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“... no man is an island, entire of itself...” (Donne, 1624)

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Table of Contents

Abstract	7
Preface.....	9
1 General Introduction	11
1.1 Selective visual attention.....	11
1.2 Principles guiding selective visual attention.....	12
1.2.1 Bottom-up saliency	13
1.2.2 Top-down contingent capture	14
1.2.3 Influences of task context	18
1.2.4 Further experience-based influences	21
1.3 Origins and explanations of inter-trial effects.....	24
1.3.1 The role of awareness	29
1.4 Overview of the remaining chapters.....	32
2 The contribution of color to attention capture effects during search for onset targets	33
3 There is more to trial history than priming in attentional capture experiments.....	55
4 The good, the bad, and the misattributed: The facilitating influence of nonconscious affect-origins on misattributions of affect.....	69
5 General Discussion	105
5.1 Summary	105
5.2 Limitations and open questions.....	107
5.2.1 Automatic attention capture by special features	107
5.2.2 Time course and temporal stability	110
5.2.3 The role of instruction and redundant features	112

5.2.4 Attribution of affect and alternative measurements	114
5.3 Perspectives.....	115
References.....	117
Zusammenfassung (german summary)	129
Curriculum Vitae.....	131

Abstract

At each moment in time, humans have to select from the visual information in their environment. Much of this visual selectivity is thought to be under voluntary control, but past experience about the utility of different sources of visual information shapes or even determines this voluntary control. The current thesis addresses three different influences of past experience on (or in) visual selectivity. Study 1 examines the influence that the experimental context exerts on voluntary attention deployment to abrupt visual onsets versus colours during visual search for onset targets. In altogether 8 experiments, I demonstrate that participants make use of colour information (sometimes in addition to onset information) where both of these features would allow successful search for onset targets. In one further experiment, I show that the usage of colours versus onsets is a matter of how efficiently these features can be used during search. Study 2 investigates trial history effects: Having experienced a feature as helpful in a recent experimental trial should prompt participants to select this feature on a subsequent experimental trial. In altogether 3 visual search experiments, I will show that this is true, even for on average irrelevant features - that is, of features that are neither shared by the target nor diagnostic of the target. Study 3 is concerned with the lacking influence of a stimulus' recent utility on its subsequent selection under subliminal conditions, in which participants remain unaware of the stimulus in question. In two experiments, I demonstrate affect-misattributions to unrelated stimuli in close temporal vicinity and away from helpful versus harmful subliminal stimuli. These affect-misattributions may be one reason why participants' past experience with subliminal stimuli (compared to stimuli of which participants are aware) do not prompt behavioural adjustments. I will elaborate on this idea together with pointing out other connections between the three studies of this thesis, as well as between the present studies and past research.

Preface

The following thesis covers a general introduction (chapter 1), three chapters with original research published in or submitted to peer-reviewed journals (chapters 2-4), and a discussion (chapter 5). The research of the thesis concerns two novel determinants of visual attention: First, the incorporation of more than the instructed search criteria into top-down templates for steering visual attention to relevant targets. Second, the capture of attention by pre-target cues that depends on the cues' most recent utility. These factors were targeted in the articles by Goller, Ditye, and Ansorge (2016), in Chapter 2, and by Goller and Ansorge (2015), in Chapter 3. A third strand of research concerns the peculiarities of the acquisition of knowledge about the utility of accessory stimuli (such as cues) under subliminal conditions (i.e., conditions of unawareness of the stimuli). This latter research is set off to some extent from the first two chapters, and is included in a manuscript by Goller, Khalid, and Ansorge (2016), reported in Chapter 4.

The rationale and the background of this thesis are motivated in the Introduction below. I will begin with a brief description of attention and one of the most fruitful approaches to study its determinants: the contingent-capture paradigm (Folk, Remington, & Johnston, 1992). In this experimental protocol, researchers have mostly tried to identify two major influences on attention: top-down control of attention on the basis of the instructions and bottom-up capture of attention due to stimulus saliency. However, I will explain that findings suggest that factors besides the instructions and stimulus saliency are also at work. Most importantly, relevant targets of visual search can often be found not only by their defining features, as these are specified in the instructions, but also by characteristics that happen to coincide with the targets, at least from time to time. In this respect, I studied the role of additional target-defining features (Chapter 2) and the role of the most recently experienced utility of additional accessory stimuli (Chapter 3) on visual attention. More information on the motivation of this

research can be found below in sections 1.1 and 1.2. However, there are limits to the usage of such redundant information. Most prominently, the utility of visual information that is presented below the threshold of aware perception does not seem to matter as much as clearly visible information of which humans become aware. Why this might be the case is explained in section 1.3 of the Introduction and is addressed in the research of Chapter 4. Importantly, this research used a different experimental procedure: masked priming instead of contingent capture. This line of research is therefore thematically not as closely related to the first two chapters as these two chapters are related to one another, but has nonetheless interesting implications for the overall topic of this thesis.

General Introduction

James (1890) once stated that “everyone knows what attention is”. Assuming that to be the case, this thesis is rendered entirely obsolete. Thank you for reading.

However, although anyone can give a subjective definition of attention and can provide some everyday examples, to this date, there is no holistic theory of attention. Luckily this thesis is restricted to the domain of visual attention and neglects the auditory or the tactile domain entirely. But even within vision, there are several assumptions on how attention operates. Attention might enhance the signal elicited by certain stimuli or tune the visual system to certain stimulus attributes while disregarding other attributes as noise. Alternatively, attention might even restrict the processing of visual stimuli to certain locations, objects or windows in time (for an overview see Wolfe & Horowitz, 2004). However, one common denominator that can be derived from these processes is that attention seems to select/prioritise some information while ignoring other information. How this selectivity is guided, is a core question of this thesis.

1.1 Selective visual attention

At each moment, we are confronted with numerous visual signals that reach our eyes, but not all of this information is registered by the observer. Although not known for certain, a central nervous system capacity limit has often been discussed as the reason for this selectivity. To understand this, consider the physiology of the eyes. Although humans possess a visual field of roughly 150° per eye, sharp and detailed vision is only possible at the fovea, a part of the retina that covers only about 2° of our visual field. Only at the fovea humans are able to perceive visual stimuli with high visual acuity, while the spatial resolution at

parafoveal (up to 2°-5° of the visual field) and peripheral (> 5° of the visual field) areas is lower. This fact necessitates us to constantly shift our eyes. With the help of sequences of fixations (a relatively still position of the eye for some time) and saccades (quick, jumping eye movements), humans sample and scrutinise information from different regions of the visual field in turn (J. Henderson, 2003, 2007). However, despite its inaccurate representation of fine detail and colour, retinal areas outside the fovea also contribute to visual processing. For example, humans are very sensitive to luminance changes and sudden onsets (appearances) of visual stimuli in peripheral vision.

Furthermore, humans can covertly shift their attention to locations in the periphery. An area or an object in the visual field can be selected for deeper processing without moving the eyes or the head to this location, such that gaze direction does not have to coincide with the current focus of attention. Shifting attention covertly does not compensate for all achievements of the human fovea, so that eye movements are still required in many situations. However, covert operations of attention are regular ingredients of overt fixations, as is clear from research demonstrating that people can miss out on information in their line of view (Deubel & Schneider, 1996; Vö, Schneider, & Matthias, 2008). In these cases attention could be restricted to certain features of a stimulus, so that other features are not seen.

1.2 Principles guiding selective visual attention

The principles that guide selective visual attention are still a matter of great debate. Two diametrical opposite positions claim that attention is initially guided either purely by the properties of the stimuli in the visual field (*bottom-up*) or by the search goals of the observer (*top-down*). Say, you are meeting with a friend for coffee. On the way to the coffee shop, the flashing lights on an ambulance driving by seem to capture your attention in a bottom-up way, without you actively

looking for them. Arriving at the coffee place, you are searching for your friend who has blonde hair. You scan the room, but you cannot find her/him. After a while you are realising that (s)he was sitting at a table right in front of you and even waved at you. Sitting down, your friend asks, “how do you like my new hair colour?”. Because you have adapted a search template for blonde, you did not notice other stimuli (or heads in this example) that were not blonde. Such a determination of the efficiency of visual search by search goals as referred to as top-down attention capture.

However, there might be also processes that guide our attention that do not fit in either of these categories, such as memory processes or past experiences (cf. Awh, Belopolsky, & Theeuwes, 2012). Such processes pose the core topic of this thesis. The following sections will review the outlined principles that guide selective visual attention more closely and will draw connections to the experiments reported throughout chapters 2, 3 and 4.

1.2.1 Bottom-up saliency

According to the bottom-up view of selective visual attention is, at least in an initial phase, captured in a stimulus driven manner by visual saliency. In a standard model, saliency is calculated as the local feature contrast between objects and their backgrounds and is a function of the summed local feature differences in colour, contrast, and orientation (Itti & Koch, 2000, 2001). Much research has been conducted to understand the automatic capture of attention capture by salient stimuli, such as colour contrasts (Theeuwes, 1991, 1992).

For example, Jonides and Yantis (1988) let their participants search for a predefined target stimulus among non-targets. In each display, a singleton (a stimulus of a unique colour, luminance, or sudden onset) was present that could be equally likely be the target or a non-target. Because the chance that the singleton

was the target was on average 50%, saliency was not informative of the most likely target position. Therefore, participants had no incentive to search for the singleton in a top-down way, but to search for the predefined target instead. According to the bottom-up account, due to its saliency, any singleton should capture attention nonetheless. This should be reflected in faster reaction times for detecting the target if it was the singleton, and slower reaction times if the target was not the singleton. Jonides and Yantis (1988) found that reaction times were higher if the singleton was a non-target distractor, reflecting attention capture by irrelevant singletons (hence bottom-up capture). However, this was only true for onset singletons, not for colour or luminance singletons. This means that salient static features, such as strong colour differences, are maybe not able to capture attention in a bottom-up way, but that only certain salient dynamic features, such as onsets, offsets or motion can do. These findings were also replicated by Belopolsky, Schreij, and Theeuwes (2010): Onset distractors elicited more efficient attentional capture than colour distractors, implying that onset singletons are more difficult to ignore. Schreij, Owens, and Theeuwes (2008; see also Schreij, Theeuwes, & Olivers, 2010) come to a similar conclusion, stating that onset stimuli always capture attention: If a colour target is preceded by an onset cue, the cue captures attention even if it does not match the top-down search-set (see next paragraphs).

1.2.2 Top-down contingent capture

If attention is entirely stimulus-driven, humans would be unable to ignore salient stimuli. If we were not able to prioritise or to suppress irrelevant visual information, we would literally not be able to look at a weak relevant signal when a strong salient stimulus competes for attention. Yet, the opposite is true. Even in the presence of highly salient stimuli, participants can strategically select objects in a top-down way and effectively ignore currently irrelevant and salient stimuli. A particularly drastic example is the inattentional blindness phenomena (for a

review see Simons, 2000). While participants are actively directing their attention to one class of objects in a top-down way (in the classical example: counting the passes of the basketball team in the white shirts), most of them do not notice an unexpected event such as a (black) gorilla walking through the scene (Simons & Chabris, 1999). This phenomenon has been extensively replicated and cannot be explained by stimulus-driven capture alone. Instead, observers must be able to steer their attention in a top-down way, by means of their search goals. Simply put, observers might use a (more or less vague) search template that they hold in their mind and everything that does not match (or corresponds to) this template is effectively ignored.

