” (Lappin, Logan, Fournier, & Hoffman, 2018, but see Dixon, 1971; Wickens, 1972 for exceptions).

The field saw a resurgence in the 1980s mostly with the seminal work of Marcel (1974, 1980, 1983). He opted for the masking paradigm which he had become familiar with while conducting studies in reading. His most-cited 1983 study was based on an earlier study by Wickens (1972) who presented a word briefly (50 - 80 ms) which was followed by a letter mask and then another word for 5 s. Participants had to judge if the second word was similar or not to the unseen word. The performance was above-chance indicating that the “unseen” word was still getting processed. Findings like these suggested that it is possible to interrupt higher levels of processing of visual information by presenting a “mask", while keeping the lower-level representations intact. This fundamental assumption of masking - now well accepted - motivated Marcel to use this paradigm to study perception without awareness. One of the problems with Wickens’ study was that there was no evidence of the participants’ awareness of the unseen word except his assertion that “the exposure duration was typically too short to result in target identification."

Taking this into consideration, Marcel designed a priming paradigm where a brief stimulus (eg., a word such as “wine") appeared followed by a pattern mask (Marcel, 1983). Following this, another word either semantically related (“drink”) or unrelated (“rope") was presented. Subjects chose the related more often suggesting that the masked word (prime) triggered semantic associations. The subliminality was ensured by reducing the delay between the prime and the mask till the accuracy of detecting the prime dropped down to chance level. This parameter was then used in the rest of the trials as it ensured that the stimulus was indeed invisible. This, in some ways, accounted for Erkisen’s earlier criticisms against introspective methods to assess awareness. It was also an improvement on Wilkens’ study which did not measure participants’ awareness of the masked word at all.

By demonstrating that meaning could be accessed without conscious awareness, these studies moved beyond the early studies which only showed discrimination without awareness. Around the same time, the repetition priming paradigm to study visual word recognition was becoming popular in the late 1980s in the study of lexical processing. In the classic study of Forster and Davis (1984), a brief prime word was presented followed by a mask (######) and a target word. The influence of the prime word was seen on the responses to the target word showing lexical processing of unconscious primes. The aim of these studies was not so much to study the extent of unconscious processing - rather to study the nature of lexical processing without the confounding effects caused by the conscious awareness of the prime. Thus, to isolate only the linguistic processing triggered by a word and rule out extralinguistic factors such as memory traces caused by the word, the primes were masked in these studies. Nevertheless, these insights from psycholinguistics helped build stronger paradigms to demonstrate unconscious processing. However, the persisting problem with several of these studies was that there was no robust measure of prime awareness making it difficult to conclude if the primes were truly invisible.

1.2 Demonstrating lack of awareness

Taking into considerations the various criticisms levied against the methodology used in unconscious processing research (Eriksen, 1960; Holender, 1986), researchers adopted a dissociation paradigm which arguably remains the most convincing way to demonstrate unconscious processing till date (Reingold & Merikle, 1988). The primary tenets of this paradigm are as follows: Two measures - “direct” and “indirect" - are sought from each participant. In the direct measure, participants are asked to detect or discriminate the critical stimulus. A chance level performance in the direct measure would confirm a lack of awareness of the critical stimulus. In the indirect measure, the influence of the critical stimulus is indirectly measured on behaviour. Thus, a combination of the direct and indirect measures would suggest unconscious processing - that stimuli that were not consciously perceived nevertheless influenced response behaviour. Thus, the lack of conscious awareness of the critical stimulus is demonstrated through objective tests rather than relying on verbal reports which was the major point of contention in the early years of unconscious processing research.

One of the strongest criteria for demonstrating unconscious processing has been to avoid a mismatch between the direct and indirect measures (Schmidt & Vorberg, 2006). According to this, the tasks for direct (testing stimulus invisibility) and indirect (testing the effect of the unconscious stimuli) have to be the same except for which stimulus serves as the target. The stimulus, the features to be responded to and the stimulus-response mappings have to be all equivalent in both the tasks. Since the indirect measure is used to conclude that the stimulus was indeed invisible even in the indirect task, any mismatch between the two tasks would make comparisons difficult.

The advantage of a forced-choice measure for assessing awareness is that chance-level performance is determined based on a signal detection measure d’ and not just accuracy (Stanislaw & Todorov, 1999). d’ is calculated based on Hit rates (how often was the stimulus present and was correctly identified?) and False alarm rates (how often was the stimulus absent but was identified as being present?). Compared to calculating accuracy (which are just Hit rates), the d’ calculation accounts for response bias. Thus, if a person has a bias towards responding “stimulus present”, then the accuracy/hit rate might be high indicating that they were correctly identifying the stimulus. But this misleading because such an individual will also have a very high false alarm rate. The d’ provides a pure measure of the individual’s ability to detect an unconscious stimulus.

Although objective tests as described above have been the most commonly used method in unconscious processing research for many years now, some researchers have nevertheless suggested that these methods do not truly capture the contents of conscious experience and that first-person approaches are necessary for the study of consciousness (Overgaard, 2008). Further, chance-level performance on a visibility task - which is sometimes considered a gold standard for establishing lack of awareness - might not necessarily indicate that. This is because of a blindsight-like phenomenon where participants perform correctly on a forced-choice task due to unconscious processes (just like blindsight patients do) even though they can’t subjectively perceive any stimuli (Koivisto & Neuvonen, 2020). Thus, subjective methods which rely on introspection rather than forced-choice task performance for measuring awareness are preferred by some researchers. Three most common subjective modes of assessment are perceptual awareness scale, confidence ratings and post-decision wagering (see Sandberg, Timmermans, Overgaard, & Cleeremans, 2010 for a review of the three methods).

The most commonly used among these is the perceptual awareness scale (PAS) is a 4-point scale that asks participants to report the clarity of their visual experience (Ramsoy & Overgaard, 2004). The four levels are No experience, vague experience, almost clear experience and clear experience. The PAS scale was first proposed by

Ramsoy and Overgaard (2004) where they showed simple shapes like a triangle, a square of different colours to the participants and asked them to describe their experience. The participants were asked to go from “no experience at all” to “a clear image” without being given any of the intermediate categories. Participants often said things like, “the position was clear, the colour was a glimpse, I had no experience of the shape”. It was seen that most participants’ experiences fell within the four categories: No experience, vague experience, almost clear experience and clear experience. Thus, this scale relies on the subjective assessment of each participant on whether they had a clear experience of the stimulus. It is possible to argue if such scales fall prey to the criticisms against introspective measures made in the late 1900s by Eriksen and others. As described earlier, the main point of criticism against introspection was that each participant would have a different threshold for deciding whether they saw something or not. The PAS scale circumvents this by including a non-binary scale where participants can rate their experience in a graded manner. Thus, the proponents of this scale argue that it captures the graded nature of conscious experience without forcing participants to choose between two categories - objectively or subjectively.

The debate on the optimum methods to demonstrate a lack of awareness is ongoing and there is no clear answer. In this thesis, I have only used objective forced-choice tasks to measure the extent of conscious awareness.

1.3 Studying unconscious processing: Taxonomy and paradigms

Before we go any further, it is important to define the term “consciousness”. Researchers vastly differ on the definitions of consciousness depending on their disciplinary affiliation. I will stick with the definitions most commonly adopted by cognitive psychologists or neuroscientists. "Consciousness" in this thesis is used to refer to the contents of conscious awareness that can be reported by an individual (Cohen et al., 2012). It is used interchangeably with the term "awareness" and I make no distinction between the two. Reducing consciousness to reportability is often criticised for ignoring the phenomenal aspects of consciousness also known as qualia. Empirical investigations into consciousness have mostly been restricted to the contents of consciousness as they can be investigated through the existing scientific paradigms. A scientific study of qualia has largely remained elusive. Some of these points are debatable and come with several caveats. I will not digress into the rich debates on consciousness. It suffices here to say that, going by the definition adopted for consciousness in this thesis, "unconscious" will refer to the lack of reportability of the contents of conscious awareness. However, several criteria need to be fulfilled for a stimulus to be referred to as "unconscious". Below, the taxonomy outlining these criteria used in this thesis will be detailed.

Dehaene et al. (2006) proposed a taxonomy for distinguishing between the different kinds of conscious and unconscious information. It was an orthogonal classification where the categorisation of a stimulus depended on two factors: bottom-up strength of that stimulus and top-down attention directed at that stimulus. This results in a 2*2 matrix as given below (Table 1.1). According to this classification, subliminal processing happens when the bottom-up strength of the stimulus is so weak that it can't trigger large-scale activation which is responsible for conscious perception. But, Dehaene et al. make a distinction between two types of subliminal processing - attended and unattended. When attention is directed at subliminal stimuli, it leads to strong feedforward activation and is said to be responsible many of the unconscious priming effects observed. The reportability of the stimulus is still low in this case making it subliminal but top-down attention triggers activations associated with the stimulus influencing behaviour, like in the case of priming. Subliminal processing without attention leads to little or no effect because lack of attention leads to weak activations not strong enough to even lead to priming. Thus, unlike many models that propose that attention and consciousness are independent and operate without each other, Dehaene et al's model assume a tight link between the two.

The other category of stimuli that individuals do not become conscious of is referred to as "preconscious" in the taxonomy of Dehaene et al. (2006). These refer to stimuli that are unconscious due to lack of top-down attention. Such stimuli have sufficient bottom-up strength and would otherwise reach consciousness in the presence of attention. Classic examples in this category include the phenomenon of attentional blink or inattention blindness. In an attentional blink, for example, a stimulus presented in quick succession of another stimulus is suppressed from awareness due to capacity limitations in attentional deployment. If the critical stimulus is presented after a longer duration, then it becomes visible suggesting that it was capable of reaching consciousness but did not due to attentional limitations. Such phenomena have been extensively studied as well to probe the psychological and neuroscientific underpinnings of conscious and unconscious processing.

Table 1.1 Taxonomy of conscious, unconscious and preconscious processes (Dehaene et al., 2006)

	Top down attention - yes	**Top down attention - No**
Weak bottom-up strength	subliminal - attended	subliminal - unattended
Strong bottom-up strength	conscious	pre-conscious

More recently, Lamme (2020) proposed an alternate framework with hierarchical levels of visual processing depending on the visibility of the stimuli and availability of attention:

1. Stage 1: fully invisible
2. Stage 2: subjectively invisible
3. Stage 3: Unattended (but otherwise subjectively invisible)
4. Stage 4: Subjectively visible and attended

Stage 1 refers to stimuli that are below that thresholds of objective visibility as measured through forced-choice tasks. Stimuli that are above the objective threshold for awareness but are subjectively invisible as measured through subjective self-reports are classified as Stage 2. Stage 3 refer to stimuli that are potentially visible are momentarily unreportable due to lack of attention. This stage of processing is similar to the preconscious category in Dehaene et al. taxonomy. The final stage (4) refers to stimuli that are attended to and are subjectively visible. There can be questions on whether stage 1 and stage 2 of Lamme's classification corresponds to subliminal - attended and unattended categories of Dehaene et al. The similarity is implied by Lamme but never explicitly addressed. One key difference between the two taxonomies is that while Dehaene's classification is orthogonal, Lamme's is hierarchical. I will not get into more details of these classifications. It would suffice to say that in this thesis, I am mainly dealing with the "subliminal" category of stimuli according to Dehaene’s classification and the Stage1 and Stage2 stimuli according to Lamme’s classification.

There are many ways to investigate unconscious processing and the issues related to it. The two main variables are the type of suppression method and the type of task/mechanism being probed. With regard to the suppression method, several methods are used to render a stimulus invisible - visual masking, binocular rivalry, object substitution masking - to name a few (see Frontiers Research Topic on “Invisible, but how?”, edited by Dubois & Faivre, 2014). Crucially, different suppression methods result in the suppression of stimulus processing at different levels. Thus, there is a functional hierarchy in unconscious visual processing determined by the method of suppression (Breitmeyer, 2014)

This thesis will solely focus on visual masking. Visual masking is the method of presenting a brief stimulus followed or/and preceded by another object on the screen (referred to as “masks”) which is expected to prevent the brief stimulus from reaching awareness. This is in part based on the feedforward model of visual processing (Lamme & Roelfsema, 2000) according to which conscious awareness of a stimulus is the result of recurrent processing triggered by the stimulus. The masks are supposed to only allow feedforward processing of the brief stimulus and prevent recurrent processing, thus masking the brief stimulus from conscious awareness.

With regard to the task and the underlying mechanism being probed, the focus in this thesis will be on two paradigms: Response priming and spatial orienting paradigms.

Response priming

The response priming paradigm grew out of the enthusiasm for the masked lexical priming studies of the 1970s and 1980s. In one of the earliest of this type, Eimer and Schlaghecken (1998) presented left or right arrows as targets. Participants were instructed to press the left button when the left arrow was presented and the right button when the right arrow was presented. Importantly, brief stimuli, known as primes were presented for 16 ms before the targets. The primes consisted of left/right arrows again. In between the primes and the targets, a “mask” comprising of left/right arrows superimposed on each other was presented. The mask was expected to prevent conscious awareness of the prime. Faster response times to the target were seen when the prime and the target were identical (eg., left prime - left target) compared to when they were not. The subliminal response priming paradigm is now popularly used to study the effect of one stimulus on subsequent response behaviour (Kiesel, Kunde, & Hoffmann, 2007). The match trials are typically referred to as compatible/congruent trials and the mismatch trials are referred to as incompatible/ incongruent. The compatibility or congruency effects are seen because the prime triggers the response associated with it. Thus, people are faster responding when the response required by the prime matches the response required by the target.

Free-choice priming is a variation of the response priming paradigm where subliminal information influences participants’ responses even when they are asked to freely choose between alternatives (eg., Kiesel et al., 2006). Schlaghecken and Eimer (2004) pioneered the free-choice paradigm by slightly modifying their response priming paradigm described above. In this task, along with left/right targets, a neutral target (a double-headed arrow) is presented on some trials. On these "free-choice" trials, participants are asked to freely choose one of the responses. It is seen that people choose the response associated with the prime presented on that trial and are faster while doing so. This illustrates that response activations triggered by masked or unconscious primes can influence seemingly free choices. This finding has also been supported by brain-imaging studies that have shown that subliminal cues can modulate activity in the voluntary-action network of the brain (eg., Teuchies et al., 2016). These evidence of subliminal free-choice priming suggest that human choices can be influenced by information that they are not even aware of and raise questions on the "voluntary" nature of our motor actions (Haggard, 2005).

Spatial orienting

The other important paradigm that has been extensively used in this thesis is the spatial cueing paradigm (Posner, 1980). There are two main varieties in this: the cue-target paradigm and the target-target paradigm (see Chica et al., 2014 for a comprehensive review of the spatial orienting paradigm). In the cue-target paradigm, a first stimulus - known as the cue - is presented either at the centre of the screen or at the periphery. Typically, no response is required for the cue. After a delay, a second stimulus - known as the target - is presented to which the participants are required to respond. In the target-target paradigm, instead of a cue followed by a target, two targets are successively presented. This thesis presents experiments only using the cue-target paradigm, so I will focus on that henceforth. The key manipulation is with regard to the type of cue which can trigger either endogenous or exogenous shifts of attention. To measure endogenous orienting, symbolic cues (eg., colour shapes or arrows) are presented at the centre of the screen. They are generally predictive of the target location, for example, a red cue can indicate that the target will appear on the left 75% of the trials. Such symbolic cues trigger attentional shifts only if they are predictive. Thus, this form of attentional orienting is referred to as endogenous or voluntary. Participants shift attention to the cued locations because it is beneficial in finding the target.

The other form of orienting is reflexive, exogenous attention capture. In this condition, cues are presented at spatial locations (eg., periphery). These cues are not predictive of the target location. Since exogenous cues are not informative of the target location, the participants typically have no incentive to pay attention to these cues. Nonetheless, it is generally seen that they capture attention. The results of this paradigm depend on several characteristics such as type of cue, time-delay between the cue and target onset (stimulus-onset-asynchrony, SOA), type of task among others (Chica et al., 2014). But, typically, at short SOAs (< 300 ms), the responses to the targets are facilitated at cued locations. As the SOA increases, this facilitation effect turns negative and responses to the uncued location are faster than responses to the cued location. This effect - termed as “inhibition-of-return” (IOR) - has been attributed to a foraging mechanism which makes people look for new locations in the visual field (Klein, 2000).

1.4 Control over unconscious processing

The early 1980s and 1990s research on subliminal perception focussed on demonstrating that the phenomenon exists while addressing the methodological problems of the past. The next phase of research has been focussed on investigating the depths and limits of unconscious processing. For example, can complex cognitive tasks, like numerical operations, be performed unconsciously (Sklar et al., 2012, but see Moors & Hesselmann, 2018)? Can scene integration be performed unconsciously (Mudrik, Faivre, & Koch, 2014)? The debate has now shifted to knowing which level of analyses are possible without conscious awareness (see Hassin, 2013 and Hesselmann & Moors, 2015 for a lively exchange). One of the threads in this line of research is investigating the extent of top-down control over unconscious processing (Ansorge, Kunde, & Kiefer, 2014). Traditionally, it has been assumed that individuals can exert cognitive control only on information that they are consciously aware of (eg., Norman & Shallice, 1986; Posner & Snyder, 1975). These early authors suggested that one of all the hallmarks of automatic processes is that it proceeds unconsciously. For instance, with regard to automatic processes, Kihlstrom (1996) stated that (p. 2):

> "*We have no conscious awareness of their operation, and we have little or no awareness of the information which they process. All that enters awareness is the final product of the automatic process. Thus, automaticity represents unconscious processing in the strict sense of the term: we have no introspective access to automatic procedures or their operations; these can be known only indirectly, by inference*."

While this implies that automatic processes are unconscious, over the years, it has also been taken to imply that ALL unconscious processes are also automatic in the sense that they are not subject to any form of control.

But over the last few years, as detailed below, a vast literature has demonstrated the influence of executive control over unconscious processing.

Executive control in these studies is primarily defined based on the model of Miyake et al. (2000). In this model, executive control is divided into three sub-components: inhibition, shifting and updating measured using tasks like Stroop, Simon, switching and n-back, to name a few. The other approach to studying control is through the concept of attentional control. Attentional control refers to the tug-of-war between different sources of attentional selection (eg., Theeuwes, 2010). This conflict is mainly between stimulus-driven and goal-driven mechanisms, but more recently a third form of attentional control known as selection-history has also been added to this framework (Awh, Belopolsky, & Theeuwes, 2012). This thesis will only be concerned with manipulation related to attentional control and their impact on unconscious processing.

1.5 Aim of the thesis

This thesis aims to examine the role of attentional control over unconscious processing. While this topic has received some attention in the literature in the past, a comprehensive study from multiple perspectives is missing. I examine *if*, *to what extent* and *under what conditions* unconscious information can influence behaviour. I have used both response priming and spatial orienting paradigm and two different methodologies: eye-tracking and manual response time. Finally, I have used both a within-subjects approach and an individual differences approach to examine group differences in unconscious processing.

As described earlier, recent work, in the last two decades, has challenged the automaticity assumption of unconscious processing. Now, there is enough evidence to show that unconscious processing can be constrained by several factors. These have been demonstrated using a variety of paradigms and predicted by different types of models. Below, I will briefly describe two models/hypotheses that are central to this claim and also most relevant to the studies undertaken in this thesis. The evidence supporting control over unconscious processing according to the predictions of each model will also be discussed.

Direct parameter specification

Neumann's (1990) direct parameter specification model was one of the earliest models to propose the role of task-sets during masked visuomotor priming. He proposed that the configuration of a task-set ensures that only relevant unconscious information is selected for further processing. Since in most priming studies, the prime and the target are identical or at least share features, the DPS account successfully explains the typical masked priming effects. Building upon this, Kiefer and colleagues proposed the "attentional sensitisation model of unconscious cognition" which proposes that processing of unconscious or briefly-visible information is susceptible to top-down control triggered by currently active task representations or attentional focus (Kiefer & Martens, 2010; Kiefer, 2012). Thus, enhanced sensitivity to processing in the task-relevant pathways and attenuation of the processing in task-irrelevant pathways results in a strategic selection of task-relevant stimuli. The model explicitly predicts that unconscious processes “should depend on available attentional resources (p.1)” and stands in stark contrast to the automaticity account of unconscious processing (Kiefer, 2012).

Ansorge and Neumann (2005) was one of the earliest studies to comprehensively demonstrate the tole of task-intentions on unconscious priming effects. They showed that priming effects based on the match between prime and target colours (or shapes) were observed only when the task was to discriminate based on the target colours (or shapes). Similarly, Kiefer and Martens (2010) showed the influence of task-sets on unconscious priming using a task-induction paradigm. Participants performed either a semantic (based on meaning: living or non-living) or a perceptual (based on shape: round or elongated) judgement on an image before performing a subliminally primed lexical decision task. Increased priming effects were observed following the semantic induction task. This is because the induction of the semantic task-set led to increased activation of the semantic processing pathways. This resulted in enhanced processing of the meaning of the subliminal primes, leading to increased priming effects on the lexical decision task. The masked priming effects have also been shown to depend on the allocation of temporal (Naccache, Blandin, & Dehaene, 2002) and spatial attention (Sumner, Tsai, Yu, & Nachev, 2006).

In a semantic categorisation task, Naccache et al. (2002) found that presenting primes and targets on a predictable time scale led to enhanced priming effects due to increased temporal attention to the prime stimuli. All these studies show that unconscious priming is susceptible to attentional control - either through the contents of the attentional set specified by task-goals or through the explicit allocation of attention.

The attentional set can be specified in various ways. Instructing certain features as relevant, cueing certain locations are some of the ways in which the attentional template can be manipulated. More recently, reward-learning has been proposed as one of the factors driving attentional selection. It is part of a larger category of factors collectively known as “selection-history”. Selection-history is the process by which previously the history of attentional selection of an individual determines future attentional allocation. For example, if you always search for your watch in a specific place in your room, then an attentional bias is created at that place. The role of such selection history driven mechanisms on response priming is unknown. In of the chapters, I take reward-learning as an example of selection-history to examine if it can modulate unconscious response priming.

Contingent capture hypothesis

The contingent capture hypothesis was proposed in a seminal study by Folk, Remington and Johnson (1992) to address if attention capture is necessarily stimulus-driven. Before this, studies, particularly by Theeuwes (1992) using his irrelevant-singleton paradigm, had shown that irrelevant distractors, for example, a red circle captures attention even when participants are looking for a green-coloured square amidst other green circles. Theeuwes concluded that attention capture by salient singletons is necessarily stimulus-driven and happens in a bottom-up fashion. Challenging this, Folk et al. (1992) showed that irrelevant salient information captures attention only when it matches the top-down goals of the participant during that task. In their study, Folk et al. (1992, Experiment 3) presented an abrupt-onset cue (white dots surrounding a box) which was followed by either an onset target (a single “x” or “=“ in white appearing on the screen) or a colour target (a red coloured “x” or “=“ in one box surrounded by three “x” or “o” in white). In the onset target condition, the task was to press left/right key depending on whether the onset target was “x” or "=". In the colour target condition, participants pressed left/right key depending on whether the red target was “x” or “=“. The same onset or colour targets were also paired with colour singleton cues (red dots surrounding one box and white dots surrounding the other 3 boxes). Thus, there were four conditions based on cue-target combinations: onset cue - onset target (match), onset cue cue - colour target (mismatch), colour cue onset target (mismatch), colour cue - colour target (match). Results showed significant cue validity effects only for the matching cues and almost no validity effects for the mismatch conditions. This suggests that only those features of the cue relevant to the current task-goals and thus part of the attentional template are successful in capturing attention.

This also helps explain Theeuwes' findings from the singleton paradigm. Since participants were looking for a unique feature (a square among circles) any cue matching this attentional template would capture attention. The irrelevant singleton distractor captured attention because it comprised of a unique feature (red among green). Thus, although irrelevant to the specific task instruction, the irrelevant singleton is nevertheless part of the attentional template active during the task. Whether goal-driven mechanisms can override stimulus-driven capture of attention is still a contentious topic and continues to be a matter of debate (Luck et al., 2020).

However, it is worth noting that while lively debates are still occurring to determine whether the contingent capture hypothesis is valid for attention capture by visible cues, not many studies have attempted to test the predictions of the hypothesis with regard to unconscious cues. The few studies that have examined this have reached the conclusion that attention capture by unconscious cues is almost entirely stimulus-driven (Schoeberl, Fuchs, Theeuwes, & Ansorge, 2015). However, this conclusion is premature (see Prasad & Mishra, 2019 for a critical review on this topic). Several experiments in this thesis are aimed at explicitly testing the predictions of contingent capture hypothesis in unconscious attention capture. Finally, the seminal study of Folk et al. focussed on feature-based attentional control. The cues and the targets either matched or mismatched at the feature-level (abrupt-onset or colour singleton). But the relevancy of the cue can also be manipulated at the spatial level (eg., Ishigami, Klein, & Christie, 2009; Ruthruff & Gaspelin, 2018) which is relatively understudied. In studies manipulating spatial relevancy, the goal is to examine if spatially irrelevant cues capture attention. The experiments in this thesis with the spatial orienting paradigm are aimed at examining the role of spatial attentional control settings (ACS) on attention capture.

1.6 Individual differences in masked visual processing

The secondary aim of this thesis is to examine group differences in unconscious processing induced by individual differences in attentional control. There is scant literature on how different groups differ with respect to their sensitivity to unconscious cues. I chose to study the hearing-impaired population as they have been shown to have enhanced attentional control compared to normal-hearing individuals (Bavelier, Dye, & Hauser, 2006).

Do deaf individuals see better? Researchers have been interested in this question for several decades and there have been opposing views with regard to the answer to this question. On one hand, there are deficit theories which, based on the notion of division of labour, argue that deaf individuals would suffer from a deficit in visual functions because the visual system has to undertake the functions of two different modalities (Mitchell, 1996). Some other theories, in contrast, have argued that the deaf brain is adaptive. Loss on auditory input leads to functional re-organisation of the auditory cortex resulting in a visual processing advantage (Dye & Bavelier, 2013). The investigations conducted in this thesis are in line with this adaptive, compensatory hypothesis rather than the deficit theories.

There are two main hypotheses in the deaf visual processing literature which put forward somewhat contradictory predictions. The dorsal stream hypothesis (Bavelier & Neville, 2002) proposes that the dorsal visual stream undergoes modification in the deaf due to hearing loss. These modifications are responsible for the changes seen in visual functioning. Since the dual-stream model proposed by Milner and Goodale (1992) proposed that the dorsal visual pathway is involved in functions related to motion and guidance of attention, the dorsal stream hypothesis explicitly predicts that the changes in deaf are attentional. These changes are expected to be more pronounced in the visual periphery. In contrast, the visual reactivity hypothesis predicts that deaf have enhanced sensitivity to visual stimuli throughout the visual field, not specific to periphery (Pavani & Bottari, 2012). Further, these changes are expected to be a result of improvement in low-level, bottom-up processing.

In sum, the exact locus of the changes observed in deaf and the mediating role of attention is still not clear. Further, these changes are by no means uniform or seen in all individuals with hearing-impairment. Several confounding factors have to be taken into account while considering neuroplasticity in the deaf (Dye & Bavelier, 2013). For instance, the experience of deafness (and the resulting neuroplasticity) can vary significantly between individuals who are born deaf (congenital deafness) in comparison to individuals who acquire deafness at a later stage due to injury or an illness. The age of acquisition of sign language is another crucial variable that decides the extent of neuroplasticity-induced changes. The studies reported in this thesis were all conducted on individuals with congenital deafness born to hearing parents.

Most studies on visual attention in the deaf have used visible cues of which the participants are consciously aware of. The existence and the extent of masked visual processing in the deaf have been rarely studied. If it is indeed true that deaf have increased visual processing capabilities, then a natural extension of this idea is to question if these changes are restricted to the conscious domain. As we have argued in detail elsewhere, understanding the effect of deaf neuroplasticity in unconscious processing can not only throw light on the deaf visual system, but it also has the potential to contribute to our understanding of the plasticity of consciousness itself (Prasad & Mishra, 2017).

1.7 Overview of the chapters

The chapters in this thesis and the experiments contained within them can be grouped under three sub-themes: the role of spatial ACS on attention capture by brief cues, the role of reward-learning on masked response priming and individual differences in masked priming/cueing. In **Chapter 2**, we study the effect of reward-learning on masked free- and forced-choice priming. Masked response priming will be used to answer this question combined with the reward-training paradigm. **Chapter 3** will be focussed on investigating if, and to what extent, spatial attentional control settings can modulate the influence of brief cues on saccadic decisions. **Chapter 4** is a registered report & replication re-examining a recent study which claimed to show evidence for "immunity to attention capture" at ignored locations. In this chapter, we will re-evaluate these claims using what we think is a more appropriate baseline to quantify attention capture. The last two chapters will be devoted to the study of individual differences. In **Chapter 5**, the spatial orienting paradigm will be used to examine attention capture by relevant and irrelevant brief cues in deaf and normal-hearing individuals. **Chapter 6** is a study of masked free- and forced-choice response priming in the deaf.

2. Reward-learning modulates masked free-choice priming

While it is known that reward induces attentional prioritisation, it is not clear what effect reward-learning has when associated with stimuli that are not fully perceived. The masked priming paradigm has been extensively used to investigate the indirect impact of brief stimuli on response behaviour. Interestingly, the effect of masked primes is observed even when participants choose their responses freely. While classical theories assume this process to be automatic, recent studies have provided evidence for attentional modulations of masked priming effects. Most such studies have manipulated bottom-up or top-down modes of attentional selection, but the role of "newer" forms of attentional control such as reward-learning and selection history remains unclear. In two experiments, with number and arrow primes, we examined whether reward-mediated attentional selection modulates masked priming when responses are chosen freely. In both experiments, we observed that primes associated with high-reward lead to enhanced free-choice priming compared to primes associated with no-reward. The effect was seen on both proportion of choices and response times, and was more evident in the faster responses. In the slower responses, the effect was diminished. Our study adds to the growing literature showing the susceptibility of masked priming to factors related to attention and executive control.

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2.1 Introduction

Masked primes influence behaviour on a range of simple tasks (Dehaene et al., 1998, 2006; Kouider & Dehaene, 2007; Greenwald, Draine & Abrams, 1996; Marcel, 1983, but see Newell & Shanks, 2014). Is it possible that we are more influenced by such masked information when they are important or valuable to us in some way? Since human perceivers attach value to external stimuli, it is likely that such additional reward information may induce greater priming effects. But, traditional theories of cognition suggest that unconscious processes are prototypical examples of automatic processes which are not prone to interference from other processes (Posner & Snyder, 1975). This has been taken to imply that only conscious processes are susceptible to any form of strategic control and nearly-invisible stimuli are outside the domain of cognitive control. But more recently, these views have changed and paved the way for more refined theories which allow for executive control over unconscious processing (Ansorge, Kunde & Kiefer, 2014; Kiefer, 2012). This is demonstrated specifically by studies that show that current task-goals (Ansorge & Neumann, 2005; Kiefer & Martens, 2010; Schmidt & Schmidt, 2010), different forms of attentional selection (Ansorge, Kiss & Eimer, 2009; Ansorge, Horstmann & Worschech, 2010; Naccache, Blandin & Dehaene, 2002) and individual differences in attention and perception (Pohl et al., 2014; Prasad, Patil & Mishra, 2017) modulate the extent of priming effects when the primes remain almost invisible. While the role of attention in masked priming has been studied using traditionally defined forms of exogenous and endogenous attention (Ansorge, Kiss & Eimer, 2009; Ansorge, Horstmann & Worschech, 2010; Sumner et al., 2007), the role of newer forms of attentional control such as reward has not been examined.

In this study we are interested in investigating if barely perceived primes that are linked to rewards of different values modulate response selection when choices are made voluntarily. Masked primes have been shown to influence response selection during simple visuo-motor tasks (Breitmeyer, Ro & Singhal, 2004; Jas ′kowski, van der Lubbe, Schlotterbeck & Verleger, 2002; Leuthold & Kopp, 1998; Neumann & Klotz, 1994; Vorberg, Mattler, Heinecke, Schmidt & Schwarzbach, 2003). Typically, a "prime" stimulus (eg., left or right arrow) is presented for a brief duration (typically less than 50 ms) followed/preceded by a mask to suppress the visibility of the prime. Participants are then presented with a target (eg., left or right arrow) stimulus to which they have to respond (eg., press left or right key). It is generally seen that responses are faster if the response required by the target (eg., left-arrow) matches the response required by the prime (eg., left arrow). In a free-choice variant of this paradigm pioneered by Schlaghecken and Eimer (2004), a neutral symbol not associated with any response (eg., a double-headed arrow) is presented as the target on some trials (followed by the primes) and the participants are asked to choose either of the responses (left or right key) on their own. In such a task scenario, participants tend to choose the response associated with the prime presented on that trial, even when they are supposed to be choosing "freely" (Kiesel et al., 2006; Prasad et al., 2017; Ocampo, 2015; O'Connor & Neill, 2010). These free-choice priming effects suggest that the primes trigger the associated response making the participants biased towards the primed-responses even when participants freely choose a response.

Attentional control over masked visual processing

Although there have been several demonstrations of masked priming of free-choice responses, it is not clear if this mechanism is completely beyond conscious control. Attentional involvement is usually not implicated since prime's influence has been taken to be 'automatic' in the conventional sense. Posner and Snyder (1975) termed a process automatic if it proceeds in the absence of conscious awareness, is not susceptible to external influences and does not depend on capacity-limited resources. However, more refined theories of automaticity (Moors & De Houwer, 2006; Kiefer, 2007) have proposed the notion of conditional automaticity where unconscious processes are automatic to the extent that they proceed without any awareness and deliberation, but are initiated subject to the top-down goals of the individual and the availability of attentional resources. The direct parameter specification (DPS) proposed by Neumann (1990) is one such theory according to which unconscious stimuli can trigger an action plan only if it matches with the currently active task intentions. For instance, Ansorge and Neumann (2005) administered a task that involved responding to black or red targets. They observed priming effects for black primes only when participants searched for black targets (Experiment 1), but not when they searched for red targets (Experiment 2). Thus, only those features of the prime that were currently useful or relevant were processed suggesting that it is possible to strategically exert control over unconscious processing (see also, Schmidt & Schmidt, 2010; Tapia, Breitmeyer, Jacob, & Broyles, 2013).

In line with this, attentional selection and prioritisation as per current goals seem to modulate the prime’s influence on response selection and action. Spatially or temporally attended primes lead to enhanced priming effects compared to unattended primes (Naccache et al., 2002; Sumner et al., 2007). For example, Naccache et al. (2002) observed that presenting targets on a predictable time scale (therefore facilitating temporal attention) lead to higher priming effects on a semantic categorisation task. Taking into account this and similar findings (Jas´kowski et al., 2002; Kiefer & Brendel, 2006; Martens et al., 2011; see Ansorge, Kiefer & Kunde, 2014 for a review), Kiefer and colleagues proposed the “attentional sensitisation model of unconscious cognition” which suggests that processing of unconscious or briefly-visible information is susceptible to top-down control triggered by currently active task representations or attentional focus (Kiefer & Martens, 2010; Kiefer, 2012). This is achieved by enhanced sensitivity to processing in the task-relevant pathways and attenuation of the processing in task-irrelevant pathways. The model, thus, explicitly predicts that unconscious processes “should depend on available attentional resources” (p.1, Kiefer, 2012). But, “attention” encompasses many forms and can be deployed in many ways (Carrasco, 2011). It is not exactly clear which forms of attention are capable of modulating the extent of masked priming and which are not.

Attentional selection mediated by reward-learning

Traditionally, attentional selection has been considered to depend on the physical properties of the stimulus (also referred to as stimulus-driven or exogenous form of attention, Theeuwes, 1992, 2010) and/or on the goals of the individual (goal-driven or endogenous form of attention, Egeth & Yantis, 1997; Folk, Remington &

Johnson, 1992; see Lamy, Leber & Egeth, 2012; Theeuwes, 2010 for reviews). Apart from these, a third form of attentional control has been proposed recently (Anderson, Laurent & Yantis, 2011a; Anderson, 2016; Awh, Belopolsky & Theeuwes, 2012) which is mediated by selection history. Stimuli that have been previously attended-to receive priority in attentional selection. Thus, current goals, physical salience and selection history all contribute to an integrated priority map (Theeuwes, 2019). Within this map, attentional priority is determined based on the strength of each of these individual factors which ultimately decides what is selected. Reward history is a form of selection history where attention is drawn to stimuli which were previously associated with reward. It is to be noted that the conceptualisation of reward as a form of attention modulator is different from reward as a psychologically motivating factor. Motivational reward is directly linked to performance (eg., giving a chocolate to a child for completing homework) and is intended to incentivise certain type of behaviour or responses. For instance, performance on a Stroop task (Padmala & Pessoa, 2010) or a Posner cueing task (Engelmann & Pessoa, 2007) is found to be enhanced when the participants are given reward contingent on their performance. Attentional reward, on the other hand, is inherently linked to certain aspects of a stimuli (feature or location) and does not depend on the participant's performance. Whether such reward-mediated attention is indeed a distinct form of attentional control (apart from endogenous and exogenous) or merely a variation of the existing categories has also been a matter of debate (Theeuwes 2018, 2019). Nevertheless, it is now commonly understood that reward-associated stimuli can modulate attentional selection.

For instance, using a negative priming paradigm, della Libera and Chellazi (2006) showed that participants found it harder to respond to a stimuli whose inhibition was previously highly rewarded. Similarly, Munneke, Hoppenbrouwers and Theeuwes (2015) showed that exogenous cues associated with higher reward lead to increased cueing effects on a Posner cueing task, compared to cues associated with lower reward. Such findings have suggested that reward-learning induces lingering biases associated with the rewarded stimuli which have been found to last days (Della Libera & Chelazzi, 2009), or even weeks (Anderson, Laurent & Yantis, 2011b) after the initial association. In most such studies, the participants are made unaware of the reward contingencies either by having separate training and testing sessions or by inducing uncertainty in the reward associations. Also, the participants are typically (falsely) led to believe that the reward points depend on their performance. These measures are taken to ensure that the observed effects are due to biases in attentional selection and not due to strategic responses to the rewarded stimuli.

Rationale for the present study

In this study, our interest was to examine whether free-choice priming is susceptible to influences by reward-learning. We adapted the masked priming design from Prasad et al., (2017) where the numbers “1” and “2” were presented as primes. The same numbers were presented as targets on forced-choice trials where participants pressed either “A” or “L depending on the target. “0” was the target on free-choice trials where participants could choose between “A” or “L. Free- and forced-choice trials were intermixed during presentation as it has been seen that free-choice priming effects are observed only when the response contingencies have already been established through forced-choice trials (Schlaghecken & Eimer, 2004). At the end of every forced-choice trial, reward points were displayed which was contingent on the target presented on that trial. That is, a reward of 10 points was given if the forced-choice target was “1” and 2 points were given for the target “2”. We also administered a control session with no reward points as a baseline measure of the masked priming effects.

On free-choice trials, participants were asked to freely choose between the response alternatives. Reward points were presented only on forced-choice trials, but not on free-choice trials. As a result, the forced-choice trials acted as the “training phase” which induced the reward-stimuli associations. The effect of these associations was dynamically tested on the free-choice trials. Several studies on reward-learning typically have a distinct training session in which the participants are trained with the reward association. The influence of this learning is subsequently examined in a testing phase without reward (see Anderson, 2016 for a review). We did not have two such separate sessions. Instead, reward learning induced in the forced-choice trials and its effects were tested dynamically on the free-choice trials since there is evidence to show that reward associations are learnt very quickly and their effects begin to show as early as on the subsequent trial (Hickey, Chelazzi & Theeuwes, 2010). Although, we present results from both free- and forced-choice trials, our primary interest was in examining if and how reward associations formed through forced-choice trials dynamically affect free-choice priming. We predicted that primes associated with higher reward would lead to enhanced free-choice priming effects on two key measures: proportion of choices and response times on free-choice trials. That is, we expected higher proportion of choices congruent with the prime on trials with high-reward primes. We also expected the RT (response time) priming effect (RT incongruent - RT congruent) to be greater for high-reward primes compared to the trials with low-reward and no-reward primes.

2.2 Experiment 1

2.2.1 Methods

Participants. Twenty healthy participants (8 females, Mean age = 23.2 years, *SD* = 2.7) were recruited for the experiment. We conducted a power analysis to determine the required sample size by including studies that have shown masked free-choice priming effects (Kiesel et al., 2006; Ocampo, 2015; Schlaghecken & Eimer, 2004). Cohen's standardised difference scores (d_z) were estimated using the reported paired-sample t test values and sample sizes (i.e., $d_z = t/\sqrt{N}$; Cohen, 1988; Hayward, Otto & Ristic, 2017; Rosenthal, 1991). The calculations were based on results reflecting differences between proportion of congruent and incongruent choices on free-choice priming trials since our main variable of interest was the choice proportion. Effect sizes estimated in this way ranged between 0.5 and 1. The power analysis for paired t tests yielded sample sizes ranging from 10 to 33 for a desired power of 0.8 with the confidence level set to 0.05. We selected a sample size to fall within this range. Power analysis was performed using the "pwr" package in R.