One of the most prominent theories claiming that even the initial phase of attentional capture is determined by top-down goals is that by Charles Folk and his colleagues (Folk, Remington, & Johnston, 1992). To test their contention, these authors used a variant of the spatial cuing paradigm (Posner, 1980), in which participants had to report the shape of a target that could appear at one out of four possible placeholder positions. Participants did not know where the next target would appear so that they had to search for the target. Each target display was preceded by a cue at one of the possible target positions that was valid or invalid. In valid conditions, cue and target were presented at the same position and in invalid conditions they were at different positions. On average, the cue was not informative of the target's position (it was equally often valid or invalid), so that the participants had no incentive to search for the cues in a top-down way. Yet, on the basis of automatic capture of attention by the cues (due to saliency), the authors expected validity effects: faster responses in valid than invalid conditions because only in the former situation, attention would already be at the target's location at the time of target onset. Crucially, the experiment was split in two different blocks. In one block, the target was defined as a colour singleton, realised as a red target symbol in one placeholder presented simultaneously with white distractor symbols in the remaining three placeholders (colour target). In the other block, the

target was defined as an onset singleton, realised as a white target symbol that appeared in one placeholder while the others remained empty (onset target). Overall, the two types of cues were used unpredictably and equally often: a colour-defined cue where one placeholder was surrounded by red dots and the remaining placeholders by white dots; and an onset-defined cue, where only one placeholder was surrounded by dots while the others remained empty. Note that the two types of cues corresponded to the two types of targets, meaning that cue and target features could either be similar (*matching*) or dissimilar (*non-matching*).

In each block only one type of target was shown (colour or onset), but both types of cues were used. Folk and colleagues reasoned that if attention was captured in a stimulus-driven way by any salient stimulus, both types of cues should capture attention and hence cause validity effects. This, however, was not the case. In blocks in which a colour target had to be found, only the colour cue elicited a validity effect, but not the onset cue. Conversely, in blocks where an onset target had to be searched for, onset cues but not the colour cues elicited a validity effect. This now classic finding was regarded as evidence that the top-down search set or template (i.e., search for onsets versus for colour) determined which stimuli (here the cues) captured attention. These results were replicated many times using various modifications to the paradigm (e.g. Anderson & Folk, 2012; Ansorge & Heumann, 2003). Also, these results were extended: Not only the features that participants search for capture attention, but also features that are similar between targets and distractors (Becker, Folk, & Remington, 2013; Folk & Remington, 1998).

Folk et al. (1992) assumed that their findings were characteristic of initial capture of attention being under voluntary control. However, Theeuwes, Atchley, and Kramer (2000) pointed out that there was a brief cue-target interval during which participants could have attended to both, the top-down matching and the non-matching, cues but during which participants could have also disengaged their attention from the non-matching cues. This circumstance could create the false impression that attention was never at the position of the non-matching cue.

In this view, initial capture of attention is under bottom-up control, while the disengagement of attention from the non-matching cue is under top-down control. Due to this control, disengagement is very fast and efficient if the cue carries a feature different from the target. This disengagement occurs in the 100 ms between cue offset and target onset. In line with this possibility, van Zoest, Donk, and Theeuwes, (2004; for a similar argumentation see Born, Kerzel, & Theeuwes, 2011) presented a target simultaneously with a distractor. Although participants knew the specific features of targets and distractors beforehand, distractors led to much longer reaction times. This decrease in performance was apparent even if the distractor shared no feature with the target (i.e., with non-matching distractors). Crucially, non-matching distractors influenced faster reaction times more than slower reaction times. This supports the notion that early stages of attention are especially prone to bottom-up processes, whereas later stages might be more top-down controlled. However, Chen and Mordkoff (2007) replicated the study by Folk et al. (1992) using a cue-target interval of only 35 ms and found no capture by onset cues during search for colour targets. This renders the disengagement account as unlikely. Additionally, electrophysiological studies showed repeatedly that top-down search sets modulate brain processes that are linked to very early attention processes (for example Ansorge, Kiss, Worschech, & Eimer, 2011).

Alternatively to attentional disengagement, Folk and Remington (1998) introduced the concept of attentional filtering costs to account for some interference effects of non-matching distractors. These costs are due to the presence of an irrelevant non-target singleton that impedes deployment of attention to the target singleton. Importantly, attention would not be shifted to such a non-target singleton. Instead, the non-target singleton would be filtered out prior to the shifting of attention to any of the locations. It remains, however, controversial how this mechanism exactly works and whether or not it entails a spatial component (for evidence for this possibility, see van Zoest et al., 2004). If

filtering entails a spatial component, a discrimination between filtering and disengagement would be quite difficult.

1.2.3 Influences of task context

Bacon and Egeth (1994) were probably the first to propose that top-down sets might not only depend on the exact instructions but also on task context. In a more general view, a number of different lines of evidence suggested that participants also pick up upon some contingencies between targets and their features that have not been instructed. These contingencies can be of use to more efficiently search for the targets. Most prominently, contextual cueing research has shown that the repeated use of the very same search displays facilitates searching the targets (Chun & Jiang, 1998, see section 1.2.4 below). This can even happen without the awareness of the participants.

Bacon and Egeth (1994) argued that if the target was a singleton (such as a single onset stimulus), participants could have adopted a *singleton detection mode*. This top-down search mode is a result of the participant's observation that the target in a visual search experiment is always a feature singleton. If this top-down search mode is adopted, any feature discontinuity in the image will match the top-down set and capture attention in a top-down contingent way, regardless of whether it is a colour, shape, orientation, or onset singleton. This search mode contrasts with the *feature search mode* that is typically instructed. According to Bacon and Egeth (1994), if both top-down singleton search and top-down feature search are possible modes to find the targets, participants might use either of these modes. In contrast, a top-down feature search mode is only enforced if the target is a non-singleton, necessitating participants to use a feature search mode, where they search for specific target-defining features.

A nice upshot of this theory is that it provides an explanation for some seemingly bottom-up capture effects of singletons: Such effects would only occur if the participants search for singletons. Indeed, many studies have replicated that attention capture by irrelevant singletons is dependent upon a fitting top-down set. If participants are forced into a feature search mode, irrelevant singletons do no longer capture attention (e.g. Eimer & Kiss, 2010). However, because this research casted doubts on the explanation of irrelevant singleton capture through stimulus-driven attention, defenders of the bottom-up view argued that feature search occurs only in more difficult displays, making the irrelevant singletons less salient in comparison to the relevant stimuli. Accordingly, attention capture by an irrelevant singleton under feature-search conditions can be reinstalled, if the relative singleton saliency is increased (Theeuwes, 2004), but even these results have been challenged (see Leber & Egeth, 2006).

In Chapter 2, I will pick up on some of these ideas and examine the role of colours versus sudden onsets for top-down contingent capture more closely. Particularly, I will test if participants pick up upon colours as additional target-defining features during search for onset targets, much as participants of Bacon and Egeth picked up upon the singleton status of their shape-defined targets. In their original demonstration, Folk and colleagues (1992) used onset cues and onset targets of the same colour, leaving it open whether top-down search-sets for onsets or colours accounted for their findings. I therefore systematically tested whether capture by coloured onset cues is facilitated by (or even entirely due to) an additional match between cue colour and target colour. This colour search set is maybe not instructed, but participants might pick up upon the colour definition in the course of the experiment, once they realise that all onset targets are of a particular colour.

Chapter 2 also addresses the possibility that what appears as more or less contingent capture could be due to more or less efficient disengagement of attention after onset cues with the same colour as the target, compared to onset

cues with a different colour than the target. This will be done by studying the time course of the effect with the help of the reaction time distribution (Ansorge, Horstmann, & Carbone, 2005; Ansorge, Priess, & Kerzel, 2013; Ansorge & Horstmann, 2007; Burnham, 2007). By looking at the target reaction time distribution, I can analyse the fastest reaction times and slower reaction times separately. If attentional disengagement plays a major role for attention capture in these experiments, I should find capture by all cues (regardless of their colour match or nonmatch) in the fastest reaction times but not in the slower reaction times. Contrary, if even initial capture is influenced by colour information, no influence of the reaction time distribution should be found.

Finally, Experiment 5 of Chapter 2, examines the role of task context more closely. Recent findings have shown that participants choose their search template on the basis of the overall utility of the features in the context of a task, such as the whether a feature can be used to distinguish a target from a distractor (Mast & Frings, 2014). I let participants search for two possible targets simultaneously. In one block, I encouraged participants to adopt a search template for onsets and discouraged a search template for colour. In a different block, I encouraged a search template for colour and discouraged a search template for onsets. Based on the encouraged search template, I found more or less efficient attention capture by onset and colour cues. Therefore, the search set seems dependent on economic considerations rather than purely on stimulus attributes. By using two different types of targets, I could also assess how the identity of the target in the previous trial influences the efficiency with which a cue in the current trials captures attention. The idea that previously encountered stimuli determines which stimuli capture our attention has received much evidence in recent years and will be discussed in the next section.

1.2.4 Further experience-based influences

The dichotomy between bottom-up and top-down largely neglects one major fact: Although participants they may be naïve in respect to the research hypothesis, they possess foreknowledge, experiences, and expectations about their environment. Take for example studies that used photographs of complex real-world scenes as search displays. In these experiments, participants have to search for a predefined item (e.g., a knife) in a certain scene (e.g., a kitchen). Here, capture of attention is not (only) guided by saliency or by a match between the stimuli in the display and the search template but also by the participants' knowledge about the world. Although real scenes may be extremely complex, they are not random. The placing of certain objects follows certain rules: Chimneys are on rooftops, lights are (mostly) on the ceiling, and knives are at (or close to) the kitchen counter. Retrieving the probability of the presence of a certain object in a scene or the location of it within a scene from memory facilitates search tremendously (Vö & Henderson, 2009, 2010). Consequently, violations of such probabilities inhibit search (Malcolm & Henderson, 2010). Such effects of scene semantics provide strong evidence that memory processes have a substantial influence on how our attention is guided.

However, memory processes do not always aid the performance in visual search tasks. Research on repeated searching through the same images shows that participants sometimes cannot improve their search performance even after extensive practice (Kunar, Flusberg, & Wolfe, 2008; Wolfe, Klempe, & Dahlen, 2000; Wolfe, Vö, Evans, & Greene, 2011). Conversely, after hundreds of trials, participants are surprisingly accurate if they have to judge whether they have seen a certain object or not (Hollingworth & Henderson, 2002). Therefore, the use of memory information may depend on whether accessing such information is more time efficient than 'pure' visual search by top-down templates defined during the instructions, and without memory aid. Additionally, abstract hints at task-relevant

memories does not facilitate search in scenes. For instance, presenting the word 'kitchen' prior to a kitchen scene does not facilitate search for kitchen-related objects (Neider & Zelinsky, 2006). However, a short glimpse of a kitchen scene that is used for the subsequent search task facilitates search if this glimpse is presented long enough (Võ & Henderson, 2010). A concrete idea about the scene that will be searched for an object, seems therefore to be necessary.

Participants cannot only (rather passively) retrieve information from memory, they can also actively form new memories about certain displays or scenes (see for instance Valuch, Becker, & Ansorge, 2013). Inhibition of return can be regarded as one such example: After visiting a certain spatial location within the visual field, revisiting the same location is associated with severe reaction times costs (Klein, 2000). Therefore, there seems to be an implicit memory process that keeps track on recently visited spatial locations. Moreover, even in scenes or displays for which no foreknowledge is retrievable from memory, memory processes can facilitate search. If target objects are repeatedly presented in the same spatial contexts of distractors, participants seem to (implicitly) learn this pattern. This leads to a facilitated search if the target is presented in the familiar context, compared to a novel context. In such contextual cueing experiments, the very fact that the participants benefit from display repetitions of which they are sometimes not even aware (Chun & Jiang, 1998, 1999) makes it clear that probably not all memory-based effects on visual attention can be subsumed under the heading of top-down control. The reason is that top-down control seems to require a deliberate decision on the side of the observers about what features to search for and attend to.