All participants reported normal or corrected-to-normal vision and provided written informed consent. All the procedures of this experiment and the subsequent experiments were approved by the Institutional Ethics Committee (IEC) of University of Hyderabad.

2.2.2 Procedure

Stimuli were designed and presented using the SR research experiment builder (SR Research, Ontario, Canada) on an LCD monitor with resolution 1024 * 768 pixels and refresh rate of 60 Hz. Participants were seated at a distance of 60 cm from the monitor. All stimuli were presented in black colour (CIE-Lab: 0.00, 0.00, 0.00) against a grey background (CIE-Lab: 63.33, 0.00, -0.00). All the participants took part in two sessions: reward session and a control (no-reward) session. It has been seen that presenting no-reward trials has different consequences depending on whether they are embedded within a reward context or not (Munneke et al., 2015). In our study, the no-reward trials were administered as a separate control session to 1) demonstrate that masked free-choice priming effects on choices and RTs could be successfully obtained through our paradigm and 2) serve as a baseline for the reward session. The two sessions were administered on the same day with session order counterbalanced across participants.

Reward session. Throughout the trial, stimuli were presented in the centre of the screen (Figure 2.1). The design was adapted from Prasad et al. (2017). Each trial in the reward session began with a fixation screen for 1000 ms followed by a prime (the digit “**1**” or “**2**” in Times New Roman, pt. 26) presented for 33 ms. Following the prime presentation, a mask (“######”, Times New Roman, pt. 26) was presented for 50 ms. The target screen was displayed next for 2000 ms or till a keypress was a registered. The target was either “**1**” or “**2**” on forced-choice trials and “**0**” on free-choice trials (Times New Roman, pt. 26). Participants were asked to press “A” on seeing “**1**” and to press “L” on seeing “**2**”. For the target “**0**”, they were instructed to freely choose and press either “A” or “L”. The target-response mapping was counterbalanced across participants. On free-choice trials, participants were instructed to choose spontaneously, “maintain some balance” while choosing and ensure that they don’t choose one response most of the time. After the target disappeared, the reward earned on that trial and the total reward points were displayed on the screen, only on forced-choice trials. A reward of two points (low-reward) was given whenever the forced-choice target was “1”. Ten points (high-reward) were given when the forced-choice target was “2”. This mapping between the target and the reward-level was counterbalanced across participants. Five points were deducted on error trials. The participants were told that the reward points depended on their performance. To ensure that the participants don't explicitly become aware of the reward association, the reward contingency (between high/low reward and the targets 1/2) was reversed on 20% of the trials. A practice session of 40 trials was administered before the experiment.

The reward session consisted of two blocks of 200 trials each, for a total of 400 trials in the experiment. Each block consisted of 120 free-choice trials and 80 forced-choice trials with the masked primes divided equally between “1” and “2”. The free- and forced-choice trials were randomly mixed in each block. The target stimuli on forced-choice trials was also equally divided between “1” and “2” mapped to the two different reward-levels. Participants were given three self-paced breaks during the experiment.

Control session. The sequence of events in the control session were similar to those in the reward session except for the presentation of the reward points. The numbers

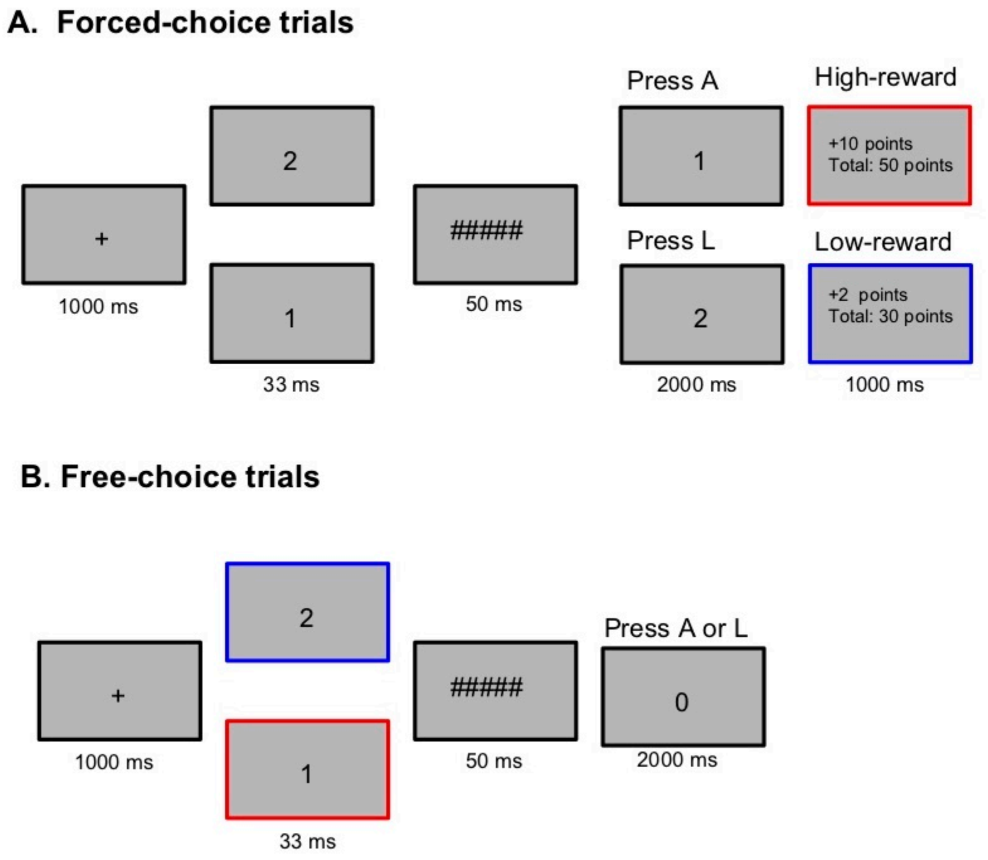

Figure 2.1. Trial structure during the reward session in Experiment 1. In the reward session forced-choice trials (A), participants were required to press A on seeing the target "1" and press L on seeing the target "2". Reward points (high: 10 or low: 2) contingent on the target were presented only on forced-choice trials. On free-choice trials (B), they were asked to freely choose between A and L. The red and blue highlights were not present in the experiment. It is used here only to demonstrate that a high-reward target on a forced-choice trial ("1" in this example) is also a high-reward prime on a free-choice trial. A control session with similar trial structure (but without the reward points) was also administered with "3" and "4" as prime/target stimuli.

"3" and "4" were used as prime/target stimuli. This was done to avoid possible carryover effects from the reward session (in those participants who performed the reward session first). The control session similarly consisted of two blocks with each block containing 200 trials (120 free-choice trials + 80 forced-choice trials). Twenty trials were given for practice before the main experiment.

Visibility tests. Two visibility tests of 120 trials each were administered after the reward and control sessions were both completed, to assess the visibility of the masked primes. The visibility tests were administered at the end to ensure that the participants don't explicitly become aware of the masked primes during the priming experiment. Henceforth, we will use "reward visibility test" to refer to the test with the primes "1" and "2" - used in the reward sessions. The "control visibility test" included primes "3" and "4". The trial structure was similar to those in the priming experiments, except for the presentation of the target and the reward points. Instead of the target used in the priming experiments, participants were presented with "1" written in the center of a grey box (2°×2°) placed on the top-left corner of the display and "2" placed inside a box on the top-right corner of the display. Participants were instructed to click on the number that matched with the prime. Similarly, the numbers "3" and "4" were placed on the screen for the control visibility test. The primes were presented for 33 ms (masked trials) or for 200 ms (control trials). A different mode of response was selected for the visibility test to ensure that previously established stimulus-response mappings don't contribute to the responses leading to "priming of awareness" (Lin & Murray, 2014).

Each block of 120 trials consisted of 100 masked trials and 20 catch trials randomly intermixed. Catch trials with longer prime duration were included as a performance check to confirm if the participants understood the instructions and were attentive to the task (Lin & Murray, 2014). The mapping between the prime stimuli and its location on the display (left or right) was counterbalanced across participants. After each mouse click, the cursor was designed to re-position at the center of the screen. There was no time constraint for the responses. Twenty practice trials were given before the main experiment. Participants could also take a self-paced break halfway through the experiment.

2.2.3 Data analysis

Data from participants who chose one of the keys on more than 75% of the trials (four participants) was discarded as it violated our instruction to "maintain some balance" between both the key choices. Free- and forced-choice trials were analysed separately. Trials with RT less than 150 ms and greater than 2.5 median absolute deviation (MAD) away from the median RT (Leys, Ley, Klein, Bernard & Licata, 2013) were excluded (Reward session: 8.1 % of free-choice and 6.14 % of forced-choice trials; Control session: 8.9 % of free-choice and 7.5 % of forced-choice trials). The MAD criterion was used because the common practice of discarding outliers 2 to 3 standard deviations away from the mean is unlikely to detect outliers correctly in small samples (Cousineau & Chartier, 2010). Trials with incorrect responses were also excluded (5.7 % and 7 % in the forced-choice trials of the reward and control session, respectively; there was no concept of "error" in free-choice trials). Mixed-effects models were used to analyse the data of all experiments using lme4 package in R environment (Bates, Sarkar, Bates & Matrix, 2007). In all the analyses, random effects for Participants were included. Please see Table 2.1 for descriptive statistics.

Forced-choice trials. Forced-choice trials were analysed first. A forced-choice trial was termed congruent if the prime matched the target. Congruency (congruent: -1, incongruent: +1) was added as a fixed effect to examine whether primes had any influence on the RTs. In the forced-choice trials, we were primarily interested in examining if the reward learning had taken place. To examine this, we analysed the forced-choice RTs as a function of the "target-type". One columns was created to compare high-reward with no-reward (HighNo: no-reward: -1, high-reward: +1) and another to compare low-reward to no-reward (LowNo: no-reward: -1, low-reward: +1). Target-type was determined based on the target-reward associations. For example, in one of the versions of the tasks, participants received high-reward when the target was 2 and low reward when the target was 1 in the forced-choice trials. Thus, for this version, all trials with target 2 were designated as target-type:high and trials with target 1 were designated as target-type: low. HighNo, LowNo and their interactions with congruency were entered as fixed effects in the mixed-effects analysis. The p-values of the effects were determined using Satterthwaite approximations to degrees of freedom, as implemented in the lmerTest function (Bates, Maechler, Bolker & Walker, 2015).

Accuracy analysis was performed on forced-choice trials by dummy coding responses into 1's (correct) and 0's (incorrect). Analysis was performed through generalized linear mixed-effects modelling (GLMM) using *glmer* function with family specified as binomial and link logit. Congruency and target-type were similarly sum-coded as in the RT analysis and entered as fixed effects. *p* values were obtained through the default output of the glmer function in R based on asymptotic Wald tests (Luke, 2017).

Free choices trials. A response on a free-choice trials was termed "congruent" if the participant chose the key (eg., "A") associated with the prime (eg., "1") presented on that trial. If not (eg., "L"), the choice was termed "incongruent". It is to be noted that "congruency" was only determined during the data analyses stage. Binomial tests comparing the proportion of congruent choices with chance (0.5) were first conducted using the binom.test function in R to examine if there was a global influence of the prime on free choices. Significantly higher proportion of choices indicate that the primes biased participants' responses irrespective of other factors. Next, the effect of other variables on congruency was examined using glmer function with family specified as binomial and link logit (similar to accuracy analysis). Congruency variable was dummy coded into 1's (congruent) and 0's (incongruent). Prime-type (HighNo and LowNo) factors were sum coded and included as fixed effects. The prime-type here indicates the reward associated with the numbers presented as primes. No target-related reward was given on free-choice trials. All main effects and interactions were included in the model. p values were obtained through the default output of the glmer function in R based on asymptotic Wald tests (Luke, 2017). Mixed effects analyses were conducted on free-choice RT using the lmer function with congruency (congruent: -1, incongruent: +1), prime-type (HighNo, LowNo) and their interactions as fixed effects. The lmerTest analysis was similar to that of forced-choice RT. The effect of the order in which the reward session was administered was analysed by entering order (reward session first: +1, reward session second : -1) as a fixed effect in the analyses.

Some studies have shown that effects of salience are short-lived (< 300 ms) and disappear at longer response latencies (eg., Donk & Van Zoest, 2008). Distributional analyses of the data was done using the vincentisation procedure (Ratcliff, 1979) to examine whether the effect of reward-learning was more evident during the faster responses. The free-choice RT data of each participant was arranged in ascending order and five bins were created by aggregating the data of each participant. Each bin comprised of the 25% of the data of each participant. Mixed effects analyses were conducted on choices and RT with Bin as the additional factor. Bin (Bin1, Bin2, Bin3, Bin4, Bin5) was dummy coded and entered as a fixed effect with Bin1 as the reference level. The interpretation of main effects in mixed effect model outputs is problematic when variables are dummy coded (Singmann & Kellen, 2019). For example, the main effect of congruency when Bin is entered as a dummy coded fixed effect refers to the difference between congruent and incongruent for Bin1 and not the difference between the congruency conditions averaged across ALL Bins which is we what we require. Thus, we will refer to the models without Bin for main effects of prime-type/congruency (in which all factors are sum-coded) and only look at the Bin related interactions in this analysis.

Prime-visibility. Trials with responses faster than 150 ms and slower than 1500 ms (Reward session: 8%, Control session: 5%) were discarded from the analysis. Accuracy on the catch trials was calculated first to assess if the participants understood the instructions and attended to the task. The prime "1" was arbitrarily designated as the signal and "2" was designated as the noise. Thus, correct responses to the prime "1" were considered as Hits and incorrect responses to the prime"2" were considered as False Alarms (FAs). Hits and FAs were corrected using the log-linear rule to adjust for occurrences of Hits and FAs of 0 or 1 (Hautus, 1995). Hit rate was calculated by dividing the number of Hits by the total number of signal trials. Similarly, FA rate was the number of FAs divided by the total number of noise trials. We computed d' on the masked trials for each participant as the difference between the z transform of Hit rate and the FA rate using the qnorm function in R. t tests comparing the d' values with chance performance (d' = 0) were conducted to assess the visibility of the primes. Paired t-tests were also conducted to examine the difference in prime visibility between the reward and control sessions.

2.2.4 Results

Forced-choice trials. The responses on congruent trials were faster compared to incongruent trials, $\beta = 19.79$, $t = 8.83$, $p < 0.001$. Accuracy was also greater on congruent trials, $\beta = -0.43$, $z = -6.07$, $p < 0.001$. These results show a clear facilitatory influence of the masked primes. Importantly, participants were faster and more accurate responding on trials where the target resulted in high-reward as opposed to no-reward, as indicated by a main effect of target-type (RT: $\beta = -7.73$, $t = -2.35$, $p = 0.019$; Accuracy: $\beta = 0.27$, $z = 2.43$, $p = 0.015$) confirming that participants indeed associated the numbers with the given reward points in the forced-choice trials (Figure 2.2). Interestingly, responses on low-reward trials were slower than no-reward trials, $\beta = 8.53$, $t = 2.57$, $p = 0.01$. Below, we examine the effect of reward-learning on the priming effect in free-choice trials.

Free-choice trials. Participants chose the congruent response (59.7 %) more often than the incongruent response in the control session ($p < 0.001$). The proportion of congruent responses (56.1 %) were also significantly greater than chance in the reward session ($p < 0.001$). This indicates that the primes had a strong influence on the choice behaviour in both reward and control sessions. The lmer analysis on the choice data further showed that the proportion of congruent choices for low-reward primes was lesser than no-reward primes, as shown by a significant main effect of prime-type for LowNo condition, β = -0.12, z = -2.91, *p* = 0.004 (Figure 2.3). There was no main effect of HighNo condition, β = 0.01, *z* = 0.33, *p* = 0.744. The distributional analysis with five Bins showed a significant interaction between Bins and reward conditions for both HighNo (Bin3: β = -0.31, *z* = -2.41, *p* = 0.016, Bin4: β = -0.59, *z* = -4.54, *p* < 0.001 , Bin5: β = -0.44, *z* = -3.43, *p* < 0.001) and LowNo conditions (Bin3: β = 0.24, *z* = 1.92, *p* = 0.054, Bin4: β = 0.43, *z* = 3.36, *p* < 0.001, Bin5: β = 0.41, *z* = 3.19, *p* = 0.001). In Bin1 and Bin2, there were more congruent choices associated with high reward primes (Bin1: β = 0.06, *z* = 3.44, *p* < 0.001, Bin2: β = 0.04, *z* = 2.18, *p* = 0.029) and fewer choices associated with low-reward primes compared to no-reward primes (Bin1: β = -0.08, *z* = -4.18, *p* < 0.001, Bin2: β = -0.04, *z* = -2.2, *p* = 0.028, Figure 2.4). There was no effect of reward condition in Bin3 (*p* > 0.2). In Bin4, high-reward primes lead to fewer congruent choices compared to no-reward (β = -0.06, *z* = -3.07, *p* = 0.002). There was no effect of low-reward in Bin4 (*p* > 0.5). Neither of the reward conditions had any effect in Bin5 (*p* > 0.1)

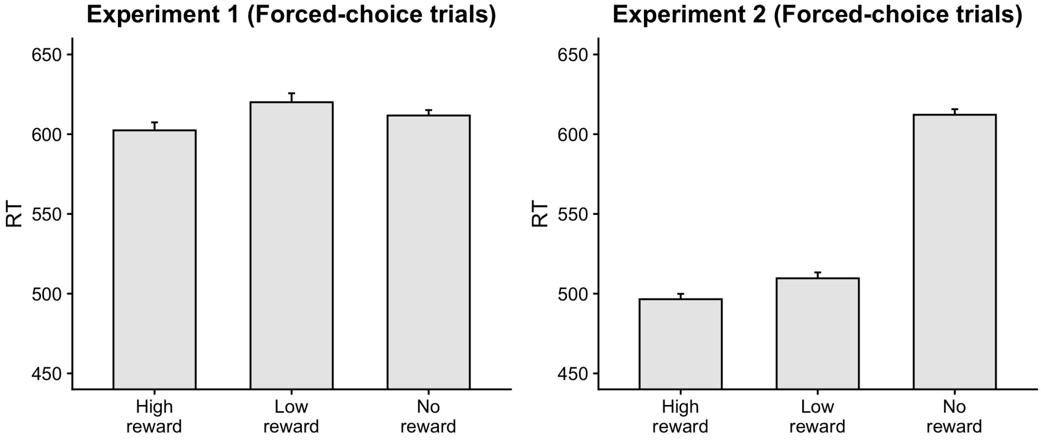

Figure 2.2. RT on forced-choice trials (Experiment 1 & 2). Faster responses were seen on forced-choice trials for targets associated with high-reward compared to no-reward.

Analysis of free-choice RTs showed faster RT for congruent incongruent responses compared to incongruent responses, β = 5.79, *t* = 3.46, *p* < 0.001. There was a significant interaction between prime-type and congruency (LowNo: β = -11.06, *t* = -4.66, *p* < 0.001) revealing substantially lower priming effects for low-reward primes compared to no-reward primes (Figure 2.3). No such interaction was observed for the HighNo condition (β = -0.07, *t* = -0.03, *p* = 0.976). There was a main effect of prime type (LowNo: β = -4.6, *t* = -1.96, *p* = 0.05) indicating that responses on trials with low reward-associated primes were faster compared to the control

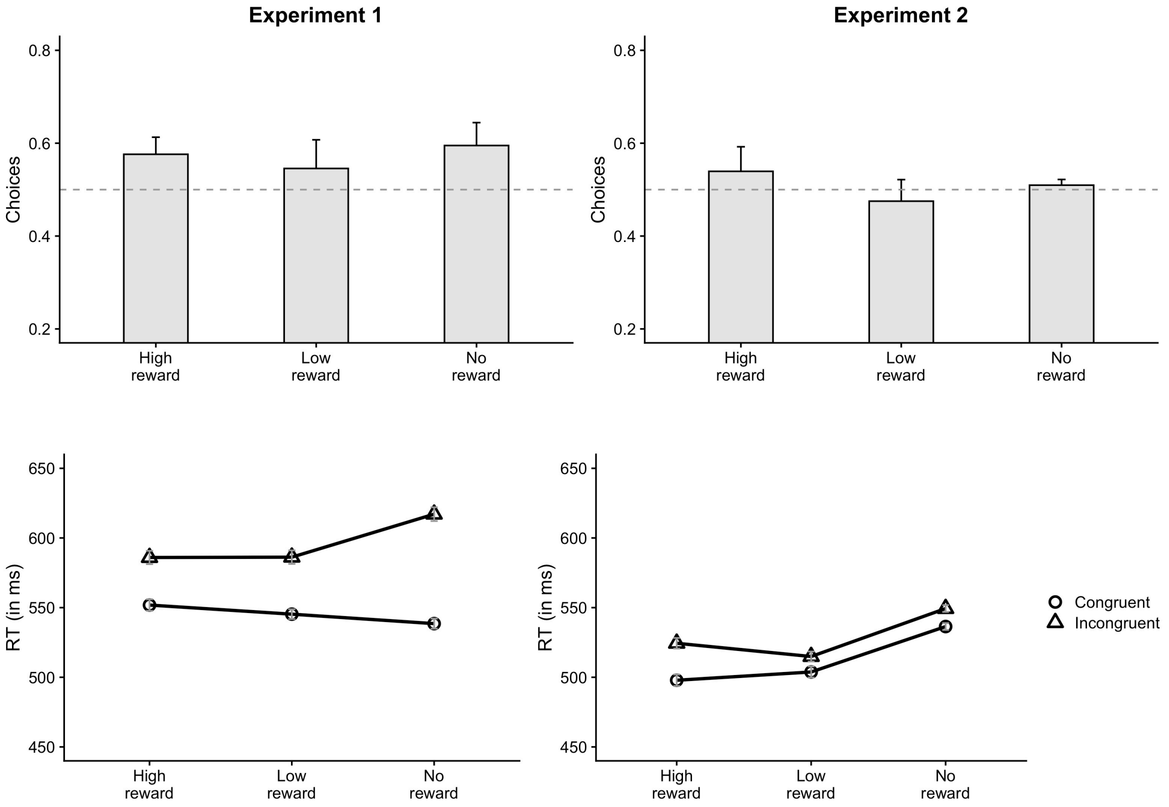

Figure 2.3. Choice and RT data (Experiment 1 & 2). In Experiment 1 (left), participants chose the congruent responses less often on low-reward trials compared to no-reward trials. The RT priming effect on low-reward trials was also lesser than no-reward trials. In Experiment 2 (right), congruent choices on high-reward trials were greater than no-reward trials. Congruent choices on low-reward trials were lesser compared to no-reward trials. No effect of reward was observed on RT priming effect.

Note: “No-reward” corresponds to the control session trials. Error bars indicate ± 1 SE. The horizontal dashed line in the choice plot indicates chance level (50%).

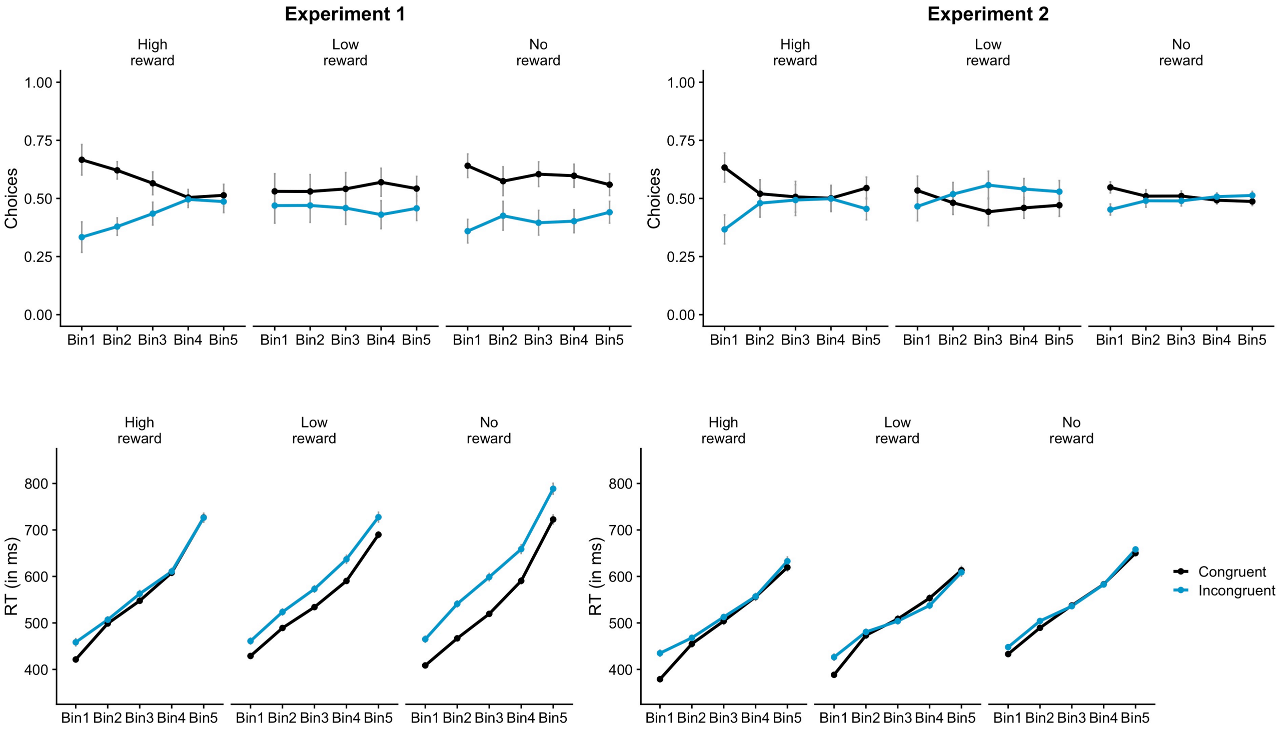

Figure 2.4. Distributional analysis of free-choice trials in Experiment 1 and 2. In Experiment 1 (left), participants chose the congruent response more often when the primes were associated with high-reward compared to no-reward only in the faster responses (Bin1 and Bin2). RT priming effects on reward trials (high & low both) were consistently lesser compared to no-reward across all Bins. In Experiment 2 (right), there were more congruent choices on high-reward trials compared to no-reward during the fastest responses (Bin1). The effect was diminished in the subsequent bins. RT priming effect was greater for high-reward primes compared to no-reward in the fastest responses (Bin1). Similar effect was seen for low-reward primes in Bin2.

condition. The distributional analysis show a significant three-way interaction between HighNo, Bin and congruency (Bin5: $\beta = -17.84$, $t = -4.13$, $p < 0.001$). The interaction between LowNo condition, congruency and Bins were also significant (Bin5: $\beta = 10.43$, $t = 2.45$, $p = 0.014$). Separate models were created for each Bin. The interaction between congruency and LowNo condition was significant in all the Bins except Bin5 (Bin1: $\beta = -7.47$, $t = -3.41$, $p < 0.001$, Bin2: $\beta = -7.70$, $t = -4.28$, $p < 0.001$, Bin3: $\beta = -8.51$, $t = -4.24$, $p < 0.001$, Bin4: $\beta = -9.46$, $t = -3.47$, $p < 0.001$) indicating reduced priming effects for low-reward trials compared to no-reward trials (Figure 2.4). Similar reduction in priming effects for high-reward trials compared to low-reward trials was seen for Bin2, Bin3 and Bin5 (Bin2: β = -3.91, t = -2.13 p = 0.034, Bin3: β = -5.41, t = -2.62, p = 0.009, Bin5: β = -12.97, t = -3.01, p = 0.003)

Table 2.1 Descriptive statistics for free-choice trials (Experiment 1 & 2)

	Experiment 1			**Experiment 2**		
	Choices	**RT**		**Choices**	**RT**	
	C	**C**	**IC**	**C**	**C**	**IC**
High-reward	57.6 (4)	552 (4)	586 (5)	53.9 (5)	498 (4)	524 (4)
Low-reward	54.5 (6)	545 (3)	586 (5)	47.5 (5)	504 (4)	515 (4)
No-reward	59 (5)	538 (4)	617 (5)	50.9 (1)	536 (2)	549 (2)

Note: Choices are given in percentage and RT in ms. C: congruent, IC: incongruent. Only congruent choices are given because proportion of incongruent choices = 1 - C. The numbers in brackets denote 1 *SE*.

Visibility test (Reward & Control sessions). Participants performed very well on the catch trials in the reward and the no-reward sessions (Reward: *M* = 95.2%, *SD* = 5.3; Control: *M* = 97.4 %, *SD* = 3.6). The prime visibility index d' deviated significantly from zero for both reward and control sessions (Reward: *d'* = 1.37, *SD* = 0.59, t (1, 15) = 9.4, $p < 0.001$, Control : *d'* = 1.52, *SD* = 0.5, t (1, 15) = 12.18, $p < 0.001$). Prime visibility did not differ between Reward and control (without reward) sessions, t (1, 15) = -0.93, $p = 0.368$.

2.2.5 Discussion

The control experiment replicated free-choice and forced-choice priming effects observed in several previous studies (eg., Kiesel et al., 2006; Prasad et al., 2017). The masked primes lead to higher proportion of congruent choices (on free-choice trials) and faster response times on congruent trials (on free- and forced-choice trials). This confirms that our design was capable of capturing masked priming effects. The analysis of the forced-choice trials of the reward session showed faster responses for visible targets associated with high-reward compared to no-reward confirming that participants learnt to associate reward with the numbers presented on the forced-choice trials.

The free-choice trials of the reward session were then compared with those of the control session to examine the effects of reward-learning on free-choice priming effect. The analysis on the proportion of choices showed that participants chose the response associated with high-reward primes more often than no-reward primes, but only in the faster responses (Bin1 and Bin2). In the slower responses (Bin4), the opposite pattern was observed. The effect of the prime on the choices was also lower for the low-reward condition compared to no-reward in the faster responses (Bin1 and Bin2). The RT data also showed a similar pattern. Priming effects (RT on incongruent trials - RT on congruent trials) on both high-reward and low-reward trials were substantially lower than priming effects on no-reward trials, irrespective of whether the responses were faster or slower.

These findings provide evidence for reward-mediated control on masked priming effects. The choice data indicates that reward-learning facilitated priming effects for the faster responses, but as the responses became slower the effect of reward-learning diminished. Faster RTs are generally considered to capture the saliency effects while longer RTs are considered to be a result of top-down processes (Theeuwes, 2018). Thus, this result is in line with studies that have shown that reward primarily modulates the perceptual saliency of the associated objects (Hickey, Chelazzi & Theeuwes, 2010).

2.3 Experiment 2

The primary objective of Experiment 2 was to replicate the findings of Experiment 1 with a different set of prime and target stimuli. This was deemed necessary because the numbers used for reward levels (“2” and “10”) were similar to the prime/target stimuli (“1” and “2”). It is possible that this similarity lead to or at least confounded the observed effects in Experiment 1. Thus, in Experiment 2, we tested the effect of reward levels using arrows as prime/target stimuli. Arrows are the most commonly used stimuli in masked priming paradigms (eg., Atas & Cleeremans, 2015; Eimer & Schlaghecken, 2003; Kiesel et al., 2006). If reward value indeed modulates masked-priming effects irrespective of the type of stimuli used, we expected to replicate the findings from Experiment 1.

2.3.1 Methods

Participants. Twenty healthy participants (10 females, Mean age = 24 years, *SD* = 2.6) took part in Experiment 2. None of them had participated in Experiment 1.

2.3.2 Procedure.

The trial structure and timing in Experiment 2 was exactly the same as in Experiment 1. The only difference was that the prime and target stimuli in the reward sessions were replaced by arrows. Participants could be presented with "<" or ">" as primes. The same arrows were also presented as targets on forced-choice trials. "<>" was presented as the target on free-choice trials. The mask was same as that used in Experiment 1. Participants were asked to press "A" on seeing "<" and "L" on seeing ">". This mapping was not counterbalanced to avoid potential slowing down due to the spatial incongruency between the arrow direction and response key location in the counterbalanced version. The mapping between reward value and target was counterbalanced as in Experiment 1. In the control session, up ("^") and down ("v") arrows were used as prime/target stimuli on forced-choice trials and their combination was presented as the target on free-choice trials. "A" and "L" were used as response keys and the target - response mapping was counterbalanced across participants. The order of reward and control sessions was also counterbalanced across participants. In the end, participants took part in visibility tests corresponding to the reward and control sessions. The number of trials in each session and the blocking levels were same as in Experiment 1.

2.3.3 Data analysis

Three participants' data was excluded because they chose one of the responses more than 75% of the times in free-choice trials. One participant did not complete the control session. Thus, the final analyses included data from 16 participants. The data analyses procedure for all the variables was similar to that used in Experiment 1 (see Table 2.1 for descriptive statistics). Outliers were discarded in the reward session (free-choice: 9 %; forced-choice: 5 %) and the control session (free-choice: 9.1 %; forced-choice: 6.4 %) based on the MAD criterion. Incorrect trials were also excluded from the analysis (reward: 3 %; control: 4.6%). In the prime visibility test, 4.5 % of trials in the reward session and 7 % of trials in the control session were discarded as outliers.

2.3.4 Results

Forced-choice trials. Congruent responses were executed faster and more accurately than incongruent responses, (RT: $\beta = 8.48$, $t = 3.46$, $p < 0.001$; Accuracy: $\beta = -0.26$, $z = 2.73$, $p = 0.006$. Participants were also faster responding on trials with high-reward and low-reward target-type compared to no-reward trials (HighNo: $\beta = -45.78$, $t = -12.62$, $p < 0.001$; LowNo: $\beta = -32.88$, $t = -9.03$, $p < 0.001$). The accuracy on high-reward trials was also greater than no-reward trials, $\beta = 0.46$, $z = 3.07$, $p = 0.002$. These results confirm that participants indeed associated the target stimuli with the given reward points (Figure 2.2).

Free-choice trials. Binomial tests showed that the proportion of congruent choices was not significantly different from chance (0.5) for both reward (50.7 %), p = 0.417 and control sessions (50.9 %), p = 0.249. The mixed-effects analysis on the proportion of congruent choices revealed a significant effect of prime-type. These main effects show that participants chose the congruent response more often for the high-reward primes compared to the no-reward primes (HighNo: β = 0.12, $z = 3.23$, $p = 0.001$, Figure 2.3). But, the proportion of congruent choices were lower for low-reward primes compared to no-reward primes (LowNo: β = -0.13, $z = 3.46$, $p < 0.001$). The distributional analysis revealed a marginally significant interaction between HighNo and Bin2 (β = -0.22, $z = -1.8$, $p = 0.072$). Separate models showed greater congruent choices for high-reward trials compared to no-reward trials in Bin1 (β = 0.06, $z = 2.9$, $p = 0.004$) and Bin5 (β = 0.04, $z = 2.07$, $p = 0.038$, Figure 2.4). The opposite effect was observed for LowNo condition, only in Bin3 (β = -0.05, $z = -2.3$, $p = 0.021$).

The free-choice RTs were faster on congruent trials compared to the incongruent trials indicating a global influence of the masked prime on response-times (β = 4.31, $t = 4.01$, $p < 0.001$). There was a significant main effect of prime-type (HighNo: β = -9.85, $t = -6.2$, $p < 0.001$; LowNo: β = -11.48, $t = -7.23$, $p < 0.001$) indicating that responses on trials with reward-associated primes were faster than the control trials. The interactions between congruency and prime-type (Figure 2.3) was neither significant for the HighNo condition (β = 2.15, $t = 1.31$, $p = 0.189$) nor for the LowNo condition (β = 0.05, $t = 0.03$, $p = 0.974$). The distributional analysis showed a significant three-way interaction between prime-type, congruency and HighNo condition (Bin2: $\beta = -5.33$, $t = -2.08$, $p = 0.037$, Bin3: $\beta = -5.27$, $t = -2.07$, $p = 0.038$, Bin4: $\beta = -7.49$, $t = -2.9$, $p = 0.004$; Bin5: $\beta = 6.48$, $t = -2.54$, $p = 0.011$). The three-way interaction between LowNo, congruency and Bin was marginally significant (Bin5: $\beta = 64.65$, $t = 1.84$, $p = 0.065$). Separate models created for each Bin showed marginally greater priming effects for high-reward trials compared to no-reward trials only in Bin1 ($\beta = 3.06$, $t = 1.66$, $p = 0.097$, Figure 2.4), but not in the subsequent Bins ($p > 0.4$). Greater priming effects for low-reward trials compared to no-reward were also seen in Bin2 ($\beta = 2.06$, $t = 1.71$, $p = 0.088$)

Visibility test (Reward and Control sessions). The mean accuracy on the catch trials in the reward and the control session was 97% (*SD* = 5.7) and 96 % (*SD* = 7.2) respectively. The prime visibility index deviated significantly from zero in the reward (Mean *d'* = 1.69, *SD* = 0.69, t (1, 15) = 9.72, $p < 0.001$) as well as the control sessions (Mean *d'* = 1.62, *SD* = 0.73, t (1, 14) = 8.9, $p < 0.001$). The paired t-test showed no significant difference between the discrimination performance in the reward and control sessions, t (1, 15) = -0.38, $p = 0.711$.

2.3.5 Discussion

Experiment 2 replicated the main finding of Experiment 1 - primes associated with higher reward lead to higher free-choice priming effects compared to no-reward primes in the faster responses. This effect was seen both in proportion of choices and RT data. Participants chose the response associated with the high-reward prime more often compared to no-reward prime in faster responses. Similarly, priming effects was greater for both high-reward primes and low-reward primes compared to no-reward primes in the faster responses. In slow responses, priming effect for high- and low-reward primes was equivalent to that of no-reward primes. The results of both experiments together provide converging evidence for the role of reward value on masked free-choice priming.

2.4 General discussion

In two experiments, we examined whether high- and low- reward values associated with masked primes modulated free-choice priming effects. The reward-learning was induced by target-contingent reward on forced-choice trials. We tested with both number (Experiment 1) and arrow primes (Experiment 2). In both experiments, we observed higher proportion of congruent choices for primes associated with high-reward as opposed to no-reward. This was observed predominantly in the faster responses in both experiments. The RT priming effect in free-choice trials was consistently reduced for high-reward condition compared to no-reward in Experiment 1. In Experiment 2, priming effect for both high- and low-reward primes were greater than no-reward primes, but this was only observed for the faster responses.

To our knowledge, this is the first set of findings that show the susceptibility of masked priming effects to reward-mediated attentional control. Most studies so far have shown that the extent of the influence of masked primes depends on current task expectations, spatial and temporal attention and other factors. We propose an additional factor, in the form of reward-learning, capable of modulating masked visuomotor priming. These findings are also in line with recent studies that have shown that masked visual processing is flexible and susceptible to top-down control (Kiefer, 2012). Models such as the attentional sensitisation model have proposed that while certain unconscious processes proceed in a manner that can be considered automatic, they are triggered subject to currently active task representations, deployment of attentional resources and other factors (Ansorge et al., 2014). In line with this, we find that reward-mediated attentional selection can modulate masked priming of free-choices.

The influence of reward-learning on free-choice priming was evident in the faster responses (in the choice data in Experiment 1 and 2, and RT data in Experiment 2). Both neurophysiological and behavioural evidence exists to suggest that reward drives attentional selection by modulating saliency of the perceptual representations of reward-associated stimuli (Bucker, Silvis, Donk & Theeuwes, 2015; Failing et al., 2015; Hickey, Chelazzi, & Theeuwes, 2010). Such saliency-based effects are expected to be short-lived. In fact, Donk and Van Zoest (2015) showed that saliency-driven attentional selection is prominent up until ~350 ms after the onset of the stimulus. Thus, our results are in line with this proposal and suggest that reward modulates perceptual salience of the masked primes and biases selection by favouring the competition towards rewarded masked primes in the early stages of visual processing. However, it is to be noted that our tentative conclusions are based on behavioural data. Neurophysiological evidence would be necessary to determine exactly which stage of visual processing during masked priming is modulated by reward-learning.

The finding of increased reward-mediated effects in the faster responses is also consistent with rapid-chase theory (Schmidt, Niehaus, & Nagel, 2006) and the feedforward model of masked priming (Lamme & Roelfsema, 2000). According to these, priming effects are a result of the feedforward signals from the prime and the target which proceed without the involvement of conscious report. The prime independently drives responses initially which are then taken over by the target. As responses get slower, an inhibitory mechanism is triggered against the primed responses leading to reduced or inverse priming effects. Our results are in line with these predictions (Panis & Schmidt, 2019; Sumner, 2007). In both the studies, RT and choice priming effects were reduced or non-existent in Bins4 and 5.