Along similar lines, researchers have argued that relatively top-down independent memory-processes might even account for what appeared to be top-down contingent capture. Belopolsky et al. (2010) showed that capture of a cue in a given trial, relies heavily on the features of the target in the previous trial. Only if the cue was of the same feature as the target in the previous trial, the cue

captured attention. According to Belopolsky et al., the preference for attending to specific features seems therefore to be (automatically) established on the basis of trial-to-trial priming of attention rather than on the basis of deliberately chosen top-down sets. The fact that search for a specific stimulus feature (such as colour, orientation or spatial frequency) becomes more efficient for search performance if this feature is repeated among multiple trials was originally described by Maljkovic and Nakayama (1994, 1996). They showed that even when repetition of the stimulus feature across trials was at chance level (i.e., the repetition of the feature was as likely as a change of the feature), repeating a target feature in a subsequent trial, led to a faster detection of the target among distractor stimuli. These authors attributed the effect to the priming of attention capture by features that the participants have recently attended to. Importantly, this effect was not conditional on a repetition of the manual reaction associated with the repeated feature. These inter-trial priming effects seem to be more bottom-up driven, because, as it is the case with contextual cueing, participants are mostly unaware of this repetition across trials (Kristjánsson & Campana, 2010) and voluntary control cannot override such effects (Kristjánsson, Wang, & Nakayama, 2002; Theeuwes & Burg, 2011; Theeuwes, Reimann, & Mortier, 2006; but see Folk & Remington, 2008 for a different view). Today however, it is clear that inter-trial priming cannot fully account for top-down contingent capture (Ansorge & Horstmann, 2007; Folk & Remington, 2008; Lamy & Kristjánsson, 2013; Worschech & Ansorge, 2012) as it was sometimes believed (Awh et al., 2012).

In Chapter 3, I will examine the nature of inter-trial priming in attention capture experiments more closely and show a new effect unrelated to priming. I analysed capture of a cue in a current trial as a function of the relation between cue and target in the previous trial. Therefore, rather than simple feature (here: colour) repetition across trials, I studied the influence of cue-target inter-trial contingencies. Capture by a cue in a current trial could be dependent on the utility of the cue shown in the previous trial (Mordkoff, Halterman, & Chen, 2008). If the

cue proved to be useful (i.e., if it was valid) in the previous trial, participants could pay more attention to the cue in the next trial. Otherwise, they should be more likely to actively ignore the cue in the next trial. I studied this effect with top-down matching cues carrying the same feature as the target and with non-matching cues carrying a different feature than the target. Hence, I tested the inter-trial contingency also with cues that should not capture attention according to the classic contingent capture hypothesis and according to the priming explanation. In particular, pure bottom-up driven inter-trial priming would not work because the features of the non-matching cues were never the same as the features of the targets in the preceding trials. Yet based on utility considerations, participants might even use the non-matching cue more and show stronger capture by it following a valid than following an invalid trial, a prediction that is further elaborated in the next paragraph.

1.3 Origins and explanations of inter-trial effects

Originally inter-trial effects were not studied in attention capture experiments, but in an Eriksen-flanker task (Eriksen & Eriksen, 1974). In this task, participants have to discriminate between two possible central target letters (e.g. 'H' or 'S'). Upon presentation, this target letter was flanked by either the same letters ('HHHHH' or 'SSSSS') or the alternative letters ('SSHSS' or 'HSHHH'). Usually, reaction times are slower in trials where the alternative letters are presented (incongruent trials) than in trials where the same letters are presented (congruent trials). This difference in reaction times between incongruent and congruent conditions is called *congruency effect*. Importantly, Gratton, Coles, and Donchin (1992), reported that the size of the congruency effect is reduced when the previous trial was incongruent compared to congruent. This *congruence sequence effect* (also called: 'Gratton effect') is considered to be an epiphenomenon

of cognitive control (Botvinick, Braver, & Barch, 2001; Verguts & Notebaert, 2008, 2009) which either leads to more focussing on the target letter or to an inhibition of the processing of the flankers. Due to this control, the interference by incongruent flankers can be reduced. The congruence sequence effect is a very robust and generalizable effect. After its initial report, it was replicated in a wide variety of tasks, paradigms and methods of measurement (Egner, Etkin, Gale, & Hirsch, 2008; Frings & Wentura, 2008; Kerns, Cohen, & MacDonald, 2004; Kunde, Augst, & Kleinsorge, 2012; Kunde & Wühr, 2006).

The role of cognitive control was challenged by Mayr, Awh, and Laurey, (2003) who argued that the congruence sequence effect could be explained by simple feature repetition priming (Maljkovic & Nakayama, 1994, 1996). In a two choice task, complete repetitions of all stimuli – targets and distractors – would (1) facilitate responding and (2) be restricted to congruent-congruent and incongruent-incongruent two-trial sequences. Accordingly, a priming effect working on top of the congruence effect would on average increase the congruence effect following preceding congruent trials and it would on average decrease the congruence effect following preceding incongruent trials. In line with this hypothetical reasoning, excluding exact stimulus repetitions across trials post-hoc from the analysis, Mayr et al. (2003) found no congruence sequence effect. However, not only exact stimulus repetitions, but also partial stimulus repetitions can mimic a congruence sequence effect (Hommel, Proctor, & Vu, 2004). Here, the stimulus features are assumed to be bound together in an episodic memory representation. In case of an exact repetition of stimuli, this representation can be easily accessed and needs not be altered. This account sparked a torrent of research that yielded remarkably inconsistent results, because some studies found congruence sequence effect also in the absence of feature repetitions (Bugg, 2008; Kunde & Wühr, 2006; Notebaert & Verguts, 2006).

It was argued that feature integration and conflict monitoring have additive effects. Notebaert, Gevers, Verbruggen, and Liefvooghe (2006) varied the

interval between a prime (here: a flanker preceding the target) and a target showing a stronger influence of feature repetitions with short intervals. This bottom-up priming is less influential with longer intervals where top-down conflict control has more time to exert its influence. As integrative as this account may seem, one major caveat remains. Feature integration would predict a facilitation of the processing of the target in a sequence of incongruent trials, given they are an exact replication of the preceding trial (e.g., HHSHH across multiple trials). Conflict control on the other hand, is associated with suppression of the irrelevant information, meaning that a sequence of intermixed incongruent trials (e.g., SSHSS followed by HHSHH) would also facilitate the response to the target (see Egner, 2007, for a similar argument). Electrophysiological evidence favours the conflict control account, because transitions between different incongruent trials are associated with a higher activation of the dorso-lateral prefrontal cortex, an area with a key role in conflict control (Botvinick et al., 2001) but not in priming. Additionally, an artificial deactivation of this brain region by transcranial magnetic stimulation abolished the congruence sequence effect completely (Stürmer, Redlich, Irlbacher, & Brandt, 2007). Recently, Weissman, Jiang, and Egner (2014) could show that even in designs where feature repetition was a priori controlled, a congruence sequence effect can be found. Additionally, they argued that cognitive control exerts more suppression of irrelevant information rather than enhancement of relevant information because if the distractor was presented at the target location, performance deteriorated. This can be regarded as evidence that spatial attention is used to focus on the target location, thus, suppressing the distractor locations.

A yet alternative view, argues that the associations between stimulus features (e.g., colour) and experimental conditions (such as congruent or incongruent) are not perfectly balanced in most studies (see Mordkoff, 2012). Take a Stroop task with four possible word colours and colour words as an example. Each congruent signal (RED, GREEN etc.) is realised more frequently than each

incongruent signal (YELLOW, GREEN etc.) if a 50% ratio of congruent and incongruent trials is to be maintained. This means that the word 'RED' is simply more likely to co-occur with the colour red. Once participants learn such contingencies, they are able to exploit them. The activation in brain areas mentioned before, are thought to reflect a mere memory bias rather than cognitive control (Schmidt, 2013a, 2013b). However, even studies that controlled for contingency learning still found a robust congruence sequence effect (Kim & Cho, 2014; Schmidt & Weissman, 2014; Weissman et al., 2014).

Gratton et al. (1992) originally explained their findings in terms of repetition expectancy. According to this explanation, participants would expect a repetition of stimulus conditions over successive trials, regardless of the actual probability of such a repetition. After a congruent trial, participants would automatically expect another congruent trial. In turn, after an incongruent trial, participants would expect another incongruent trial. The behavioural consequences of this account are similar to cognitive control: In expectation of an incongruent trial, participants would focus more on the target, in expectation of a congruent trial, their cognitive control would be loosened. However, the time course of these influences should be different. The (reactive) cognitive control is assumed to be a rather fast process that decays over time while (proactive) expectancies need time to be created and grow stronger over time. Using various intervals between prime and target, Egner, Ely, and Grinband (2010) found results that are best reconciled with cognitive control: a smaller congruency effect if the time interval between prime and target was longer. These results might be influenced by the task context however (Duthoo, Wühr, & Notebaert, 2013).

The description of cognitive control as an automatic and reactive process may, however, not be justified. For example, the size of the congruence sequence effect is strongly influenced by the proportion of congruent and incongruent trials (Logan & Zbrodoff, 1979; Tzelgov, Henik, & Berger, 1992). Therefore, cognitive control is not only exerted on a trial-by-trial basis but is also based on the overall

context. Even more compelling evidence comes from Crump, Gong, and Milliken (2006). Using a priming task, these authors presented prime-target pairs (equiprobably) either above or below the horizontal meridian of the screen. Importantly, pairs above the meridian were congruent in 75% of all trials, while pairs below the meridian were congruent in only 25% of all trials. Although the overall congruency of the prime-target pairs was at chance level, the congruency effect was larger in the highly congruent compared to the highly incongruent context. This context-specificity indicates a very flexible adaptation process that incorporates information across multiple trials and therefore cannot be short-lived.

As mentioned in the last section, Chapter 3 studies if attention capture effects in the contingent-capture paradigm are also affected by the cue's utility in the preceding trial. I compared attentional capture effects following valid and invalid trials, and expected to see more evidence of attentional capture following valid trials (similar to a congruence sequence effect). Also, this was done both with matching and with non-matching cues and including the additional factor of a match of the cue to the top-down set in the preceding trial, too. In this way, I was able to test if cue utility had an impact on attentional capture, regardless of inter-trial priming. To note, in each trial a target was shown following a cue so that the non-matching cue was never primed by the attended-to targets. Especially the cueing sequence effects of the non-matching cues would thus not be open to an explanation in terms of more or less inter-trial priming, an interpretation that was further backed-up by a control experiment, in which neither cue nor target positions could ever repeat from trial to trial. Finally, as the cue and the target positions in Chapter 3 were shown uncorrelated at each of four possible locations, all the different conditions were exactly of the same frequency, ruling out that any kind of contingency learning accounted for the expected inter-trial contingencies.

1.3.1 *The role of awareness*

Related to the foregoing, at least reactive cognitive control in response to a stimulus implies that participants become aware of the stimuli that elicit more or less conflict (Ansorge, Kunde, & Kiefer, 2014). Although there are many processes that do not require conscious perception (for a review see Van den Bussche, Van den Noortgate, & Reynvoet, 2009), some higher cognitive functions seem to depend on conscious input – that is, input of which the participants become aware. For example, bottom-up priming can occur without awareness, but a strategic control of a top-down set cannot be adjusted without awareness of the stimuli suggesting this kind of reactive control. It follows that if conflict control accounts for the congruence sequence effect, this effect should only be found with consciously perceived conflicting stimuli. Indeed, a large body of research suggested that there is no congruence sequence effect under nonconscious or subliminal conditions (Ansorge, Fuchs, Khalid, & Kunde, 2011; Frings & Wentura, 2008; Greenwald, Draine, & Abrams, 1996; Kunde, 2003). In contrast, simple (within-trial) priming effects can be found in neurological patients who have lost the ability to create new memories (e.g. Warrington & Weiskrantz, 1968).

More recently, a small congruence sequence effect under nonconscious conditions could also be established (Desender, van Lierde, & van den Bussche, 2013; van Gaal, Lamme, & Ridderinkhof, 2010). However, this congruence sequence effect was always weaker with nonconscious input than with conscious input. Also, it is debated whether these studies truly found conflict control to nonconscious inputs, or whether the congruence sequence effect was rather caused by some conscious by-product of the nonconscious conflict, such as processing fluency (Desender, Opstal, & van den Bussche, 2014) or an increased vulnerability to errors in incongruent conditions (Jaśkowski, Skalska, & Verleger, 2003). For example Kinoshita, Mozer, and Forster (2011) argued that participants that are not aware of the conflicting primes, they might be aware of the difficulty of a trial,

which in turn could elicit behavioural adaptation. Usually, incongruent trials are more difficult than congruent trials and congruence and difficulty are confounded in most studies. Additionally, since nonconscious primes only leave weak and short-lived memory traces (Haynes, Driver, & Rees, 2005), the effects of such primes should become weaker as reaction times increase. In general, participants tend to respond faster after congruent trials (Lupker, Brown, & Colombo, 1997) and slower after incongruent trials (Verguts, Notebaert, Kunde, & Wühr, 2011). Therefore, the smaller congruency effect following preceding incongruent trials might simply be caused by the relatively longer time interval (and, therefore, more decay of the weak memory traces) since the last trial, compared to preceding congruent trials (Kinoshita & Hunt, 2008).

More recently, Reuss et al. (2014) could show that contextual effects (see Crump et al., 2006) can also be found in subliminal conditions, thus arguing for a nonconscious exertion of cognitive control. This compelling finding might however still be explained by participants having a 'gut feeling' that previously a conflict has occurred without being sure. Desender et al. (2014) showed conflict control after trials were participants thought they were conflict-loaded (i.e. incongruent), even though the trial in question was in fact congruent.

In Chapter 4, I examine the origins of the weaker congruence sequence effects under subliminal conditions. It has been proposed that conflict adaptation is not caused by conflict per se, but rather by the aversive emotion that conflict triggers. This could explain why adaptation effects are weaker if participants are rewarded following incongruent trials (van Steenbergen, Band, & Hommel, 2009). Dreisbach and Fischer (2011) showed that the mere experience of reward is sufficient to exert adaptation effects. They presented participants with number words either printed in an easy- or hard-to-read font. Performance was greatly enhanced following a hard-to-read trial. This result has an interesting implication, because conflict could not have driven the adaptation effect. The task used by Dreisbach and Fischer contained no conflict in the sense of contradictory

information regarding motor responses (prime versus target). This task simply manipulated difficulty. Fritz and Dreisbach (2013) recently further supported the notion that conflict-stimuli elicit negative affect. They presented congruent and incongruent Stroop stimuli prior to a neutral symbol that participants had to rate as either positive or negative. The proportion of negative ratings was significantly higher in trials with an incongruent compared to a congruent Stroop. Two things are of particular interest here. First, negative affect might be the means by which conflict (or difficulty) is consciously experienced. Second, such affect can also be attributed to a stimulus that is entirely unrelated (but in close temporal vicinity) to the stimuli causing this negative affect.

Such affect misattributions may also be the common denominator to reconcile the inconsistent results of conscious and nonconscious congruence sequence effect. Research has shown that affect misattributions are stronger with nonconsciously than consciously perceived stimuli (Bornstein & D'Agostino, 1992; Murphy & Zajonc, 1993). The reason is that without awareness of the true affect origin, it is easier to misattribute the currently felt affective response to a stimulus in close temporal vicinity (Bornstein & D'Agostino, 1992). However, a correct attribution of negative affect to the conflict as the true affect origin could be critical for adaptation (cf. van Steenbergen et al., 2009). Thus, as compared to conscious priming, nonconscious priming could lead to both more affect misattributions and lower congruence sequence effects. I tested this possibility in Chapter 4. Using a priming task with a nonconscious and a conscious condition, I compared the influence of congruent and incongruent prime-target pairs on misattributions – that is, on the rating of neutral symbols. A demonstration of more misattributions under nonconscious than under conscious conditions would be indicative that negative affect correctly attributed to the incongruent prime-target pair could indeed be responsible for conflict adaptation. Also, if this is true, it should be possible to also show more evidence of adaptation (i.e., of a congruence sequence effect) in conscious than nonconscious priming conditions. Chapter 4's study can,

thus, shed further light on inter-trial effects (such as in Chapter 3) in general and can point towards the mechanisms that drive the impact of stimulus utility on information processing and attention. To this day, the role of affective responses to helpful/harmful (i.e. valid/invalid) stimuli on attention capture remains largely unnoticed.

1.4 Overview of the remaining chapters

Each of the following three chapters consists of an original manuscript describing the studies, their results, and the conclusions in more detail. Chapter 5 presents a general discussion which summarises the main findings of the studies and outlines open questions that require additional research. At the beginning of the next three chapters, the publication status of the respective manuscript indicates whether it has been already published in its present form, or whether it is currently under review. In case of a manuscript under review, the finally published manuscript might well differ from the version included in this thesis. The interested reader should consider retrieving the published version of the manuscript online at a later point in time. Additionally, each beginning of the next three chapters also informs about the contributions of the respective authors in more detail.

The contribution of color to attention capture effects during search for onset targets

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FG and UA developed the study concept and the study design. FG programmed and conducted the experiments, data analysis and interpretation. FG drafted the manuscript, TD and UA added to the manuscript and provided critical revisions. All authors approved the final version of the manuscript for submission.

The contribution of color to attention capture effects during search for onset targets

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Abstract The literature on top-down contingent capture is concerned with the question of what constitutes a search set. Is it restricted to single stimulus properties such as color or onsets, or can such sets be more complex? In nine experiments ($N = 140$), we tested whether cueing effects during search for onset targets were affected by cue color. According to the classic theory of contingent capture (Folk, Remington, & Johnston, *Journal of Experimental Psychology: Human Perception and Performance*, 18, 1030–1044, 1992), during search for onset targets, cues capture attention on the basis of a match between the cue's onset and top-down control settings directed to the target onsets. However, such cueing effects were based on cues of a color similar to the target color. Therefore, matches of the cue color to the target color could have contributed to the effects. Indeed, here we found cueing effects when the cues and targets were of the same color, but not when they were of different colors (Exps. 1a, 1b, 4a, and 4b). In addition, same-color cueing effects were stronger than different-color cueing effects (Exps. 2a, 2b, 3a, 3b, and the white-target conditions of Exp. 5). In Experiment 5, we also identified efficient search for only one target color as a critical prerequisite for the differences between cueing by color-similar and -dissimilar onset cues. We conclude with a discussion of the contributions of cue-to-set color matches, deallocation of attention, and intertrial priming to what appear to be top-down contingent-capture effects based on abrupt onsets.

Keywords Cueing · Attention capture · Onset · Color · Contingent capture

At any time, humans perceive only some of the available visual information and ignore the rest. This selectivity is called *visual attention* and can be both stimulus-driven and top-down-dependent (Bundesen, Habekost, & Kyllingsbæk, 2005; Wolfe, 1994). Strong evidence for top-down attentional control comes from *contingent-capture effects*, indicating that the capture of attention is contingent on attentional control settings (Folk, Remington, & Johnston, 1992). For example, when looking for a lemon, observers might set their attentional control settings so as to find yellow (or yellower) objects in the environment (see Becker, Folk, & Remington, 2010).

Originally, Folk et al. (1992) argued for the existence of two types of top-down control settings: settings for color (or static features) and settings for abrupt onsets (or dynamic features). This proposal was later revised when it became clear that control settings can even be selectively directed to only one specific color (Folk & Remington, 1998). In the present study, we investigated the contribution of control settings for a specific color in cases that originally had been attributed to contingent capture by abrupt onsets. This is necessary because both color and onsets could have contributed to top-down contingent-capture effects for abrupt-onset stimuli, as we will explain next.

Folk et al. (1992) instructed participants to search for two different types of targets. In one block, participants searched for a white abrupt-onset target, and in the other block, they searched for a red color target presented together with white distractors. Prior to the target, two types of cues were presented: white onset cues shown in isolation, or red color cues shown together with white nonsingletons. Importantly, both types of cues were presented at either the same position as the

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target (valid condition) or at a different position than the target (invalid condition) and were not informative about the target position. In line with top-down contingent attentional capture, a selective cueing effect was found: Reaction times (RTs) were faster in valid than in invalid conditions, but only if the cue matched the top-down settings for the targets. If participants searched for a white abrupt-onset target, a white onset cue resulted in a cueing effect, but a red color cue did not; if participants searched for a red color target, a red color cue triggered a cueing effect, but a white onset cue did not.

Originally, Folk et al. (1992) assumed that participants used static control settings to search for the color targets. As a consequence, any pop-out color stimulus matching these control settings should capture attention. These authors also argued that participants used dynamic control settings to search for the onset targets, and that therefore any onset stimulus would match this control setting and capture attention. However, Folk and Remington (1998) revised these views when they observed that participants were able to ignore green color cues when searching for red color targets, and to ignore red color cues when searching for green color targets. In light of these findings, Folk and Remington argued that their participants set up different static control settings directed to just a specific color. This finding implies that the properties of the control settings depend on the task at hand.

Importantly, Folk and Remington (1998) did not revise their view on the mechanisms responsible for the cueing effects in the target-onset conditions, although the use of different color control settings could also explain these effects. In Folk et al. (1992), the onset cues were white and the color cues were red. Therefore, a control setting for white in the onset-target conditions would have led to the same results as a control setting for abrupt onsets: capture by white and no capture by red color cues. In the present study we tested this possibility, because two findings have suggested that the top-down contingent capture by onset cues might be explained by search for colors alone.

First, search for onset targets seems to depend on target color: With red abrupt-onset targets, participants showed a stronger cueing effect with reddish than with greenish abrupt-onset cues. With green onset targets, the opposite was found (Ansorge & Heumann, 2003, 2004; Ludwig & Gilchrist, 2002; see also Anderson & Folk, 2010, for similar findings with color singleton targets). This should not be if search were for abrupt onsets only. However, there was a residual cueing effect even with the color-dissimilar cues in most of these conditions. With the present study we aimed to extend these findings and to determine the time course of this cueing effect: By analyzing RTs in quintiles, we wanted to determine at which point in time the cueing effects for more and less color-similar onset cues start to differ from one another. Possibly attention is captured by both target-color-similar and -dissimilar cues, but participants are better at

deallocating their attention away from dissimilar than from similar cues (Theeuwes, Atchley, & Kramer, 2000). If deallocation plays a role, we would expect to find larger cueing effects for color-dissimilar cues among the faster responses that would decrease as RTs increased. If, however, dissimilar cues do not capture attention in any way, we would expect to find no cueing effects by color-dissimilar cues from the fastest RT quintile onward. Admittedly, deallocation could also take place prior to the fastest RTs, rendering null cueing effects of the dissimilar cues in the first RT quintile not entirely conclusive. Yet, theoretically, the quintile analysis could at least yield a pattern predicted by the deallocation account.