The results from RT data in Experiment 1 seem to be inconsistent with this explanation. RT priming effects were reduced for high-reward primes (compared to no-reward) even for the fastest responses which is in contradiction with the saliency-based explanation. We would have expected higher RT priming effects for high-reward condition in the faster responses. This is possibly related to the finding that the reward-related effects were stronger in Experiment 2 compared to Experiment 1. This is supported by considerably faster RT for both high- and low-reward targets compared to no-reward (high: 496 ms, low: 510 ms, no: 612 ms) in the forced-choice trials in Experiment 2. In contrast, the difference between high-reward (602 ms) and no-reward (612 ms) targets on forced-choice trials was much lesser in Experiment 1 suggesting that reward-learning on forced-choice trials was stronger in Experiment 2 compared to Experiment 1. Further, reward-related effects persisted in the free-choice data even for slow responses in Experiment 2, whereas they disappeared for the slow responses in Experiment 1. Thus, since the efficiency of reward-learning on forced-choice trials in Experiment 1 was weak to begin with, it is possible that its subsequent effects on free-choice trials dissipated quickly resulting in reduced effects on response times. It is also possible that the participants inhibited the prime-related response to follow the instruction to maintain "some balance" while choosing. This could also explain the inconsistent effects across different time bins in both the experiments. Another potential reason for the inconsistent effects could be low power of our experiments. Although we performed a power analysis, we selected a sample size (20) well within the upper limit of the desired range (33). Further, apart from the number of participants, the total number of trials per each participant also contributes to the overall power of a study. We acknowledge that we might have observed more robust effects with an increased number of participants/trials.

In the choice and RT analysis of both the experiments, barring few exceptions, the effect of reward-learning was generally lower for low-reward condition compared to no-reward. This could be because we administered reward and no-reward trials (control session) in separate blocks. Thus, in the reward session, participants either got high- or low-reward which could have lead to the devaluation of the low-reward trials. We might have observed a graded effect (high > low > no) if we had intermixed no-reward trials with reward trials (Munneke, Hoppenbrouwers, & Theeuwes, 2015 for one such example).

It is is important to note that interpreting the role of reward-learning on forced-choice trials in our study is not straight-forward. On every forced-choice trial, the prime could be linked to either high- or low-reward. Similarly, the target could also be associated with either high- or low- reward. Thus, high-reward targets are greatly facilitated by congruent primes (which are also high-rewarded) as opposed to low-reward targets which are poorly facilitated by congruent primes (which are low-rewarded). This is an artefact that complicates interpreting the effect of reward-learning on forced-choice priming. The origin of the problem is obvious. We used the forced-choice trials to train the reward associations in the participants. One way to get around this problem is to have a separate training session where the prime/target stimuli are associated with reward points. While this doesn't completely solve the problem, the issues caused by immediate appearance of reward points following forced-choice targets can be avoided. Another, probably more efficient way would be to associate reward with prime locations so that the strength of prime processing is modulated independent of the target (Sumner et al., 2006, Schmidt & Seydell, 2008).

It is possible to question whether the reward was getting associated with the prime or the target since both were displayed on every forced-choice trial before the reward was presented. We believe that the reward was associated with the targets since the number of points gained depended on the type of target. There was no pattern associated with the presentation of the primes. Hence, it is not possible for any systematic learning to have taken place associating the reward levels with the type of prime. Another potential concern could be that we did not have a separate training sessions to induce reward associations. One of the criticisms against evaluating such dynamic learning of reward values through a single experiment where the rewarded stimuli is also task-relevant (instead of having distinct training and testing sessions) is that participants might strategically select reward-predictive stimuli. Thus, these effects might indicate motivation-driven response behaviour rather than a reward-mediated alteration in the attentional bias. However, this criticism may not apply to our results because reward points were not presented on the free-choice trials. Further, the reward-predictive stimuli (that is, "1" and "2") were not task-relevant on the free-choice trials as the participants only had to respond to the free-choice target ("0") and the reward-associated stimuli ("1" and "2") were masked.

Still, it is possible to question if the observed effects were indeed because the rewarded-primes triggered the corresponding responses more often. Instead, it can be said that participants simply chose the key-press (on a free-choice trial) that previously resulted in a high-reward (based on their experience with forced-choice trials), irrespective of the prime's influence. Thus, it is possible that the observed effects were not mediated through the primes and instead, reward directly influenced the responses. For instance, participants could have pressed A more often on free-choice trials (irrespective of the prime) simply because they received high-reward whenever they pressed A (on forced-choice trials). We do not think is likely as the reward contingency on the forced-choice trials was reversed on 20% trials to prevent any such explicit strategies from being formed. We also analysed our data to rule out this confounding explanation of our effects. We tested if the proportion of trials on which the participants chose the response associated with reward (irrespective of the prime) was significantly greater than chance (See Appendix A for detailed description of the analyses). In both the experiments, participants did not choose the response associated with reward significantly more than chance, (Experiment 1: t (1, 15) = 0.41, $p = 0.69$; Experiment 2: t (1, 15) = 1.5, $p = 0.157$). Thus, we can tentatively rule out prime-independent explanations for the reward-based effects seen in our study. However, since this is one of the first studies to examine the role of reward-learning on free-choice priming, we acknowledge that further studies are necessary to draw strong conclusions and rule out alternate explanations.

The objective of this study was to investigate the influence of reward-learning on masked free-choice priming. Are such free-choice priming effects restricted to response priming paradigms with identical primes/targets as used in this study? Testing the influence of the prime independent of the response can be done, for example, by using the semantic priming paradigm. Interestingly, free-choice priming effects have been demonstrated in semantic priming as well. In one such study, Ocampo (2015) administered a free-choice version of the classic number magnitude judgement task by Dehaene et al. (1998). On forced-choice trials in this task, participants saw a target (2,4,7 or 9) and were asked to press the left button if the target was less than 5 and press the right button if the target was greater than 5. On free-choice trials, people could freely choose between the two responses. Either novel (eg., 3 or 8) or repeat (eg., 2 or 7) primes were presented before the target. As expected, congruency effects were seen on forced-choice trials. Importantly, on free-choice trials, participants chose the prime-congruent responses more often and were faster while doing so. This effect was the same for novel and repeat primes suggesting that existing stimulus-response links do not necessarily enhance free-choice priming effects.

A possible limitation could be our use of points as reward. Most studies on reward-mediated attention have used financial reward where participants are given money based on their final reward scores (eg., Anderson et al., 2011; Della Libera & Chelazzi, 2006; Munneke et al., 2015). Here, we used just the display of reward points to manipulate attention. Although we did find the expected effects even without the use of financial reward, it is possible to question whether the mere display of points-earned is enough to manipulate attentional selection. Several studies have shown that reward does not always have to be monetary and that other tokens such as points (Shomstein & Johnson, 2013), food (Pool et al., 2014) and social reward (Anderson, 2015) can have a similar influence. The mere presentation of numerical reward points as feedback has the same influence as providing monetary reward on complex behaviour such as anticipatory control (Adam, Bays & Husain, 2012). These findings can be justified based on the idea of a common neural currency in the brain (Levy & Glimcher, 2012) according to which we encode the subjective value of different type of rewards on a common scale.

The discrimination performance on the visibility test was significantly greater than chance level performance in Experiment 1 and 2. Thus, the effects observed here can't be confidently generalised to stimuli that are completely below the threshold of awareness. Several masked priming studies with brief stimuli aiming to examine unconscious processing have similarly observed above-chance performance in the prime visibility test (eg., Pohl et al., 2014; Prasad et al., 2017; Sumner, 2008). It is to be noted that the objective prime visibility test is an overestimation of the true visibility of the primes during the experiment. This is because in the visibility test, the participants are explicitly informed about the primes making them task-relevant which could create an attentional template to look for the prime. These conditions are different from those in the main experiment where the participants are completely unaware of the nature of the primes. Further, a dissociation between the extent of prime awareness and the priming effect is commonly observed (eg., Francken et al., 2011; Mattler, 2003) suggesting that priming effects can be independent of the visibility of the primes.

More recently, Koivisto and Neuvonen (2020) observed above-chance performance on an objective prime discrimination task even when participants subjectively reported to have seen "nothing". Further, it has been shown that high prime-target similarity can lead to lower discrimination performance on the visibility test. This could be due to confusions and mistakenly responding to targets instead of primes on incongruent trials (Khalid, König, & Ansorge, 2011). To avoid this, we did not present the target in the visibility tests. But this gives rise to another problem: it violates the exhaustiveness criteria for unconscious processing (Reingold & Merikle, 1988). That is, the prime visibility test was not exactly same as the main priming task. Several researchers have suggested that the prime visibility test must be identical with the main priming task because differing task demands can modulate the visibility measure (Eriksen, 1960). This could be a serious limitation of our study. All these points indicate that it is necessary to conduct more studies with stricter control on the prime awareness measure. The diversity of methods in the existing literature to induce lack of awareness and measure it has lead to controversies on the most suitable method (Rothkirch & Hesselmann, 2017). One possible alternative, other than using objective visibility tests like the one used in this study, is to rely on subjective measures of awareness such as the perceptual awareness scale (PAS, Ramsøy & Overgaard, 2004). In sum, we acknowledge that our awareness measures could have limitations which prevent us from making strong conclusions about reward-related influences on “unconscious” processing.

Conclusion

The relationship between attention and awareness is one of the mostly intensely debated topics in cognitive science. Thus, determining the sources of attentional capture and selection has implications not just for research on masked processing, but to general theories of attentional selection as well. In this set of experiments, we show the influence of reward-mediated attentional selection on masked priming of free-choices. Although these results are preliminary and need further replications, they add to the growing literature concerning the depths and limits of masked visual processing.

3. Examining the influence of spatial attentional control settings on unconscious cueing of free saccades

Subliminal cues have been shown to capture attention and modulate manual response behaviour. But, the impact of subliminal cues on eye movement behaviour is not well-studied. In two experiments, we examined the following issues: Can subliminal cues influence constrained free-choice saccades? If yes, is this influence under strategic control as a function of task-relevancy of the cues? On each trial, a display containing four filled circles at the centre of each quadrant was shown. A central coloured circle indicated the relevant visual field on each trial (Up or Down in Experiment 1; Left or Right in Experiment 2). Next, abrupt-onset cues were presented for 16 ms at one of the four location. Following the presentation of the cue, participants were asked to freely choose and make a saccade to one of the two target circles in the relevant visual field. The analysis of the frequency of saccades, saccade endpoint deviation and saccade latency revealed a significant influence of the relevant subliminal cues on the spatial and temporal properties of the saccadic decisions. Latency data from both experiments showed reduced capture by spatially-irrelevant cues under some conditions. These results indicate that spatial attentional control settings as defined in our study could modulate the influence of subliminal abrupt-onset cues on eye movement behaviour. We situate the findings of this study in the attention-capture debate and discuss the implications for the subliminal cueing literature.

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3.1 Introduction

It is well known now that stimuli below the threshold of awareness can influence our responses (Breitmeyer & Öğmen, 2006; Dehaene, Changeux, Naccache, Sackur, & Sergent, 2006; Kouider & Faivre, 2017). While the effect of subliminal visual information on instructed responses is well studied (eg., Dehaene et al., 1998; Neumann & Klotz, 1994; Kiesel, Kunde, & Hoffmann, 2007), the susceptibility of voluntary eye movements to subliminal cues is not clear. Most laboratory tasks in cognitive psychological research involve responding to targets based on specific instructions. But, in our daily lives, we often choose between two or more alternatives freely, and under no specific constraints. The mechanisms involved in such "free" decisions and the factors influencing them has been an important topic of research (Haggard, 2008). Our interest was to examine if subliminal visual information can bias eye movement behaviour when participants make constrained free-choice saccades. Additionally, we also investigated if such influence is under strategic control induced by task-goals. The nature of control mechanisms at work when eye movements are triggered by subliminal cues is largely unknown. Can spatially-irrelevant subliminal cues be ignored such that they don't influence eye movement behaviour (eg., not look at the notification light on the phone while reading a book)? We examined these questions using a variant of the spatial cueing paradigm (Posner, 1980) with subliminal abrupt-onset cues on a constrained free-choice saccade task.

In one of the first studies to show the influence of masked, subliminal cues on "free" responses, Schlaghecken and Eimer (2004) asked participants to press the “left” key on seeing a left arrow, “right” key for a right arrow (forced-choice trials) and choose either of the keys for a double-headed arrow (free-choice trial). The visible targets were preceded by masked left/right arrows presented for 16 ms. On the free-choice trials, participants chose the key corresponding to the masked arrow more often than chance (in this case, 50%) suggesting that masked, subliminal visual information can influence responses even when they are freely chosen. Although it is close to two decades since this landmark study, there have only been a handful of studies examining the role of masked visual information on free choices (eg., Kiesel et al., 2006; Mattler & Palmer, 2012; Ocampo, 2015; O’Connor & Neill, 2011; Prasad et al., 2017) and specifically, free eye movements (Huang, Tan, Soon, & Hseih, 2014).

Eye movements are the primary mechanism through which we gather visual information and become aware of objects around us. But, what decides where we look? In line with previous models of oculomotor selection (Findlay & Walker, 1999; Trappenberg, Dorris, Munoz, & Klein, 2001), Godjin and Theeuwes (2002) proposed the concept of an oculomotor priority map in which saccade programming occurs through a single priority map where information from different sources is integrated. Thus, goal-relevance, physical salience and other factors compete together to drive eye movement behaviour. Godjin and Theeuwes (2002) arrived at this model based on data that showed that participants were slower making a saccade to the target (a grey open circle among red open circles) when an additional irrelevant distractor (red open circle) was suddenly presented (far from the target) than when it was not. The exogenous signals from the sudden onset of the distractor interfered with saccade planning to the target thereby slowing it down. Thus, the oculomotor selection is determined by the integration of several competing programs and the resolution of this competition determines where the eyes land (but see Kramer, Irwin, Theeuwes, & Hahn, 1999 for an alternative oculomotor model). So far, factors such as selection-history, reward learning and emotion representations been shown to compete for selection in the oculomotor priority map and direct eye movements (Belopolsky, 2015). Our interest in this study was to see if subliminal cues can compete for selection in endogenously generated saccades and bias eye movements towards the location of the cues.

There are existing studies that demonstrate the influence of subliminal cues on saccade metrics (Mulckhuyse & Theeuwes, 2010a; Van der Stigchel, Mulckhuyse, & Theeuwes, 2009; Weichselbaum, Fuchs, & Ansorge, 2014). But, most of these studies have used tasks where participants were instructed to make eye movements to specific locations. To our knowledge, there is only one study which has examined the influence of subliminal cues on free-choice saccades. Huang et al. (2014) asked participants to freely choose to saccade to one of the four placeholders (white, horizontal Gabor patches) on the screen. Prior to the eye movement response, subliminal cues were presented for 33 ms at one or more locations. The cues were white vertically oriented Gabor patches which were rendered invisible by presenting a mask display (a grid of 25 white, horizontally oriented Gabor patches) for 260 ms preceding and following the cue. The authors observed that participants were more likely to saccade to the cued location compared to chance and were also faster when they did so compared to other locations. Since this is the only study so far to have shown such an effect, we wanted to re-examine it and also investigate the time-course of this effect.

Our additional interest was to examine if it is possible to strategically control the influence of subliminal cues on free eye movements. The relative contributions of stimulus-driven and goal-driven factors to the influence of subliminal cues have been a point of debate (see Ansorge, Horstmann, & Scharlau, 2011; Mulckhuyse & Theeuwes, 2010b; Prasad & Mishra, 2019 for reviews). Many researchers have argued that subliminal cues capture attention in a purely stimulus-driven manner - that is, irrespective of their relevancy to the current task (eg., Schoeberl, Fuchs, Theeuwes, & Ansorge, 2015). Evidence for this comes from studies that show attention-capture by task-irrelevant cues. For instance, Weichselbaum et al. (2014) presented white or black subliminal distractors (filled circles) while participants were asked to make a saccade to a white target (open diamond). The authors observed oculomotor capture by the cues (slower saccade latency to the target in the presence of a distractor) irrespective of whether they had the same (white cue - white target) or different contrast polarity (black cue - white target) compared to the target. On the other hand, some others have found that attention capture by subliminal cues is contingent on attentional control settings (ACS) generated by the task-goals (Ansorge, Kiss, & Eimer, 2009; Ansorge, Horstmann, & Worschech, 2010; Lamy, Alon, Carmel, & Shalev, 2015). While there is no eye movement study in support of this, Ansorge et al. (2010) demonstrated top-down contingent capture by relevant masked cues on a task that required participants to search for a colour-defined target (eg., red) and discriminate based on its shape (diamond or square). The cues were colour singletons (eg., single green or red shape surrounded by three other red or green shapes) whose visibility was diminished by backward masking. The shape of the cue could either match or mismatch that of the target creating response congruency between the cue and the target. This was included with the additional goal of examining the extent of response activation by congruent cues which we won't discuss here. Importantly, location cue validity effects were seen only for the target-matching (red) but not for non-matching (eg., green) colour singleton cues lending support to the hypothesis that only goal-relevant masked cues capture attention.

Most studies with masked/subliminal peripheral cues have similarly manipulated relevancy based on feature match/mismatch between the cues and the targets where the task-relevance is established based on a feature such as colour, shape or onset type of the cues (along the lines of contingent-capture studies pioneered by Folk, Remington, & Johnston, 1992; see Prasad and Mishra, 2018 for a detailed tabulation of such studies). But to our knowledge, no study so far has examined if spatial relevancy can modulate attention capture by brief, nearly-invisible cues. Although, the role of spatial ACS in modulating attention capture has been studied using *visible* cues (Gaspelin & Ruthruff, 2017; Ishigami, Klein and Christie, 2009; Yantis & Jonides, 1990). Yantis and Jonides (1990, Experiment 2), for instance, used black, central arrow cues which always indicated the direction of the target letter. Participants were equally fast in identifying the target (E or H) when the target was an abrupt-onset compared to when one of the distractors was an abrupt-onset. This showed that voluntary attention to a specific location can override exogenous attention capture (in this case, by the abrupt-onset distractor).

Similarly, Ishigami et al. (2019) showed reduced attention capture by peripheral cues presented outside of the spatial ACS. They were also the first to demonstrate that multiple locations can be ignored (or attended-to) depending on the task-goals. Spatial ACS in this study was induced by instructing a group of participants to look for targets in the vertical visual field (in a display arranged in the form of a plus sign with four black-coloured figure-8 placeholders). Another group was assigned to the horizontal condition. The task was to identify a black coloured-digit (2 or 5) in the relevant visual field and press a key accordingly. Peripheral cues, created by brightening one of the figure-8 placeholders, were presented for 100 ms at the relevant-valid, relevant-invalid and irrelevant-invalid locations. Faster RT in the irrelevant-invalid condition compared to the relevant-invalid condition was seen suggesting that the irrelevant cues were not as efficient as relevant-invalid cues in capturing attention. These studies show that spatial attentional control settings can successfully modulate or even prevent capture by irrelevant peripheral cues.

It is well-known that spatial and feature-based attention differ with regard to their time-course, strength and flexibility (Carrasco, 2011; Hayden and Gallant, 2005; Liu, Stevens, & Carrasco, 2007). Thus, the role of top-down attention on subliminal processing should depend on which form of top-down attention is being manipulated (Kanai et al., 2006; Prasad & Mishra, 2019). As mentioned before, no study so far has examined the role of spatial ACS with subliminal cues on free eye movements. On each trial, participants were instructed to make a single saccade to one of the two target circles in the upper or lower visual field. A central coloured circle indicated the relevant locations for that trial (eg., blue: up, green: down). These two types of trials appeared randomly. Thus, within the relevant visual field (eg., Up), participants were free to choose either of the two locations (eg., upper left or upper right). We will refer to this as “constrained free-choice” (see Huestegge et al., 2019 for a similar terminology) to distinguish it from previous free-choice studies where there were no additional constraints on participants’ responses (eg., Huang et al., 2014). Prior to the saccade response, a subliminal cue was presented for 16 ms either in one of the two relevant locations or in one of the two irrelevant locations. The cues were expected to be masked from awareness due to the short presentation duration and the immediate display of placeholders following the cue. On a separate block of one-third of the trials, no cue was presented. We speculated that the sudden appearance of the cue on some trials might provide an alerting benefit irrespective of its location relevancy. Thus, to prevent the alerting mechanisms from confounding the orienting mechanisms triggered by the cue, cue absent trials were presented in a separate block.

We also manipulated the cue-target stimulus-onset asynchrony (SOA) to include three levels: 33 ms, 50 ms and 100 ms. Huang et al. (2014) did not manipulate SOA as they were presumably interested in obtaining the basic facilitatory effect of the subliminal cues. We wanted to replicate and extend their study by investigating the time course of these effects. The three levels of SOA were chosen based on the existing free-choice studies with manual responses that have observed facilitatory effects at short SOA and inhibitory effects at longer SOAs of a similar range (O’Connor & Neill, 2011; Prasad et al., 2017; Schlaghecken & Eimer, 2004).

Thus, we expected facilitation at 33ms and 50 ms SOA and inhibition at 100 ms SOA. The SOA condition was blocked because most studies examining the time-course of free-choice priming effects have used blocked SOA condition (O'Connor & Neill, 2011; Prasad et al., 2017; Schlaghecken & Eimer, 2004). O'Connor and Neill (2011) explicitly compared the effects of blocked vs. mixed SOAs on a free-choice priming study (Experiment 1a and 1b) using a design similar to Schlaghecken and Eimer (2004). They recommended blocking SOA in future research as it led to clearer effects of the masked primes on free-choice responses compared to the mixed SOA condition.

We measured the frequency, endpoint deviation, accuracy and latency of the saccades. First, in line with many previous studies, we expected the subliminal cues at relevant locations to influence saccades. We expected a higher frequency and faster latency of the saccades to the cued location. We also predicted the saccade end location to deviate more towards the cued location. Next, if it is possible to selectively filter out task-irrelevant information, irrelevant cues should not interfere with saccadic responses as much as relevant cues. As seen with Ishigami et al. (2009), the responses on trials with irrelevant invalid cue should be faster compared to relevant invalid cue trials.

3.2 Experiment 1

3.2.1 Methods

Sample size selection. The sample size was determined using a power analysis ("pwr" package in R). The effect size was estimated to be from 0.5 to 0.9, based on previous research on subliminal cueing of eye movements where saccade latency was measured (Huang et al., 2014; Mulckhuyse & Theeuwes, 2010b; Van Zoest & Donk, 2010). Cohen's standardised difference scores (dz) were estimated using the reported paired-sample t-test values and sample sizes (i.e., $dz = t/\sqrt{N}$; Cohen, 1988). In the only study so far that has examined the influence of subliminal cues on the frequency of free saccades (Huang et al., 2014, one-cue condition), the sample size was 23 and the effect size 0.5. The power analysis yielded a sample size with a range of 11 to 33 with the desired power of 0.8 and a confidence level of 0.05. We selected a sample size that was within this range.

Participants. Twenty-four participants (9 female, Mean age = 22.71 years, SD = 2.33) took part in the experiment. All participants reported normal or corrected-to-normal vision and provided written informed consent. All the procedures of this experiment and the subsequent experiments were approved by the Institutional Ethics Committee (IEC) of the University of Hyderabad.

Apparatus. Experiment builder software (SR Research Ltd., Ontario, Canada) was used for stimuli presentation. Stimuli were presented in a dimly lit room on an LCD monitor with 1280 * 1024 resolution and 60 Hz refresh rate. Eye movement data were recorded using Eyelink 1000 desktop mount eye tracker with a sampling rate of 1000 Hz. A chin-rest and a forehead bracket were used to stabilise the head and maintain a viewing distance of 70 cm for all participants.

3.2.2 Procedure

The experiment began with a 9-point calibration. Each trial started with a fixation cross surrounded by four black filled circles of diameter 0.8° on a grey background (CIE-Lab: 63.33, 0.00, -0.00). A fixation trigger was used to ensure that the display did not move to the next screen unless the participant fixated on the cross for a minimum of 500 ms. The four circles were placed 7° from the centre and were equidistant from each other (Figure 3.1). This was followed by a central coloured circle of diameter 1° (blue CIE-Lab: 22.83, 58.29, -94.73 or green CIE-Lab: 37.08, -40.51, 41.55). The coloured circles henceforth referred to as "ACS signal" indicated the relevant visual field for the participants on that trial. For instance, a blue circle indicated that the participants had to make a saccade to one of the two circles in the upper visual field. The green circle indicated that one of the two circles in the lower visual field were the targets. The mapping between the colour of the ACS signal and the visual field was counterbalanced across participants. Next, the outline of one of the four black circles turned white for 16 ms, acting as the cue. The cue appeared in one of the two relevant locations on one-third of the trials and in one of the two irrelevant locations on the other one-third of the trials. The cue was equally likely to appear at each of the four locations. On the remaining one-third of the trials, no cue was presented. A display screen consisting of the central coloured circle and the four black circles was then presented for a variable duration of 17, 33 or 84 ms following which the ACS signal disappeared at which stage the participants were required to make their response. Thus, there were three levels of SOA between cue onset and the offset of the ACS signal: 33 ms, 50 ms, and 100 ms.

The participants were instructed to quickly look at one of the two black circles in the relevant visual field of the trial as soon as the ACS signal disappeared. They were given a maximum of 1000 ms to initiate the eye movement response, failing which they were prompted with a message "Too slow!" written in red (50 pt, Times New Roman) on a black background accompanied by a loud beep. The participants were also instructed to maintain fixation throughout the trial until the response. When eye movements away from the fixation were detected (before the response stage), the trial was aborted with a message written in red “Trial aborted” accompanied by a beep. An error message was similarly given if the saccades landed in a wrong (irrelevant) location. A blank screen was presented for 500 ms after every trial.

There were a total of 540 trials in the experimental session divided into two blocks: 360 trials with cue and 180 trials without the cue. This was the first level of blocking within which the trials were further divided equally between three levels of SOA (33, 50, 100 ms) and were presented in blocks. Within each SOA block, the trials were again divided equally between the two ACS signals (up and down) and presented randomly. The subliminal cues were presented in a relevant location half the times and in the irrelevant location half the times. Thus, in total there were 180 trials with a relevant cue and 180 trials with an irrelevant cue. Thus, the probability of the cue appearing was 25 % at each location. The trials in each block were presented randomly. The order of presentation of the blocks was counterbalanced across participants. The experiment lasted about 45 minutes with a break after every 120 trials. A practice session of 30 trials was administered first.

After the main experimental session, an objective visibility test was administered to assess the participants’ level of awareness of the cues. The same sequence of events as in the Main experiment was followed (only with relevant and irrelevant cues) except the participants were asked to identify the location of the subliminal cues. They were instructed to make a guess even if they were not sure about the cue's location. A four-button (arranged in the form of a plus sign) Cedrus RB series response pad (SR research) was used to collect the responses. Participants pressed the button that was spatially congruent with the cue's location.

A total of 150 trials were administered which consisted of 138 experimental trials + 12 control trials presented together. The experimental trials had 16 ms cue duration like in the main experiment; the control trials included cues of 500 ms duration. The objective of including control trials was to assess if the participants understood the instructions and performed accurately when the cues were clearly visible. The 150 trials were divided into three SOA blocks of 50 trials each. Each SOA block consisted of 25 trials with cues in relevant locations and 25 trials with cues in irrelevant locations. The order of presentation of the trials within each block and the blocks themselves was randomised.

3.2.3 Data analysis

On each trial, the first saccade that originated within an imaginary square of 4° width around the fixation cross and landed within 4° of four target circles was considered. Three participants' data were discarded because more than 25 % of their saccades landed more than 4° away from the target circles. From the remaining participants' data, 9 % of trials were discarded based on this criterion. Saccade latency was defined as the time taken to initiate a saccade following the disappearance of the ACS signal. Frequency distribution of saccades (Figure 3.2) revealed two distinct peaks which are considered as a hallmark of the presence of two types of saccades: express and regular saccades (Fischer & Ramsperger, 1984;

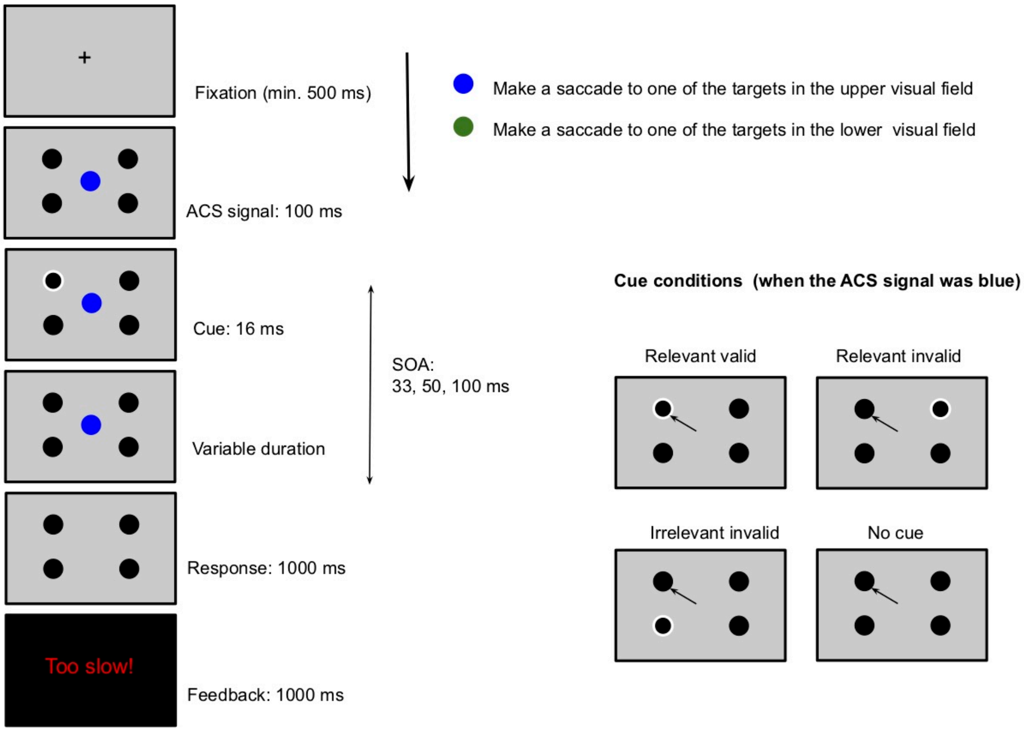

Figure 3.1. Experimental trial structure in Experiment 1. The ACS signal (blue central circle) shown in the example required the participants to make a saccade to one of the target circles in the upper visual field. In this example, the subliminal cue was presented in one of the relevant locations. The possible conditions as a function of cue-location and choice of saccade location are displayed on the right. The cue was not present during the eye-movement response. It is depicted so in the figure (on the right) to provide clarity on the experimental conditions. The same trial structure was used in Experiment 2. The only difference was that participants were asked to make a saccade to one of the target circles in the left or right visual field.
Note: Only negative feedback were given: "Incorrect", "Too slow" and "Trial aborted".

Fischer & Weber, 1993). Express saccades are those with a latency between 80 ms and 130 ms (15 % of the total saccades). Remaining saccades with the latency greater than 130 ms were considered as regular saccades. Saccades with latency less than 80 ms were discarded as being anticipatory (10.5 %).

Separate analyses were performed on express and regular saccades. The upper limit for outliers among the regular saccades was calculated using the median absolute deviation (MAD) criterion as the more common method of discarding responses based on the standard deviation is considered to be not as effective in detecting outliers in smaller samples (Cousineau & Chartier, 2010; Leys, Ley, Klein, Bernard & Licata, 2013). Regular saccades with the latency greater than 2.5 MAD away from the median latency were discarded for each participant (10.7 %). On the remaining filtered trials, accuracy analysis was performed. Next, only those trials in which the saccades landed in the correct interest area (based on the spatial ACS of that trial) were considered for other analyses (88.28 %). The saccades could land in the relevant locations (correct saccades) or the irrelevant locations (categorised as errors). The saccades that landed correctly in one of the relevant locations could either be at the cued location or the opposite location. Thus, trials were categorised into four types as a function of the location of the cue and the landing position of the saccade: relevant valid cue (cue location matches the correct saccade end location), relevant invalid cue (cue location opposite to the correct saccade end location, in the relevant visual field), irrelevant invalid cue (cue location in the irrelevant visual field) and no cue. The results from the analysis of regular saccades are reported below. The express saccades analysis lead to mostly non-significant results and are reported in the supplementary material.

Choice rate. The choice rate was calculated as the proportion of correct saccades to the cued location (relevant valid) and the opposite location (relevant invalid). Only relevant cue trials were included because participants could only choose one of the two relevant locations. To assess if the subliminal cues biased saccadic decisions when they were relevant, d' was calculated on the choice rate (following the procedure of Mattler & Palmer, 2013; Prasad et al., 2017) for each ACS signal type (Up and Down). For instance, for the ACS signal Up, the upper left cue was

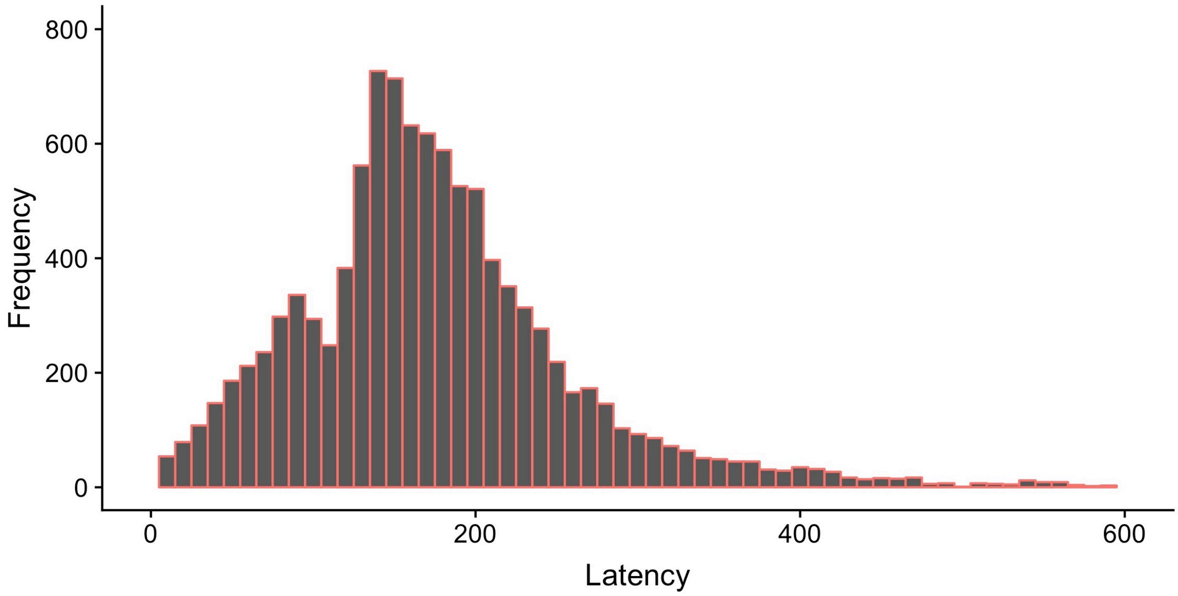

Figure 3.2. Frequency distribution plot of saccade latency in Experiment 1 showing two distinct peaks for express (80 ms - 130 ms) and regular saccades (> 130 ms)

arbitrarily designated as the signal and the cue on the upper right was considered as noise. Hits referred to saccades that landed on the upper left quadrant when the cue was on the upper left. Saccades that landed on the upper left quadrant for the Upper right cue were considered as false alarms (FA). d' prime was calculated as the difference in z transformed values of hit rates and false alarms. d' was similarly calculated for ACS signal Down. Hits and false alarm rates of 0 or 1 were corrected using the log-linear rule (Hautus, 1995). A t-test was performed comparing the mean d' value with chance (0). Next, to examine the effect of SOA and ACS signal, we constructed a linear mixed-effects model using the lmer function. The effect of SOA was analysed by constructing two columns: SOA100_50 (SOA 100 ms: +1, SOA 50 ms: -1) and SOA100_33 (SOA 100 ms: +1, SOA 33 ms: -1). ACS signal was also sum coded (Up: -1, Down: +1). All these factors were entered as fixed effects.

Accuracy. Our objective was to compare the accuracy level across relevant (R), irrelevant (IR) and no-cue trials. It was not possible to compare the accuracy between relevant valid and relevant invalid saccades here because this was a constrained free-choice task where a saccade to either of the relevant locations (valid or invalid) was considered correct. The difference between the frequency of correct saccades to relevant valid and relevant invalid is captured in the choice rate analysis described above. In the accuracy analysis, we primarily wanted to examine the accuracy in following the ACS signal as a function of cue type. d’ was calculated for accuracy. Correct responses to ACS signal Up (saccades to one of the upper locations) were considered as Hits and Incorrect responses to ACS signal Down (saccades to one of the upper locations) were considered as false alarms. “Relevancy” was included as a fixed effect in the analysis by creating two columns. One column compared irrelevant cue- and no-cue trials (IR_No; +1: irrelevant cue, -1: no cue) and another column compared the relevant- and no-cue trials (R_No; +1: relevant cue, -1: no cue). SOA100_33, SOA 100_50 and their interactions with relevancy were also added as fixed effects.

Saccade endpoint deviation. The mean horizontal deviation of the saccade landing position from a central vertical line was calculated in degrees (following Wang, Satel, Hilchey, & Klein, 2012). For each ACS signal, the endpoint deviation was calculated such that saccades landing to the right of the central line had a positive sign, those landing to the left had a negative sign. Thus, a positive (rightward) deviation in the presence of a right cue and a negative (leftward) deviation in the presence of a left cue would indicate that the subliminal cues have facilitated the saccade endpoint deviation. In contrast, a positive deviation in the presence of a left cue and a negative deviation in the presence of a right cue would indicate inhibition of the cued location.

Trials with relevant and irrelevant cues were analysed separately. Mixed-effects analysis was performed with cue location (left: -1, right: +1), SOA (SOA100_33 and SOA50_33), ACS signal and their interactions as fixed effects.

Saccade Latency. Mixed-effects analysis was performed on saccade latency using the lmer function. There were three levels of the "Condition" variable: relevant valid, relevant invalid, and irrelevant invalid. No cue condition was not included in the analysis as it lacks the alerting component involved in all the other conditions. Thus, it differed from the other conditions not only in terms of orienting but also in terms of alerting. The effect of condition was analysed by creating two columns: conditionRIV_RV (relevant invalid: +1, relevant valid: -1) and conditionRIV_IR(relevant invalid: +1, irrelevant invalid: -1). SOA100_33, SOA 100_50, ACS signal and their interactions with condition were also added as fixed effects.

In all the analyses involving d' (frequency and accuracy), Participants was entered as a random effect. For the latency and endpoint deviation analyses, both Participants and Items were included as random effects. All main effects and interactions were included for each model. lmerTest function was used to obtain p values using Satterthwaite approximations to degrees of freedom (Bates, Maechler, Bolker & Walker, 2015).

Cue localisation. Trials with response times greater than 130 ms were discarded from analysis (3.4 %). Accuracy on experimental and control trials was calculated. Cue visibility index was calculated by creating pairs of cue locations: Q1 - Q2, Q1 - Q3, Q1 - Q4, Q2 - Q3, Q2 - Q4, Q3 - Q4 (Q1, Q2, Q3, Q4 represent each of the four quadrants). For each pair, the cue appearing at one location (for eg., Q1) was arbitrarily designated as the signal and the cue appearing at the other location (for eg., Q2) was designated as noise. Correct responses to the signal (selecting Q1 when the cue appeared in Q1) were considered as Hits and incorrect responses to the noise (only those responses that were same as the correct response to the Hits, for a given pairing - for example, selecting Q1 when the cue appeared in Q2) were considered as false alarms (FA). Hits and FAs were corrected using the log-linear rule to adjust for occurrences of Hits and FAs of 0 or 1 (Hautus, 1995; Prasad, Patil, & Mishra, 2017). Hit rate and FA rate were calculated by dividing the Hits and FAs by the total number of signal and noise trials, respectively. The Hit rates and FA rates across the 6 pairs of cue locations (Q12, Q13, Q14, Q23, Q24, Q34) were averaged to yield a single Hit and FA rate. The mean d' was computed as the difference of the z transform of the mean Hit rate and the mean FA rate using the norm function in R. One-sample t-tests were conducted to examine if the cue visibility significantly differed from chance (0). Paired sample t-tests were conducted to compare the visibility between cues at relevant and irrelevant locations.

Additionally, we calculated Bayes factors to supplement the results as frequentist statistics is not appropriate for accepting a null hypothesis - that the performance on cue visibility test is equivalent to the chance level. Bayesian hypothesis testing involves setting up two models with two contrasting hypotheses and adjusting the likelihood of each model based on the evidence (data obtained). We tested for model H0 defined as cue visibility being same as the chance level against model H1 defined as better cue visibility compared to chance level. We performed one-sample t-tests using JASP comparing d' with chance level (0). Bayes factors (BF01) were computed which quantify the relative evidence for the two competing hypotheses. According to a commonly accepted convention (Jeffreys, 1961; Kruschke, 2011), $3 > BF01 > 1$ denotes anecdotal evidence, $10 > BF01 > 3$ denotes moderate evidence and $BF01 > 10$ denotes strong evidence *for* the null hypothesis. Similarly, $1/3 < BF01 < 1$ denotes anecdotal evidence, $1/10 < BF01 < 1/3$ denotes moderate evidence and $BF01 < 1/10$ denotes strong evidence for the alternate hypothesis. $BF01 = 1$ suggests that the data is inconclusive.