A second line of research suggests a role for top-down contingent capture by color in search for onset targets: Using black abrupt-onset targets, Fuchs, Theeuwes, and Ansorge (2013) showed a selective cueing effect with black abrupt-onset cues, and no cueing effect with white ones. This pattern reversed during search for white onset targets. However, Fuchs et al. used a detection task that was not perfectly sensitive to attentional capture effects. In light of these considerations and findings, we tested whether the contingent-capture effect of onset cues in a discrimination task (see Folk et al., 1992) is facilitated by an additional match between cue color and a target-directed top-down setting, or whether the cueing effects of onset cues can even be entirely explained by the degree to which these cues match the attentional control settings for colors. Furthermore, we set out to test how contingent attention capture by onset stimuli is boosted, or even entirely explained, by contingent capture by color.

Experiment 1

In Experiment 1, participants searched for white abrupt-onset targets. In Experiment 1a, either a white abrupt-onset cue (presented in isolation) or a white color cue (presented together with three red nonsingletons) preceded the target. If participants set their control settings for abrupt onsets, white onset cues should lead to a cueing effect, whereas white color cues should not. In contrast, if participants set their control settings for color, both of the cues should lead to cueing effects.

This would also be predicted if participants search for any kind of pop-out target (Bacon & Egeth, 1994; Leber & Egeth, 2006), or if pop-out features attract attention in a stimulus-driven way (Theeuwes, 2010). We addressed these possibilities in Experiment 1b, in which participants also searched for a white abrupt-onset target, but now this target was preceded by a white color cue (presented together with red nonsingletons) or a red color cue (presented together with white nonsingletons). If all pop-out stimuli attract attention, regardless of their color similarity to the searched-for targets, the white and the red color cues should both lead to cueing effects in Experiment 1b. However, if control settings for

colors were at work, we would expect the white but not the red color cues to elicit a cueing effect. Finally, if control settings were directed to onsets, no cueing effect would be expected in Experiment 1b, because both of the cues were color cues.

Method

Participants Sixteen psychology students at the University of Vienna participated in Experiments 1a (ten female, six male; $M_{\text{Age}} = 22.89$ years, $SD_{\text{Age}} = 2.86$) and 1b (11 female, five male; $M_{\text{Age}} = 22.33$ years, $SD_{\text{Age}} = 2.13$), respectively. Here and in the following experiments, each student received partial course credit, had normal or corrected-to-normal visual acuity and normal color vision, and participated in only one of all nine experiments. Prior to all experiments, informed consent was obtained from all participants. In Experiments 1a and 1b, one participant was excluded because of an error rate exceeding 20 %.

Apparatus Stimuli were presented on a 19-in. LCD monitor (Acer B 193) with an aspect ratio of 4:3 and a resolution of 1,024 × 768 pixels at a vertical refresh rate of 75 Hz. Viewing distance was 57 cm, and participants' heads were supported by a chin and forehead rest. Manual responses were recorded as keypresses of the left and right index fingers on a standard keyboard. The experiment was controlled using MATLAB 7.7.0 (MathWorks Inc., Natick, MA, USA) and the Psychophysics Toolbox (Brainard, 1997; Pelli, 1997).

Stimuli All stimuli were presented against a black (CIE Lab coordinates: 0.8 cd/m²; 0.1/−0.1) background. Four gray (75.5 cd/m²; −5.3/−18.8) square-shaped outline placeholders (1.5° × 1.5°) were presented at the corners of an imaginary diamond around a central gray fixation point (0.5°). The center-to-center distance between the fixation point and the placeholders was 5.0°. On each trial, a white (137.7 cd/m²; −7.8/−28.4) onset target was shown in the center of one of the placeholders. This target was equally likely an “X” or an “=” (1.33° × 1.33°). In Experiment 1a, one half of all cues (four filled dots with diameters of 0.25°) were white color cues centered around one placeholder and presented together with red nonsingletons (74.2 cd/m²; 94.4/98.3) centered around the other placeholders. The other half of all cues were white onset cues presented alone. In Experiment 1b, instead of the white onset cues, red color cues (presented together with white nonsingletons) were used (see also Fig. 1).

Procedure Figure 1 illustrates the sequence of events in a trial. After fixation (600 ms), the cueing display was presented for 50 ms, followed by another fixation display for 100 ms. Next, the target was shown for 50 ms on top of the fixation display. After the offset of the target, a blank screen was presented until participants reported the target's identity (“X” or

“=”) via a keypress. Stimulus-to-response mappings were constant within but counterbalanced across participants. RTs were measured to the nearest millisecond as the time intervals between target onset and keypress. Release of the response key started the next trial. Participants were instructed to answer as quickly and accurately as possible. If no keypress was registered within 1,550 ms, participants received onscreen feedback (“react faster!”), and the trial counted as an error.

From trial to trial, cue positions, target positions, and target identities varied randomly, but across trials, both cues and targets were counterbalanced across all four positions. The cue and target positions were 25 % valid, 75 % invalid, and uncorrelated across trials. Invalid cues appeared randomly but equally often at each of the three remaining positions, other than that of the target. The cues were not informative about the target's identity. Each of the two possible target shapes was presented equally often. The different conditions were presented in a pseudorandomized order.

Results

RTs of Experiment 1 Error trials and all trials with an RT below 150 ms were removed (4.97 %). The RTs for each participant and variable combination were sorted in ascending order and into equally sized quintiles: The fastest 20 % of all RTs were sorted into Quintile 1, the slowest 20 % of all RTs into Quintile 5, and the other RTs binned accordingly in between. We computed cueing effects as the differences of the mean correct RTs in invalid trials minus the mean correct RTs in valid trials, separately for each participant as well as for each step of the variable cue type (with varying steps; here, white onset cue vs. white color cue in Exp. 1a, and white color cue vs. red color cue in Exp. 1b) and for each quintile. Unless otherwise stated, all cueing effects are significantly different from zero (all $ps < .05$). Throughout this article, all results were processed in a corresponding way and further analyzed by a repeated measurements analysis of variance (ANOVA), with cue type and quintile as within-subjects variables. Where appropriate, degrees of freedom were adjusted by the Greenhouse–Geisser correction. For better transparency, the uncorrected degrees of freedom are reported. All reported pairwise comparisons were Bonferroni-corrected, meaning that the critical p value of significance was multiplied by the number of comparisons.

RTs of Experiment 1a White color cues matched a potential top-down set for color, whereas white onset cues would have matched top-down sets for onsets and color. The analysis showed a main effect of quintile, $F(4, 56) = 8.34$, $p < .001$, indicating that the cueing effects were different between quintiles (26, 26, 27, 31, and 48 ms, respectively, from the 1st to the 5th quintile). Pairwise comparisons revealed that the cueing effect in the 5th quintile was significantly larger than those

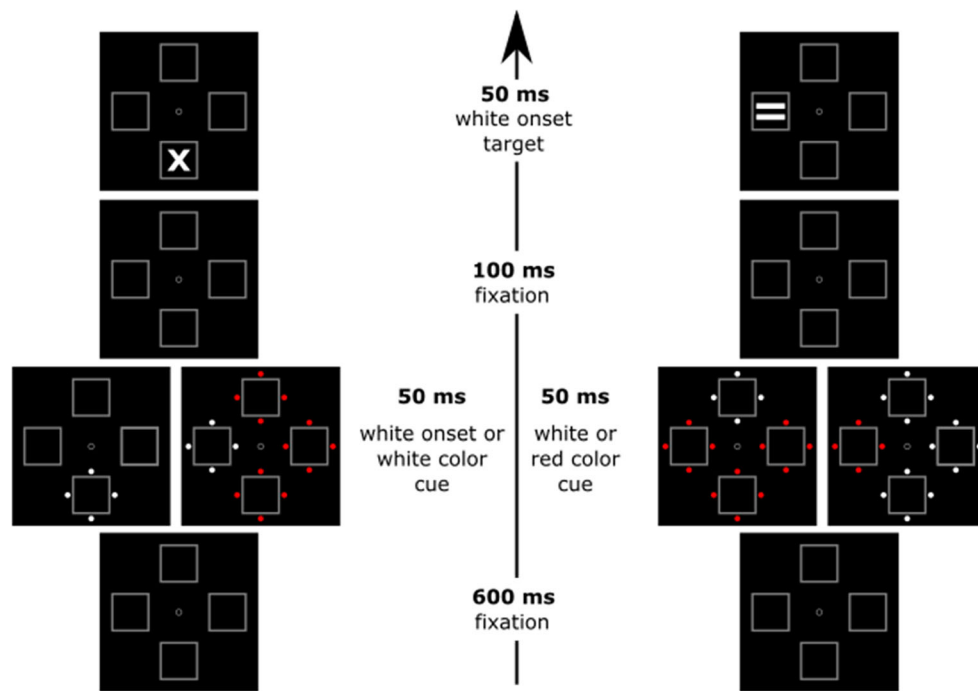


Fig. 1 Example sequence of Experiment 1a (left), and an example sequence of Experiment 1b (right), both with time flowing from bottom to top. First, a fixation display was shown, followed by a cue display, another fixation display, and finally a target display.

In Experiment 1a, the white color cues and the white onset cues were used equally often, and in Experiment 1b, the white color cues and the red color cues were used equally often. The figure is not drawn to scale

in all other quintiles (all p s < .011). No other differences between the quintiles and no main effect of cue type were found. Figure 2 (upper panel) shows the cueing effects as a function of quintile and cue type. No other effects were found, all F s < 1.91, all p s > .121.

Error rates (ERs) of Experiment 1a The arcsine-transformed ERs were subjected to a repeated measurements ANOVA, with the variables validity (valid, invalid) and cue type (white color cue, white onset cue). The results showed no significant effects, all F s < 1.05, all p s > .322.

RTs of Experiment 1b The data were handled as in Experiment 1a, which led to the exclusion of 5.03 % of all trials. White color cues matched a potential top-down set for colors, whereas red color cues did not match any set, be it for color or onsets. The analysis showed a main effect of cue type, $F(1, 14) = 137.08$, $p < .001$. The cueing effect was selectively present for white color cues (32 ms), $t(14) = 10.14$, $p < .001$, but inverted for red color cues (–11 ms), $t(14) = -4.34$, $p < .001$. No other effects were found, all F s < 0.67, all p s > .617. The lower panel of Fig. 2 illustrates these cueing effects.

ERs of Experiment 1b The analysis of the ERs was analogous to that in Experiment 1a and yielded no significant results, all F s < 2.54, all p s > .134.

Discussion

In both experiments, participants searched for white onset targets. In Experiment 1a, the cueing effects of white onset and white color cues were of the same size, although only the white onset cues would have matched a control setting for onsets. These results can be explained by control settings for target color, in which case top-down contingent attention capture of both cues would be predicted.

The results of Experiment 1a could also be explained as reflecting capture by pop-out. However, in Experiment 1b, the cueing effect was restricted to the white color cue. With the red color cue, the cueing effect was even inverted, although this cue was also a pop-out stimulus. Thus, the presence of a match of the cue to the set for target colors seemed to be both sufficient (Exp. 1a) and necessary (Exp. 1b) for a standard cueing effect of the onset cues.

Yet the following caveats should be taken into consideration. The inversion of the cueing effect in Experiment 1b might indicate active suppression of the red color cue, inhibiting target processing at the position of this cue (cf. Lamy, Leber, & Egeth, 2004; but see Carmel & Lamy, 2014). Alternatively, in Experiment 1b's red color cue conditions, attention could have been deployed to all three white nonsingletons in the cueing display, or randomly to one of these nonsingletons at a time. This in turn could have led to RT costs if the target was shown at the actually least attended

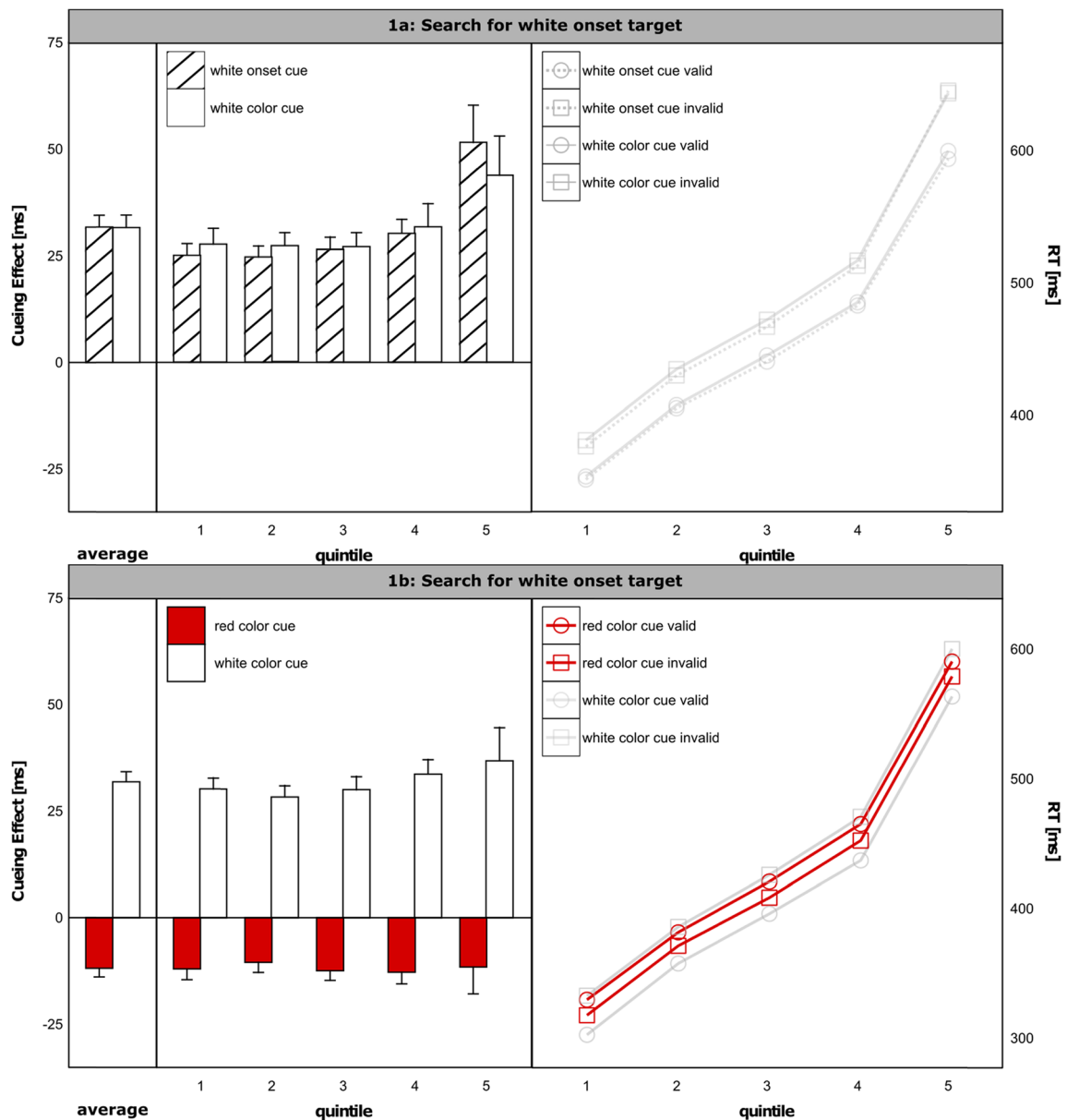


Fig. 2 From left to right: Cueing effects (invalid RT minus valid RT) as a function of cue type, averaged across quintiles; cueing effects as a function of cue type, split for the RT quintiles; and RTs as a function of

cue validity (circles = valid, squares = invalid), cue type, and quintile (x -axis). The top panel depicts the results of Experiment 1a, and the bottom panel those of Experiment 1b

position of the red nonmatching cue. Whatever the exact principle, these results support an interpretation in terms of a cueing effect that was contingent on a top-down control setting for the target's white color.

Another caveat is that a search setting for chromaticity and a search setting for luminance transients could both explain our results, because the white stimuli always were the highest-luminance transients in the display. If participants searched for luminance transients to find the white targets, a white onset cue and white color cue would have matched such a control setting in Experiment 1a. In the same manner, more attention capture by the white color cues in Experiment 1b could be

explained. At variance with this explanation, a top-down setting for luminance transients would have predicted more capture by white onset cues than by white color cues in Experiment 1a, because the white color cues would have suffered from a lower difference by which the cue's luminance stood out among the weaker but still luminant red nonsingletons. The issue whether sets for chromaticity or luminance accounted for the findings cannot ultimately be decided on the basis of the present results alone, and will be addressed in Experiments 2b, 3, 4, and 5.

A third caveat concerns the utility of a top-down setting for color as a means to ignore the red color cues in Experiment 1b.

Maybe the participants in Experiment 1b selected attentional control settings for the target's white color because this allowed them to both attend to the targets and ignore 50 % of the uninformative cues—namely, the red color cues. Note that this incentive was also provided in most other contingent-capture experiments (e.g., Folk et al., 1992; Folk & Remington, 1998).

In any case, the difference between the cueing effects of matching white color cues and of nonmatching red color cues was present from the fastest responses onward. This is also in line with a contingent-capture account, since control settings are assumed to be set up in advance and can therefore effectively determine which stimuli capture attention directly, from stimulus onset onward (Ansorge & Horstmann, 2007; Burnham, 2013). In contrast, positive evidence for more deallocation (Theeuwes, 2010; Theeuwes et al., 2000) with nonmatching cues than with matching cues would have consisted of a significant cueing effect by nonmatching cues in the earliest quintile(s) and its decrement with increasing RTs (as more time would have passed since the initial capture). The fact that the predicted interaction between cue type and quintile was not found means that no positive evidence supported the deallocation explanation.

Experiment 2

In Experiment 2, we tested whether cueing effects of onset cues during search for onset targets depended on a match between cue color and attentional control settings. In Experiment 2a, participants searched for white onset targets and onset cues were of either white or red color. Under the perspective of contingent capture by onsets, both cues should induce a cueing effect, but under the perspective of contingent capture by colors only white onset but not red onset cues should induce a cueing effect.

Above we discussed that it is also possible that participants actively searched for luminance transients. Another possibility is that they searched for colors and onsets simultaneously. Under both these perspectives, in Experiment 1a, the cueing effect for the red onset cue might only be reduced as compared to that of the white onset cue. If participants actively searched for the white onset targets' luminance transients, the red onset cues of a moderate luminance would capture attention to some degree but not as effectively as the white onset cues. On the other hand, if search was directed to both colors and onsets, the white onset cue would capture attention according to a fit to two control settings and, thus, more effectively than the red onset cue that would capture attention only on the basis of its matching to one control setting for onsets.

To discriminate between these two possibilities, in Experiment 2b, participants searched for red onset targets, preceded by red onset or white onset cues. The red onset cues should lead to a cueing effect if participants adopted a control

setting for color or maybe for onsets and color. The white onset cues should lead to a cueing effect if participants adopted a search setting for onsets or luminance transients only. If participants searched for luminance transients to find the onset targets in Experiments 1a and 1b, we would expect the same results in these experiments: more efficient capture by the white onset cues with their higher luminance than by the red onset cues.

Method

Participants Sixteen participants were tested in Experiments 2a (12 female, four male; $M_{\text{Age}} = 23.25$ years, $SD_{\text{Age}} = 2.86$) and 2b (seven female, nine male; $M_{\text{Age}} = 22.20$ years, $SD_{\text{Age}} = 1.82$), respectively. In Experiment 2b, one participant was excluded due to more than 20 % error trials.

Stimuli and procedure In Experiment 2a, the participants searched for white onset targets, and the cues were either white onset cues or red onset cues. In Experiment 2b, the participants searched for red onset targets, and the cues were either white onset cues or red onset cues. The procedure closely resembled Experiment 1; see also Fig. 3.

Results

RTs of Experiment 2a Data handling was the same as in Experiment 1, and 5.23 % of all data were removed from the analysis. The cueing effect was systematically influenced by cue type, $F(1, 15) = 8.23$, $p = .011$. With a white onset cue, the cueing effect was significantly larger than with a red onset cue (28 vs. 22 ms). We also found a main effect of quintile, $F(4, 60) = 10.92$, $p < .001$, indicating that the cueing effect increased with the RTs (17, 19, 23, 28, and 38 ms, respectively, from the 1st to the 5th quintile). Pairwise comparisons showed that the cueing effect of the 5th quintile was significantly larger than in the 1st to 3rd quintiles (all $ps < .002$), and the cueing effect in the 4th quintile was significantly larger than that in the 1st quintile ($p = .040$). No other effects were found, all $F_s < 1.81$, all $ps > .139$; see also Fig. 4.

ERs of Experiment 2a This analysis was analogous to that in Experiment 1. No significant results were found, all $F_s < 2.44$, all $ps > .139$.

RTs of Experiment 2b Taken together, 6.57 % of all trials were removed. Now the red onset cue (30 ms) elicited a significantly larger cueing effect than the white onset cue (16 ms), $F(1, 14) = 19.72$, $p < .001$. Furthermore, the cueing effect increased with RTs (16, 17, 21, 26, and 36 ms, respectively, from the 1st to the 5th quintile), $F(4, 56) = 10.06$, $p < .001$, and was significantly larger in the 5th quintile than in the 1st, 2nd, and 3rd quintiles (all $ps < .015$). No other effects were found, all $F_s < 0.21$, all $ps > .933$, see also Fig. 4.

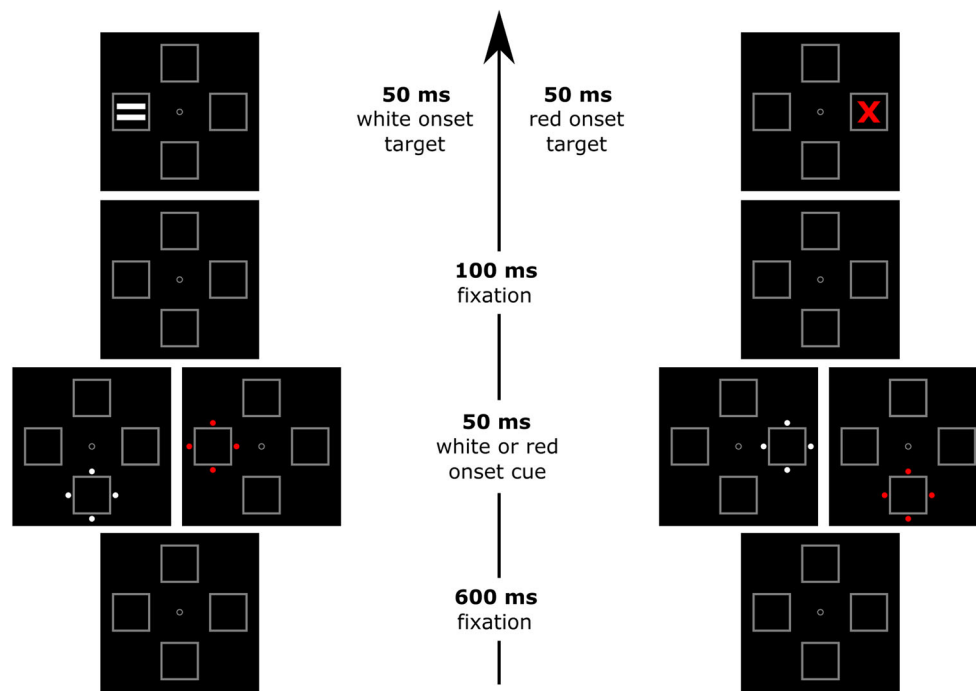


Fig. 3 Example sequences of Experiments 2a (left panel) and 2b (right panel). In Experiment 2a, participants had to identify a white onset target, and in Experiment 2b, they had to identify a red onset target. In both

experiments, the target was preceded equiprobably by a red or a white onset cue. The figure is not drawn to scale

ERs of Experiment 2b ERs were higher with a red onset cue (7.26 %) than with a white onset cue (5.17 %), $F(1, 14) = 11.84$, $p = .004$. No other effects were found, all F s < 3.14, all p s > .584.