3.2.4 Results

Choice rate. Overall d' prime was significantly greater than chance level suggesting that saccades landed at the location of the relevant cue more often than the opposite location, t (1, 20) = 2.02, $p = 0.057$. Mixed-effects analysis revealed a significant effect of SOA on d' (SOA 100 ms vs. SOA 33 ms: $\beta = -0.19$, $t = -2.37$, $p = 0.019$). The proportion of saccades to the cued location (see Table 2.1 for means and Figure 3.3A) were much higher at 33 ms SOA compared to 100 ms SOA. Separate t-tests were performed on d' at each SOA. d' was significantly greater than chance at 33 ms, t (1, 20) = 2.4, $p = 0.026$, but not at 50 ms SOA , t (1, 20) = 1.22, $p = 0.236$ and 100 ms SOA, t (1, 20) = -1.28, $p = 0.215$. There was no effect of ACS signal, $\beta < 0.001$, $t = 0.01$, $p = 0.994$, suggesting that the influence of relevant cues did not vary as a function of whether participants were told to look up or down. The interactions between ACS signal and SOA were not significant either ($t < 1.5$).

Saccade endpoint deviation (horizontal amplitude). There was no main effect of (relevant) cue location, $\beta = 0.23$, $t = 1.43$, $p = 0.15$ (Figure 3.3B). There was a significant interaction between cue location and SOA at 100 ms compared to 33 ms, β = - 0.49, *t* = - 2.05, *p* = 0.041. Saccades landed towards the location of the cue at 33 ms SOA (leftward deviation for left cues and rightward deviation for right cues). This effect was reversed at 100 ms SOA (leftward deviation for right cues and rightward deviation for left cues) which indicated that the cued locations were inhibited. There was a significant main effect of ACS signal, β = 0.54, *t* = 3.37, *p* < 0.001, indicating greater positive (rightward) deviation whenever participants were instructed to look Up, irrespective of the cue location.

Spatially- irrelevant cues did not affect the saccade endpoint deviation, β = -0.01, *t* = - 0.08, *p* = 0.938 (Figure 3.3C). The interaction between irrelevant cue location and SOA was also not significant either, 100 ms: β = - 0.05, *t* = - 0.23, *p* = 0.821; 50 ms: β = - 0.05, *t* = - 0.23, *p* = 0.817.

Saccade Latency. Latency on relevant valid and relevant invalid trials across SOAs were comparable, β = 1.17, *t* = 0.98, *p* = 0.326 (Figure 3.3D). However, participants were faster on relevant valid trials compared to relevant invalid trials at 33 ms as indicated by a significant interaction between SOA and condition (100 ms vs 33 ms: β = 3.84, *t* = 2.14, *p* = 0.032). This pattern was also reversed at 50 ms which was significant compared to 33 ms SOA (50 ms vs 33 ms: β = -3.21, *t* = -1.97, *p* = 0.049). There were no overall differences in latency between IR invalid cue trials and relevant invalid trials, β = -1.16, *t* = -1.14, *p* = 0.255 either. There was a significant interaction between SOA and IR invalid cue condition (100 ms vs 33 ms), β = -6.73, *t* = -4.43, *p* < 0.001. Participants were faster in making saccades in the IR invalid condition compared to relevant invalid condition at 33 ms, but the pattern was reversed at 100 ms SOA (see Table 3.1 for means). Saccades were faster when the instruction was to look up vs. down as indicated by a main effect of ACS signal, $\beta = 1.59$, $t = 1.97$, $p = 0.049$. Saccades were also faster on trials with 100 ms SOA compared to 33 ms SOA (100 ms vs 33 ms: $\beta = -12.41$, $t = -10.27$, $p < 0.001$). None of the other interactions were significant ($t < 1.3$).

Accuracy. Participants were less accurate on IR cue trials ($M = 86$ %, $SD = 9$) compared to no cue trials ($M = 89$ %, $SD = 7$), $\beta = -0.12$, $t = -2.26$, $p = 0.025$ (Figure 3.3E). The accuracy on relevant cue ($M = 90$ %, $SD = 7$) trials was also greater compared to no cue trials, $\beta = 0.14$, $t = 2.48$, $p = 0.014$. There was also a main effect of SOA indicating greater accuracy on 100 ms SOA trials ($M = 92$ %, $SD = 7$) compared to 33 ms SOA trials ($M = 86$ %, $SD = 7$), $\beta = 0.21$, $t = 3.89$, $p < 0.001$.

We next examined if there were more error saccades to the location of the irrelevant cue as opposed to the opposite location. The overall d' value across SOA was not significantly different from chance t (1, 20) = 0.25, $p = 0.804$. A mixed effects analysis on the d' values revealed a nonsignificant effect of SOA (50 ms vs 33 ms: $\beta = -0.08$, $t = -0.77$, $p = 0.443$; 100 ms vs 33 ms: $\beta = -0.01$, $t = -0.12$, $p = 0.904$).

Table 3.1. Descriptive statistics (Experiment 1)

	Choice rate (relevant valid)	**End location (horizontal amplitude)**		**Saccade latency**			
		Left cue	**Right cue**	**Relevant valid**	**Relevant invalid**	**IR invalid**	**No**
33 ms	0.55 (0.08)	-0.31 (7.18)	0.87 (7.28)	206 (61)	213 (68)	203 (56)	203 (60)

50 ms	0.52 (0.09)	-0.84 (7.39)	-0.12 (7.44)	198 (48)	195 (45)	198 (48)	199 (57)
100 ms	0.49 (0.08)	0.47 (7.5)	-0.21 (7.47)	182 (41)	183 (50)	196 (65)	184 (48)

Note: Latencies are given in ms. Horizontal amplitude of endpoint deviation is given in degrees. Numbers in bracket denote +1 SD. Since the choice rate on relevant-valid and relevant-invalid trials is complementary, values are given only for relevant-valid trials. IR: irrelevant.

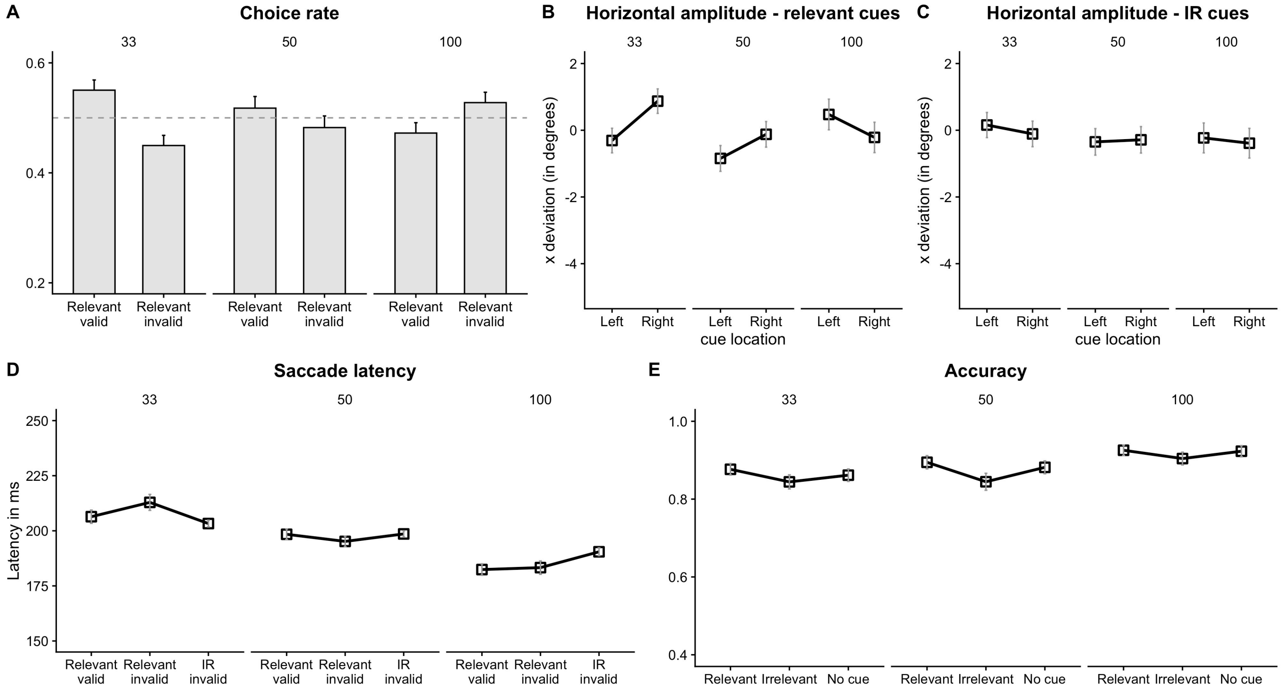

Figure 3.3. Results from Experiment 1 (A) There were more saccades to the relevant valid location at 33 ms and 50 ms SOA compared to relevant invalid location (B) The horizontal deviation of the saccade end location showed that saccades landed towards the cued locations at 33 ms and away from the cued locations at 100 ms SOA, only when the cues were relevant (C) but not when they were irrelevant (D) Facilitation followed by inhibition was observed in latency of saccades to the (relevant) cued location. But latency on IR invalid cue trials was faster compared to relevant invalid trials at 33 ms. This pattern was reversed at 100 ms SOA (E) Participants were less accurate on IR cue trials compared to no cue trials, accuracy on relevant cue trials was also greater compared to no cue. Note: The choice-rate and accuracy data are plotted in terms of proportion for ease of comprehension. The statistical analysis was performed on d' values. The dashed horizontal line represents chance level (0.5)

Cue localisation. The accuracy on control trials (*M* = 59.1 %, *SD* = 30) and experimental trials (*M* = 40.8 %, *SD* = 18.1) were both significantly different from chance level (25 %), *t* (1, 20) = 5.21, *p* < 0.001 and *t* (1, 20) = 3.99, *p* = 0.001 respectively. The cue visibility index (*M* = 0.5, *SD* = 0.77) was also significantly greater than zero, *t* (1, 20) = 2.97, *p* = 0.007. Paired t -tests revealed no significant difference between the visibility for cues at relevant (*M* = 0.52, *SD* = 0.7) and irrelevant locations (*M* = 0.46, *SD* = 0.9), *t* (1, 38.51) = -0.21, *p* = 0.832.

Bayesian statistics showed that there was moderate evidence to accept the alternate hypothesis (BF01 = 0.16).

3.2.5 Discussion

The subliminal cues had a significant influence on saccadic decisions when they were spatially-relevant. The frequency of saccades to the cued location was greater than the saccades to the opposite location. We also observed significant leftward deviation in the saccade endpoint for cues on the left and rightward deviation for cues on the right at 33 ms suggesting that the cues facilitated the saccade endpoint deviation at short SOA. This pattern was reversed at 100 ms SOA indicating inhibition of the cued location. Participants were also faster making saccades on relevant valid trials, but only at the short SOA (33 ms). All these results indicate that we found the classic pattern of facilitation at short SOA followed by (not so significant) inhibition at longer SOA commonly observed in studies with peripheral cues (Klein, 2000). Thus, we found converging results to conclude that spatially-relevant cues biased both the spatial and temporal characteristics of constrained free-choice saccades in a manner similar to the influence on instructed responses.

We had hypothesised that if the participants can filter out spatially-irrelevant cues to some extent, then the participants should be faster on IR invalid cue trials compared to relevant invalid trials. We found exactly that at 33 ms SOA. The pattern was reversed at 100 ms in line with the inhibitory effects seen with relevant cues. Thus, the trials with IR invalid cues captured less attention than relevant invalid trials suggesting that participants were less influenced by IR cues. Interestingly, participants made more errors on IR-cue trials compared to no-cue trials indicating that IR cues did influence saccadic responses. Thus, we can only conclude that while participants could not completely ignore the IR cues, there was reduced capture by IR cues.

3.3 Experiment 2

We can conclude two things from Experiment 1: First, subliminal cues influenced saccadic decisions. Second, there was reduced capture by spatially-irrelevant cues. In Experiment 2, we sought to replicate these findings with a slightly different design. It is well known that spatial biases play a role in determining eye movement behaviour (Previc, 1990; Nuthmann & Matthias, 2014; Ossandón, Onat, & König, 2014; Thomas & Elias, 2011). To examine if the observed findings could be generalised and was not restricted to the way we defined the ACS (up vs. down), we conducted another experiment with a different type of ACS (left vs right). Participants were asked to choose one of the target locations on the left or right depending on the ACS signal at the centre. Based on the results of Experiment 1, we expected 1) the subliminal cues to similarly influence frequency of saccades, endpoint deviation and latency when the cues were spatially relevant 2) the latency on IR invalid cue trials to be faster than relevant invalid trials at short SOA 3) accuracy on IR cue trials to be worse compared to no cue trials.

3.3.1 Methods

Participants. Twenty-two individuals (19 males, Mean age = 25.27 years, SD = 3.19) took part in the experiment. None of them had participated in the previous experiment.

3.3.2 Procedure

The apparatus and stimuli were exactly same as Experiment 1. The procedure was similar to that of Experiment 1, except the participants were asked to choose one of the two targets in the left and right. Thus, blue and green circles indicated Left and Right direction respectively for half of the participants (and vice-versa for other half of the participants). The number of trials and the blocking levels were same as Experiment 1.

3.3.3 Data analysis

The analysis procedure was similar to that of Experiment 1. Four participants' data was discarded from final analysis because more than 25% of the saccades landed more than 4° away from the target circles (irrespective of the ACS). From the remaining participants' data, 10 % of the trials were discarded based on this criterion; 18 % saccades were discarded for being anticipatory. 16.5 % of trials were express saccades (130 ms > latency > 80 ms, Figure 3.4) and were analysed separately (Supplementary material). Among the regular saccades, 10.6 % of the trials were discarded as outliers based on the upper limit determined by MAD criteria. Error trials constituted 10.25 % of the trials. In the cue visibility test, 6% of the trials were

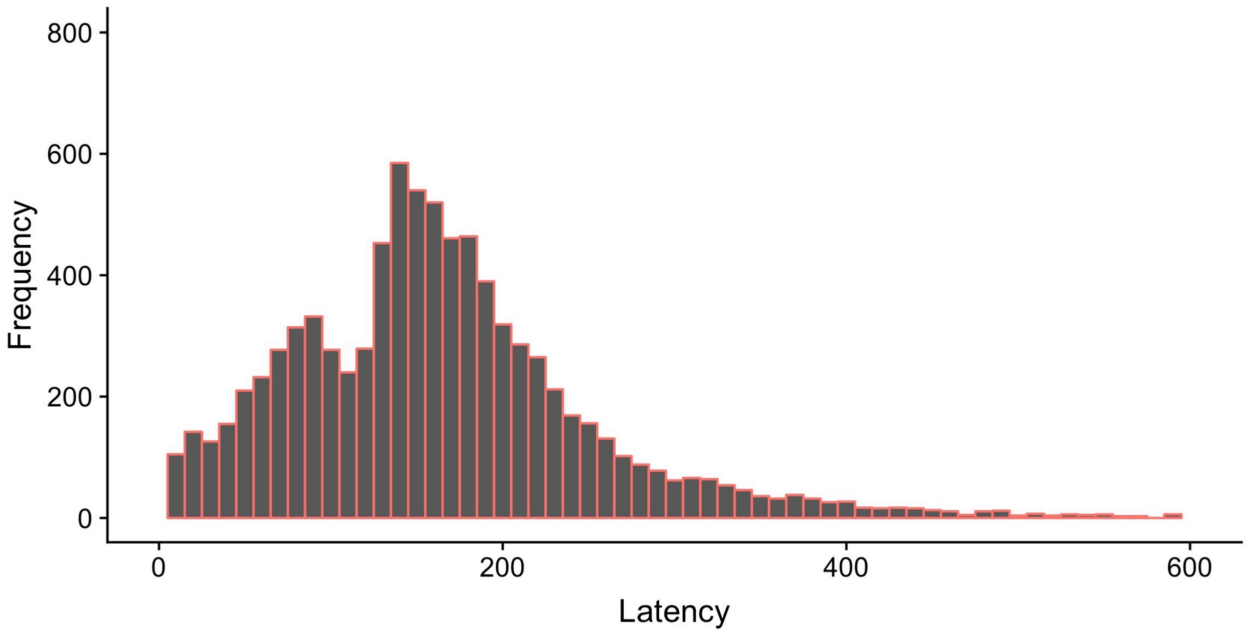

Figure 3.4. Frequency distribution plot of saccade latency (Experiment 2) showing two distinct peaks for express (80 ms - 130 ms) and regular saccades (> 130 ms)

discarded as outliers (RT < 130 ms)

In the choice rate analysis, d’ was calculated for each ACS signal (Left and Right). For instance, for ACS signal Left, saccades to the upper left quadrant on trials with upper left cue were considered Hits. Saccades to the upper left quadrant on trials with lower left cue were considered False Alarms. d’ was similarly calculated for ACS signal Right. Mixed effects analysis was done on d’ values with ACS signal (Left: -1, Right: +1) and SOA (SOA100_33, SOA 100_50) as factors. In the saccade endpoint deviation analysis, vertical amplitude was calculated. This was because the ACS signals in Experiment 2 were Left or Right. For each type of signal, the cue could be located either up or down. The deviation of the saccade landing position (vertical amplitude) from a central horizontal line was calculated. A positive number indicated a downward deviation and a negative number indicated an upward deviation compared to the central horizontal line. Mixed effects analysis on saccade endpoint deviation was performed with cue location (up: -1, down: +1), ACS signal and SOA as fixed effects. For the accuracy analysis, d' prime was calculated by considering correct (leftward) responses to ACS signal Left as Hits and incorrect (leftward) responses to ACS signal Right as False Alarms. Relevancy (IR_No and R_No) and SOA were entered as fixed effects. Saccade latency was analysed by entering condition (conditionRIV_RV and conditionRIV_IR), SOA and ACS signal as factors into the mixed-effects model.

3.3.4 Results.

Choice rate. The frequency of saccades to the relevant valid location was not significantly greater than chance, t (1, 17) = 1.45, p = 0.165. lmer analysis on d' showed that SOA did not significantly modulate the frequency of saccades to the relevant valid location, (50 ms vs. 33 ms: $\beta = 0.12$, $z = 1.6$, $p = 0.113$; 100 ms vs. 33 ms: $\beta = - 0.11$, $z = -1.39$, $p = 0.152$) (Figure 3.5A). Individual t tests on d' values were performed separately at each SOA. Saccades to the relevant valid location were greater than chance at 50 ms, t (1, 17) = 2.17, $p = 0.044$ but not at 33 ms SOA, t (1, 17) = 0.35, $p = 0.733$ and 100 ms SOA, t (1, 17) = -1.39, $p = 0.185$ (see Table 3.2 for descriptive means). There was no effect of the ACS signal (- $\beta = 0.04$, $z = -0.67$, $p = 0.506$) suggesting that the instruction to look Left or Right did not modulate the influence of the cues on choice rate.

Saccade endpoint deviation (vertical amplitude). There was a main effect of cue location, $\beta = 0.40$, $t = 2.05$, $p = 0.041$ (Figure 3.5B) suggesting that the deviation from a central horizontal line was relatively more positive (downward) for Down cues compare to Up cues. This indicates an overall facilitatory effect of the cues on saccade deviations. This facilitatory effect was greater at 50 ms SOA compared to 33 ms SOA, β = 0.53, *t* = 1.97, *p* = 0.049 as indicated by a significant interaction between cue location and SOA. There was a significant three-way interaction between ACS signal, cue location and SOA, β = -0.7, *t* = -2.37, *p* = 0.018. Separate models on data corresponding to each ACS signal showed that SOA and cue location interaction was present only for the ACS Left condition (50 ms vs 33 ms: β = 09.92, *t* = 2.44, *p* = 0.015; 100 ms vs 33 ms: β = -1.12, *t* = -2.8, *p* = 0.005), but not for ACS Right condition (50 ms vs 33 ms: β = 0.04, *t* = 0.12, *p* = 0.907; 100 ms vs 33 ms: β = 0.31, *t* = 0.71, *p* = 0.479). There was a main effect of ACS signal, β = 0.85, *t* = 4.28, *p* < 0.001 indicating that saccade deviation was more downward for ACS signal Right compared to Left.

Spatially irrelevant cues did not influence saccade endpoint deviation, β = 0.02, *t* = 0.11, *p* = 0.910 (Figure 3.5C). The interactions with SOA or ACS signal were not significant either (*p* > 0.5)

Saccade Latency. Saccade latency on the relevant valid cue trials was lesser compared to the relevant invalid cue condition, β = 3.44, *t* = 2.46, *p* = 0.014 (Figure 3.5D). This effect was marginally greater at 50 ms SOA compared to 33 ms SOA, β = 3.38, *t* = 1.77, *p* = 0.078. The facilitatory effects observed at 33 ms and 50 ms SOA turned into inhibition at 100 ms as revealed by a significant interaction between SOA and condition (100 ms vs 33 ms), β = -4.56, *t* = -2.17, *p* = 0.03. The latency on irrelevant invalid cue trials was faster compared to the latency on relevant invalid cue trials, β = -2.3, *t* = -1.9, *p* = 0.058. This pattern was observed at 33 ms SOA, but the opposite pattern was observed at 50 ms SOA (50 ms vs 33 ms: β = -3.28, *t* = -1.98, *p*

= 0.048). There was no main effect of ACS signal, $\beta = -1.23$, $t = - 1.28$, $p = 0.199$. It did not interact with any of the conditions either ($t < 1.3$). Shorter overall latencies were observed on trials with 100 ms SOA compared to 33 ms SOA, $\beta = -15.85$, $t = -10.81$, $p < 0.001$. Latency on 50 ms SOA trials was greater compared to 33 ms SOA, $\beta = 7.37$, $t = 5.58$, $p < 0.001$.

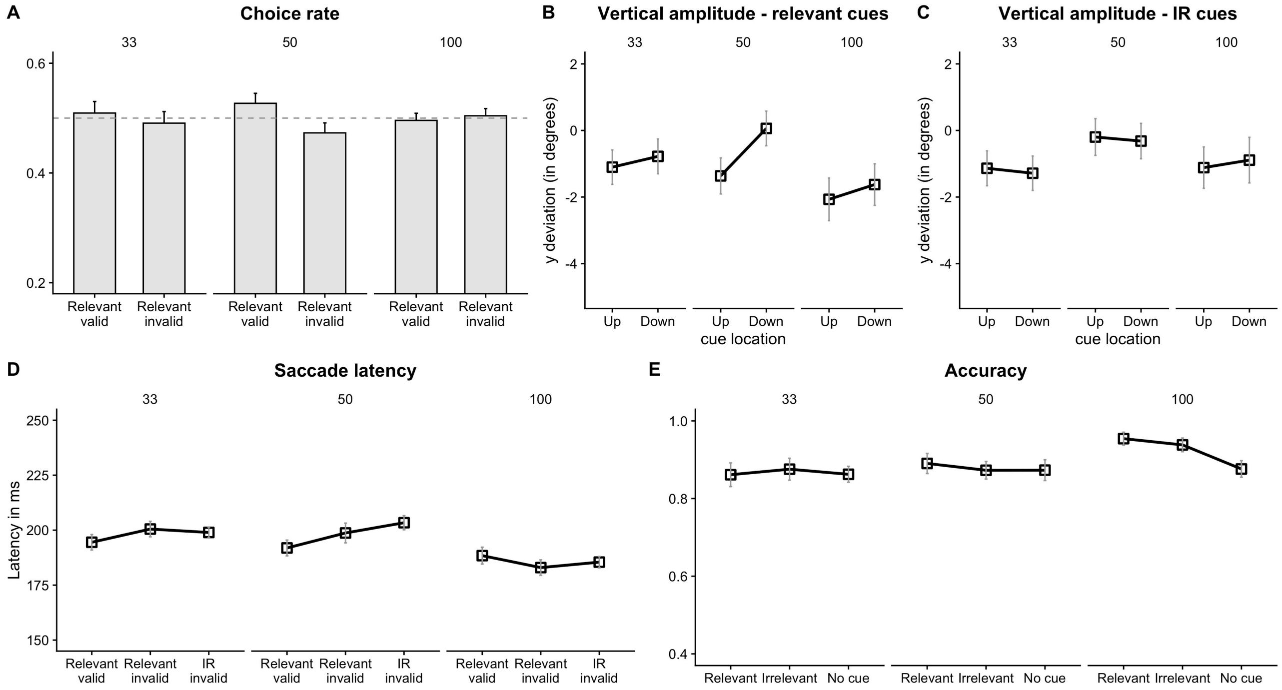

Figure 3.5. Results from Experiment 2 (A) Frequency of saccades to the cued location was significantly greater than chance at 50 ms SOA. (B & C) The vertical deviation of the saccade end location was relatively more downward (more positive) for "down" cues compared to "up" cues at all SOAs, only when the cues were relevant (D) Relevant cues facilitated saccade latency at 33 ms and 50 ms SOA which turned into inhibition at 100 ms SOA. Saccades on trials with IR invalid cues were faster compared to relevant invalid trials at 33 ms SOA. This pattern was reversed at 50 ms (E) No differences in accuracy were found across different cue conditions. Note: The choice-rate and accuracy data are plotted in terms of proportion for ease of comprehension. The statistical analysis was performed on d' values. The dashed horizontal line represents chance level (0.5)

Table 3.2. Descriptive statistics (Experiment 2)

	Choice-rate (relevant-valid)	**End location (horizontal amplitude)**		**Saccade latency**			
		Up cue	**Down cue**	**Relevant valid**	**Relevant invalid**	**IR invalid**	**No**
33 ms	0.51 (0.09)	-1.1 (8.78)	-0.78 (8.77)	194 (58)	200 (59)	198 (58)	204 (57)
50 ms	0.53 (0.08)	-1.36 (8.75)	0.06 (8.94)	192 (61)	199 (72)	203 (75)	204 (59)
100 ms	0.49 (0.05)	-2.07 (8.92)	-1.62 (8.79)	188 (54)	183 (48)	185 (50)	180 (41)

Note: Latencies are given in ms. Horizontal amplitude of endpoint deviation is given in degrees. Numbers in bracket denote +1 SD. Since the choice rate on relevant-valid and relevant-invalid trials is complementary, values are given only for relevant-valid trials. IR: irrelevant-cue.

Accuracy. There was no difference in accuracy between the relevant cue ($M = 90$ %, $SD = 11$) and no cue trials ($M = 87$ %, $SD = 9.6$), $\beta = -0.09$, $z = 1.2$, $p = 0.232$. The accuracy on IR cue trials ($M = 89$ %, $SD = 10$) was also comparable to no-cue trials, $\beta = 0.01$, $z = 0.20$, $p = 0.841$ (see Table 3.2 for condition-wise mean values and Figure 3.5E). SOA did not modulate accuracy on either of these types of trials (100 ms vs 33 ms: $\beta = -0.01$, $t = -0.14$, $p = 0.887$; 50 ms vs 33ms: $\beta = 0.04$, $t = 0.6$, $p = 0.55$).

The analysis on error saccades revealed that the number of error saccades at the location of the irrelevant cue was comparable to the other location, t (1, 17) = -0.54, $p = 0.593$. SOA did not have any influence either (100 ms vs 33 ms: $\beta = 0.04$, $t = 0.48$, $p = 0.634$; 50 ms vs 33ms: $\beta = -0.02$, $t = -0.21$, $p = 0.835$). Separate t tests performed at each SOA revealed nonsignificant differences compared to chance ($p > 0.5$).

Cue localisation. Participants performed significantly better than chance level (25 %) on control trials (*M* = 43.98 %, *SD* = 30), *t* (1, 17) = 2.67, *p* = 0.016. Accuracy on the experimental trials (*M* = 33.58 %, *SD* = 17.4) was marginally greater than chance level performance, *t* (1, 17) = 2.09, *p* = 0.052 respectively. The cue visibility index (*M* = 0.31, *SD* = 0.72), even though very small, was significantly different from zero, *t* (1,17) = 1.83, *p* = 0.085. The visibility did not differ significantly between the cues at relevant (*M* = 0.17, *SD* = 0.7) and irrelevant locations (*M* = 0.38, *SD* = 0.73), *t* (1, 33.94) = 0.86, *p* = 0.393.

Bayesian hypothesis testing revealed anecdotal evidence to accept the null hypothesis (BF01 = 1.04).

3.3.5 Discussion

We conducted Experiment 2 to verify if the findings of Experiment 1 could be generalised across different types of spatial ACS. Participants made more saccades to the cued location at one of the shorter SOAs (50 ms). The vertical amplitude of the saccade endpoint deviation was also influenced by the relevant cues. The “up” cues lead to more relatively upward deviation than the “down” cues and vice versa. This effect was also greater at 50 ms SOA compared to 33 ms SOA. The interaction with SOA was only observed for ACS signal Left, but not for ACS signal Right. Latency on relevant valid trials was faster than relevant invalid trials at the shorter SOAs (33 ms and 50 ms). The pattern reversed at 100 ms SOA. Like in Experiment 1, these results provide converging evidence for facilitation followed by inhibition effect of the spatially relevant cues. However, the evidence of inhibition of cued locations was seen only in saccade latency, but not in choice rate and saccade deviation. The possible reasons for weak inhibition are discussed in the general discussion. Finally, the latency on IR invalid trials was faster than relevant invalid trials at 33 ms but the pattern was reversed at 50 ms SOA suggesting reduced capture by IR cues only at 33 ms SOA. Interestingly, the cues had no influence on the accuracy of saccades irrespective of their relevancy.

3.4 General Discussion

We examined the influence of subliminal cues presented at spatially-relevant or irrelevant locations on constrained free-choice saccades. The relevancy of the cues was established based on spatial ACS. In Experiment 1, participants were asked to make a saccade to one of the target circles in the upper visual field or the lower visual field depending on the colour of the central circle. In Experiment 2, the instruction was to choose one of the target circles in the left or right visual field. In both the experiments, we observed that relevant subliminal cues influenced saccadic responses measured through choice rate, saccade endpoint deviation and saccade latency. Our objective was also to examine if and to what extent spatially-irrelevant cues influence saccadic decisions. In both the experiments, saccade latency data showed reduced capture by spatially-irrelevant cues in some conditions. Further, subliminal cues at IR cue locations lead to more errors in Experiment 1. These findings show that spatial ACS can modulate the influence of subliminal cues on constrained free-choice saccades.

Time course of subliminal cueing of constrained free-choice saccades

The present results add to the growing evidence regarding the depths and limits of subliminal cueing of eye movements. We have shown that abrupt-onset subliminal cues can bias saccadic responses even when the saccade locations are chosen freely by the participants within some constraints. We expected the relevant subliminal cues to influence both the spatial (choice rate and saccade endpoint deviation) and temporal properties (latency) of the saccades. In both experiments, we observed a higher frequency of saccades to the (relevant) cued location at shorter SOAs (33 ms and 50 ms) which turned into inhibition at 100 ms SOA. A similar pattern was observed in the latency of the saccadic decisions. Participants were faster making saccades on relevant valid trials compared to the relevant invalid cue trials at short SOAs (33 ms and 50 ms). Inhibitory effects - defined by faster latency on relevant invalid cue trials compared to relevant valid trials were seen at longer SOA. The facilitatory effects obtained at the shorter SOAs are in confirmation with Huang et al. (2014)'s results who also showed that subliminal cues can influence free saccadic decisions.

The pattern of facilitation at short SOA followed by inhibition at long SOA is a classic finding in studies with peripheral cues in a Posner cueing paradigm (Posner, 1980). The "negative" effect typically observed in peripheral cueing at long SOA (> 100 ms) is known as the inhibition-of-return which arises due to the reluctance of the oculomotor system to revisit recently attended locations (Klein, 2000). However, IOR is commonly observed in response time measures in Posner cueing tasks. In such tasks, participants are slower responding to targets when they are presented at the cued locations as opposed to the uncued locations at long SOAs.

For the first time, we have shown an inhibitory effect of subliminal cues on both frequency and latency of constrained free choice saccades. The existence of such inhibitory effects even when people freely make decisions about where to look is in line with the underlying explanation of IOR as a foraging mechanism of the visual system which helps individuals seek out new information. We acknowledge that the inhibition was not strong (and statistically significant) in all measures. This can probably be attributed to our relatively “short” long SOA. It is likely that we have captured only the beginning of the inhibitory effects and that strong inhibition will be observed at longer (> 150 ms) SOAs. The other possibility is that the inhibitory effects were weakened by the blocking of SOA. In a peripheral cueing study with double targets, Wang et al. (2012) observed IOR at 50 ms only when the 50 ms SOA trials were intermixed with 600 ms SOA trials (Experiment 2), but not when 50 ms SOA trials were presented alone (Experiment 1). The authors suggest that the temporal uncertainty in the appearance of the targets could have lead participants to disengage attention faster from cued locations resulting in observable IOR in the mixed condition. In contrast, the incentive to disengage from the cued location was lower in Experiment 1 as the participants could reliably (temporally) predict the appearance of the target using the cue. We might have similarly observed stronger inhibitory effects if we had intermixed the SOA conditions. Thus, while blocking SOA might be preferable in motor priming studies, it may not be so while investigating attentional mechanisms, especially using eye movements. Future studies should note this and choose the appropriate design.

Our results show that subliminally presented stimuli in the visual field can also generate activity in the oculomotor priority map and guide oculomotor selection. We found converging evidence for this across different eye movement measures: choice rate, saccade latency and saccade endpoint deviation. This is in line with previous studies that have suggested that a saliency map can be constructed without full awareness of the corresponding objects (Hsieh, Kolas, & Kanwisher, 2011). Pop-out without awareness demonstrated by Hseih et al., 2011 is one such example where participants were presented with a display containing a feature singleton among homogenous distractors. The entire display was masked from awareness, but participants performed better on an orientation discrimination task at the location of the feature singleton than at other locations. This suggests that the location of the feature-singleton pops out and grabs attention without participants' awareness. This was possible because the feature-singleton altered the saliency map even though it was suppressed from awareness. These results contribute to our growing understanding of the factors that guide overt visual selection.

The role of spatial ACS in subliminal cueing of free-choice saccades

To our knowledge, this study is the first to examine the role of spatial ACS on subliminal cueing of free saccades. We had hypothesised that if spatial ACS can modulate the influence of irrelevant cues, saccade latencies on trials with irrelevant invalid cues should be faster than relevant invalid cue condition at short SOA. In contrast, if the spatial ACS fails to filter out the irrelevant cues, then the performance on IR invalid cue trials should be comparable to or worse than the relevant invalid cue condition. In Experiment 1, participants were faster in the presence of IR invalid cues at shorter SOAs and slower at long SOAs. Thus, cues at irrelevant locations did not capture attention as much as a relevant invalid cue. This suggests that the spatial ACS modulated the responses on IR cue trials. We observed this effect in Experiment 2 as well but only at 33 ms SOA. The accuracy data from Experiment 1 appears as an anomaly to the preceding argument. Participants made more errors on trials with IR cues compared to no cues suggesting that the IR cues captured attention causing more errors. An analysis of error saccades did not suggest that participants were more likely to make an error saccade to the IR cue location. Further, the effect of cue-relevancy on accuracy was seen only in Experiment 1. This indicates that the errors were more likely a result of the failure of the ACS in Experiment 1 rather than the irrelevant cues triggering eye movements. Thus, on trials where participants successfully managed to saccade to the correct locations, they were able to inhibit the IR cues.

This type of research has the potential to contribute to our understanding of the role of top-down attention on subliminal visual processing. They also help move forward the debate regarding the relationship between attention and consciousness. Showing that top-down attention can be directed at stimuli below the threshold of awareness suggests that attention does not necessarily require consciousness. Such dissociations can also inform us of the functional role of consciousness. While conducting such investigations, it is important to examine various domains of attention. As discussed in the introduction, several studies have shown that feature-based top-down attention can modulate the extent of subliminal processing (Bahrami et al., 2007; Kanai, Tsuchiya, & Verstraten, 2006; Wyart and

Tallon-Baudry, 2008). In our study, we find some evidence to suggest that location-based top-down attention was able to reduce the influence of irrelevant subliminal cues. But, we acknowledge that the results with respect to the influence of irrelevant cues are not entirely conclusive. Since this is the first study to examine the influence of spatial ACS on subliminal cueing, we hope that these results will provide an impetus into more research on this topic which is necessary to evaluate the strength of these findings and arrive at robust conclusions.

It is possible to question if we would have observed stronger effects of spatial ACS if the trials had been blocked for each ACS. Previous studies examining the role of spatial ACS on attention capture with visible cues have administered trials with only one type of ACS to a participant (Gaspelin & Ruthruff, 2017; Ishigami et al., 2009) We did not have such blocking because many researchers have pointed out that top-down contingent capture effects seen in earlier studies could be explained through inter-trial priming effects arising due to blocking (eg., Belopolsky, Schreij & Theeuwes, 2010; Theeuwes, 2013, 2018). Thus, any top-down effects we might have observed could have been attributed to bottom-up inter-trial priming due to repeated occurrence of targets in particular locations. To account for this criticism, we only administered mixed sessions.

Limitations

It is important to alert the readers to the fact that our no-cue condition is not the most appropriate baseline for measuring the influence of irrelevant cues. The abrupt-onset of a cue (irrespective of its location) also serves as an alerting signal to the visual system. This alerting mechanism is absent in the no-cue condition. For this reason, we did not compare the saccade latency on critical trials with no cue condition, which would have been a more appropriate comparison. This can be corrected by, for example, using an auditory tone on all the trials simultaneous to the presentation of the cue or no-cue (as used by Ishigami et al., 2009).

Further, our use of the term "subliminal" might be called into question as the participants performed better than chance level in the cue localisation task possibly indicating that they were at least partially aware of the subliminal cues. It should be, however, noted that Bayesian analysis of Experiment 2 cue localisation indicated that there was anecdotal evidence to accept the null hypothesis. Nevertheless, the overall pattern of results in both Experiments was intriguing because we used 16 ms cues which are commonly used in subliminal cueing studies (Ivanoff and Klein, 2003; Mulckhuyse & Theeuwes, 2010a; Van der Stigchel et al., 2009; Weichselbaum et al., 2014). One possible reason why the cues could be localised better than chance is that the participants only had to detect the location of the cues. It is commonly observed in such studies that participants see “something” on the screen but can’t discriminate the exact identity of the cue. Thus, d’ is more likely to be at chance level when the cue visibility test involves some form of discrimination based on their identity rather than just detecting their presence. Since our cue visibility test was easy, participants might have been able to detect the cues with better than chance accuracy.

It is to be noted that several previous studies on subliminal cueing have similarly found above-chance performance on the cue visibility test (eg., Schoeberl et al., 2015; Weichselbaum et al., 2014). The cues are nevertheless argued to be subliminal in such cases sometimes for several reasons. First, participants in these studies often subjectively report to not have seen the cues. Second, the visibility index obtained on objective tests is considered to be an overestimation of the true awareness of the cues in the main experiment session. This is because the participants are explicitly informed about the nature of the cue and asked to pay attention to it in the visibility tests. However, they have no such explicit knowledge of the subliminal cues during the main experiment session. In line with this, some studies have found that the performance on the cue awareness test is independent of the cue's influence on responses (Francken, van Gaal, & de Lange, 2011). Third, it is impossible to rule out cueing effects in the visibility test itself. That is, the presence of the cues could have triggered responses associated with that location - similar to the cueing effects observed in the main experiment. Support for this "blindsight" like phenomenon comes from a recent study by Koivisto and Neuvonen (2020) where participants performed a discrimination task and gave a subjective rating within a single response. The authors observed above-chance performance on the discrimination task even when participants reported to have seen "nothing" on the screen.

Given this, when can we truly say that participants are not conscious of certain stimuli? One of the major problems plaguing research on subliminal processing is the diversity of methods to induce and measure awareness (Rothkirch & Hesselmann, 2017). While some people use objective visibility tests like the one we used, others rely on trial-wise subjective reports. In a paper comparing various subjective awareness measures, Sandberg, Timmermans, Overgaard, and Cleeremans (2010) concluded that a graded perceptual awareness scale (PAS; Ramsøy &

Overgaard, 2004) provided the strongest correlation between awareness and performance. As a consequence, graded subjective ratings, particularly PAS has been a popular method to elicit subjective reports of awareness and provide a better alternative to forced-choice discrimination tasks.

In sum, we acknowledge that we can't be confident that similar cueing effects on eye movements will be seen for cues that are completely invisible. However, like in many previous studies mentioned above, none of the participants verbally reported having "seen" the cues during the main experiment when questioned during a post-experiment briefing session. But, since we did not use a subjective awareness scale on a trial-to-trial basis, we can't entirely rely on the verbal reports of the participants. Further studies with stricter control on the cue visibility are necessary to determine if the findings from our study can be generalised to subliminal cueing.

Conclusion

In conclusion, we have demonstrated that 1) subliminal cues can affect saccadic decisions even when these decisions are voluntary 2) Spatial ACS can modulate the influence of irrelevant cues under certain conditions. We observed this across two studies with different types of spatial relevancy. This study is a small contribution towards examining *if*, *when* and *how* attentional control can be exerted on capture by subliminal cues during free-choice eye movement behaviour. Such studies have the potential to contribute to our understanding of subliminal cueing as well as general theories of attention and eye movements.

4. Re-examining immunity to attention capture

In 2018, Ruthruff and Gaspelin used a modified spatial cueing paradigm in which targets were presented at two locations while abrupt-onset cues could be presented at four locations. They found that performance following cues presented at irrelevant locations was no worse than following no cue or following a centrally presented cue. They concluded, as conveyed by the title of their paper ("Immunity to attentional capture at ignored locations") that a spatial attentional control setting had eliminated capture of attention. This conclusion was reached by comparing response time to targets on cue-absent vs. irrelevnat cues condition. We administered the exact same task in Experiment 1 and observed that responses on irrelevant trials were faster compared to cue absent trials providing support for the "immunity to attention capture claim" made by Ruthruff and Gasplein (2018). However, cue absent trials may not be the most appropriate baseline condition as they lack the alerting benefit provided by cue-present trials. Thus, equivalent response times (RTs) on trials with absent cues and irrelevant cues observed in Ruthruff and Gaspelin (2018) could have been due to the lack of this alerting benefit. We tested this in Experiment 2 by additionally including a warning beep on every trial as an alerting signal. With this methodological change, we observed that responses were slower on irrelevant trials compared to the cue absent trials suggesting interference from irrelevant locations. This study underscores the importance of using the appropriate baseline while testing attention capture.

This chapter has been accepted as a stage 1 proposal in the registered replication format. Stage 2 manuscript is under review: Prasad, S. G., Mishra, R. K. (under review). Re-examining attention capture at irrelevant (ignored) locations. *Journal of Experimental Psychology: General*.

4.1 Introduction

The extent to which attention capture by spatially uninformative peripheral events is contingent on the top-down goals of the participants has been a topic of debate for several decades now. While many have argued that attention capture by peripheral cues is automatic (eg., Jonides, 1981; Jonides & Yantis, 1988), others have shown evidence that voluntary attentional control settings (ACS) can override such capture under certain conditions (eg., Lien, Ruthruff, & Johnston, 2010; Yantis & Jonides, 1990). In a seminal study, Folk, Remington and Johnston (1992) demonstrated that abrupt-onset cues only captured attention when the task was to look for abrupt-onset targets and color-singleton cues only captured attention when looking for color-singleton targets. Thus, only those cues that shared the task-relevant properties of the target captured attention. This led the authors to propose the "contingent involuntary orienting hypothesis" according to which peripheral cues capture attention only when they match the feature-based attentional control settings in place during that trial. This influential hypothesis (foreshadowed in the quote from Posner, 1980, at the beginning of our paper) triggered a mountain of research exploring the role of feature-based ACS in modulating attention capture (for one review, see Theeuwes, 2010).

If, and to what extent, a spatial ACS can modulate attention capture has been relatively less studied (but see, Berggen & Eimer, 2018; Lachter, Forster, & Ruthruff, 2004; Leonard, Balestreri &Luck, 2014; Ishigami, Klein, & Christie, 2009; Theeuwes, 1991; Yantis & Jonides, 1990, Exp. 2). In a seminal example, Yantis and Jonides (1990, Exp. 2), used a central arrow cue to reliably (100% of the time) indicate the location of the target letter (E or H). The final display consisted of a target letter and three distractor letters. On each trial, either the target or one of the distractor letters was an abrupt-onset while the other (distractor/target, respectively) was created by deleting elements of an already present figure 8. So long as there was sufficient time for the participant to focus their attention on the cued location.Responses were equally fast in these two conditions suggesting that voluntary spatial attention directed by the central cue could eliminate any capture of attention by an irrelevant peripherally presented onset stimulus.

Ishigami and colleagues (Ishigami, Klein & Christie, 2009; Ishigami, Hamm, Satel & Klein, 2012) extended this line of research, using figure 8 markers, in several ways. First, rather than encouraging focussed attention at one peripheral location, they encouraged focussed attention at two locations on opposite sides of the fixation stimulus. Second, their manipulation of attention was not based on a visually presented stimulus (e.g., not an arrow at fixation); rather it was based on an instruction to focus attention on two of the four figure 8s (either above/below fixation; or to the left/right of fixation) which was reinforced by the fact that targets (2 or 5 generated by deletion of 2 elements of an 8) were only presented at these two locations. Third, the to-be-ignored stimulus was not an abruptly appearing new object, but rather was the brief brightening of any one of the 4 figure 8 placeholders. Fourth, in separate studies they sought converging evidence about the perceptual system's representation of uninformative peripheral stimuli that were presented at relevant versus irrelevant location: either by exploring their power to generate illusory line motion (Ishigami et al, 2009) or by comparing the early, sensory-related

ERP components, elicited by these cues (Ishigami et al., 2012). In both studies Ishigami et al. observed stronger capture (slower responses to invalidly cued targets) from cues presented at relevant (putatively attended) locations than from cues presented at irrelevant (putatively unattended) locations and concluded that "spatial ACS can be established to attend to two different locations. (IKC'09, p. 449-450)."

Recently, Ruthruff and Gaspelin (2018) tested if abrupt-onset cues presented at spatially-irrelevant locations can be completely ignored. Participants saw a search array consisting of four letters arranged in a cross and, as were Ishigami's participants, were told to attend to either the horizontal letters or the vertical locations (Figure 4.1). Prior to the search display, abrupt-onset cues were presented either at the relevant or the irrelevant locations. On one-third of the trials, no cues were presented. As expected, participants' responses were faster when the cue and target locations matched. As in Ishigami's experiments, response times following an invalid cue at a relevant location were significantly slower than those following an invalid cue at an irrelevant location. Importantly, there was no difference in performance (RT and Accuracy) between the cue absent and the irrelevant-cue trials. This lack of cost due to the presence of an irrelevant cue led the authors to conclude, as implied in their title that there was "immunity to attentional capture at ignored locations."

A potential methodological problem with Ruthruff and Gaspelin (2018) is that the cue absent trials lacked the possible benefit from alertness that any cue could provide on the cue present trials. Relative to all the conditions with cues, this alertness confound could have slowed down the responses on the cue absent trials causing an overestimation of RT on these trials. Such an overestimation due to a lack of alertness in this baseline condition might mean that the “immunity from capture” inference was incorrect. Exploring this possibility is the objective of this registered replication.

Recognizing this possibility, Ruthruff and Gaspelin (2018) attempted to address this confound in Experiment 2 by including a central cue condition which was expected to be a more appropriate baseline condition. The authors found equivalent performance on cue absent, central cue and irrelevant cue trials. Although we agree that such a central cue adequately addresses the alertness confound described above, we believe that it introduces a different confound: the central cue could have slowed down responses due to delayed disengagement from a recently changed central fixation. Posner and colleagues (Posner, Walker, Friedrich & Rafal, 1984) proposed that covert attention orienting could be understood in terms of a sequence of three operations: *disengaging* from the current focus, *moving* to the new focus and *engaging* there. It is well established that activation of the system responsible for maintaining fixation, whether from visual stimulation near the fovea (e.g., Walker, Deubel & Findlay, 1997) or electrical or neurochemical activation of the rostral pole of the superior colliculus (Munoz & Wurtz, 1993), increases the time to disengage attention overtly. Our concern with the central cue condition used in Ruthruff & Gaspelin’s Experiment 2 is simply that a corresponding effect might apply to covert orienting. Thus, we believe that both Experiments 1 and 2 from this paper are potentially compromised by a baseline condition that was in one way or another confounded and therefore that immunity from attention capture at irrelevant locations was not convincingly demonstrated.

We have highlighted the research of Ishigami et al (2009, 2012) for several reasons. Firstly, when compared to Ruthruff and Gaspelin's methods, despite the different nature of the cues and targets, there are striking methodological similarities. In both lines of research:There were four locations (above/below and left/right) of fixation where spatially uninformative peripheral cues might be presented; for different groups of participants targets could only be presented at 2 of these 4 locations; and consequently, there were three analogous conditions on trials with cues: valid and invalid (for cues at relevant locations), and invalid cues at irrelevant locations, which will be referred to simply as irrelevant cues. Secondly, the patterns of results from these three cue-conditions, as can be seen in Figures 4.2A, 4.2B, 4.2C and 4.2D are remarkably similar from both lines of research: Relevant cues generated robust cueing effects (invalid minus valid) in both lines of research. And relevant to the question about spatial attentional control settings, both lines of research found that reaction times to targets was faster following invalid cues at irrelevant locations than following invalid cues at relevant locations.

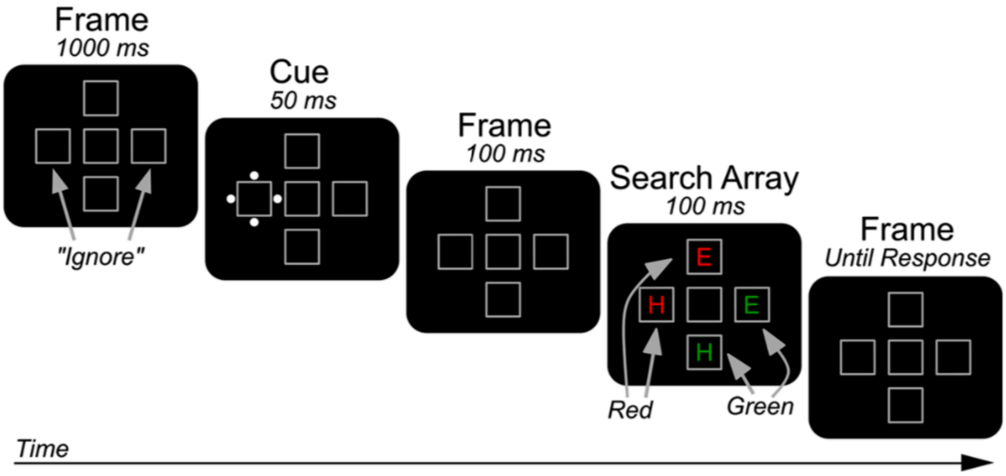

Figure 4.1. Sequence of events on a sample trial from Ruthruff and Gaspelin (2018).

Thirdly, there are several noteworthy differences two of which we will emphasize here. One important difference was the nature of the targets and the nature of the array when the target was presented. The target in Ishigami et al.'s experiments was a change of one figure 8 into a 2 or 5. Thus at the time of the presentation of the target, the location containing the target was both the only location where there had been a post-cue change in shape, and the target could be considered a singleton. In contrast, in Ruthruff and Gaspelin the target letter (E or H) was defined by color (for different participants, red or green) and at the time of target presentation colored letters appeared in all 4 locations. Thus at the time of target presentation, the target was not a singleton and any attention allocated to the wrong locations might result in an incorrect response. One consequence of this difference is that Ruthruff and Gaspelin's participants would have had even greater motivation to allocate their attention to the relevant locations because not doing so would result in erroneous responses. Another important difference is that at the time of the cue (or, on cue absent trials, at the time when a cue would have been presented) Ishigami et al. (2009) presented an auditory warning signal (this feature was not necessary, and hence not included, in Ishigami et al., 2012 because the cue absent condition itself was dropped). The purpose of this highly reliable auditory warning was to ensure that participants would be relatively equally alert/prepared for the target regardless of the cue condition.

Comparing the findings of comparable conditions from Ishigami et al. and Ruthruff and Gaspelin (Figure 4.2A, B, D) reveals that while Ishigami et al. (2009) found slower responses on irrelevant trials relative to no-cue trials, Ruthruff and

Gasplein found equivalent RTs on these two types of trials. Importantly and crucially to the current replication study, all trials in Ishigami et al. (2009) - with and without cue - were accompanied by a warning beep as an alerting signal. As a result, the different cue conditions were equated in terms of the alerting signal and only differed from one another in terms of the orienting mechanism. We propose to incorporate this methodology of Ishigami et al. (2009) within the design of Ruthruff and Gaspelin (2018) and examine if the different pattern of results obtained in the two studies can be attributed to the lack of the alerting signal in Ruthruff and Gaspelin.

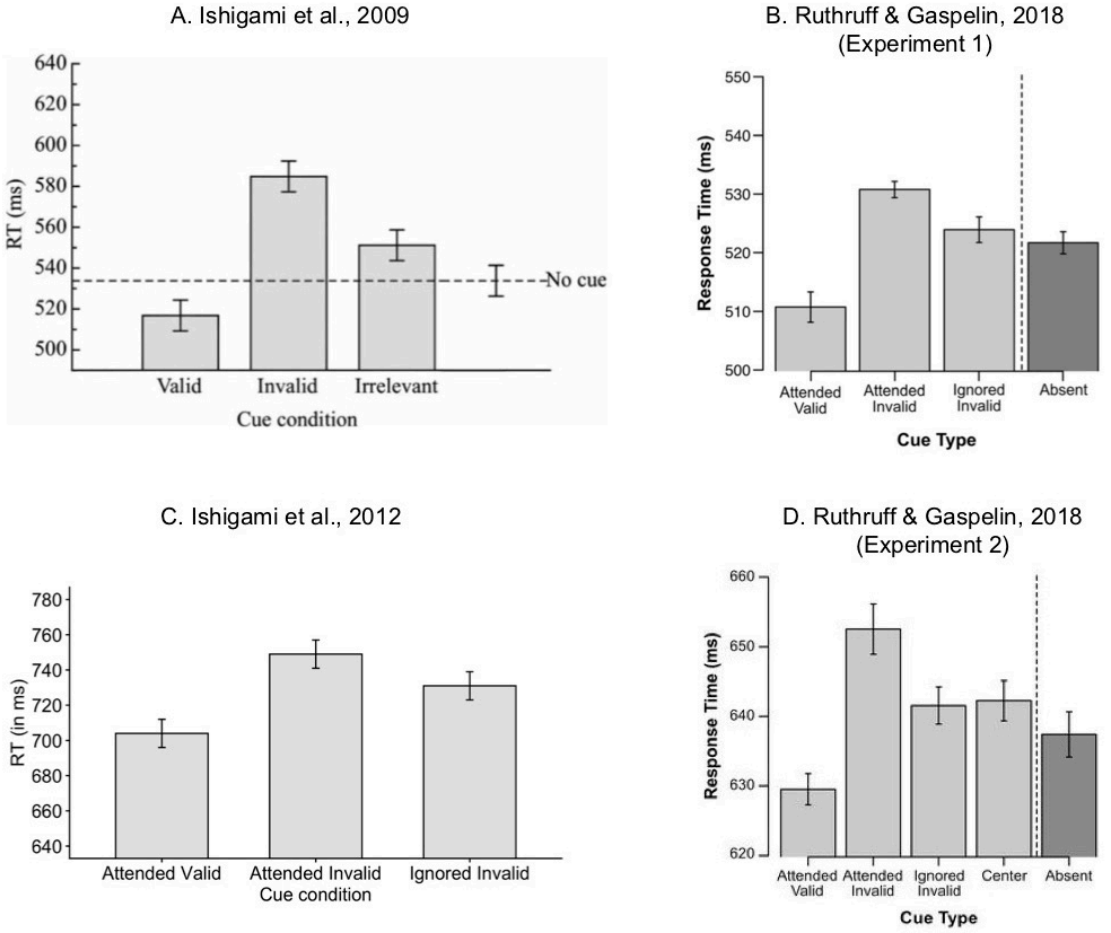

Figure 4.2. Response times for different cue conditions in A) Ishigami et al. (2009,Experiment 2, blocks 2-3), B) Ruthruff and Gaspelin (2018, Experiment 1), C) Ishigami et al. (2012) and D) Ruthruff and Gaspelin (2018, Experiment 2).

Experiment 1 will be a direct replication of Ruthruff and Gaspelin (2018, Experiment 1) and Experiment 2 will additionally include a warning beep on every trial simultaneous with the presentation of the cue. In Experiment 1, we expect to find the same pattern of results as Ruthruff and Gaspelin (2018) - cue validity effects for relevant cues and equivalent RTs on irrelevant-cue trials when compared to cue-absent trials. In Experiment 2, we expect the warning signal to speed up responses on cue absent trials. As a result, equivalent performance on cue-absent and irrelevant cues previously observed in Ruthruff and Gaspelin (2018) might no longer be seen. Instead, we might observe reduced attention capture by irrelevant-invalid cues as seen in Ishigami et al. (2009) rather than complete immunity from attention capture as reported by Ruthruff and Gaspelin (2018). Alternatively, it is possible that the increased motivation to focus attention on the instructed locations associated with Ruthruff and Gaspeclin's, methods as described above, was the key factor responsible for their finding of "immunity to capture." If this is true, then we should replicate results of Ruthruff and Gaspelin (2018) even with the inclusion of the warning signal.

4.2 Experiment 1

4.2.1 Methods

Participants. The Experiment was administered online to 40 participants (25 female, 15 male, mean Age = 26.6 years, SD = 4.4). All participants were healthy adults. All participants had normal or corrected-to-normal vision. The experimental procedures were approved by the Institutional Ethics Committee of University of Hyderabad.

4.2.2 Procedure

The experiment was designed in PsychoPy and data were collected online using PsychoJS (v2020.1) in combination with Pavlovia. Chrome, Safari and Firefox browsers were used by the participants. Detailed instructions were given to the participants to minimise distractions while doing the experiment. The participants were asked to place themselves 60 cm away from the screen. Since the screen dimensions varied across participants, the stimulus dimensions and the distances between them were controlled by through a calibration process. In this process, a rectangular box was displayed on the screen and the participants were asked to adjust the size of the box to match that of a standard debit/credit card. Since the size of a debit/credit card is universal, it is possible to calculate the degrees in visual angle subtended by a debit/credit at a distance of 60 cm. Using this and the size of the card in pixels, for each participants' screen, a base pixel/degree value (number of pixels equivalent to 1 degree) was calculated and used as the base unit for all stimulus sizes/ distances. For instance, if the px/deg was 52 for a particular screen at a viewing distance of 60 cm, and a placeholder had to be presented for 2.5 visual angle, then the stimulus size for the placeholder was fixed at (52 * 2.5) px.

The experimental procedure of Ruthruff and Gaspelin (2018) was followed exactly. Each trial started with five placeholder boxes (grey, 2.5° in width and height) presented for 1000 ms. Four boxes were arranged in a cross formation and there was one box at central fixation (Figure 4.1). Next, an abrupt-onset cue consisting of four white dots surrounding one of the peripheral boxes were presented for 50 ms. After 100 ms, four letters (2° in height and width) randomly picked between E and H were presented on the screen. On each axis (horizontal and vertical), there was one red letter (RGB: 255, 0, 0) and one green letter (RGB: 0, 151, 0). Each participants was randomly assigned to either the horizontal or the vertical axis group. The participants in the horizontal axis group were asked to look for a red (or green) coloured letter in the horizontal axis (left/right) and report its identity. The vertical axis participants had to search for the target letter in the vertical locations (up/down). The location of the cue was selected randomly such that the cue was non informative of the target location. Cue-absent trials (one-third of all trials) followed the same trial sequence except during the cue duration, the empty placeholders were presented. The remaining two-third trials were equally divided between cues at relevant and irrelevant locations.

There was a practice block of 64 trials at the beginning. Next, participants completed 12 blocks of 64 trials each. After each block, participants received RT and Accuracy feedback.

4.2.3 Data analysis

Trials with Response Times (RTs) less than 200 ms and greater than 1500 ms were discarded as outliers (2.2 %). Errors were also excluded from RT analysis (6.1 %). One participant's data had to be excluded as their error percentage was too high (51 %).

4.2.4 Results

The results are presented in Figure 4.3 and Table 4.1.A one-way repeated measures ANOVA on mean RT showed a significant effect of cue condition, F (3, 114) = 19.11, $p < 0.001$, $\eta_p^2 = 0.335$. Next, three sets of planned t-test were undertaken:Participants were faster responding on valid trials (563 ms) compared to invalid trials (582 ms), t (1, 38) = 5.13, $p < 0.001$, $d = 0.823$ indicating capture at relevant locations. Responses on irrelevant trials were faster compared to invalid trials, *t* (1, 38) = 2.13, *p* = 0.04, *d* = 0.34 indicating reduced capture by irrelevant cues. t-tests comparing irrelevant-invalid and absent conditions also showed a significant difference. Responses on irrelevant trials (575 ms) were significantly faster than absent trials (582 ms), *t* (1, 38) = 3.61, *p* < 0.001, *d* = 0.58. This result is different from Ruthruff and Gaspelin who found no difference between irrelevant-invalid and absent trials.

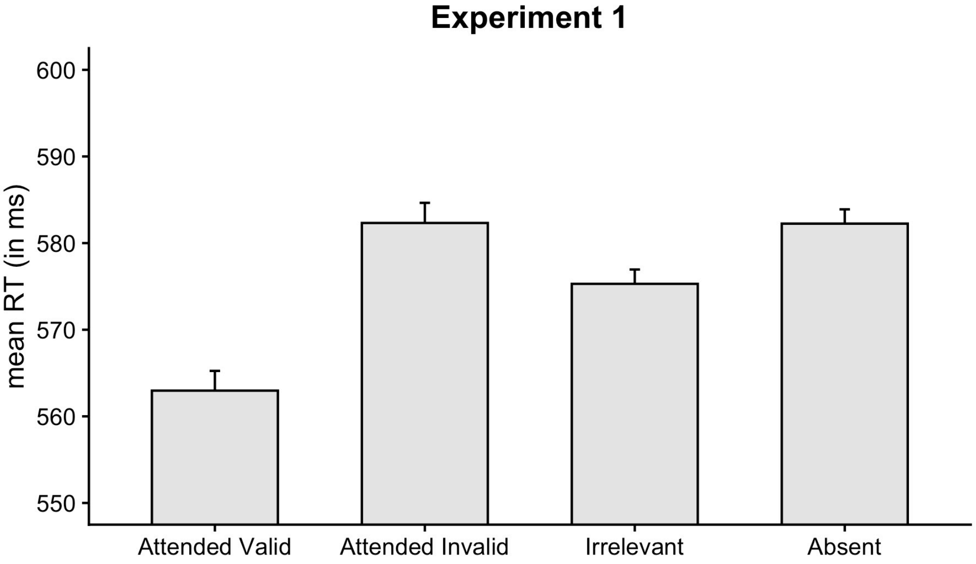

Figure 4.3. Response times as a function of cue condition (Experiment 1). Error bars represent +1 SE.

Table 4.1. Mean RT and Error rates for all cue conditions (Experiment 1)

	Mean RT (SE)	Error rate (SE)
Absent	582 (1.6)	5.2 (0.9)
Irrelevant	575 (1.6)	6.5 (1.2)
Invalid	582 (2.3)	7.3 (1.2)
Valid	563 (2.3)	5.7 (1)

The analyses of error rates revealed a significant main effect of cue condition, *F* (3, 114) = 7.08, $p < 0.001$, $\eta_p^2 = 0.16$. Pairwise t-tests showed that error rates were higher for invalid trials compared to valid trials, *t* (1, 38) = 2.61, $p = 0.013$, $d = 0.42$. Errors on irrelevant trials were lower than on invalid trials, *t* (1, 38) = 2.36, $p = 0.023$, $d = 0.38$. Error rates on invalid trials were greater than on absent trials, *t* (1, 38) = 2.77, $p = 0.009$, $d = 0.44$.

4.2.5 Discussion

Here we replicated the task used in Experiment 1 of Ruthruff and Gaspelin (2018). As expected from previous research, participants were faster and more accurate responding on valid trials compared to the invalid trials. In other words cues presented at relevant locations captured attention. Participants were also faster and more accurate following irrelevant than invalid cues. This demonstrates reduced capture from irrelevant cues. We also observed faster RTs on irrelevant trials compared to the absent trials which is different from the observation of equivalent RTs on these two conditions in Ruthruff and Gaspelin. Whereas it is possible to argue that this finding reflects "immunity to attention capture" (irrelevant RT is not worse than that in the cue absent condition) we believe that both in this experiment and in Ruthruff and Gaspelin (2018, Experiment 1), the RTs on absent trials were overestimated due to the lack of an alerting benefit on absent trials. Indeed, the relatively slow and yet accurate responding in the cue-absent condition of our experiment is consistent with a low level of alertness in this conditions (Posner, Klein, Summers & Buggie, 1973). Precisely to avoid this possibility and to equate alertness across the different conditions we administered Experiment 2 with a warning signal on every trial.

4.3 Experiment 2

4.3.1 Methods

Participants. Experiment 2 was administered on 40 participants (14 female, mean age = 28.5 years, SD = 3.8) who did not take part in Experiment 1.

4.3.2 Procedure

The stimuli and the procedure were the same as that used in Experiment 1 except for the following change. On all trials, an auditory warning signal was presented for 50 ms. The onset of the warning signal was simultaneous with the presentation of the abrupt-onset cue on cue-present trials. On cue-absent trials, the warning signal preceded the target by the same foreperiod (SOA=150 ms).

4.3.3 Data analysis

The data of three participants who accuracy was less than 60% was discarded. 1.9% of the trials were discarded as outliers as the RT was greater than 1500 ms or less than 200 ms. Errors (4.1%) were discarded from the RT analyses. The same analyses procedure of Experiment 1 was followed.

4.3.4 Results

The results are presented in Figure 4.4 and Table 4.2.A one-way repeated measures ANOVA on mean RT showed a significant effect of cue condition, *F* (2.13, 76.7) = 10.79, $p < 0.001$, $\eta_p^2 = 0.23$. Pairwise planned t-tests showed, as had been seen in Experiment 1, faster RTs on valid trials compared to invalid trials, t (1, 36) =

3.77, p < 0.001, d = 0.62, and faster RTs on irrelevant trials compared to invalid trials, t (1, 36) = 3.59, p < 0.001, d = 0.59. In contrast to what we found in Experiment 1 and what was reported by Ruthruff & Gaspelin, RT on irrelevant trials was slower than absent trials, t (1, 36) = 2.43, p = 0.02, d = 0.4 indicating capture by cues at irrelevant locations.

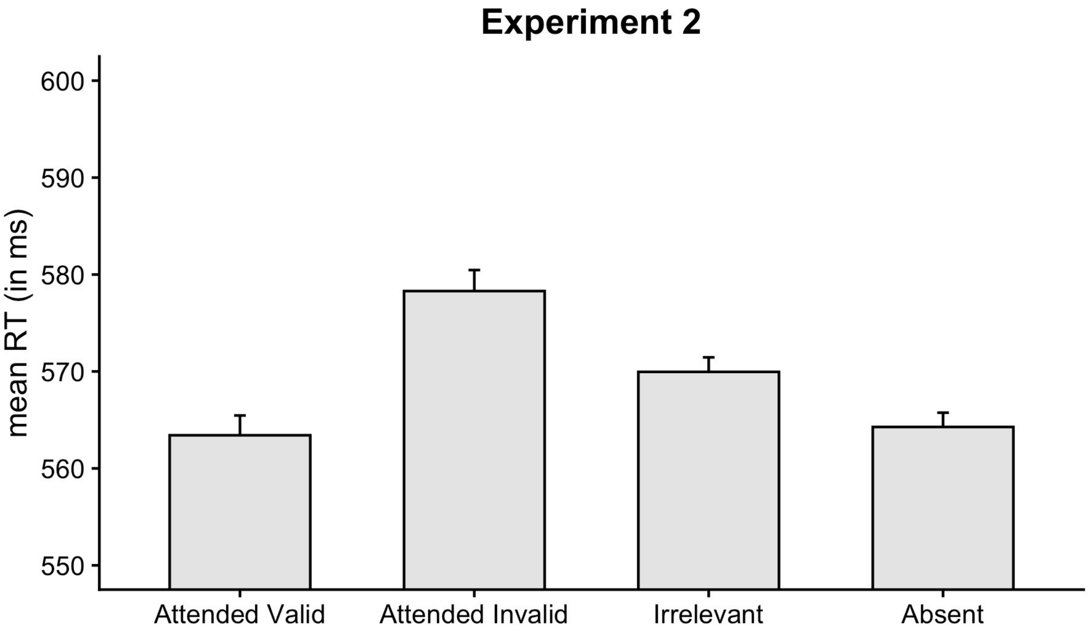

Figure 4.4. Response times as a function of cue condition (Experiment 2). Error bars represent +1 SE.

Table 4.2. Mean RT and Error rates for all cue conditions (Experiment 2)

	Mean RT (SE)	Error rate (SE)
Absent	564 (1.5)	3.8 (0.6)
Irrelevant	570 (1.5)	4 (0.6)
Invalid	578 (2.2)	5.3 (0.8)
Valid	563 (2)	3.4 (0.6)

There was a significant main effect of cue condition on errors, *F* (2.24, 80.6) = 5.61, *p* = 0.001, η_p^2 = 0.13. Pairwise t-tests showed fewer errors on the valid trials compared to invalid trials, *t* (1, 36) = 3, *p* = 0.005, *d* = 0.49. Error rates on absent trials were equivalent to the error rates on irrelevant trials, *t* (1, 36) = 0.57, *p* = 0.575, *d* = 0.09. There were also fewer errors on irrelevant trials compared to invalid trials, *t* (1, 36) = 3.35, *p* = 0.002, *d* = 0.55.

4.3.5 Discussion

In this experiment, with an auditory warning signal on every trial two key findings from Experiment 1 and the prior research of Ruthruff and Gaspelin (2018) and Ishigami et al. (2009; 2012) were replicated: cues at relevant locations captured attention and cues at irrelevant locations captured attention less effectively. The key question was whether performance on trials following irrelevant cues would be equivalent to that on cue absent trials as in Ruthruff and Gaspelin (2018, Experiment 1). Contradicting their "immunity to capture" inference, when alertness was equated across conditions and therefore performance in the cue-absent condition was not artificially elevated due to a low level of alertness, performance on irrelevant trials was significantly worse than that on cue absent trials.

4.4 General discussion

In this paper, we aimed to re-examine the claim of immunity from attention capture at irrelevant locations made by Ruthruff and Gaspelin (2018). Ruthruff and Gaspelin's assertion of immunity from attention capture at irrelevant locations was based, in part, on equivalent RTs on cue absent and irrelevant trials in their

Experiment 1. We speculated that the RTs on cue absent trials had been overestimated due to a lower level of alertness in this condition. When this confound was addressed in our Experiment 2 by the inclusion of a warning signal on all trials, the irrelevant RTs were found to be slower than cue absent RTs.

Recognizing the possible contribution of alertness differences in their Experiment 1, in their Experiment 2, Ruthruff and Gaspelin incorporated a different baseline condition that entailed presenting the cue around the fixation stimulus. RTs on these trials were equivalent to RTs on trials with irrelevant cues. Although this finding might be taken as evidence for immunity from capture, as mentioned in the introduction, an increase in information at fixation might cause a difficulty in disengaging from the central location. We are not aware of any direct evidence to support the possibility that disengaging from fixation might be more difficult when fixation had been recently cued, but we are planning to test this idea.

Ruthruff and Gaspelin (2018) attempted to provide stronger evidence for spatial filtering in Experiment 3 by presenting cues that either matched the target or the distractor color. Instead of four white dots appearing as abrupt-onset cues, one of the placeholder boxes changed to either the target color or the distractor color and the other three boxes turned into a neutral color (blue or yellow). Robust cueing effects were found for target-colored cues at relevant locations, but not for distractor-colored cues at relevant locations. This was as expected (based on Folk et al., 1992 and many other studies). The key was to test if the target-colored cues capture attention when presented at ignored locations. It was seen that RTs for ignored target-colored cues were equivalent to RTs for ignored distractor-colored cues indicating that the feature-based irrelevancy of the cues did not result in a cost when the cues were presented at ignored locations. Thus, the authors concluded that even potent color cues whose features are task-relevant failed to capture attention when presented at ignored locations. It is important to note that this experiment only shows that the extent of attention capture is equivalent for both target-matching and distractor-matching cues at ignored locations. It does not directly indicate immunity to attention capture for target-colored cues at ignored locations because unlike Experiment 1 and 2, a critical baseline condition was missing in this experiment.

As noted in the discussion of Experiment 1, we found RTs on cue absent trials to be slower than ignored RTs. This was different from the finding in Ruthruff and Gaspelin's Experiment 1 where equivalent RTs were found between these two conditions. This was surprising since both the experiments used the same methods - except that the our studies were web-based whereas the Ruthruff and Gaspelin's studies were done in the lab. We can only speculate on the exact causes of this discrepancy. It is possible that the slowness of the responses on the cue absent trials in the absence of the warning signal was exaggerated in the online version. Importantly, regardless of the explanation, the instability of the RTs on no-cue trials points to the inadequacy of this condition as a baseline when alertness has not been controlled.

A difference between our study and that of Ruthruff and Gaspelin (2018) is the mode of data collection. Due to the onset of COVID-19, the data for both of our experiments were collected through a web-based platform. The experiment by Ruthruff and Gaspelin was conducted in a laboratory environment. Studies comparing the timing accuracy on web-based experimental platforms with lab-based studies have shown that response times in web-based studies are typically shifted by a constant value, in comparison to lab-based studies (Bridges, Pitiot, MacAskill, & Peirce, 2020). Several recent studies examining the efficiency of web-based experiments have concluded that web-based platforms such as jsPsych, Lab.js, Gorilla among others are capable of providing accurate timing as long as relative measures are used. In line with this, the cueing effects at relevant locations obtained in our study (Experiment 1: 19 ms, Experiment 2: 15 ms) were in the same range as in Ruthruff and Gaspelin (Experiment 1: 20 ms). The only difference was the overall mean RT in both of our experiments (Experiment 1: 575 ms, Experiment 2: 569 ms) was slightly slower compared to Ruthruff and Gaspelin (Experiment 1: 522 ms). Thus, we have no reason to believe that the precision of RT measurement was in anyway compromised in our study due to the online implementation.

To summarise, across all the studies discussed so far - Ruthruff and Gaspelin (2018, Experiment 1, 2 and 3), Ishigami et al. (2009, 2012) and the two experiments described in this paper - responses on trials following irrelevant cues are faster than those following invalid cues. This provides a robust demonstration that spatial attentional control settings can attenuate attention capture at irrelevant locations. We demonstrated that the conclusions arrived at by Ruthruff and Gaspelin regarding complete immunity of attention capture at ignored locations was premature due to the inappropriate baseline used for comparison in their Experiment 1. When we controlled for alertness differences, performance on irrelevant trials was significantly worse than on cue absent trials (in both RT and accuracy), a pattern distinctly contrary to the assertion of “immunity” from capture. We believe that this finding provides indirect evidence to support our concern with the central cue baseline Ruthruff & Gaspelin relied on in their Experiment 2. Nevertheless, we recognize that in the absence of direct evidence for a disengage cost associated with a recently cued fixation stimulus, it is equally plausible to argue the reverse: The central cue baseline could be the “better" one and it might point to some inadequacy of our “no cue baseline with auditory warning signals on all trials”.Thus, we would like to end with the note that it could indeed be possible that peripheral events at irrelevant locations can be completely ignored, but immunity from capture cannot yet be confidently endorsed.

5. Individual differences in masked visual processing I: Studying attentional control over unconscious cueing in the Deaf

Auditory loss in deaf individuals has been associated with an enhancement in the visual modality. Visual attention is one domain where such plasticity-induced changes have been observed, although which specific attentional mechanisms are improved is still not clear. Using a modified spatial cueing paradigm, we examined attention capture in deaf and normal-hearing participants. Brief abrupt-onset cues were presented for 16 ms either in attended or ignored locations. The to-be-attended locations for each trial were indicated by a horizontal or a vertical bar at the centre of the screen. These were presented either in vertical- or horizontal-only blocks or mixed together. We observed greater negative cueing effects in the NH group compared to deaf. These results suggest that the deaf are not better at disengaging attention from cued locations compared to normal-hearing individuals. Additionally, the deaf showed greater capture by cues at ignored locations in the slower responses. These findings shed further light on orienting mechanisms in the deaf and help in understanding the specificity of plasticity-induced changes in the deaf.

This chapter is under consideration for publication at *Neuropsychologia:* Prasad, S. G., Patil. G. S., Somashekarappa, V. & Mishra, R. K. (under review). Attention capture by brief abrupt-onset cues in deaf individuals. *Neuropsychologia*.

5.1 Introduction

It is well known that neuroplasticity leads to changes in visual attention in the deaf (see Bavelier, Dye, & Hauser, 2006; Dye & Bavelier, 2013; Pavani & Bottari, 2012 for reviews). Enhanced processing of visual information in the deaf has been seen, for example, in simple detection tasks (Bottari, Nava, Ley, & Pavani, 2010; Loke and Song, 1991), change blindness (Bottari et al., 2008), selective attention and filtering tasks (Dye, Baril, & Bavelier, 2007; Dye, Hauser, & Bavelier, 2009; Hauthal, Neumann, & Schweinberger, 2012; Proksch & Baveleier, 2002; Sladen et al., 2005), especially in the periphery. However, these attentional differences in the deaf are not widespread. In several other tasks such as, brightness discrimination (Bross, 1979) and contrast sensitivity (Finney and Dobkins, 2001), comparable performance between deaf and normal-hearing is observed. Understanding the specificity of the visual processing advantage in the deaf is important for several reasons. It helps determine which aspects of visual processing are amenable to plasticity-related changes and also narrow down the locus of neuroplasticity in the brain. In this study, our objective was to examine if congenitally deaf adults who have learnt sign language since childhood show differences in attentional orienting to brief, peripheral cues compared to normal-hearing. Additionally, we also examined if the deaf demonstrate better endogenous control over attentional orienting.

Most researchers agree that the deaf and normal-hearing individuals primarily differ in the spatial distribution of attentional resources in the visual field (Dye & Bavelier, 2013). While normal-hearing (NH) individuals show enhanced processing at the fovea, the deaf have been shown to possess enhanced peripheral attention. One way this has been demonstrated is by using the classic cue-target paradigm pioneered by Posner (1980) which remains the hallmark for examining attentional orienting to peripheral cues (Chen et al. 2006; Colmenero et al. 2004; Parasnis and Samar, 1985; Prasad, Patil, & Mishra., 2015; Xingjuan et al. 2011). In this paradigm, a peripheral cue is followed by a target either at the same location (cue-valid trials) or at a different location (cue-invalid trials). Responses are facilitated on cue valid trials and thus, the typical finding is that participants are faster responding to targets on the cue-valid trials compared to the cue-invalid trials. In one of the earliest studies on deaf visual attention, Parasnis and Samar (1985) administered a target detection task with central cues. The deaf showed faster response-times on cue-invalid trials compared to Normal-hearing (NH). Colmenero et al. (2004, Experiment 1) similarly found that the invalid peripheral cues caused lesser interference in the deaf, while the amount of facilitation caused by valid cues was comparable between deaf and NH. Taken together, these findings have led some researchers to conclude that while cue-related *benefits* may not always be observed in the deaf, they are more likely to be better at disengaging attention from cued locations (Dye & Bavelier, 2013).

However, not all studies have found faster/better disengagement from cued locations in the deaf. Xingjuan et al. (2011) administered a simple detection task with cue-target SOAs ranging from 350 ms to 1250 ms They found that the cues were inhibited at all the SOAs and the participants were faster on cue-invalid trials compared to the cue-valid trials. This pattern, known as inhibition of return (IOR) is said to reflect the disengagement of attention from cued locations. In Xingjuan et al. (2011), the time course, as well as the magnitude of inhibition, did not differ between the deaf and NH group. Jayaraman et al. (2016) also found no differences between deaf and NH in either time course or magnitude of inhibition. It is worth noting that Xingjuan et al. (2011) measured inhibition through manual response times whereas Jayaraman et al. (2016) used eye movement measures. Interestingly, in another study recent study with eye movement measures, Prasad et al. (2015) compared deaf and NH on a saccadic localisation task and measured saccade latency to the target. They found enhanced facilitation in the deaf at short SOA (150 ms) indicating enhanced cue-related benefits compared to NH. Thus, in spite, of several studies examining attentional orienting in the deaf, it is still not clear if the deaf are better at orienting attention to a cued location, or disengaging from it or neither. To sum up, the findings from the Posner cueing paradigm in the deaf paint a mixed picture thus warranting a re-examination.