Discussion

An onset cue of a color similar to the searched-for onset target captured attention more efficiently than a cue of a color dissimilar to the onset target. This was found during search for white onset targets (Exp. 2a) and red onset targets (Exp. 2b). Given that there was no interaction between the variables cue type and quintile, these experiments again provide no direct support for more deallocation of attention following the less similar cues. Furthermore, these results point to a role of top-down attentional control settings for color during search for onset targets and cannot be explained by a control setting for luminance transients. A control setting for luminance transients would have led to more capture by white onset cues than red onset cues, regardless of whether the white onset cues matched (Exp. 2a) or did not match (Exp. 2b) a control setting for colors.

In contrast to Experiment 1b's target-dissimilar color cue condition, here we found significant residual cueing effects of the target-color-dissimilar onset cues (22 and 16 ms). Taken in isolation, the residual cueing effect of the color-dissimilar onset cue could be due to a control settings directed to onsets (Folk et al., 1992) or to stimulus-driven capture by just any

abrupt onset (Schreij, Los, Theeuwes, Enns, & Olivers, 2014; Schreij, Theeuwes, & Olivers, 2010). When Folk et al. (1992) used a red color target (under otherwise the same conditions as in the present experiment), a white onset cue created a zero cueing effect. This suggests that in the present experiment an attentional control setting for onsets was likely necessary for the residual cueing effects of onset cues with a color different from the target, meaning that participants searched for colors and onsets to find the targets. This interpretation would be in line with other contingent-capture effects, indicative of top-down search for two different features at the same time (Adamo, Pun, Pratt, & Ferber, 2008; Irons, Folk, & Remington, 2012). However, this interpretation begs the question as to what might have encouraged the participants to use a top-down set for onsets *and* for colors in the present experiment because, in contrast to prior studies, it was not necessary to search for both features to find the targets. One reason could be that participants used both of these features in the control settings because both features allowed searching for all targets with equal efficiency. This would imply that sets for color should not play a role during search for onset targets, in which color search would be less efficient (see Exp. 5).

Experiment 3

In Experiments 1 and 2, the red cues and targets had lower luminance transients and contrast than the white cues and

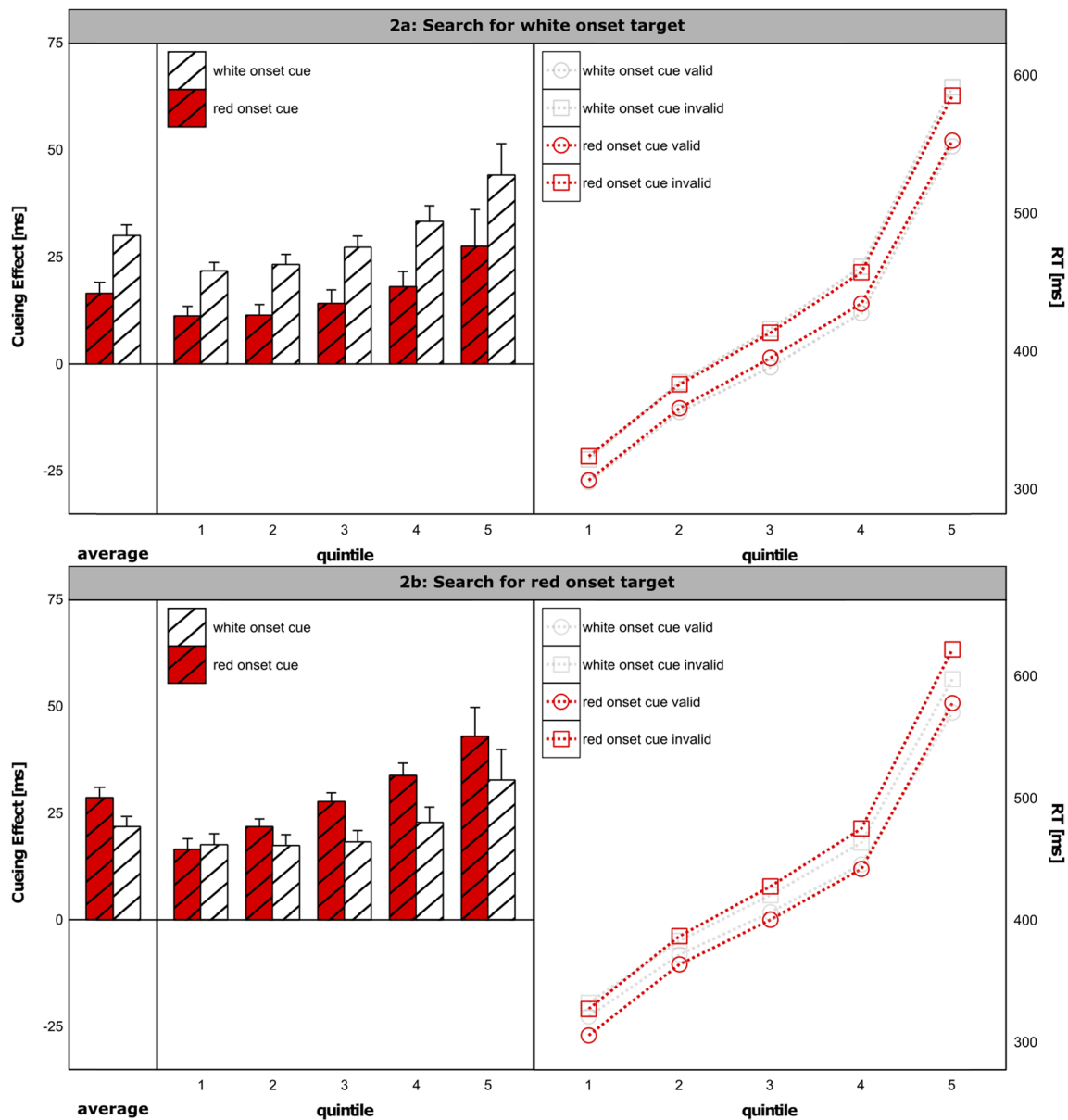


Fig. 4 The top panel depicts the results of Experiment 2a, and the bottom panel those of Experiment 2b

targets. Therefore, we tested whether the results of Experiment 2 would hold with the contrast and strength of the luminance transient equated for achromatic onset cues and onset target colors (see Fuchs et al., 2013). Participants searched for light onset targets (Exp. 3a) or for dark onset targets (Exp. 3b), and the cues in both experiments were light or dark. If top-down control settings are directed to target onsets or luminance transients, we expected both kinds of onset cues to capture attention to similar extents in Experiments 3a and 3b because the strengths of the luminance transients of the light and the dark stimuli were carefully equated for their resulting contrasts. The same prediction would be made on the basis of stimulus-driven capture by all onset cues and on the basis of capture contingent on control settings for onset singletons.

However, if control settings were directed to achromatic target colors (much as to target chromaticity in Exp. 2b), we would expect cueing effects by light but not by dark onset cues during search for light onset targets, and by dark but not by light onset cues during search for dark onset targets.

Method

Participants Sixteen participants were tested in Experiment 3a (ten female, six male; $M_{Age} = 21.69$ years, $SD_{Age} = 3.44$), and another 16 participants (nine female, seven male; $M_{Age} = 22.36$ years, $SD_{Age} = 6.03$) were tested in Experiment 3b. In Experiment 3b, two participants were excluded owing to more than 20 % errors.

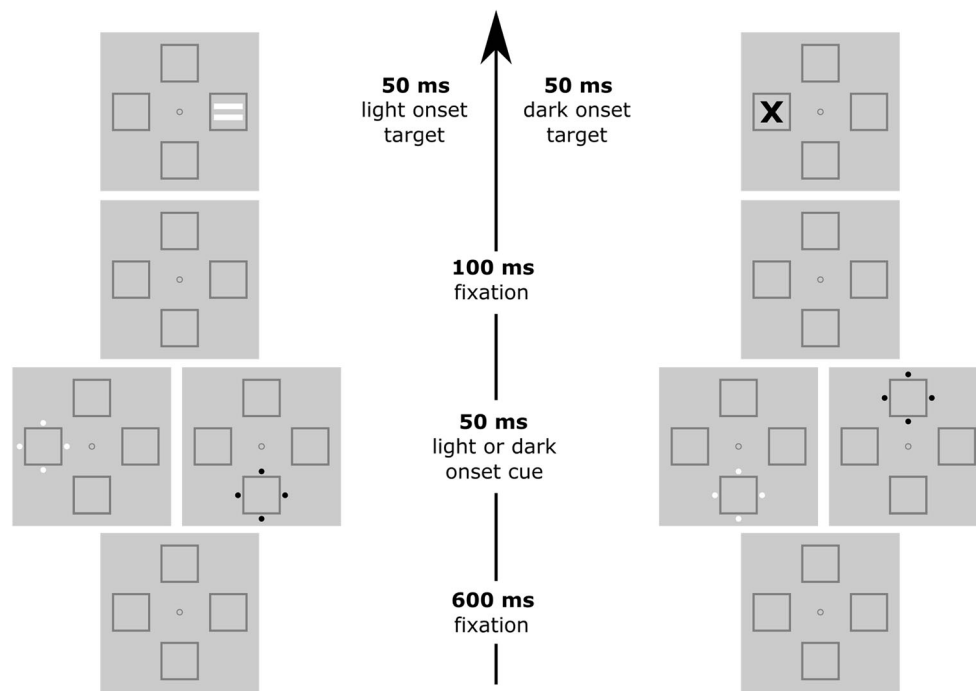


Fig. 5 Example sequences of displays in trials of Experiments 3a (left panel) and 3b (right panel). In Experiment 3a, participants had to identify a light onset target, and in Experiment 3b they

had to identify a dark onset target. In both experiments, the target was equiprobably preceded by a light or a dark onset cue. The figure is not drawn to scale

Stimuli and procedure The stimuli and procedure were the same as in Experiment 2, with the following exceptions; see also Fig. 5. The background was gray (75.5 cd/m^2 ; $-5.3/-18.8$). The dark onset cues of Experiments 3a and 3b, as well as the placeholders and the central fixation point in Experiment 3b, had a luminance of 13.3 cd/m^2 ; $-1.8/-9.1$. The light onset cues of Experiments 3a and 3b, as well as the placeholders and the central fixation point in Experiment 3a, had a luminance of 137.7 cd/m^2 ; $-7.8/-28.4$. The dark and light stimuli were thus of equal stimulus-background Weber contrasts (± 0.82) and created equally strong luminance transients.