We used brief peripheral cues of 16 ms duration. There is a large body of work on the hearing population that shows that attention can be captured by brief subliminal cues (eg., Ivanoff & Klein, 2003; McCormick, 1997). These studies have demonstrated that attentional selection can also be driven by nearly-invisible stimuli. But similar investigations in the deaf population are missing. The processing of subliminal stimuli is said to be mediated by the dorsal stream pathways (eg., Brogaard, 2011; Milner, 2012; but see Hesselmann, Darcy, Rothkirch, & Sterzer, 2018). The source of enhanced attention in the deaf may also lie in the functioning of dorsal visual stream pathways. The dorsal route hypothesis implicates the dorsal steam pathways in the plasticity-induced attentional changes in the deaf, especially in the periphery (Bavelier & Neville, 2002). The primary evidence for this comes from neuroimaging studies which showed enhanced recruitment of Medial Temporal Area - one of the main targets of dorsal stream projections - in the deaf in response to motion stimuli (Bavelier et al., 2001; Fine et al., 2005). Given that brief subliminal cues recruit dorsal stream areas and that dorsal stream functioning is enhanced in the deaf, we used brief cues to maximise the expected differences between the deaf and NH participants (Prasad, Patil, & Mishra, 2017; Prasad & Mishra, 2018).

Additionally, we also wanted to examine if the deaf display enhanced endogenous control over attention capture. Bottari et al. (2008) administered a change blindness task where two displays were presented separated by a blank screen. The task was to detect whether a change was present or absent in the second display and provide a manual response. Due to the insertion of the blank screen, participants could not detect the change due to abrupt changes in the display and had to deploy purely endogenous attention. The results showed comparable detection accuracy in deaf and NH when they were told to focus their attention at a particular location (center or periphery). Such findings lead researchers to conclude that the changes in the deaf are mostly linked only to the involuntary, exogenous components of attention and that endogenous components in the deaf don’t drive the observed visual processing advantages (Bottari et al., 2010). In contrast, in a recent study using a visual search task (Heimler et al., 2015), participants were asked to find the target (a line with a specific orientation) in the presence of a salient-coloured distractor and make a saccade to its location. The deaf were less influenced by the salient distractors than the normal-hearing participants. The deaf were also slower in responding than their hearing counterparts. Thus, it possible that the deaf are better at implementing goal-driven processes when they make slow and strategic responses (Bavelier et al., 2000; Neville and Lawson, 1987; Sladen et al., 2005).

To examine this within the Posner cueing paradigm, we used a modified version of the design pioneered by Folk, Remington and Johnston (1992) to study the role of top-down control on attention capture. Folk et al. (1992, Experiment 1) asked participants to search for a single white target (x or =) that appeared suddenly on the screen (abrupt-onset target) in one block of trials. In another block, participants searched for a red target (x or =) surrounded by three other characters in white. In both the blocks, white abrupt-onset cues were presented before the targets. The authors found that a white abrupt-onset cue captured attention only while searching for a white abrupt-onset target and not while searching for a red colour target. This effect which is known as "contingent capture" - is said to reflect top-down control over salient information because only those cues that match the attentional control settings (ACS) induced by the current task-goals capture attention. Most studies on top-down control in attention capture even on NH participants have manipulated *feature-based* ACS, in line with the Folk et al (1992) study. That is, the cue relevancy is manipulated at the level of the feature of the cue (eg., colour vs onset). To our knowledge, contingent capture with spatial ACS (relevancy manipulated based on cue location) in the deaf has not been studied (but see, Yantis & Jonides, 1990; Ishigami, Klein, & Christie, 2009; Ruthruff & Gaspelin, 2018 for such studies in NH). The changes in spatial attention in the deaf as seen in spatial cueing tasks are well documented (Chen et al. 2006; Colmenero et al. 2004; Prasad et al., 2015; Xingjuan et al. 2011). Given this, it is necessary to probe whether the capacity for goal-directed attention capture by spatial cues is also enhanced in the deaf correspondingly.

We adapted and modified the paradigm used by Ruthruff and Gaspelin (2018) who examined the role of *spatial* ACS on attention capture in NH. A target display consisting of four boxes arranged in a cross was presented to the participants. On each trial, they were asked to search for a specific coloured letter (E or M) either in the horizontal direction or in the vertical direction. A central bar (horizontal or vertical) at the beginning of every trial (ACS signal) indicated the to-be-attended locations for that trial. Peripheral cues were presented for 16 ms which could appear in either attended or ignored locations. The horizontal and vertical trials were either blocked or mixed together. This was done since studies showing top-down contingent capture effects (with visible cues) have mostly consisted of a single target-defining feature for a block of trials (eg., Büsel, Voracek, & Ansorge, 2018; Folk et al., 1992; Folk & Remington, 2006). It has been argued that inter-trial priming can account for some of these findings (Belopolsky, Schreij, Theeuwes, 2010; Theeuwes, 2013). Belopolsky et al. (2010) used the contingent-capture paradigm of Folk et al. with one major change - while the presentation of target type (abrupt-onset or colour target) was blocked in Folk et al. (1992), it was mixed in Belopolsky et al. (2010). An instructional cue indicated the type of target (onset or colour-singleton) on every trial. The objective was to examine if only those cues that matched the target set would capture attention as found in the original study by Folk et al. (1992) even when the target-type changed dynamically from trial-to-trial. This was not observed. Both types of cues captured attention as seen through manual response times irrespective of the target set (Experiment 1 and 2) leading the authors to conclude that top-down control over task-irrelevant cues is not possible and that original effects of Folk et al. (1992) could be attributed to the repeated appearance of a target-feature across trials.

Taking this into consideration, the target-defining aspects (in this case, attended locations) were also mixed during the presentation to rule out bottom-up statistical learning. There were four cue conditions depending on the ACS signal and the location of the cue: attended valid (cue in one of the attended locations, target at cue location), attended invalid (cue in one of the attended locations, target at the opposite attended location), ignored invalid (cue on one of the ignored location, target in one of the attended locations) and cue absent trials. Our predictions were as follows:

1) Based on our previous findings on a similar population (Prasad et al., 2015), we hypothesise that the congenital early deaf individuals should show enhanced cue validity effects (RT invalid - RT valid) for the attended cues compared to the normal-hearing individuals.
2) If cross-modal plasticity in the congenitally deaf individuals leads to better endogenous control over attention capture, we should see reduced attention capture by ignored cues manifest as faster RTs in the ignored invalid cue condition compared to the attended invalid conditions since the ignored invalid cues will not delay the responses as much as the attended invalid cues. In contrast, if the deaf can't exert endogenous control and filter out the spatially-irrelevant cues, then we should see slower or comparable responses on ignored invalid trials compared to attended invalid trials.

5.2 Experiment

5.2.1 Methods

Sample size. Power analysis was performed ("pwr" package in R) to determine the sample size. The effect size was estimated based on studies comparing attentional orienting in deaf and NH (Colmenero et al., 2004; Prasad et al., 2015). In all the studies, the effect sizes for group comparisons of cueing effects were used/calculated. Wherever effect sizes were not available, Cohen's standardised difference scores (dz) were estimated using the reported independent sample t-test values or F values and sample sizes (Cohen, 1988). The effect size was estimated to lie between 0.6 and 0.86. The power analysis yielded a sample size with a range of 22 to 44 participants in each group for the desired power of 0.8 and a confidence level of 0.05. We selected a sample size that was within this range.

Participants. Fifty-seven participants took part in the study where 27 were deaf individuals (8 females, Mean age = 25 years, SD = 4) and 30 were normal-hearing individuals (11 females, Mean age = 23, SD = 2). The deaf participants were recruited from the deaf enabled education centre in Hyderabad, India. All the deaf participants were diagnosed with congenital deafness with profound sensorineural hearing loss. They were all born to parents with normal hearing and received education in special schools for the deaf in which the primary medium of communication was Indian Sign Language (ISL). They reported having acquired sign language at an average age of 8 years and reported high proficiency in ISL (Mean self-rating score: 3) in sign language-use on a 3-point scale with 3 being the highest. All the above-mentioned information was obtained through a questionnaire from our previous studies (Prasad et al., 2015, 2017). The NH participants were students at the University of Hyderabad. All the participants provided written informed consent for their participation in the study. One of the co-authors of the study who is a speech pathologist with good proficiency in sign language took consent from and explained the instructions to the deaf participants. The institutional ethics committee of the University of Hyderabad approved the experimental procedures.

5.2.2 Procedure

Each participant was randomly assigned to attend to the red-targets (CIE-Lab: 54.42, 81.14, 70.65) or the green targets (CIE-Lab: 54.27, -54.11, 55.20). Participants were instructed to look for the coloured target (red or green depending on their assignment) in the attended locations (up and down for vertical bars; left and right for horizontal bars). The task was to report the identity of the target letter (E or M) by pressing a key on the keyboard (A and L labelled as E and M, this mapping was counterbalanced across participants).

All the stimuli were presented on a 15-inch monitor with a screen resolution of 1280 * 768 pixels in black on a grey (CIE-Lab: 63.33, 0.00, -0.00) background. Each trial started with a fixation cross (0.3° × 1°) displayed at the centre of the screen for 1000 ms (Figure 5.1). The display consisted of four square placeholder boxes (1.6°) arranged in the form of a plus sign above, below, to the left and right of the fixation cross. The placeholders were equidistant from the fixation (6°). A horizontal (1.5° × 0.6°) or vertical bar (0.6° × 1.5°) was presented at the centre of the screen (ACS signal). The orientation of the bar signalled the relevant locations for the trial.

Vertical bar indicated that up and down were relevant; horizontal bar indicated that left and right were relevant. Following the ACS signal, the outline of one of the boxes thickened for 16 ms serving as the abrupt-onset cue. After a duration of 100 ms during which the placeholders remained on the screen, the central bar disappeared and the search targets were presented. The target letter (in the relevant location; eg., red E) was presented along with a distractor letter in a different colour (eg., green M) in the opposite location. The two letters were also presented in red and green in the irrelevant locations, but the letter which was the target on that trial (eg., E) was never presented in the target colour (eg., red).

The experiment consisted of 640 trials divided into two blocks of 320 trials each (Blocked, Mixed). In each block, there were 160 trials with vertical bars and 160 trials with horizontal bars. These two types of trials either appeared randomly mixed together (Mixed ACS) or were blocked (Blocked ACS). The 320 trials in each block were equally divided between attended valid (80), attended invalid (80), ignored invalid (80) trials and cue absent trials (80). A break was given after every 80 trials. The trials within each block and the order of presentation of the blocks were randomised for each participant. A practice session of 20 trials was administered to the participants at the beginning of the experiment.

After the main experiment, all participants completed a cue localisation task where they were shown the exact same sequence of events as in the main experiment. After the target screen, the placeholder boxes were presented with a question mark in between. The participants were explicitly informed about the cue and were asked to identify its location. Responses were collected using a 4-button response pad with the

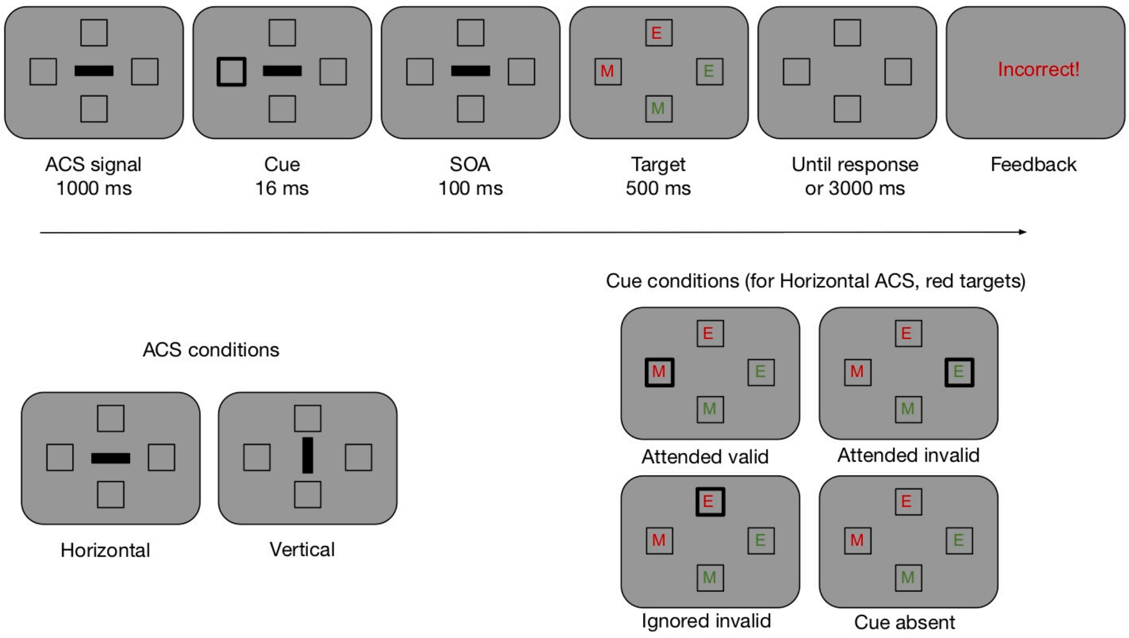

Figure 5.1. Sequence of events and experimental conditions in the experiment. The central ACS signal directed the participants to either attend to the horizontal boxes or the vertical boxes. These were either blocked or mixed. One set of participants were instructed to look for red targets, another set had to look for green targets. In this example, participant had to search for the red target (M) in the attended locations (horizontal) and press a button accordingly. The target, in this example, appears at the cued location. This illustrates a trial in the attended valid condition.

buttons arranged in a cross. Participants were instructed to press the button spatially congruent with the cue's location and to press the central button for the cue absent condition. There were a total of 128 trials equally divided between Mixed and Blocked ACS which were further divided into attended valid, attended invalid, ignored invalid and cue absent conditions.

5.2.3 Data analysis

Trials with no responses and with RTs lesser than 120 ms or greater than 2.5 median absolute deviation (MAD) were excluded from the median RT for each participant (9.3 % in deaf participants' data and 5.5 % in NH data). The MAD criterion was used as the common method for discarding responses using standard deviation is not considered to be as effective in detecting outliers in smaller samples (Cousineau & Chartier, 2010; Leys, Ley, Klein, Bernard & Licata, 2013). Trials with inaccurate responses were also discarded from RT analysis (14.4 % in deaf participants' data and 5.8 % in NH data). One deaf participant's data with less than 60 % accuracy was discarded.

Linear mixed-effects analysis on Response time (RT) and Accuracy using the lme4 package in R were performed. This analyses method was chosen as frequentist statistics such as ANOVA make assumptions of independence between observations which is clearly violated in a repeated measures design. Mixed models provide accurate and generalisable estimates of the effect taking into account the dependency among data points (Singmann & Kellen, 2019).

RT was analysed using the lmer function. lmerTest function was used to obtain p values using Satterthwaite approximations to degrees of freedom (Bates, Maechler, Bolker & Walker, 2015). Accuracy variable was dummy coded into 1's (Correct) and 0's (Incorrect) and analysed using the glmer function. p values were obtained through the default output of the glmer function in R based on asymptotic Wald tests (Luke, 2017). In both the analyses, cue condition was entered as a fixed effect by creating two separate columns - one comparing attended valid with attended invalid and another comparing attended invalid with ignored invalid[1]. This was done since cue condition was a factor with 3-levels and had to be broken down into 2-level factors. Thus, condition_IV_V (attended invalid: +1, attended valid: -1) and condition_IR_IV

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[1] We did not include cue-absent trials in the analysis as these trials lacked the alerting benefit provided by the presence of a cue. Thus, these two conditions not only differ in terms of the orienting mechanism, but also in the alerting mechanism. Any difference (or the lack of it) between cue-absent and the other cue conditions could not be solely attributed to differences in attentional orienting. This point has relevance for the analysis of ignored cues and is elaborated on further in the discussion.

(ignored invalid: +1, attended invalid: -1) were entered as fixed effects along with ACS type (mixed: +1, blocked: -1) and Group (deaf: +1, NH: -1). All main effects and interactions were included in the models. Participants and Items were included as random effects in all the models. Items referred to the trials administered to the participant. We chose this model based on the recommendation to use the "maximal random effects structure justified by the design" (Barr, et al., 2013). The random slopes for each fixed effect were not included as data with smaller sample sizes tend to not converge when random slopes are included (Singmann & Kellen, 2019).

Several researchers have suggested analysing RT distributions instead of solely relying on mean RT (eg., Balota & Yap, 2011). Time-course analysis of RTs can also shed light on attentional mechanisms as stimulus-driven influences are typically seen at shorter responses and goal-driven influences at longer responses (van Zoest & Donk, 2005, 2008). To examine if the goal-driven effects in our study are more evident in the slower responses, distributional analysis was done using the vincentisation procedure (Ratcliff, 1979). RT data of each participant was arranged in ascending order and five bins were created. Bin was dummy-coded with Bin1 as the reference level and entered as a fixed effect in all the models along with cue condition, ACS type and group. The interpretation of main effects in models with dummy coded variables is problematic (Singmann & Kellen, 2019, p.25). For instance, the main effect of cue condition (in the model with Bin as a dummy-coded factor) refers to the difference between the cue conditions for Bin1, and not across all the Bins. For this reason, we will refer to the Bin-related interactions in this model and refer to the previous model (without Bin) for all other effects.

Cue localisation. The cue localisation accuracy was analysed after discarding trials with response time lesser than 120 ms (12 % in NH and 11 % in the deaf). Cue visibility index (d') was first calculated for each cue location paired with the cue absent condition: up cue - no cue, down cue - no cue, left cue - no cue, right cue - no cue. For each pair, the cue appearing at one location (for eg., up) was designated as the signal and the no cue condition was designated as noise. Hits (correct responses to the signal) and False alarms (FA, incorrect responses to the noise) were calculated. Hit rate and FA rate was calculated by dividing the Hits and FAs by the total number of signal and noise trials, respectively. Occurrences of Hits and FAs of 0 or 1 were adjusted using the log-linear rule (Hautus, 1995; Prasad, Patil, & Mishra, 2017). The d' for each pair of locations was computed as the difference of the z transform of the Hit rate and the FA rate using the norm function in R. The average d' of all these pairs of locations was then taken as the final d' score.

5.2.4 Results

Alerting effect. To assess if the presence versus absence of the cue influenced performance, we analysed RTs for targets and Accuracy as a function of cue-presence (present: +1, absent: -1), ACS type (mixed: +1, blocked: -1) and Group (deaf: +1, NH: -1). The cue-present condition included RTs from all trials with a cue - attended valid, attended invalid and ignored invalid.

RT: The responses to cue-present condition were faster compared to the no-cue condition, $\beta = -3.79$, $t = -2.44$, $p = 0.015$ (Figure 5.2). This difference was greater for the deaf group, as revealed by a significant interaction between cue-presence and group, β = -2.86, *t* = -2.16, *p* = 0.031. The alerting benefit for the deaf was also greater in the blocked ACS condition compared to the mixed ACS condition as indicated by a significant three-way interaction between ACS type, group and cue presence, β = 2.71, *t* = 2.04, *p* = 0.041 (See Table 5.1 for descriptive means for all cue conditions)

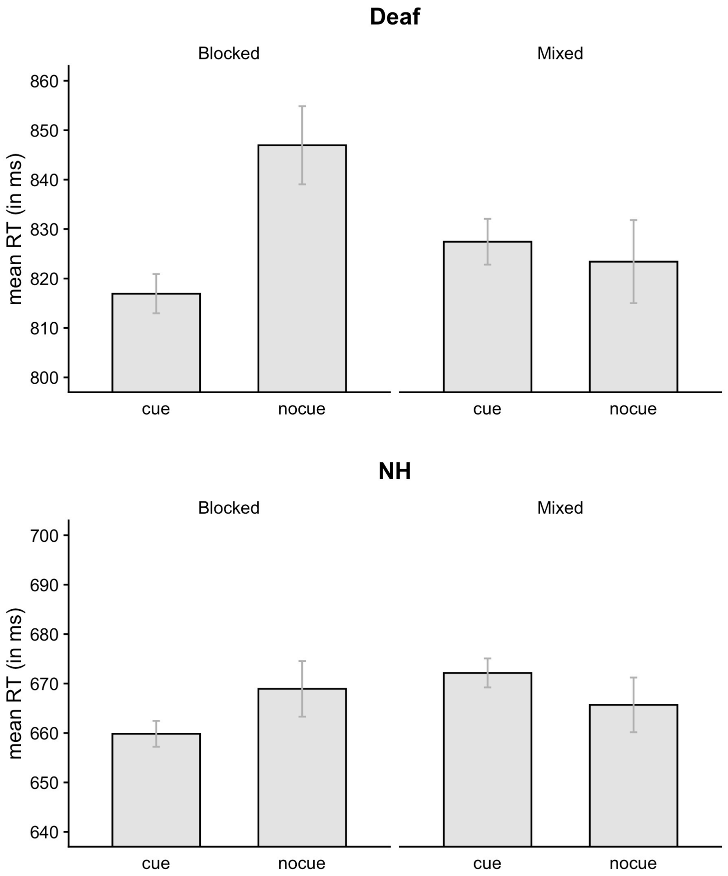

Figure 5.2. Alerting effect. RTs for cue present and cue absent conditions for both the groups. Greater alerting effect for the deaf compared to NH in the Blocked ACS condition. Note: Error bars indicate ± 1 SE. The y-axis range is different between the plots, but the scale (60 ms) is the same for each plot to facilitate comparison.

Accuracy: Accuracy on the cue present and cue absent trials were equivalent, β = 0.04, *z* = 1.44, *p* = 0.150. There was no significant interaction between Group and cue-presence, β = 0.001, *z* = 0.05, *p* = 0.963.

Orienting effect. **RT:** There was no significant difference between RT on attended

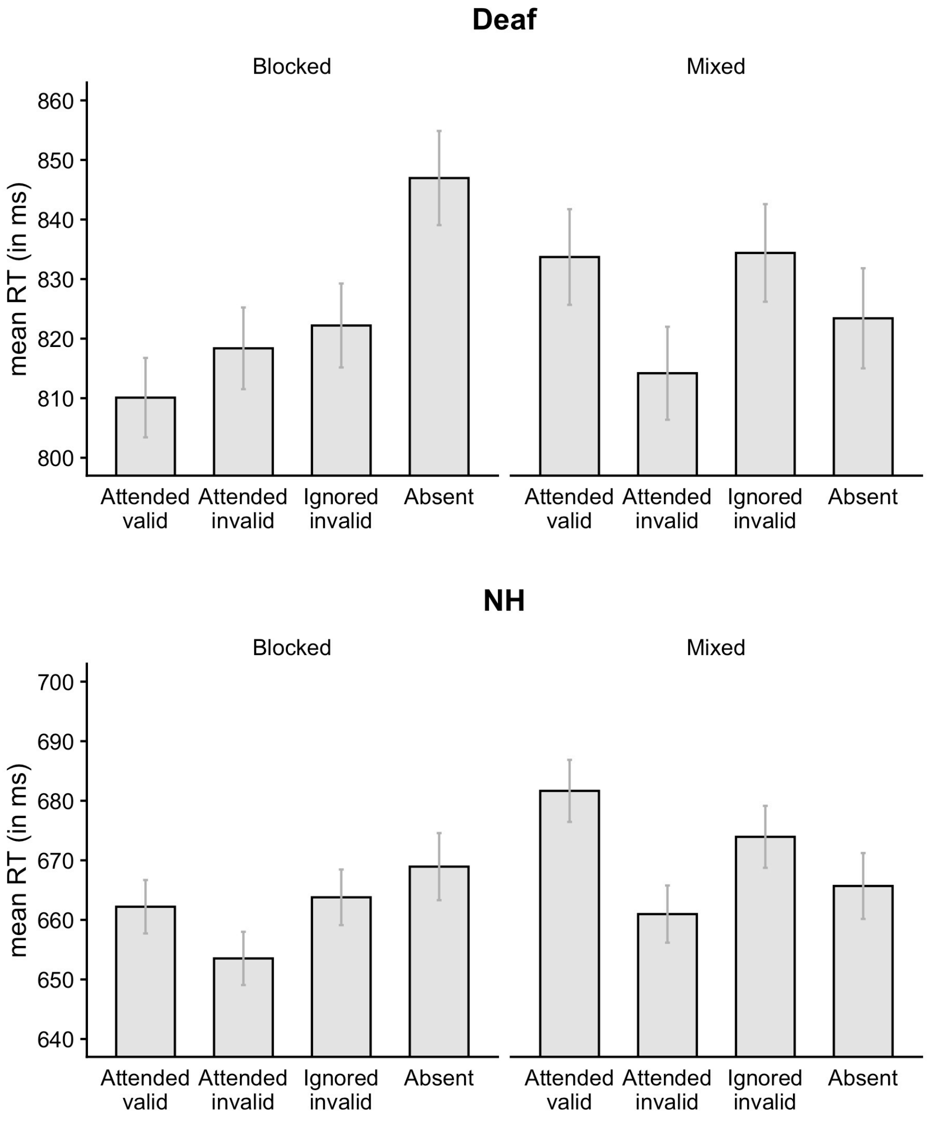

Figure 5.3. Orienting effect. RTs as a function of cue condition and ACS for both the groups. Faster responses were seen on attended invalid trials compared to attended valid trials, but only in the NH group. The negative cueing effects were also seen only in the mixed ACS condition. No differences between RTs on ignored and attended invalid trials were observed.
Note: Error bars indicate ± 1 SE. The y-axis range is different between the plots, but the scale (60 ms) is the same for each plot to facilitate comparison.

valid trials and attended invalid trials, β = -7.3, *t* = -1.14, *p* = 0.256. RT on ignored invalid trials was equivalent to the attended invalid trials, β = 1.63, *t* = 0.44, *p* = 0.66. There was a significant interaction between condition_IV_V and group, β = 11.18, *t* =

2.04, $p = 0.041$ indicating that RT on attended valid trials was greater than RT on attended invalid trials, but only in the NH group (Figure 5.3). No such differences were observed for the deaf group. RTs on attended valid trials were also greater compared to attended invalid trials but only in the mixed ACS condition, as revealed by a significant interaction between condition_V_IV and ACS type, $\beta = -12.84$, $t = -1.99$, $p = 0.046$. The deaf participants were overall slower than NH participants, $\beta = 72.52$, $t = 3$, $p = 0.004$. Participants were also overall faster in the blocked ACS condition compared to the mixed ACS condition, $\beta = 9.98$, $t = 3.8$, $p < 0.001$.

RT distributional analyses showed significant three-way interactions between Group, Bin and condition_V_IV (Bin3: $\beta = 18.44$, $t = 1.83$, $p = 0.067$; Bin4: $\beta = 33.92$, $t = 3.37$, $p = 0.001$; Bin5: $\beta = 33.4$, $t = 3.35$, $p = 0.001$, Figure 5.4). There were also significant interactions between Group, Bin and condition IR_IV (Bin4: $\beta = 18.59$, $t = 3.2$, $p = 0.001$; Bin5: $\beta = 20.34$, $t = 3.54$, $p < 0.001$). Separate analyses done on individual bins showed a main effect of condition_IV_V in Bins 1,2 and 3 (Bin1: $\beta = -21$, $t = -5.34$, $p < 0.001$; Bin2: $\beta = -23.38$, $t = -6.2$, $p < 0.001$; Bin3: $\beta = -11.64$, $t = -2.54$, $p = 0.011$). There were significant interactions with Group in Bins3, 4 and 5 (Bin3: $\beta = 10.77$, $t = 2.35$, $p = 0.019$; Bin4: $\beta = 25.41$, $t = 4.55$, $p < 0.001$; Bin5: $\beta = 26.72$, $t = 3.09$, $p = 0.002$) indicating that cue validity effects for attended cues were greater in the NH group compared to the Deaf group in the slower responses. The main effect of condition_IR_IV was also significant in Bins1, 2, 4 and 5 (Bin1: $\beta = -10.1$, $t = -4.46$, $p < 0.001$; Bin2: $\beta = -9.53$, $t = -4.35$, $p < 0.001$; Bin4: $\beta = 10.48$, $t = 3.25$, $p = 0.001$; Bin5: $\beta = 18.41$, $t = 3.69$, $p < 0.001$) indicating slower responses on ignored invalid trials compared to attended invalid trials. The interaction

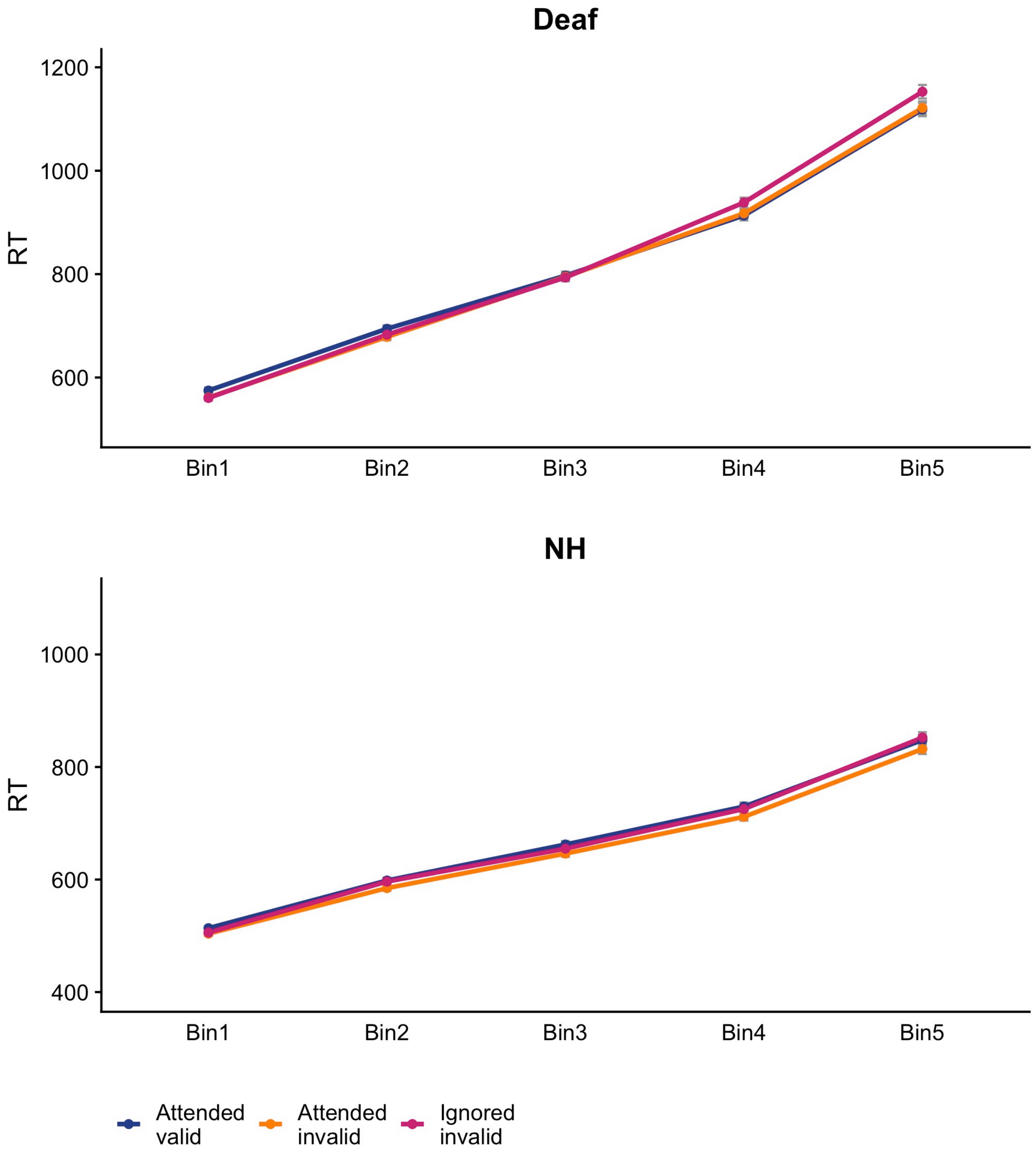

Figure 5.4. Condition-wise mean RTs for the 5 bins for each group. NH showed greater negative cueing effects (RT attended invalid < RT attended valid) in the slower responses (Bin 3, 4 and 5). deaf showed greater slowing down due to ignored cues (RT ignored invalid > RT attended invalid) in the slower responses (Bin 4 and 5)

Note: Error bars indicate ± SE. The y-axis range is different between the plots, but the scale (600 ms) is the same for each plot to facilitate comparison.

between Group and condition_IR_IV was significant in Bins 4 and 5 (Bin4: β = 12.76, $t = 3.96$, $p < 0.001$; Bin5: β = 15.13, $t = 3.04$, $p = 0.002$) indicating that the difference between ignored and attended invalid RTs was greater for the deaf group compared to NH group in the slower responses.

Accuracy: NH participants were more accurate in responding to the targets compared to the deaf participants, $\beta = -0.38$, $z = -2.62$, $p = 0.009$. Accuracy was also greater in the blocked ACS condition compared to the mixed ACS condition, $\beta = -0.25$, $z = -6.02$, $p < 0.001$. None of the other effects were significant.

Table 5.1. Descriptive mean RTs of all conditions for deaf and NH data.

	Blocked ACS				
	Alerting effect		Cue validity effect		
Cue conditions	Present	Absent	Attended valid	Attended invalid	Ignored invalid
deaf	817 (4)	847 (8)	810 (7)	818 (7)	822 (7)
NH	660 (3)	669 (6)	662 (4)	653 (4)	664 (5)
	Mixed ACS				
	Alerting effect		**Cue validity effect**		
Cue conditions	Present	Absent	Attended valid	Attended invalid	Ignored invalid
deaf	827 (5)	823 (8)	834 (8)	814 (8)	834 (8)
NH	672 (3)	666 (5)	682 (5)	661 (5)	674 (5)

Note: All values are given in ms. The numbers in brackets denote 1 SE. RT "present" is the average RT across attended valid, attended invalid and ignored invalid conditions.

Cue-localisation: The mean accuracy on the cue localisation task was significantly greater than chance level (20%) for normal-hearing participants ($M = 37.6$ %, $SD = 10.8$, $t(1, 29) = 8.91$, $p < 0.001$) and the deaf ($M = 21.7$ %, $SD = 3.6$, $t(1, 24) = 2.33$, *p* = 0.028). The cue visibility index (d') did not differ between the two groups, F (1, 53) = 1.12, *p* = 0.294. Individual t-test on each group showed that d' was significantly greater than zero for the deaf (*M* = 0.85, *SD* = 1.08, *t* (1, 24) = 3.96, *p* < 0.001) and marginally greater than zero for normal-hearing participants (*M* = 0.48, *SD* = 1.46, *t* (1, 29) = 1.81, *p* = 0.081).

5.3 Discussion

Using a modified spatial cueing paradigm, we investigated attention capture by cues at attended and unattended locations in the deaf. The NH participants showed greater negative cueing effects for the attended cues compared to the deaf. This difference was greater in the slower responses. Ignored invalid cues lead to greater slowing down than attended invalid cues in the slower responses suggesting that spatially-irrelevant cues were not filtered. This effect was greater in the deaf compared to the NH group.

We observed faster responses to attended invalid cues compared to attended valid cues in the mixed session which was a surprising finding. Such a negative effect could be related to inhibition of return which is usually observed in studies with peripheral cues at long SOAs between the cue and the target (Klein, 2000; but see Carmel & Lamy, 2014 for an alternative interpretation of negative cueing effects). Such inhibitory effects typically emerge 200 - 300 ms after the presentation of the cue. The duration between cue onset and ACS signal offset in our study was 116 ms which can be considered too short to induce IOR. However, the emergence of negative effects at short SOAs in a cue-target paradigm is not without precedence (eg., Danziger & Kingstone, 1999; Tassinari et al., 1994; Tipper et al., 1997). Klein

(2000) proposed that both facilitation and inhibition may be present simultaneously after the presentation of the cue and net facilitation or inhibition emerges depending on their relative strength. It has also been suggested that the emergence of negative cueing effects depends on the "orienting strategies" adopted by the participants for a given task (Lupianez et al., 2001). In line with this, IOR has been found to appear sooner when the participants are motivated to disengage attention from an exogenously cued location under endogenous orienting (Danziger & Kingstone, 1999). Similarly, we believe that strong endogenous orienting induced by the central ACS signal lead to faster disengagement from the cues resulting in IOR.

Negative cueing effects were also seen only in the mixed ACS condition compared to blocked ACS. The demanding context created by the mixed ACS may have led to a stronger deployment of endogenous control compared to the blocked ACS trials. The evidence for negative cueing effects as a result of stronger top-down control comes from Belopolsky et al. (2010). In Experiment 1 and 2 of their study, they used an instructional cue to indicate the upcoming target type (RED: colour singleton target; WHITE: abrupt-onset target) and found capture effects even when the cue did not match the target set (eg., abrupt-onset cue with a colour-singleton target). In Experiment 3 and 4, they created conditions for stronger deployment of top-down control. In Experiment 3, after the instructional cue (eg., RED), the other target type (abrupt-onset) was presented on some trials to which the participants were required to withhold their response. In Experiment 4, participants could choose the target feature they wanted on every trial. In both these experiments, negative cueing effects for irrelevant cues were observed. The authors argued that, while irrelevant cues lead to capture in Experiment 1 and 2, greater top-down control in Experiment 3 and 4 lead to greater inhibition of the cues resulting in negative effects. In sum, the mixed ACS condition may have led to greater deployment of endogenous control as a result of which higher inhibition was applied to the cues. It is to be noted that we observed inhibitory effects for the attended/relevant cues as opposed to Belopolsky et al. (2010) and a few others (eg., Lamy, Leber, & Egeth, 2004) who have observed negative effects only for the irrelevant cues. Since the spatial ACS was changing dynamically from trial-to-trial, it is possible that inhibition was applied to all cues to deal with the demanding context.

Greater negative cueing effects observed in the NH group compared to the deaf was an unexpected finding. As mentioned in the introduction, several studies have found that the deaf are better/faster at disengaging attention from cued locations. We observed the opposite effect. The NH participants were better at disengaging from cued locations compared to the deaf participants. This result is more in line with studies that have similarly not observed better disengagement in the deaf (eg., Jayaraman et al., 2016). There could be several explanations for this finding. The lack of cueing effects in the deaf could mean that the cues were not successful in orienting attention at all. But this seems unlikely as ignored cues were successful in capturing attention, as shown by slower responses compared to attended invalid trials. Thus, if we view cueing effects as a net effect of parallely running facilitation and inhibition, lack of cueing effects in the deaf could be due to facilitatory effects nullifying the inhibitory effects in the deaf. In the NH participants, it is possible that the inhibitory effects were stronger than the facilitator effects resulting in net cueing effects. Thus, these findings are more in line with studies that have shown increased facilitation in the deaf rather than increased disengagement (Prasad et al., 2015). However, we completely acknowledge that this is indirect evidence and there could be easily be alternate explanations. But, we do believe this is an interesting pattern in data which deserves to be examined further.

If our argument - that inhibitory effects and facilitatory effects nullified each other in the deaf in the mixed ACS session - is correct, then we should observe greater positive cueing effects in the deaf in the blocked ACS condition. But, we did not observe this which is in contrast to our prediction based on our previous study (Prasad et al., 2015) on a similar population. At the moment, we can only speculate on the reasons for this discrepancy. One possible reason could be that we used an abrupt-onset cue with a colour target as we closely followed the design of Ruthruff and Gaspelin (2018). Prasad et al. (2015) used an abrupt-onset cue with an abrupt-onset target. It is well known within the visual attention literature that cues that match the target template have greater effects than cues that don't match (Folk et al., 1992). We might have observed stronger cueing effects (both in deaf and NH) with colour-singleton cues. Thus, it is perhaps important to choose a cue type closely matching the target to maximise the orienting effects. It is worth noting that, descriptively, we observed positive cueing effects (8 ms) in the deaf but negative cueing effects in the NH group (-9 ms) in the blocked ACS condition. This suggests that facilitatory effects are perhaps stronger in the deaf compared to the NH group

The alerting effect was also significantly greater in the deaf compared to the NH group in the blocked ACS condition. This fits nicely with previous findings of enhanced reactivity in the deaf. Several studies have shown that deaf participants are faster responding on simple detection tasks which do not involve spatial orienting which has led some researchers to conclude that the locus of the visual processing advantage in the deaf lies in the early stages of visual processing (Bottari et al., 2010; 2011).

The deaf participants were also significantly slower in responding than their hearing counterparts. This is in contrast to some studies with Posner cueing paradigm which find faster responses in the deaf (Colmenero et al., 2004; Xingjuan et al., 2011). However, slower responses in the deaf have been observed before in both manual reaction times (eg., Sladen et al., 2005) and eye movement measures (eg., Heimlar et al., 2015). These studies argue that deaf are more slow and strategic in their responses. It is important to note that Sladen et al. used a flanker task and Heimlar et al. used a visual search task. The cueing studies cited before, which showed faster responses in the deaf, used a simple detection task. Ours was a feature-search task comparable in difficulty with those of Sladen et al. and Heimlar et al. Task-difficulty may play a role in the response speed of the deaf participants. Tasks that require more top-down control might elicit slow and strategic responses from the deaf.

Our secondary interest was also to examine the extent of endogenous control over attention capture in the deaf. We found that the cues presented at spatially-irrelevant locations captured attention indicated by slower responses compared to attended invalid condition. This effect was greater in the deaf in the slower responses indicating that they were less successful in ignoring the spatially-irrelevant cues.

Thus, the present study aligns with the account of greater stimulus-driven capture in the deaf. However, we note that these findings must be interpreted with caution. Previous studies showing reduced capture for ignored cue observed faster RTs in the ignored invalid condition compared to the attended invalid condition. Since the ignored cues did not *slow down* responses as much as the attended invalid cues, it was concluded that ignored cues captured less attention than the attended cues. However, all such previous studies on NH participants blocked the spatial ACS signals (eg., Ishigami et al., 2009; Ruthruff and Gaspelin, 2018). Ours is the first study with mixed spatial ACS trials. Since we observed negative cueing effect in the mixed ACS condition, it is not straightforward to determine what would qualify as evidence for reduced capture by ignored cues. Further studies with a range of SOAs in the mixed ACS condition are necessary to draw firm conclusions.

One of the limitations of the current study is that the cue absent trials lacked the alerting benefit involved in all the cue present trials. Participants were significantly faster on cue present trials compared to cue absent trials demonstrating a clear alerting benefit. Therefore, the cue present and cue absent conditions were not comparable. Comparing RT on ignored cues with RT on cue-absent trials would have lead us to falsely conclude that ignored cues were filtered out in the blocked session (because RT ignored < RT cue absent) whereas the ignored cues clearly slowed down the RTs compared to the attended invalid cues. In sum, we were not in a position to examine whether the ignored cues were completely *immune* from attention capture as seen in Ruthruff and Gaspelin (2018) who found comparable RTs on cue absent and ignored invalid trials using a similar design. Future experiments aiming to test for complete immunity should address this confound such that cue absent and present trials can be effectively compared.

Further, the results from the cue localisation task were intriguing because we presented the cues for a brief duration (16 ms) which is commonly the presentation duration in subliminal cueing studies (Mulckhuyse & Theeuwes, 2010; Van der Stigchel et al., 2009; Weichselbaum et al., 2014). and yet participants performed better than chance on the localisation task. Thus, we acknowledge that, strictly speaking, the cues can't be called "subliminal". One reason for this could be that participants were explicitly informed about what the cue looked like and when it would appear. As a result, participants could have deployed top-down attention to the cues in the visibility test. It is known that such top-down attention can boost visual awareness (Dehaene et al., 2006). Since this was the case only in the cue localisation and not in the main experiment (where the participants had no knowledge about the cues), it is possible that the d' obtained in the cue localisation task were overestimated. In line with this, some studies have found that the subliminal priming effects are independent of performance on a prime-visibility test (Francken, van Gaal, & de Lange, 2011). Further, it has also been seen that participants can subjectively report to have seen nothing, and yet perform better than chance on an objective visibility test (Koivisto & Neuvonen, 2020). One way to address this is to use a subjective measure of awareness such as the perceptual awareness scale which asks participants to report the quality of their experience in a graded manner (Ramsøy & Overgaard, 2004). Although, it is worth noting that it is still not clear if which method of assessing awareness - subjective or objective - is more precise (Rothkirch &

Hesselmann, 2017). To summarise, a lot more research is needed to conclusively establish lack of awareness. However, we acknowledge that the findings from our study can't be directly generalised to subliminal attention

Conclusion

In sum, we found that deaf are worse at disengaging attention from a cued location and also show greater capture by irrelevant cues compared to NH participants. These results are contradictory to what we predicted. These findings, along with the observation of greater alerting effect in the deaf support the view that changes in deaf visual processing are linked to stimulus-driven components of attention. These findings shed further light on the specificity of plasticity-induced changes in deaf visual attention.

6. Individual differences in masked visual processing II: Hearing impairment enhances masked free-choice priming

Compensatory changes as a result of auditory deprivation in the deaf lead to higher visual processing skills. In two experiments, we explored if such brain plasticity in the deaf modulates processing of masked stimuli in the visual modality. Deaf and normal-hearing participants responded to targets either voluntarily or by instruction. Masked primes related to the response were presented briefly before the targets at the center and the periphery. In Experiment 1, targets appeared only at the foveal region whereas, in Experiment 2, they appeared both at the fovea and the periphery. The deaf showed higher sensitivity to masked primes in both the experiments. They chose the primed response more often and also were faster during congruent responses compared to the normal hearing. These results suggest that neuroplasticity in the deaf modulates how they perceive and use information with reduced visibility for action selection and execution.

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6.1 Introduction

Much of what we do not see and report nevertheless influences our actions and thought. Many studies in the domain of visual perception have shown that subliminally presented primes can influence action selection and control (Dehaene et al., 1998; Dehaene & Naccache, 2001; Neumann & Klotz, 1994; Kiefer, 2012; Kunde, Kiesel, & Hoffmann, 2003). Such unconscious states give rise to overt actions later (Hommel, 2017). Although unconscious priming as an effect and as a paradigm remains debated, studies tightly controlled for prime visibility show a substantial influence of the primes on later behaviour (e.g. Eimer & Schlaghecken, 2003). It's not clear if variables related to individual differences in biological and cognitive profiles should influence the processing of hardly-visible stimuli. One such variable is neural reorganisation as a result of sensory deprivation as it happens in the deaf or the blind. Here, we examine if the deaf show higher sensitivity to masked priming. We exploit the deaf participants since they have been found to perceive and react faster to visual stimuli (e.g. Bottari, Caclin, Giard, & Pavani, 2011; Parasnis & Samar, 1985). Additionally, we also explored if the priming effects vary for the foveal and peripheral regions, as the deaf have been shown to be particular faster in discrimination at the periphery (e.g. Lore & Song, 1991).

Cortical reorganisation following a lack of input to the auditory cortex has been observed in the Deaf (Finney, Fine, & Dobkins, 2001; Seymour et al., 2016). Mainly, structural and functional enhancement in the dorsal processing stream has been reported in the deaf (see Bavelier, Dye, & Hauser, 2006 for a review). As a result, the deaf have been shown to be more sensitive to information in the visual periphery and also display higher spatial attention particularly at the visual periphery (Bavelier et al., 2006; Bosworth & Dobkins, 2002; Dye, Hauser, & Bavelier, 2008; Lore & Song, 1991; Neville & Lawson, 1987; Pavani & Bottari, 2012; Prasad, Patil, & Mishra, 2015).Although many have found that the Deaf are faster with visible stimuli compared to the normal hearing, we hypothesize that they should be highly sensitive to stimuli in the environment that have reduced visibility.

The source of the predicted enhanced priming effects in the deaf may lie in the selective manner in which the dorsal stream functions (Bavelier et al., 2000; Neville & Lawson, 1987; Stevens & Neville, 2016, see also Dye & Bavelier, 2013; Pavani & Bottari, 2012 for reviews) The dorsal route hypothesis, as it is called, makes explicit predictions about superior spatial attentional abilities in the deaf. Visual spatial attention is redistributed more widely in the deaf extending to the periphery (Bosworth, Petrich, & Dobkins, 2013; Hauthal, Sandmann, Debener, & Thorne, 2013; Proksch & Bavelier, 2002; Seymour et al., 2016). For instance, Proksch and Bavelier (2002) observed that the Deaf processed peripheral distractors more while performing a search task at the center of the visual field, compared to normal hearing individuals. In consonance with the dorsal route hypothesis, ERP data (N1 component) showed preferential processing of 'motion' stimulus (dorsal) compared to 'color' stimulus (ventral) in the deaf (Armstrong, Neville, Hillyard & Mitchell,, 2002). But does such a reorganisation of the ‘dorsal’ stream lead to higher sensitivity to information that is hardly visible in the deaf?

Most researchers so far have focused on the peripheral visual advantage in the deaf to perceivable visible stimuli. It is clear that the deaf show higher sensitivity to stimuli presented at the visual periphery. Although, it is not certain if this advantage is attentional or perceptual given the different paradigms and tasks researchers have used. Given this background, we explored if the deaf show higher sensitivity to masked primes which in turn would influence their action related choices? Additionally, we sought to examine if such an effect is particularly higher at the visual periphery for the deaf. It has been suggested that deaf perform better with these visual tasks if the attentional demands are high. We used masked primes to modulate free- and forced-choice visuomotor actions in the deaf and the normal hearing participants (Figure 6.1). In Experiment 1, the primes were presented either at the periphery or center whereas targets appeared only at the center. In Experiment 2, the target location always matched the prime location. This was done to test whether the peripheral advantage in Deaf would still be seen when the attentional demands are low (Experiment 2).

We also manipulated the mask-target SOA based on a previous study examining central-peripheral masked priming effects (Schlaghecken & Eimer, 2000). In this study, masked primes were presented briefly followed by targets. Left or right pointing arrows were used as primes/targets and several of them were superimposed on each other to create the mask. Participants were asked to respond to the direction of the arrow. Crucially, the mask-target SOA was varied from 0 ms to 192 ms. A positive compatibility effect (PCE, faster responses on compatible trials) at shorter SOAs and negative compatibility effect (NCE, faster responses on incompatible trials) at longer SOAs is commonly observed when mask-target SOA is varied. The NCE is said to represent a self-inhibition mechanism which causes the suppression of the initial activation of the prime. Accordingly, in this study, PCE was observed at shorter SOAs (< 96 ms) which turned into NCE at longer SOAs (> 96 ms) as expected but only for central primes. For peripheral primes, PCE was observed even at longer SOAs. The explanation for this so-called central-peripheral asymmetry (CPA) was that the activation-followed-by-inhibition account is valid only when primes are presented at the fovea. At other locations, the perceptual strength of the primes is not strong enough to cross the activation threshold and result in inhibition. However, it is known that Deaf have high perceptual sensitivity even at the periphery. Thus, such an asymmetry between central and peripheral priming effects should be observed only in normal-hearing participants but not in the Deaf. In sum, we expected normal-hearing participants to display CPA at long SOA similar to previous studies (Schlaghecken & Eimer, 2000). However, we expected no such asymmetry in the Deaf. We expected these effects on both free- and forced-choice trials as NCE has been observed in both types of trials (Kiesel et al., 2006).

6.2 Experiment 1

The main aim of Experiment 1 was to test the hypothesis that the Deaf should show enhanced masked priming effects. Deaf and normal-hearing participants completed a masked priming task in which they were asked to make responses to targets ("forced-choice" trials) or voluntarily choose a response between two alternatives ("free-choice" trials). We expected the Deaf to show higher priming effects on both free- and forced- trials. That is, we expected the proportion of congruent choices (on free-choice trials) and RT priming effect (RT incongruent – RT congruent, on free- and forced-choice trials) to be higher in the Deaf. A prime discrimination test was administered after the main experiment to test for the visibility of the primes. Based on prior evidence (Bavelier et al., 2000, 2001; Finney & Dobkins, 2001), we did not expect to see any differences between Deaf and normal-hearing in the prime visibility index (*d* prime).

6.2.1 Method

Participants. Twenty-eight hearing-impaired and twenty-seven normal-hearing participants initially took part in the main priming study. Prime visibility data from three hearing impaired individuals and one normal-hearing participant was not available (due to technical problems, failure of the participant in understanding instructions, etc.). Thus, data from twenty-five hearing impaired individuals (4 female, Mean age =24.4 years, SD = 4.3) and twenty-six normal-hearing individuals (10 female, Mean age = 24.7 years, SD = 4.8) were finally considered for the study. The deaf participants were from Deaf Enabled Education Centre. All deaf participants were congenitally deaf, born to hearing parents and suffered from profound sensorineural hearing loss. They received education in special schools for the deaf in which the primary medium of communication was Indian Sign Language. They reported having acquired sign language at an average age of 9.4 years and reported high proficiency in sign language use (Mean self-rating score: 3; see Prasad et al., 2015 for details of this scale). Participants with normal hearing were all students at the University of Hyderabad. All participants reported normal or corrected-to-normal vision and gave informed consent for their participation in the study. Instructions to the hearing impaired participants were given by one of the co-authors who is a speech therapist with good proficiency in sign language. The study protocol was approved by the Institutional Ethics Committee (IEC) at University of Hyderabad. The methods were in accordance with the guidelines and regulations of IEC. None of the participants had been diagnosed with any psychiatric/neurological condition.

6.2.2 Procedure

Stimuli were designed and presented using the SR research experiment builder (SR Research, Ontario, Canada) on an LCD monitor with resolution 1024 * 768 pixels and refresh rate of 60 Hz. Participants were seated at a distance of 60 cm from the monitor. The stimuli were presented in black against a white background. Every trial started with a fixation cross at the centre of the screen for 1000 ms (See Figure 6.1A). Following the fixation cross, a prime was presented for 33 ms. Next, a line mask ("######", Times new roman, pt. 26) was presented for 50 ms followed by a blank screen of variable duration (0 ms/150 ms). The primes constituted of the digits "1" or "2" (Times new roman, pt. 26) and were located either at the centre or the periphery (21-degree eccentricity to the left/right of the fixation). There were two types of trials, "Free-choice" and "Forced-choice" which were presented randomly. On the forced-choice trials, the blank screen was followed by the presentation of the target ("1" or "2") at the centre of the screen. Participants were asked to press "A" for "1" and "L" on the keyboard for "2". On free-choice trials, participants were presented with "0" at the centre after the blank screen and were asked to freely choose their response between "A" and "L". Participants were asked to "choose freely and spontaneously" without following a particular pattern. Participants were given a maximum of 3000 ms to respond to the target on forced-choice trials or to choose a response on free-choice trials. The mapping between the target and the

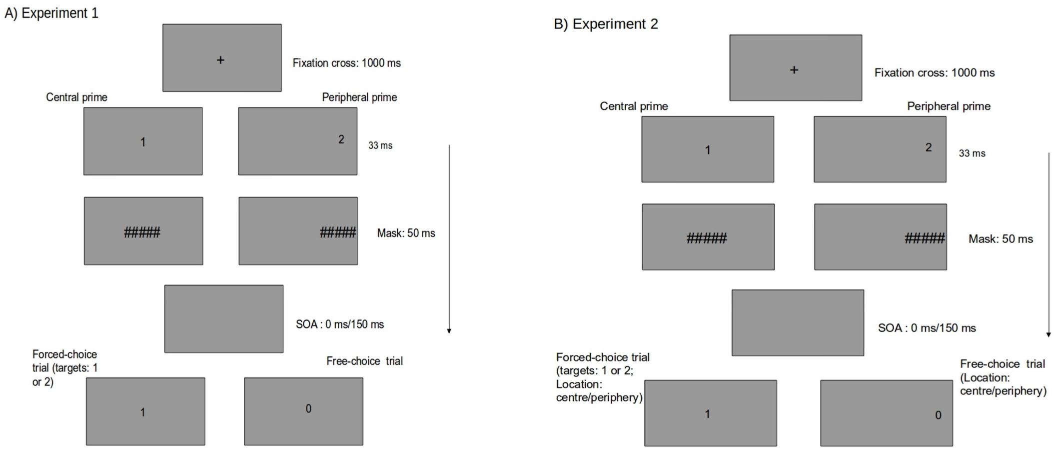

Figure 6.1. Experimental design. (A) Experiment 1. Primes (1 or 2) are presented at centre or periphery followed by a mask (#####). Participants then respond to the target cue (forced-choice trials) or choose between two alternative responses (free-choice trials). The target numbers (1, 2 or 0) are always presented at the centre. B) Experiment 2. The design is similar to Experiment 1. The only difference is that targets also presented at periphery, always matching the prime location.

response keys on the keyboard was counterbalanced across participants.

After the main experiment, participants performed a prime visibility task. Primes (1 or 2) were presented for 33 ms followed by a mask for 50 ms. After a variable duration (0, 150 ms), participants were presented with a screen displaying “1 or 2?” for 5000 ms. Participants were asked to press the key corresponding to the number they detected (that is, 1 or 2 on the keyboard).

Design. There was a total of 400 trials divided into two blocks of 200 trials each. Each block consisted of 120 free-choice trials and 80 forced-choice trials. The 120 free-choice trials in each block were divided into 60 trials with central primes and 60 with peripheral primes. Each of these 60 trials was further divided into 30 trials with 0 ms SOA and 30 trials with 150 ms SOA. The 80 forced-choice trials in each block were also divided equally between the eight types of trials: prime location (2) * SOA

(2) * congruency (2) with ten trials in each condition. A forced-choice trial was defined as "congruent" when the prime matched with the target (e.g. prime – 1, target - 1) and as "incongruent" when there was a mismatch (e.g. prime – 2, target - 1). On free-choice trials, if the participant chose the response indicated by the prime, it was designated as a "congruent" trial (e.g. when participants pressed "A" when the prime was 1) and vice versa. The trials in each block were presented in a random sequence. A break of two minutes was given after one block. Each participant was given twenty practice trials before the start of the experiment.

6.2.3 Data Analysis

Free- and forced-choice trials were analysed separately (See Table 6.1 for descriptive statistics). Statistical analysis was performed using SPSS (IBM Corp.). F and p values along with partial eta squares are mentioned for all the relevant main effects and interactions. Pairwise comparisons on significant interactions were performed using Least significant differences posthoc tests.

Forced-choice trials: Trials with no responses were discarded (0.7 %). Response times on forced-choice trials above and below 2SD (Standard deviation) of the Mean RT of each participant were also discarded (4.4 % in the Deaf and 4.8 % in the normal-hearing). Further, only correct trials were considered for the main analysis. The percentage of errors was 7.8 % for the Deaf and 5.1 % for the normal-hearing. Error analysis was performed separately by computing *d'* values. Correct responses to the target "1" were defined as hits and incorrect responses to "2" were defined as false alarms. Hits and false alarm rates of 0 or 1 were corrected using the log-linear rule (Hautus, 1995). Please note that the mean values for different conditions are mentioned in percentage for the ease of comprehension.

Free-choice trials: In line with previous studies on free-choice priming, stringent data trimming procedure was not applied to free-choice trials (e.g. Mattler & Palmer, 2012; Schlaghecken & Eimer, 2004). Instead, only responses faster than 100 ms (Deaf: 0.03 % and Normal-hearing: 0.08 %) and trials with no responses (0.8 %) were discarded. We computed *d′* values for each participant for each condition. A response was considered as a hit if the participants chose "A" when the prime was "1" and as a false alarm if the participants chose "A" when the prime was "2" (The calculation was adjusted accordingly when the mapping was opposite). ANOVA was performed on the *d′* values. Individual t-tests comparing with chance level performance (*d′* = 0) were also performed wherever appropriate. Means are, however, reported in percentage.

Table 6.1. Descriptive Statistics – Experiment 1

			Central primes				**Peripheral primes**			
			0		**150**		**0**		**150**	
			C	**IC**	**C**	**IC**	**C**	**IC**	**C**	**IC**
Free-choice	**Choices**	**Deaf**	57	43	62	39	55	45	57	43
			(15)	(15)	(20)	(20)	(16)	(16)	(15)	(15)
		NH	53	47	52	48	49	51	51	49
			(9)	(9)	(9)	(9)	(8)	(8)	(11)	(11)
	RT	**Deaf**	689	761	625	757	717	778	654	681
			(168)	(295)	(137)	(295)	(254)	(305)	(148)	(263)

		NH	646 (198)	659 (187)	602 (205)	630 (227)	638 (208)	649 (210)	631 (200)	625 (191)
Forced-choice	**Error rate**	**Deaf**	0.6 (1)	1.2 (2)	0.6 (1)	1.9 (3)	0.9 (1)	0.9 (2)	0.7 (1)	1.1 (0.2)
		NH	0.5 (1)	0.8 (2)	0.5 (1)	1 (2)	0.6 (1)	0.3 (1)	0.6 (1)	10.8 (0.1)
	RT	**Deaf**	648 (143)	704 (182)	594 (155)	681 (181)	691 (202)	703 (200)	673 (148)	694 (182)
		NH	637 (168)	656 (155)	565 (136)	590 (122)	629 (159)	626 (164)	597 (148)	585 (126)

Note: Mean values with SD in brackets. C: Congruent, IC: Incongruent, NH: Normal-hearing, “0” and “150” refer to mask-target SOA in ms, Choices and error rates are given in percentage (%). RT is given in ms.

6.2.4 Results

Free-choice trials. Congruent choices were more in number (54.6 %) than incongruent choices (45.4 %) , t (1, 50) = 3.24, p = 0.002. ANOVA was then performed on d' values of proportion data with congruency (congruent, incongruent), prime location (central, peripheral), SOA (0, 150) as within-subjects factors and group (Deaf, normal-hearing) as a between-subjects factor. There was a significant main effect of group, , F (1, 49) = 5.78, p = 0.02, η_p^2 = 0.10. indicating that Deaf (57.9 %) made higher proportion of congruent choices than normal-hearing (51.3 %, Figure 6.2A). Individual t tests revealed that d' differed significantly from zero for the Deaf (d' = 0.32), t (1, 25) = 3.1, p = 0.005 but not for normal-hearing participants (d' = 0.05), t (1, 24) = 1.44, p = 0.16. There was also a main effect of prime location, F (1, 49) = 5.65, p = 0.02, η_p^2 = 0.10 indicating that the participants made higher proportion of congruent choices for central primes (56%) compared to peripheral primes (53.2 %). The main effect of SOA [$F(1, 49) = 1.11$, $p = 0.3$, $\eta_p^2 = 0.02$] and all of the interactions were nonsignificant (Fs < 1)

Primes had an influence on response times on free-choice trials, as indicated by a main effect of congruency on RT on free-choice trials, $F(1, 49) = 8.25$, $p = 0.006$, $\eta_p^2 = 0.14$. Congruent trials (650.18 ms ms) had faster responses than incongruent trials (692.54 ms). To further examine the effect of other variables on priming effect, ANOVA was performed on "RT priming effect" which was calculated as the RT difference between incongruent and congruent trials. Prime location, SOA and Group were considered as factors. There was a significant effect of group, $F(1, 49) = 4.34$, $p = 0.04$, $\eta_p^2 = 0.08$ reflecting a larger effect of the primes for the Deaf (73.07 ms) compared to the normal-hearing participants (11.64 ms, Figure 6.2B). Priming effect for central primes (61.35 ms) and peripheral primes (23.36 ms) differed, as indicated by a main effect of prime location, $F(1, 49) = 4.44$, $p = 0.04$, $\eta^2 = 0.08$. The interaction between prime location and group was not significant, $F(1, 49) = 1.12$, $p = 0.28$, $\eta_p^2 = 0.02$. There was no main effect of SOA ($F < 1$). None of the interactions were significant (Fs < 3)

Forced-choice trials. Repeated measures ANOVA was first performed on RT with congruency (congruent, incongruent), prime location (central, peripheral) and SOA (0 ms, 150 ms) as within-subjects factors and Group (Deaf, normal-hearing) as between-subjects factors. There was a main effect of congruency on RT, $F(1, 49) = 14.26$, $p < 0.001$, $\eta_p^2 = 0.22$ indicating that participants were faster responding on congruent trials (629.16 ms) compared to incongruent trials (654.82 ms). Further analysis of RT

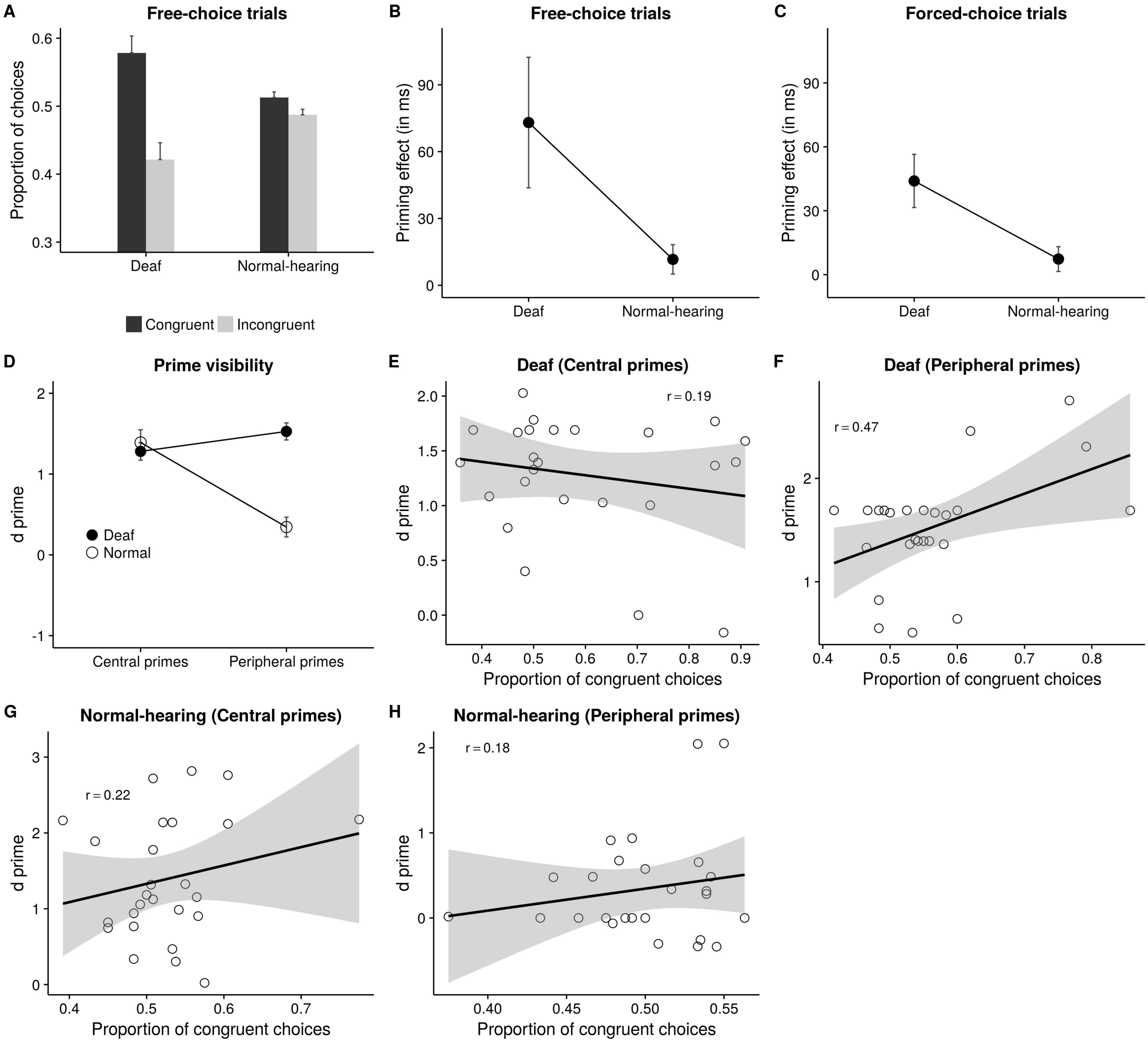

Figure 6.2. Experiment 1 results. (A) Deaf made more congruent choices on free-choice trials compared to normal-hearing. The RT priming effect (RT incongruent – RT congruent) is larger for the Deaf on both free-choice (B) and forced-choice (C) trials. (D) Deaf are better at discriminating primes, only at periphery. (E) – (H) Plots of correlation between proportion of congruent choices at the two prime locations for the two groups. The effect was significant (p = 0.02) only for the Deaf at periphery (F).

priming effect (incongruent RT – congruent RT) revealed higher priming effect for the Deaf (44 ms) compared to normal-hearing (7.32 ms) participants, $F(1, 49) = 7.28$, $p = 0.01$, $\eta_p^2 = 0.13$, as shown in Figure 6.2C. A main effect of prime location, $F(1, 49) = 20.26$, $p < 0.001$, $\eta_p^2 = 0.29$ showed larger priming effects for primes for central primes (46.65 ms) compared to peripheral primes (4.67 ms). The interaction between prime location and group was not significant, F (1, 49) = 1.95, $p = 0.17$. Neither the main effect of SOA [F(1, 49) = 1.26, $p = 0.27$, $\eta_p^2 = 0.02$] nor its interactions with group [F(1, 49) = 1.89, $p = 1.7$, $\eta_p^2 = 0.04$] and location [F(1, 49) = 1.05, $p = 0.31$, $\eta_p^2 = 0.02$] was significant. The interaction between prime location, SOA and Group, F (1, 49) = 0.01, $p = 0.91$, $\eta_p^2 < 0.001$ was not significant either.

Percentage of errors on forced-choice trials (there was no concept of error on free-choice trials) was also analysed with prime location, congruency, SOA and Group as factors. The ANOVA revealed a significant effect of congruency, F(1, 49) = 4.12, $p = 0.048$, $\eta_p^2 = 0.08$ indicating that participants made more errors on incongruent trials (1 %) than congruent trials (0.6 %). The Deaf did not differ with respect to normal-hearing, F(1, 49) = 0.41, $p = 0.52$, $\eta_p^2 = 0.01$. There was a significant interaction between prime location and congruency, F(1, 49) = 10.03, $p = 0.003$, $\eta_p^2 = 0.17$ indicating higher percentage of errors ($p = 0.003$) on incongruent trials (1.2 %) than congruent trials (0.5 %), but only for central primes. A significant interaction between SOA and congruency was also observed, F (1, 49) = 62.31, $p < 0.001$, $\eta_p^2 = 0.56$.

Prime visibility. Signal detection measure d' was calculated to assess prime visibility. The prime “1” was considered as Signal and “2” as Noise. Consequently, correct responses to the prime “1” qualified as Hits and incorrect responses to “2” qualified as False alarms (FA). Prime visibility was above chance level for each group: $d' = 1.4$, t (1, 25) = 7.16, $p < 0.001$ for the Deaf and $d' = 0.87$, t (1, 25) = 8.26, $p < 0.001$ for the normal-hearing participants.

To examine whether the two groups differed on the ability to discriminate between the two primes, repeated measures ANOVA was performed on *d′* with prime location as a within-subjects factor and Group as a between-subjects factor. There was a main effect of group, $F(1, 49) = 15.43$, $p < 0.001$, $\eta_p{}^2 = 0.24$ indicating that the Deaf individuals ($d' = 1.4$) could discriminate the primes better than the normal-hearing participants ($d' = 0.87$). Central primes ($d' = 1.34$) were discriminated better compared to peripheral primes ($d' = 0.94$), as shown by a main effect of prime location, $F(1, 49) = 12.4$, $p = 0.001$, $\eta_p{}^2 = 0.2$. The interaction between prime location and group was significant, $F(1, 49) = 32.45$, $p < 0.001$, $\eta_p{}^2 = 0.4$ (Figure 6.2D). Post-hoc analysis showed that Deaf participants were able to discriminate the primes better than normal-hearing only when they were presented at periphery (Deaf: 1.53 vs. normal-hearing: 0.34), but not at center (Deaf: 1.28 vs. normal-hearing: 1.39).

Correlational analysis. To examine if visibility of the primes lead to the priming effects seen in the main task, linear regression analysis was performed with proportion of congruent choices as the dependent variable and *d′* as the predictor variable separately for each prime location and each group. The analysis revealed a significant correlation between *d′* and proportion of congruent choices for the Deaf group but only for primes presented at periphery, $r = 0.47$, $p = 0.02$ and not for those presented at center, $r = 0.19$, $p = 0.36$ (Figure 6.2E and 6.2F). No significant correlations between *d′* and proportion of congruent choices were observed for the normal-hearing group either at center, $r = 0.22$, $p = 0.29$ or at periphery, $r = 0.18$, $p = 0.38$ (Figure 6.2G and 6.2H). Linear regression analysis was also performed with

RT priming effect (in free- and forced-choice trials) as the dependent variable. Similar results were found for RT on free-choice trials (Appendix B).

6.2.5 Discussion

Deaf participants showed a greater influence of the primes on choices and response times in both free- and forced-choice trials. Participants also made fewer errors on congruent trials, although no group difference was found in the percentage of errors. No effect of SOA was observed on any of the variables. Based on earlier evidence of central-peripheral asymmetry we expected the PCE to turn into NCE at long SOA (150 ms) only for central primes but not for peripheral primes in normal-hearing participants. Additionally, due to the peripheral advantages in the Deaf, we expected PCE to turn into NCE for both central and peripheral primes at long SOA. The failure to observe these could be due to several reasons. One could argue that the dissimilarity of our masks and the targets could be one factor since NCE is best observed when masks consist of task-relevant features (Schlaghecken et al., 2007). Further, according to the activation followed by inhibition account, self-inhibition is triggered at longer SOAs since there is no longer any perceptual information triggered by the primes. Thus, NCE is commonly observed when the primes are completely invisible (Eimer & Schlaghecken, 2003; Mattler, 2006). Although this point is debatable, a negative correlation between prime visibility and size of NCE has been observed suggesting that the magnitude of self-inhibition goes up as visibility is reduced (see Sumner, 2008 for a review). Thus, the lack of NCE in this experiment could also be attributed to the above chance level discrimination of the primes. Finally, an important difference between our study and previous studies that have observed central-peripheral asymmetry is the location of peripheral primes. While most such studies presented peripheral primes at less than 6 degrees away from the fixation, peripheral primes in our study were presented at 21 degrees (in line with previous studies on Deaf that have observed a peripheral advantage). This could be one of the reasons why PCE did not turn into NCE at longer SOAs for peripheral primes even for hearing impaired participants in spite of their heightened perceptual sensitivity.

Nonetheless. these data provide evidence for higher masked priming in the deaf. The visibility index also correlated significantly with priming effects for the peripheral primes in the Deaf. Thus, our results also suggest enhanced perceptual advantages for the Deaf at the periphery to masked stimuli. In the next experiment, we examined if the effect we observed had any attentional component.

6.3 Experiment 2

In Experiment 1, we observed enhanced priming effects in the Deaf for both central and peripheral primes compared to the normal hearing participants. Studies have shown that varying prime/target location induces attentional demands on the participants (Schlaghecken & Eimer, 2000; Wang et al., 2016). Existing evidence also suggests that peripheral visual processing advantage is found in the Deaf when selective attention is engaged either by a central task or if the peripheral stimuli are unpredictable (see Bavelier et al., 2000 for a review). Accordingly, we observed higher priming effects at the periphery (also, at center) for the Deaf in Experiment 1 where selective attention was engaged at the fovea as a possible target location. In

Experiment 2, we examined if the peripheral advantage in Deaf would disappear if the peripheral attentional engagement were less demanding. We tested this by also presenting the targets at the periphery (apart from the center). Thus, both the Deaf and normal-hearing participants' attention was focussed to peripheral and central locations as possible target locations. Here, we expected the Deaf to show higher priming effects only at the center but not at the periphery. The SOA manipulation was maintained for the sake of consistency between Experiment 1 and 2 so that any differences in the effects in Experiment 2 could only be attributed to the manipulation of attentional engagement and not due to the differences in the mask-target SOA between the two experiments.

6.3.1 Method

Participants. Thirty hearing-impaired and thirty normal hearing individuals took part in the main priming study out of which prime visibility data from five hearing-impaired and two normal-hearing participants was not available. Hence, data from twenty-five hearing-impaired individuals (23 male, Mean age = 25.2 years, SD = 3.6) and twenty-eight normal-hearing individuals (22 male, Mean age = 24.9 years, SD = 3.9) was eventually considered for all analysis. The characteristics of the Deaf participants were similar to those who participated in Experiment 1. The average age of acquisition of sign language was 7.4 years and mean self-rating score on sign language proficiency was 2.3. None of the participants who took part in Experiment 1 were recruited for this Experiment. The experimental protocols were approved by the Institutional Ethics Committee (IEC) at University of Hyderabad. The methods were carried out in accordance with the regulations of IEC. All participants gave informed consent for their participation in the experiment.

6.3.2 Procedure

The procedure was the same as in Experiment 1 except as noted below. In Experiment 1, primes were presented either at the centre or the periphery whereas targets were always presented in the centre. In this Experiment, the prime and the target location always matched (Figure 6.1B). Thus, peripheral primes were followed by targets at the periphery and central primes were followed by targets at the centre. The trial structure and the design of the experiment remained same.

The sequence of events in the prime visibility task was similar to the main Experiment. Similar to Experiment 1 prime visiblity test, participants were asked to judge if the prime was “1”or “2” and respond through the keyboard within 5000 ms.

6.3.3 Data Analysis

Similar data trimming and analysis procedure were used as in Experiment 1.

Forced choice trials. Trials with no responses (0.9 %) and trials with RT above or below 2 SD of the Mean RT of each participant (Deaf: 4.6 % and normal-hearing: 4.2 %) were discarded. Error trials were excluded from analysis (Deaf: 15.7 % and normal-hearing: 4.1 %). The overall range of the error percentage across the hearing-impaired participants was 0 – 48 %. We found that the percentage of errors for the Deaf participants was fairly high because four of the participants gave inaccurate responses on more than 40% of the trials. The mean error percentage excluding these four participants was 6.4 %. However, we decided to retain the data from these participants as the analysis after discarding them did not give different results. Errors were analysed by calculating *d'* similar to Experiment 1.

Free choice trials. 0.3 % of the trials with no responses and 0.15 % of trials with RT > 100 ms were discarded from analysis. The analysis procedure for the proportion of choices and RT was same as experiment 1.

6.3.4 Results

Free-choice trials. Participants chose responses congruent with the prime (52.8 %) more often than incongruent responses (47.2 %), t (1, 52) = 4.26, $p < 0.001$ Repeated measures ANOVA was performed on *d'* with prime location, SOA and congruency as within-subjects factors and group as a between-subjects factor (see Table 6.2 for descriptive statistics). There was a marginally significant effect of Group, $F(1, 51) = 4.04$, $p = 0.05$, $\eta_p^2 = 0.07$ indicating higher proportion of congruent choices in the Deaf (54.2 %) compared to normal-hearing participants (51.5 %, Figure 6.3A). Individual t-tests showed that *d'* differed significantly from zero for both Deaf (0.16) and normal-hearing participants (0.06), t (1, 24) = 4.64, $p = 0.001$and t (1, 27) = 2.45, $p = 0.02$, respectively. Proportion of congruent choices was also higher for central primes (54.7 %) than peripheral primes (51 %), as suggested by a main effect of prime location, $F(1, 51) = 5.46$, $p = 0.02$, $\eta_p^2 = 0.09$. There was a significant interaction between prime location and Group, $F(1, 51) = 5.26$, $p = 0.026$, $\eta_p^2 = 0.09$. Pairwise comparisons showed that the Deaf and normal-hearing group differed only at the centre (Deaf: 57.5 % vs normal-hearing: 50.8 %, $p = 0.016$) but not at periphery (Deaf: 51.8 % vs normal-hearing: 51.3 %, $p = 0.73$). The effect of SOA and its interaction with Group were both non significant, $F(1, 51) = 1.58$, $p = 0.21$, $\eta_p^2 =$

0.03 and $F(1, 51) = 3.2$, $p = 0.08$, $\eta_p^2 = 0.06$ respectively. None of the other effects were significant (Fs < 1)

Table 6.2. Descriptive Statistics - Experiment 2

			Central primes				**Peripheral primes**			
			0		**150**		**0**		**150**	
			C	**IC**	**C**	**IC**	**C**	**IC**	**C**	**IC**
Free-choice	**Choices**	**Deaf**	54	46	61	39	49	51	53	47
			(9)	(9)	(13)	(13)	(17)	(17)	(18)	(18)
		NH	51	49	52	48	52	48	51	49
			(6)	(6)	(8)	(8)	(11)	(11)	(12)	(12)
	RT	**Deaf**	604	642	494	560	701	646	529	548
			(143)	(158)	(98)	(132)	(173)	(144)	(122)	(111)
		NH	610	621	610	624	631	639	605	607
			(127)	(132)	(145)	(134)	(137)	(111)	(155)	(148)
Forced choice	**Error rate**	**Deaf**	1.3	1.8	1.8	2.8	2	2	1.9	2.2
			(1.4)	(2.5)	(1.7)	(3.5)	(2.6)	(1.8)	(2.2)	(2.5)
		NH	0.4	0.5	0.4	0.6	0.6	0.4	0.5	0.7
			(0.6)	(0.5)	(0.5)	(1.1)	(0.8)	(0.4)	(0.6)	(0.8)
	RT	**Deaf**	773	799	684	691	819	818	713	726
			(180)	(201)	(188)	(168)	(191)	(197)	(208)	(196)
		NH	600	610	560	588	611	611	555	581
			(98)	(103)	(97)	(93)	(91)	(80)	(99)	(89)

Note: Mean values with SD in brackets. C: Congruent, IC: Incongruent, NH: Normal-hearing, “0” and “150” refer to mask-target SOA in ms, Choices and error rates are given in percentage (%). RT is given in ms.

The ANOVA on response times in free-choice trials revealed a marginally significant effect of congruency, $F(1, 51) = 2.89$, $p = 0.09$, $\eta_p^2 = 0.05$. Congruent trials (597.9 ms) elicited faster responses than incongruent trials (610.78 ms). ANOVA was then performed on RT priming effect (incongruent RT – congruent RT) which revealed no effect of group on priming effect, $F(1, 51) = 0.11$, $p = 0.74$, $\eta_p^2 = 0.002$. Prime location had a significant effect, $F(1, 51) = 15.94$, $p < 0.001$, $\eta_p^2 = 0.24$. The RT priming effect for central primes (31.55 ms) was higher compared to peripheral primes (-9.25 ms). There was a significant interaction between prime location and group, $F(1, 51) = 10.65$, $p = 0.002$, $\eta_p^2 = 0.17$ (Figure 6.3B). Pairwise comparisons showed that the two groups differed in priming effect only when primes were presented at the center (Deaf: 50.79 ms vs normal-hearing: 12.31 ms, $p = 0.04$) but not at periphery (Deaf: -23.35 ms vs normal-hearing: 4.85 ms, $p = 0.13$). There was a significant effect of SOA, $F(1, 51) = 5.08$, $p = 0.03$, $\eta^2 = 0.09$ indicating higher priming effects for 150 ms SOA (27.48 ms) compared to 0 ms SOA (-5.18 ms). The interaction between SOA and group was significant, $F(1, 51) = 5.45$, $p = 0.02$, $\eta_p^2 = 0.1$. Pairwise comparisons showed that priming effect was higher (marginally significant, $p = 0.05$) for the Deaf (46.96 ms) compared to normal-hearing (7.99 ms) participants at 150 ms SOA but not at 0 ms SOA ($p = 0.2$) None of the other effects were significant (Fs < 3).

Forced-choice trials. Repeated measures ANOVA was performed on RT with prime location (central, peripheral), SOA (0 ms, 150 ms) and congruency (congruent, incongruent) as within-subjects factors. Group (Deaf, normal-hearing) was considered as a between-subjects factor. Responses on congruent trials (664.61 ms) were faster compared to incongruent trials (678.24 ms), as shown by a marginally significant effect of congruency, $F(1, 51) = 3.87$, $p = 0.05$, $\eta_p^2 = 0.07$. To examine

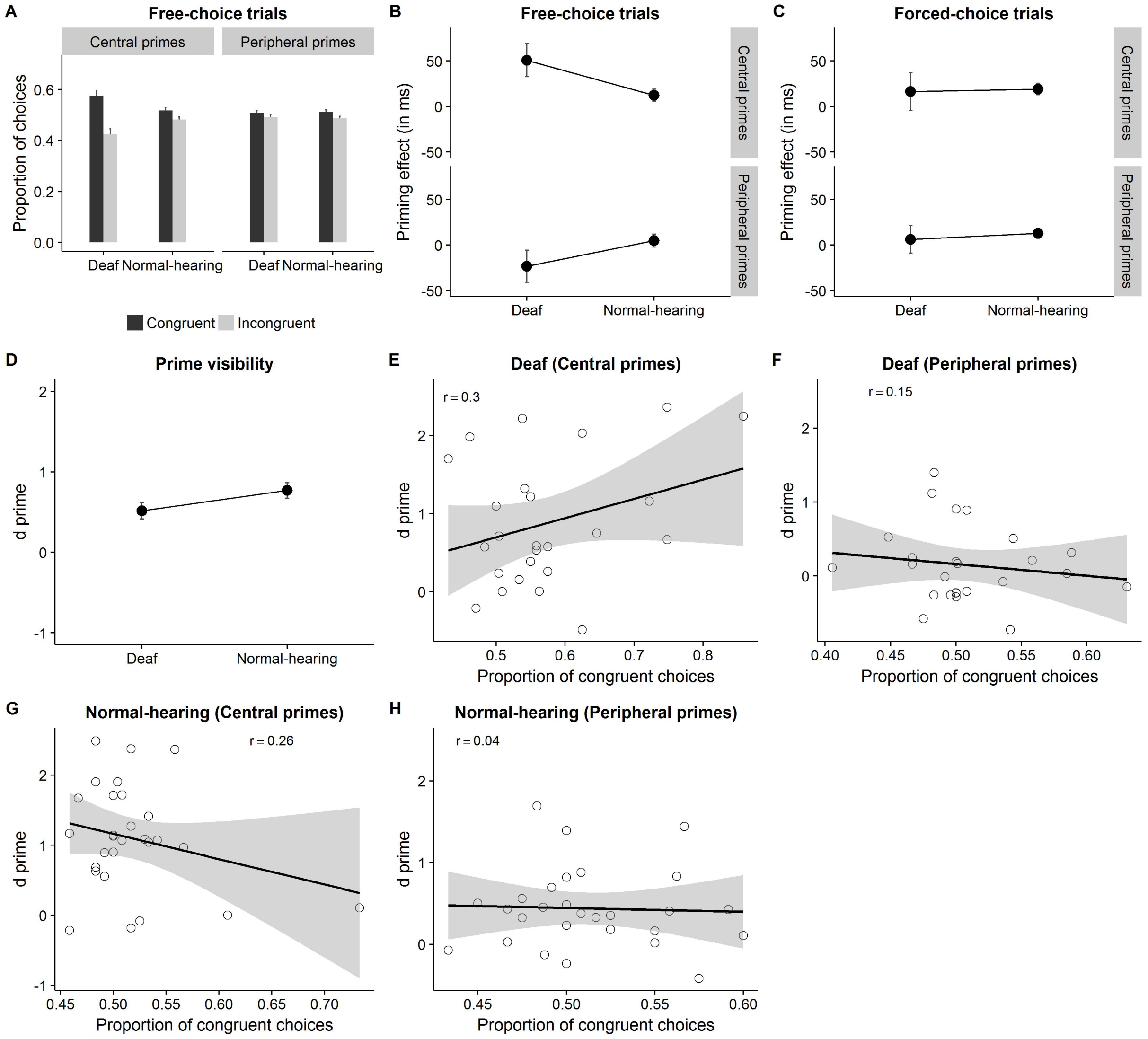

Figure 6.3. Experiment 2 results. (A) Proportion of congruent choices is higher for Deaf compared to normal-hearing, only for central primes. (B) Group difference on priming effect on free-choice trials, only for central primes (C) No group difference in priming effect on forced-choice trials (D) Marginally significant group difference in prime visibility index (E) – (F) Correlation plots between proportion of congruent choices and d prime. None of the correlations were significant.

whether this priming effect was modulated by the other factors, repeated measures ANOVA was performed on RT priming effect (RT on incongruent trials – RT on congruent trials) with prime location, SOA and group as factors. There was no group difference in the RT priming effect, $F(1, 51) = 0.11$, $p = 0.74$, $\eta_p^2 = 0.002$ (Deaf: 11.35 ms vs normal-hearing: 15.92 ms, Figure 6.3C). Prime location had no significant effect, $F(1, 51) = 4.82$, $p = 0.49$, $\eta_p^2 = 0.009$. The interaction between prime location and Group, $F(1, 51) = 0.03$, $p = 0.87$, $\eta_p^2 = 0.001$ was not significant either. The main effect of SOA was not significant (F < 1). None of the other interactions were statistically significant (Fs < 2).

ANOVA on d' values for error data revealed a significant effect of group, $F(1, 51) = 14.01$, $p < 0.001$, $\eta_p^2 = 0.21$. Deaf participants made larger number errors (2 %) than normal-hearing participants (0.5 %). Participants were in general more error-prone on incongruent trials (1.4 %) than congruent trials (1.1 %), $F(1, 51) = 9.43$, $p = 0.003$, $\eta_p^2 = 0.16$. The interaction between congruency and group was not significant, $F(1, 51) = 1.11$, $p = 0.3$, $\eta_p^2 = 0.02$. The interaction of congruency with prime location was not significant either, $F(1, 51) = 2.45$, $p = 0.12$, $\eta_p^2 = 0.05$. There was a significant effect of SOA, $F(1, 51) = 8.16$, $p = 0.006$, $\eta_p^2 = 0.14$ indicating higher percentage of errors for trials with 150 ms SOA (1.3 %) compared to 0 ms SOA (1.1 %).

Prime visibility. *d'* was calculated using a procedure similar to that of Experiment 1. Prime discrimination was above chance level for each group, $d' = 0.52$, $t\ (1, 24) = 5.16$, $p < 0.001$ for the Deaf, $d' = 0.77$, $t\ (1, 27) = 8.76$, $p < 0.001$ for the normal-hearing participants. ANOVA was performed on d' with prime location and group as factors. The analysis revealed a marginally significant effect of Group, $F(1, 51) = 3.32$, $p = 0.07$, $\eta_p^2 = 0.06$ (Figure 6.3D). Normal-hearing participants were better at discriminating primes ($d' = 0.52$) compared to the Deaf participants ($d' = 0.77$). Central primes ($d' = 0.99$) were discriminated better than peripheral primes ($d' = 0.29$), $F(1, 51) = 35.36$, $p < 0.001$, $\eta_p^2 = 0.41$. Interestingly, there was also a main effect of SOA, $F(1, 51) = 13.43$, $p = 0.001$, $\eta_p^2 = 0.21$. Primes presented with 150 ms

SOA were discriminated better (*d′* = 0.84) compared to those with 0 ms SOA (*d′* = 0.44). None of the other effects were statistically significant.

Correlational analysis. Linear regression analysis was performed with proportion of congruent choices as dependent variable and *d′* as the predictor separately for each group at both centre and periphery. No significant correlation was observed between the two variables for the Deaf group when the primes were presented at centre, $r = 0.31$, $p = 0.14$ as well as at periphery, $r = 0.15$, $p = 0.48$ (Figure 6.3E and 6.3F). Similarly, for the normal-hearing participants, no significant correlations between prime visibility and proportion of congruent choices were observed for central primes, $r = 0.26$, $p = 0.19$ and peripheral primes, $r = 0.04$, $p = 0.84$ (Figure 6.3G and 6.3H). Linear regression analysis with RT priming effect on free- and forced-choice trials as the dependent variable is reported in Appendix B. No significant effects were observed.

6.3.5 Discussion

In this experiment, we observed that the two groups did not differ on the degree of priming for peripheral primes when attentional demand was higher. Deaf showed higher priming effects on response times and proportion of choices on free trials but only for the central primes. No group differences were observed on forced-choice priming effects. Similar to the results of Experiment 1, both groups could discriminate the primes at higher than chance level. Normal-hearing participants were marginally better at discriminating primes compared to the Deaf. However, no significant correlations were observed between priming effect and prime discrimination index for either of the groups at either of the locations. The results suggest that peripheral advantage in the deaf emerges only under certain conditions, mostly when their attention is spread over a large area.

6.4 General discussion

Our results provide strong evidence of enhanced masked priming in the deaf. We have shown that the peripheral advantage is seen when the attentional demands are higher (Experiment 1). These results extend previous findings and show that even processing of masked stimuli is modified as a result of neuroplasticity. For the first time, we have shown that sensory deprivation in the deaf not only facilitates processing of perceivable visual events but also visual cues that are hardly visible. Importantly, such cues could influence choices of voluntary actions and facilitate actions themselves. The results thus extend earlier findings that have shown superior peripheral processing in the deaf (e.g. Lore & Song, 1991; Proksch & Bavelier, 2002). They also extend results obtained with the normal-hearing population that have shown the influence of subliminal or masked stimuli on conscious decision making in visuomotor actions (Bottari et al., 2010; Kiesel et al., 2006).

Enhanced processing of peripheral primes in the Deaf was observed only in Experiment 1 since participants' selective attention was engaged at centre due to the repeated appearance of the target. This difference disappeared in Experiment 2 where the targets could appear both at the periphery and at centre. This made the task attentionally less demanding at the periphery. This is in line with previous studies where no peripheral differences between Deaf and normal-hearing have been observed when the tasks were not attentionally demanding (Bosworth & Dobkins, 2002; also see Bottari et al., 2010). Our results suggest that sensitivity to masked primes is modulated by the redistribution of attentional resources between the centre and the periphery (Dye, Hauser, & Bavelier, 2009; Proksch & Bavelier, 2002) Further, in both the experiments, we observed an enhanced processing of central primes in the Deaf. Enhanced processing of foveal stimuli has not been consistently found in previous studies (Bottari et al., 2010; Dye, Hauser, & Bavelier, 2009; Hauthal, Neumann, & Schweinberger, 2012). Thus, whether plasticity-related changes in Deaf also result in higher processing of stimuli presented at fovea is still an open question (Dye, 2016).

We expected the time course of priming effects to be influenced differently in Deaf and normal-hearing participants. The mask-target SOA had no significant influence on the prime's influence in both the experiments. Further, we did not observe the central-peripheral asymmetry in normal-hearing individuals seen in previous studies (e.g. Schlaghecken & Eimer, 2000). The possible reasons for this discrepancy have been discussed. However, it 's hard to draw firm conclusions since we only manipulated two levels of SOA. Lingnau and Vorberg (2005) examined the time course of response priming effects by systematically varying mask-target SOA, stimulus size and eccentricity. They observed that several factors could influence the onset of response inhibition and that NCE might be missed by measuring priming effects at only one or two SOAs. Thus, future work aiming to examine central/ peripheral priming effects in deaf and normal-hearing participants should systematically vary both stimulus eccentricity and mask-target SOA thereby shedding more light on the time course of these effects.

A possible limitation of our study is that the prime visibility was not at chance level (but see Pohl et al., 2014 for similar results). However, the prime discrimination measure is an overestimation of the actual visibility of the primes in the main experiment since the participants are explicitly made aware of the primes in the prime discrimination test and are asked to focus on detecting the primes. Further, we did not observe any correlations between prime discriminability index and the priming effects in masked priming tasks, except at periphery in Experiment 1. Budnik et al. (2013) had participants perform a masked priming task with Gabor patches as primes presented at both centre and periphery. The main task consisted of responding to the orientation of a visible Gabor patch. A clear difference between central and peripheral priming effects was observed even though prime visibility was controlled for (by equating the discrimination performance at centre and periphery using a staircase procedure) suggesting that separate mechanisms are likely responsible for the unconscious activations triggered by the primes and the visibility of the primes. Thus, our results support many previous studies which show that the influence of the masked primes on subsequent behaviour is independent of the level of awareness of the primes. (e.g. Francken, van Gaal, & de Lange, 2011; Mattler, 2013).We acknowledge that we can’t be confident that these effects will still be observed with stimuli that are entirely invisible and unconscious. Recently many have recommended newer methods to ensure and evaluate invisibility of the primes (Lin & Murray, 2014; Shanks, 2016). Future work examining the role of auditory deprivation on unconscious processing should consider these newer methods and employ stronger procedures to ensure the invisibility of the primes.

In sum, we have shown that sensory deprivation in the deaf can enhance the attentional and visual systems to the extent that these systems become highly sensitive to even information that is hardly visible. Such information not only influenced the choice of free actions but also the actions themselves. Additionally, these effects in the deaf are modulated by the redistribution of the attentional resources between the fovea and periphery. However, from these results, one cannot say if the prime is influencing the action itself or mental states related to the action. These questions need to be further examined from the point of view of brain plasticity and individual differences.

7. Conclusion

This thesis attempted to answer the following question: can explicit control settings modulate unconscious processing - both at the level of response priming and the level of attention capture. The answer is yes. We find that several factors - such as reward-learning, spatial attentional control settings and individual differences in attention - all can either facilitate or suppress masked visual processing. These effects were seen in both eye movements and response time measures. This adds to the existing literature which suggests that equating unconscious processing with automaticity is not accurate. What kind of information influences our responses depends on the overall goals or task-intentions.

Chapter 3 showed that spatial attentional control settings can reduce capture by masked cues in a spatial orienting paradigm. Cues at spatially-irrelevant locations caused lesser interference than cues at relevant locations suggesting that participants could ignore the irrelevant cues. These effects were seen across several eye movement measures. Chapter 5 examined these issues in the deaf population and found that the deaf are more susceptible to capture by irrelevant cues. There was no evidence to suggest that the deaf could successfully ignore the spatially-irrelevant cues.

It is important to note that even though we found evidence for reduced attention capture by cues at irrelevant locations in the studies described in Chapter 3 and 5, the baseline condition used in these studies was not appropriate. In both these studies, the RTs on the cue-absent condition was considered the baseline with which RTs on other conditions were compared. The assumption was that equivalent RTs on cue-absent and ignored conditions would suggest immunity from attention capture at ignored locations. However, as shown in Chapter 4, cue absent trials are not the most appropriate baseline to determine if the irrelevant cues have been ignored or if they have captured attention. Thus, we acknowledge that our studies in Chapter 3 and 5 did not have the correct baseline to warrant conclusions on whether the subliminal cues at irrelevant locations were completely ignored.

The role of ACS on masked free-choice response priming was similarly manipulated within and between participants. In Chapter 2, reward-learning was used to modulate ACS. To begin with, we observed robust free-choice priming effects in both the studies confirming the existence of these effects with a slightly novel paradigm. We used numbers 1 and 2 in these studies as primes/forced-choice targets which were never used earlier. Our findings show that masked priming can indeed bias free-choice responses and influence the response times as well. Further, reward-learning modulated these priming effects confirming that masked priming was susceptible to this "third-form" of attentional control as well. In Chapter 6, the deaf showed greater free- and forced-choice priming effects compared to normal-hearing individuals. This provides robust evidence of neuroplasticity-induced individual differences in masked priming effects.

7.1 Comparing conscious and unconscious cueing

It is also worth noting that in the studies described in Chapter 5, negative cueing effects were seen for the NH group. This was unexpected because, based on the study of Ruthruff and Gaspelin (2018), we expected robust positive cueing effects for NH. Both our study and Ruthruff and Gaspelin (2018) used white abrupt onset cues with colour targets. The only difference between the two studies was the type and duration of the cue. In Ruthruff and Gaspelin (2018), the cues were four white dots surrounding the placeholder and were presented for 100 ms. In our study, the cues were white flashes presented for 16 ms. Thus, it is likely that the unconscious nature of the cues was responsible for the negative cueing effects. But, why?

One possible reason could be the differences in conscious and unconscious cueing effects. Negative cueing effects are typically seen at cue-target SOAs > 300 ms. But in Chapter 5, negative cueing effects were seen at cue-target SOA of 116 ms which was surprising. Similarly, in the constrained free-saccade study discussed in Chapter 3, negative cueing effects were seen at 100 ms cue-target SOA. Most other studies that have examined attention capture by masked or subliminal cues have used very short SOAs (~ 16 - 50 ms) and observed positive cueing effects. One previous study that can shed light on the findings from this thesis is Ivanoff and Klein (2001). In this study, masked cues were presented for 15 ms followed by a go or a no-go target. The task was to press a button when the go target was presented. There were two cue-targets SOAs: 105 and 1005 ms. Crucially, there were two experimental conditions: cue report and no report. In the cue report condition, in addition to the events described above, participants were asked to report the presence and the location of the masked cue at the end of every trial. No such report regarding the cue was elicited in the no-report condition. The same set of participants were administered both the condition (with no-report condition administered first). Significant facilitation at short SOA and non-significant facilitation at long SOA was seen in the cue-report condition. Interestingly, non-significant facilitation at short

SOA and a significant inhibition at long SOA was seen in the no-report condition. The authors concluded that the cue-report condition led to the establishment of an attentional control setting (ACS) incentivising engagement with the cue resulting in significant cue-related facilitation at short SOA. The ACS also weakened the disengagement process leading to non-significant facilitation at long SOA. However, when the ACS was absent, the facilitation by the cue at short SOA disappeared and only the disengagement at long SOA remained resulting in a significant IOR.

This could explain the discrepancy between the unconscious cueing effect in NH group data described in Chapter 5 and the conscious cueing effects of R&G. Since we did not have a trial-by-trial measurement of cue awareness and there was no other incentive to engage with the cue there might not have been much facilitation associated with the cue. However, this suggests that we should have seen no differences between cue valid and invalid trials - or non-significant cueing effects. Instead, in both the studies (Chapter 3 and Chapter 5), we observed significant negative cueing effects at short SOA. This can not be explained by the absence of the cue-related facilitation alone.

To explain this, we have to go by alternate theories of IOR that model facilitation and inhibition as simultaneously occurring processes (Danzinger & Kingstone, 1999; Klein, 2000). This is in contrast to the traditional perspective which assumes that cueing effects follow a biphasic pattern where facilitation is temporally followed by inhibition. Rather, recent findings show that the net cueing effects are a result of the competition between facilitatory and inhibitory processes. Typically, facilitation is greater at shorter SOAs resulting in overall positive cueing effects.

Similarly, inhibitory processes are stronger at longer SOAs resulting in IOR. But, in some cases, it is possible that this pattern is not followed. For instance, in our studies being discussed here, the facilitation at short SOA could have been very weak because the cues were not exactly part of the attentional template. The task was to search for a colour target and report its identity. The contingent capture hypothesis argues that attention capture is seen only by cues that are part of the attentional template induced by the task goals. Since the cues in our study were not relevant to the attentional template (to search for the colour target), there was much less incentive to engage with the cue. In the absence of any facilitation, it is possible that the disengagement processes dominated the responses resulting in net negative cueing effects. Thus, unconscious cues seem to have a different set of constraints that determine when and what type of cueing effects are seen. The time-course, as seen here in comparison with Ruthruff and Gaspelin (2018), could be different depending on the strength of facilitation and inhibition triggered by the cues.

Testing this would require comparing attention capture by abrupt-onset cues for a colour target with abrupt-onset cues for abrupt-onset targets using both conscious and unconscious cues. If my speculation is correct, then we should observe robust positive cueing effects at short SOAs when the unconscious cue matches the target template, that is, for abrupt-onset cues with abrupt-onset targets. If this is found, it would be mean that contingent capture is in some ways stronger for unconscious cues. Abrupt-onset cues can capture attention even while looking for colour targets - when the cues are conscious but not when they are unconscious. Since unconscious effects are weak and short-lived, it is possible that strong effects are seen only when the cues match the current task goals. Of course, this is just speculation at this point and needs further empirical testing. But, these findings underscore the necessity of comparing contingent-capture effects in conscious vs. unconscious cueing to establish the breadth of the phenomena.

7.2 The role of blocked vs. mixed attentional control settings

Another less-studied, but potentially relevant factor in studies on attentional control is the blocking or mixing of the attentional template. The question is, does the attentional template have to constantly appear in a blocked manner for goal-driven control mechanisms to be triggered? In the classic study of Folk et al. (1992), the task set or the attentional template did not change between the trials. The participants were asked to look for an abrupt-onset target in one block and for a colour target in another block (Experiment 3). Thus, participants had to maintain only one template in a given set of trials and not dynamically shift between the two. Would the same effects be seen if the task-set changed from trial to trial?

Belopolsky et al. (2010) tested this in a study where an instructional cue at the beginning of the trial indicated the task for that trial - WHITE (respond to the white abrupt-onset target) or RED (respond to the red target). This was the only difference compared to the Folk et al. (1992) study. While Folk et al. (1992) blocked the presentation of the target-type, Belopolsky et al. (2010) had a mixed presentation. The hypothesis was that if it is truly possible to exert top-down control over salient information, then knowing the type of the upcoming target should set in place the control settings required for that task and lead to selective processing of relevant features. But, this was not found. Both types of cues captured attention, irrespective of the type of target presented on a trial. This led the authors to argue that the classic contingent capture effects observed in Folk et al. (1992) and other similar later studies were due to the blocked design of these studies. They proposed that inter-trial priming and not goal-driven control accounted for these effects. The argument was that, when the same type of target is presented across several trials, it leads to bottom-up priming of features related to this target. Thus, cues that are perceptually similar to the target are processed more. Since this influence is a low-level, perceptual priming effect, the observed effects can not be attributed to top-down control.

In another study examining the same issue, Lien, Ruthruff, and Johnston (2010, Experiment 3) similarly asked participants to look for a coloured target. The target colour randomly changed from trial to trial (red or blue or green). On each trial, target-coloured, distractor-coloured or neutral-coloured singleton cues were presented before the target display. Crucially, at the beginning of each trial, a prompt was used to inform the participants about the colour of the upcoming target. Thus, on a trial where the participants had to look for a red target, a white-coloured “R” was presented at the beginning of the trial. The authors hypothesised that if goal-driven control can be maintained even under fluctuating ACS, then stronger capture should be seen for target-coloured cues compared to distractor-coloured cues. This is exactly what was found. The effect was replicated across multiple experiments leading the authors to conclude that rapidly changing ACS can nevertheless modulate attention capture such that relevant cues capture more attention than irrelevant cues.

The studies presented in this thesis suggest the view that rapidly changing ACS should modulate attention capture just as blocking would. In Chapter 3, for instance, the relevant visual field (up vs down) was signalled by a central coloured circle. The up and down signals were randomly presented together. We observed faster saccade latencies on the irrelevant cue trials compared to the attended invalid trials (at short SOA) suggesting reduced capture by irrelevant cues. This provides evidence that mixed ACS was effective in regulating attention capture. However, since we did not compare blocked vs. mixed ACS conditions, we are not in a position to conclude if the participants could have ignored the irrelevant cues more effectively in the blocked design compared to the mixed ACS condition.

Chapter 5 contains studies with both blocked and mixed ACS for comparison. Here, there was no significant effect of the ACS condition on cueing effects. Same effects were seen in both blocked and mixed ACS condition for the NH group. But, it is important to note that the studies discussed in this thesis used spatial ACS where the block vs. mixed comparisons described earlier (Belopolsky et al., 2010, Lien et al., 2010) used feature-based ACS. To our knowledge, there has been no study so far that has compared blocked vs. mixed ACS in the spatial domain. It is possible that it is easier to switch between two different spatial control settings (horizontal vs vertical) or between different properties with the same feature (red vs. blue colour, Lien et al., 2010), but much harder to switch between completely different feature-based settings (onset vs. colour) as in Belopolsky et al. (2010).

Why is this issue even important? It is because it has direct implications to the attention capture debate. Based on evidence from studies such as Belopolsky et al. (2010), several researchers have argued that there is nothing “top-down” about contingent-capture. If inter-trial priming can explain the effects found in contingent-capture studies, then it has the potential to seriously threaten goal-driven accounts of attentional selection. To counter these, it is necessary to show that contingent capture is seen in both blocked and mixed ACS conditions in some circumstances and provide suitable justification when differential effects are observed. Thus, it is important for future research examining spatial or feature-based contingent capture to pay more attention to this and establish the boundary conditions for this classic finding.

7.3 Motor priming vs. spatial orienting

The effects of attentional control were seen across two different paradigms: the response priming paradigm and the spatial orienting paradigm. One striking aspect of the result was that in studies with the spatial orienting paradigm, negative cueing effects were seen at relatively short SOA (100 ms, Chapter 3 and 5). Such negative effects at short SOAs are typically seen in the response priming paradigm (Kiesel et al., 2007). In the spatial orienting paradigm, negative cueing effects are typically seen at SOAs longer than 300 ms (although some studies have observed them at shorter SOAs, Danziger & Kingstone, 1999). It is possible to question if observing these negative effects in the cueing paradigm might indicate that they reflect motor priming and not attentional orienting. This - and not the differences between unconscious and conscious cueing as explained in section 7.2 - could be the reason for the unexpected IOR-like effects seen in Chapter 3 and 5. This is especially possible in Chapter 3 which involved an eye movement task. Thus, can we be confident that these effects were a result of attentional orienting and not oculomotor priming?

Schoeberl and Ansorge (2017) tested if subliminal cues influence eye movements primarily via attentional or oculomotor planning. They presented peripheral subliminal cues before a pro- and anti-saccade task. The authors expecting robust cueing effects for both types of task. This is because the cues were expected to orient attention to the location opposite to the cue since the participants knew that the "real" target was at the location opposite to the presented target. This knowledge would trigger orienting to the right location on valid trials but the opposite location on invalid trials resulting in robust cueing effects, even in the anti-saccade task. But, if the cues only triggered eye movements without necessarily affecting the orienting mechanisms, then the cue would trigger eye movements to the presented target - which would be the "wrong" location on valid trials but the right location on invalid trials. As a result, there should be minimal cueing effects on the anti-saccade task if the subliminal cues only trigger oculomotor plans. This is exactly what the authors found. Based on these findings, they concluded that subliminal cues primarily modulate attentional orienting and have a minimal effect on eye movement planning.

If this finding is to be relied upon, then we can argue that even our findings from Chapter 3 reflect attentional orienting and not oculomotor priming. However, it is worth noting that the distinction between oculomotor motor priming and attentional selection is relevant only if one believes in a dissociation between the two. Many researchers have argued that attention and eye movement systems have some interconnections, but are certainly independent in several aspects (Hunt, Reuther, Hilchey, & Klein, 2019). Some other theories, most notably, the pre-motor theory of attention proposes that attentional selection is always accompanied by an oculomotor plan which may or may not be executed (Rizzolatti et al., 1987). This debate regarding the relationship between attention and eye movements is complex and remains unresolved.

7.4 Masked visual processing in the deaf

Two chapters in this thesis were devoted to examining individual differences in masked visual processing. This is a relatively under-studied topic in unconscious processing research. While it is important to find out general principles of the mechanisms involved in unconscious processing, findings related to individual differences can help establish boundary conditions of the phenomena. With this aim, we compared congenitally deaf and normal-hearing individuals on both attentional orienting (Chapter 5) and response priming (Chapter 6) tasks.

In Chapter 6, we observed robust effects of hearing-status. The deaf group showed greater free- and forced-choice priming effects compared to the NH group. The effect was seen in both proportion of choices and RTs. This adds to the few studies that exist in the literature on individual differences in masked visual priming. It is not straightforward to conclude what mechanisms were responsible for the differences between deaf and NH observed in our study. We hypothesised that increased attentional abilities in the deaf would lead to enhanced priming effects. But, it is important to point out that we did not explicitly manipulate attention and the masked priming task is strictly not an "attention task". While masked priming crucially depends on available attentional resources (Kiefer, 2012), there are several other levels of processing where the deaf could have differed from NH participants. One way to establish that the effects seen in Chapter 6 were indeed due to attentional differences would have been to administer a separate task measuring attention and correlating performance between the attention task and the masked priming task. In sum, since we did not have an independent measure of attention or an explicit manipulation of attention in the priming task, attributing the observed effects to attentional differences between the two groups is not completely justified. Further research is needed to conclude deaf and NH individuals differ at what levels of processing in a masked priming task.

The results of Chapter 5 were not as clear. Contrary to our expectations, negative cueing effects were seen in the deaf group which were lower in magnitude compared to the NH group. This is also in contradiction to one of our previous studies on a similar population (Prasad, Patil, & Mishra, 2015). In this study, deaf and NH individuals were administered a cueing task where onset cues (highlighting of one of the four placeholder boxes) were presented before an onset target (a black disc). The task was to localise the target and make an eye movement to its location. Saccade latencies were measured which indicated robust positive cueing effects for both the groups. Importantly, the cueing effects were greater for the deaf. Several differences between the two studies could account for this discrepancy. To begin with, Prasad et al. (2015) did not have a condition where the ACS was rapidly changing. This is comparable to the Blocked ACS condition in the experiments reported in Chapter 5. In the blocked condition, there were descriptive differences between valid and invalid trials, but in the expected direction. That is, RT on valid trials was faster than RT on invalid trials but the difference was non-significant. Thus, as mentioned in the discussion section of Chapter 5, the negative cueing effects were most likely due to the mixed ACS condition.

Another key difference between the studies in Chapter 5 and Prasad et al. (2015) was the cue-target relationship. In Prasad et al. (2015), abrupt-onset cues were presented with abrupt-onset targets whereas, in Chapter 5, abrupt-onset cues were presented with colour singleton targets. A strong version of the contingent capture hypothesis predicts weaker cueing effects in Chapter 5 due to mismatch between cue and target features. This could explain why we didn’t see robust positive cueing effects even in the blocked ACS condition.

These results indicate that whether and in what conditions plasticity-induced changes are seen in the deaf is a complex problem. Apart from theoretical considerations outlined previously, the etymology of deafness also plays a critical role. It is well-known that cause of deafness, the hearing status of parents and the age of acquisition of sign language all play a key role in determining the changes in visual processing caused by deafness (Dye & Bavelier, 2013). All the deaf participants recruited in the studies presented here were congenitally deaf and born to hearing parents. They had acquired sign language early in their childhood. It is worth noting that it is not possible to generalise the findings presented in this thesis to other kinds of deaf individuals.

7.5 The importance of using the right baseline to measure capture

The studies with the spatial orienting paradigm in this thesis were concerned with investigating if irrelevant spatial cues capture attention. Cue absent trials were used as a baseline to determine if capture occurred. This methodology was adopted from Ruthruff and Gaspelin (2018). In that study, if responses on the irrelevant cue trials were equivalent to that on cue absent trials, it was concluded that the irrelevant cues were ignored. However, as pointed out in the discussions of Chapter 3,4 and 5, cue absent and cue present trials are not directly comparable. In chapter 4, we re-evaluated the findings of Ruthruff and Gaspelin (2018) by attempting to equate the cue-present and absent conditions on the alerting mechanism. We found that RTs on cue-absent trials are overestimated in such studies since the cue-absent trials lack the alerting benefit present in other trials with cues. When the alerting component was equated across all cue conditions - by presenting warning signal - we found reduced absent RTs when compared to ignored RTs (Experiment 2) suggesting that there was no complete immunity to attention capture at ignored locations. Comparing the results of Experiment 1 and 2 (Chapter 4), it is evident that the warning signal considerably speeded up responses on the cue-absent trials. Importantly, this study illustrates the significance of having an appropriate baseline condition in spatial orienting studies to avoid reaching misleading conclusions.

Mack and Jonides (1984) were among the first researchers to argue that all cue conditions in a spatial orienting paradigm should be matched “on physical appearance, on the potential to alert subjects generally, and on ease of encoding”. They carefully discussed the various ways in which a neutral condition can be flawed and become incomparable with other cue conditions. For these reasons, researchers using the spatial orienting paradigm have mostly calculated net cueing effects (invalid RT - valid RT) instead of calculating costs (invalid RT - neutral RT) or benefits (neutral RT - valid RT). This convention seems to be inspired out of methodological concerns regarding the neutral condition. However, using a neutral condition become unavoidable in circumstances where it is necessary to determine IF capture has occurred - as is the case in the experiments described in this thesis. In these situations, it is important to be careful about the neutral condition and whether it is equated with the other cue conditions in all aspects except the ability to orient attention to a location.

7.6 Limitations & the big picture

Several limitations have already been discussed within each chapter and the sub-sections earlier in this section. One key limitation is related to the high d’ values observed in several studies. d primes are expected to be at the chance level to conclusively classify a stimulus as subliminal. This criterion was violated in most of the chapters presented here. Thus, I acknowledge that it is not possible to easily generalise these findings to unconscious processing. However, it is worth noting that demonstrating a lack of awareness is one of the most challenging issues facing the field today. The reasons for this have been discussed in detail in the discussion sections of Chapter 3, 5 and 6 which will not be repeated here. Apart from the methodological challenges, philosophical issues related to the study of consciousness are also relevant to this issue.

Consciousness has been notoriously hard to define. While the studies presented in this thesis do not directly address the issue of consciousness, a definition of “unconscious” processing crucially depends on how “consciousness” is defined. Most laboratory studies operationalise consciousness as reportability. A participant is said to be conscious of a stimulus if they can report its identity or some features related to its identity. However, this definition of consciousness has left out a vast domain related to the subjective, experiential aspects of our consciousness. Qualia or the “redness of red” has been largely excluded from experimental investigations. The philosopher Ned Block has classified these two types of consciousness as phenomenal and access consciousness (Block, 1995). Access consciousness refers to the contents of our conscious experiences. For example, when you see a red apple and you can report that you saw a red apple, you are accessing the contents of our consciousness of the red apple. But there is something it is like to see a red apple. You are having a subjective, private experience of seeing red - the “redness” of red. This is referred to as phenomenal consciousness.

Most scientific approaches to consciousness have focussed on the access part of consciousness - referred to as “easy” problems by David Chalmers (Chalmers, 1995). But, the “hard” problems - investigating the phenomenal experiences - have remained outside most empirical investigations. One reason is that the scientific study of consciousness was considered taboo till the late 1980s and early 1990s. Crick and Koch’s first paper on possible neurobiological line of research to solve the problem of consciousness was published in 1990 (Crick & Koch, 1990). The association for the scientific study of consciousness (ASSC) - the largest body for the scientific study of consciousness - was founded in 1994. Thus, even though cognitive science began in the 1960s it took another 2-3 decades to bring consciousness research within the purview of experimental research. Thus, the aim in the beginning years, was to tackle the problems which were difficult - but well within the scope of objective, falsifiable, empirical science. Subjective experiences, by definition, fall out of this category. Thus, perhaps it was necessary to temporarily ignore some of the thorny issues to make some start in experimental consciousness research. But, now that it is close to 3 decades since consciousness research started, researchers are increasingly beginning to feel the need to consider some aspects of the “hard” problem (Overgaard, Mogensen, & Kirkeby-Hinrup, 2020).

A phenomenological approach to cognitive science takes first-person experiences seriously while theorising about the mind. Several researchers have been calling for the incorporation of phenomenological descriptions into cognitive science experiments (Gallagher & Sørensen, 2008; Overgaard, 2008). In one of the earliest studies to do so, Lutz et al. (2002) presented a random dot pattern to the participants. They were asked to report their experience when the random dots turned into a discrete shape. Participants were found to describe their experiences in many ways, like: “I had a growing sense of expectation but not for a specific object; however, when the figure appeared, I had a feeling of confirmation, no surprise at all”. The authors found correlations between the phenomenological experiences and the ERP waveforms. Based on this, they argued that much of the neural variability observed across participants in EEG/ERP recordings could be due to the subjective differences in attentional or other states in the participants. Getting a phenomenological report from the participants themselves could help understand this data better, rather than reducing it to “noise”. This is, in essence, the argument made in favour of first-person approaches by the majority of people who advocate for it. That it can be used to supplement the existing empirical approaches and can provide additional insights (but see, Dennett, 2001)

Such a phenomenological movement with cognitive science inspired the development of the Perceptual Awareness Scale (Ramsoy & Overgaard, 2004) which is widely used today by researchers studying subliminal processing. It is an improvement compared to the introspection-ist methods used in the early days of research on unconscious processing because it does not dichotomise the subjective experience (yes, I saw it vs no, I did not see it). Rather, it is a graded measure of the quality of conscious experience. However, it can be argued that even the PAS scale is subject to some of the same criticisms against subjective methods made by Eriksen (1960) and others. Mainly, the problem of establishing common criteria for reporting experiences remains.

I have traced the history of the developments in consciousness research because a good understanding of the past is necessary to critically evaluate the methods used today. Demonstrating a lack of awareness has been notoriously hard and the field has oscillated periodically between relying on subjective, introspectionist methods and objective methods. There is no consensus so far on which method is more reliable (Rothkirch & Hesselmann, 2017). I started my PhD research with the belief that objective methods are more reliable because they are externally verifiable and for that reason, more “scientific”. This is probably a reflection of my 5 years of training in Physics during my Master's degree. But, at this point, based on a combination of my intuitive understanding and domain knowledge, I am beginning to think that subjective measures are indispensable, especially when it comes to consciousness.

To conclude, the scientific study of consciousness and unconscious-ness is one of the final frontiers in our endeavour to understand the mind. I hope this thesis is a small contribution towards that while being aware that a lot more research is needed to make strong arguments related to the questions addressed in this thesis. I also hope that the skepticism regarding unconscious processing will only stimulate more research to understand the boundary conditions of this fascinating phenomenon rather than stifle it. I will end with a quote from Block's (2016) essay which nicely summarises my view on the study of unconscious processing (p. 452):

> *"All experimental paradigms in psychology have weaknesses (and very often, also strengths), but studies of unconscious perception are problematic in a special way. We do not have a scientific account of what consciousness is, and without such an account, all cases of unconscious perception lack scientific proof. Of course the same is true of conscious perception: we can not prove scientifically that you are having conscious visual experiences while reading this. This is not to say, however, that there is no science of conscious and unconscious perception."*

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Appendix A

In one version of the task in Experiment 1, participants received high-reward on seeing target 1 and low-reward for target 2 on forced-choice trials. They were asked to press A for target 1 and L for target 2. The table below gives all possible prime-response combinations on free-choice trials and the reward-levels for both prime-type and response-type (for Experiment 1, as an example). The analysis was done based on "response-type" in both experiments to examine if mere response-related associations lead to the observed effects in a prime-independent manner.

Prime	**Prime-type**	**Response**	**Response-type**
1	High-reward	A	High-reward
2	Low-reward	A	High-reward
1	High-reward	L	Low-reward
2	Low-reward	L	Low-reward

Appendix B

Table B1. Linear regression analyses on RT priming effect as a function of d' (Experiment 1)

Group	Prime location	Forced-choice trials	Free-choice trials
Deaf	Centre	0.22	0.07
	Periphery	0.32	0.49*
Normal-hearing	Centre	0.12	0.08
	Periphery	0.18	0.28

Note: Table include *r* values.
$^*p < 0.05$

Table B2. Linear regression analyses on RT priming effect as a function of d' (Experiment 2)

Group	Prime location	Forced-choice trials	Free-choice trials
Deaf	Centre	0.26	0.34

	Periphery	0.21	0.004
Normal	Centre	0.17	0.12
	Periphery	0.17	0.37[†]

Note: Table include *r* values.
[†] $p < 0.01$

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