Results

RTs of Experiment 3a Overall, 6.46 % of all trials were removed. The significant main effects of quintile, $F(4, 60) = 6.37$, $p = .016$, and cue type, $F(1, 15) = 12.84$, $p = .002$, are both best explained in terms of their interaction, $F(4, 60) = 4.34$, $p = .004$. For target-color similar light onset cues, the cueing effect grew with RTs (21, 26, 32, 38, and 61 ms, respectively, for the 1st to the 5th quintile) and was significantly different in the 5th quintile than in the 1st, 2nd, and 3rd quintiles (all $ps < .028$). For the target-color dissimilar dark onset cues, the cueing effect differed nonsignificantly between 14 and 18 ms. In each quintile, the cueing effect of the light onset cue was

significantly larger than the cueing effect of the dark onset cue (all $ps < .022$). In addition, the cueing effect for the dark onset cue was not significantly different from zero in the fifth quintile, $t(15) = -1.09$, $p = .292$. For these results, see also Fig. 6.

ERs of Experiment 3a Participants made more errors in invalid (6.26 %) than in valid (5.17 %) trials, $F(1, 15) = 9.96$, $p = .007$. No other effects were found, all F s < 4.33 , all $ps > .521$.

RTs of Experiment 3b After the exclusion of errors (6.11 %), the ANOVA showed significant main effects of quintile, $F(4, 52) = 3.17$, $p = .028$, and cue type, $F(1, 13) = 21.99$, $p < .001$, but no interaction ($p = .149$). The cueing effect was significantly larger for target-color-similar dark (32 ms) than for dissimilar light onset cues (13 ms). Furthermore, the cueing effect grew numerically with RTs (16, 18, 21, 27, and 32 ms, respectively, from the 1st to the 5th quintile), but none of these differences were significant (all $ps > .110$).

ERs of Experiment 3b The ERs were higher in invalid (5.70 %) than in valid (4.65 %) trials, $F(1, 13) = 5.47$, $p = .036$. Furthermore, we observed a main effect of cue type, $F(1, 13) = 7.39$, $p = .018$. Participants made more errors with dark (5.69 %) than with light (4.66 %) onset cues.

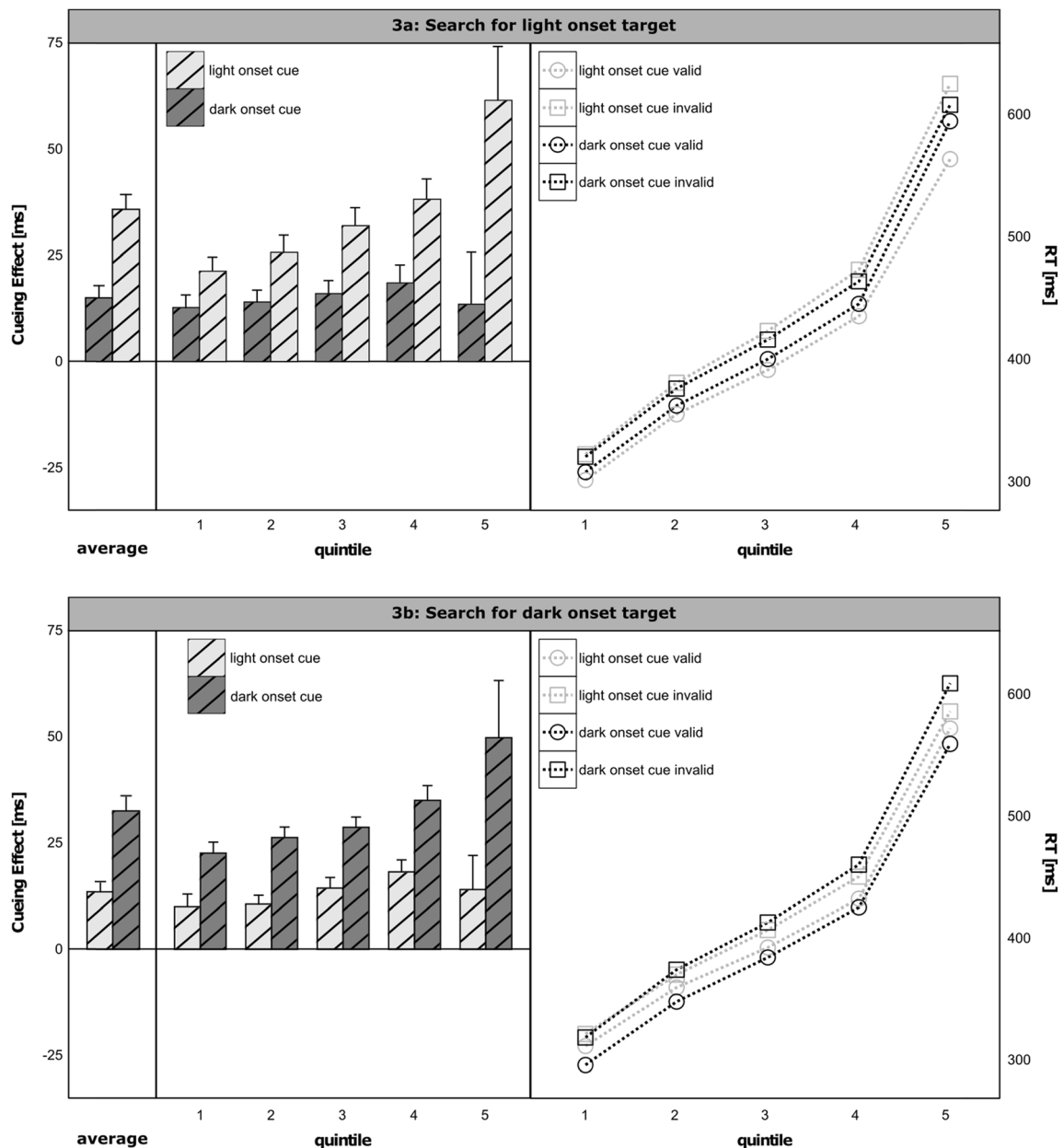


Fig. 6 The top panel depicts the results of Experiment 3a, and the bottom panel those of Experiment 3b

Discussion

Experiment 3 showed that when participants searched for an onset target of a particular achromatic color, onset cues of this particular color led to a stronger cueing effect than did onset cues of a different color but of equally strong contrast and luminance transient. During search for a light onset target, light onset cues captured attention more efficiently than dark onset cues (Exp. 3a), but during search for a dark onset cue this pattern reversed, and dark onset cues captured attention more efficiently than light ones (Exp. 3b). This pattern conceptually replicates the findings of Experiment 2 and again shows that the use of a color-directed attentional control

setting does not critically depend on the selective presence of a stronger luminance transient (or contrast difference) at the position of the onset cues. In addition, as in Experiment 2, we found residual cueing effects by onset cues of a color dissimilar to the searched-for target.

These results are only partly in line with prior findings by Fuchs et al. (2013), in which even reversed cueing effects for the less-matching target-color-dissimilar dark and light onset cues were found. A number of methodological differences might be responsible for the different results. For example, Fuchs et al. (2013) used only two possible cue and target positions, whereas in the present study four target positions were possible. Active suppression of an irrelevant color cue

might be easier with just two than with four potential cue positions. Also, the cue–target interval was shorter (16 ms) in Fuchs et al.’s than in the present study (150 ms). It is possible that a closer temporal proximity of the cue and target calls for more active suppression of the irrelevant cues, or that it took a while for the residual capture effect of the less-matching cues to build up, so that this was overlooked with the short interval. However, this conclusion is not supported by the present results, because the cueing effect of the less-matching dark cue in Experiment 3a did not increase as a function of RTs.

In contrast to Experiments 1 and 2, we also found an interaction between cue type and quintile, at least in Experiment 3a, but this interaction was not predicted by the deallocation account. The interaction was due to an increasing cueing effect in color-matching cue conditions across the quintiles of the RT distribution. Again, in line with a contingent-capture explanation, already among the fastest responses, the cueing effect of the light onset cue that would have matched a top-down control setting for target colors was stronger than that of the dark onset cue, which would not have matched the same control setting.

Experiment 4

Experiment 4 was a conceptual replication of Experiment 1b, but with luminance-equated light and dark color cues. In Experiment 4a, a light onset target was used, and in Experiment 4b, a dark onset target was used. In both experiments the color cues were equally likely to be light or dark. If contingent capture by color was at work, a cueing effect of light color cues (in Exp. 4a; of dark color cues in Exp. 4b), but not of dark color cues (in Exp. 4a; of light color cues in Exp. 4b) would be expected. In contrast to these predictions, on the basis of a top-down control setting for onsets, we would expect to find no cueing effects with any of the color cues. Finally, if the participants used an attentional control setting for singletons, or if just any singleton captured attention in a stimulus-driven way, a cueing effect in all of the cue type conditions would be expected.

Method

Participants Sixteen participants were tested in Experiments 4a (nine female, seven male; $M_{\text{Age}} = 23.69$ years, $SD_{\text{Age}} = 3.59$) and 4b (ten female, six male; $M_{\text{Age}} = 23.73$ years, $SD_{\text{Age}} = 9.11$), respectively.

Stimuli and procedure The stimuli and procedure were the same as in Experiment 3, with the exception of additional nonsingletons that were used in the displays (see Fig. 7): With a dark color cue, white nonsingletons were presented

around all three of the remaining placeholders, and with a light color cue, dark nonsingletons were shown around all three of the remaining placeholders.

Results

RTs of Experiment 4a Due to the usual data handling, 6.50 % of all trials were excluded. A main effect of cue type, $F(1, 15) = 142.28$, $p < .001$, was further modulated by its significant interaction with quintile, $F(4, 60) = 3.82$, $p = .008$. For the light color-matching cue, the cueing effect increased with RTs (32, 33, 34, 36, and 42 ms, respectively, from the 1st to the 5th quintile), although none of the differences between quintiles were statistically significant (all $ps > .790$). For the dark color-nonmatching cue, on the other hand, the cueing effect became numerically more negative (–13, –15, –15, –17, and –25 ms, respectively, from the 1st to the 5th quintile), but the differences were again not significant, all $ps > .200$. In each quintile, the cueing effects of the light color-matching cues were significantly larger than that of the dark color nonmatching cues, all $ps < .001$. For the results, see also Fig. 8.

ERs of Experiment 4a We observed no significant effects in the ERs, all $Fs < 2.25$, all $ps > .154$.

RTs of Experiment 4b Out of all trials, 5.82 % were removed from the analysis. We obtained results analogous to those of Experiment 4a: a main effect of cue type, $F(1, 14) = 106.83$, $p < .001$, that interacted with quintile, $F(4, 56) = 3.99$, $p = .006$. In each quintile, the cueing effect of the matching dark color cue was stronger than that of the nonmatching light color cue, all $ps < .001$. However, the interaction was due to the fact that the cueing effect of the dark color cues grew across RTs (24, 26, 30, 31, and 39 ms, respectively, from the 1st to the 5th quintile), with a significant difference between the 1st and 5th quintiles, $p = .019$. In contrast, for the light color cues, the cueing effect was more constant across RTs (–8, –7, –8, –9, and –9 ms, respectively, from the 1st to the 5th quintile), but it was not significantly different from zero in the fifth quintile, $t(14) = -1.71$, $p = .110$.

ERs of Experiment 4b The analysis of the ERs yielded no significant results, all $Fs < 2.00$, all $ps > .179$.

Discussion

Experiment 4 confirmed the findings of Experiment 1b: There was a cueing effect for light cues when participants searched for light targets, and for dark cues when participants searched for dark targets. In other words, the cueing effect was restricted to the top-down matching color cues. However, we even found reversed cueing effects, indicative of an active

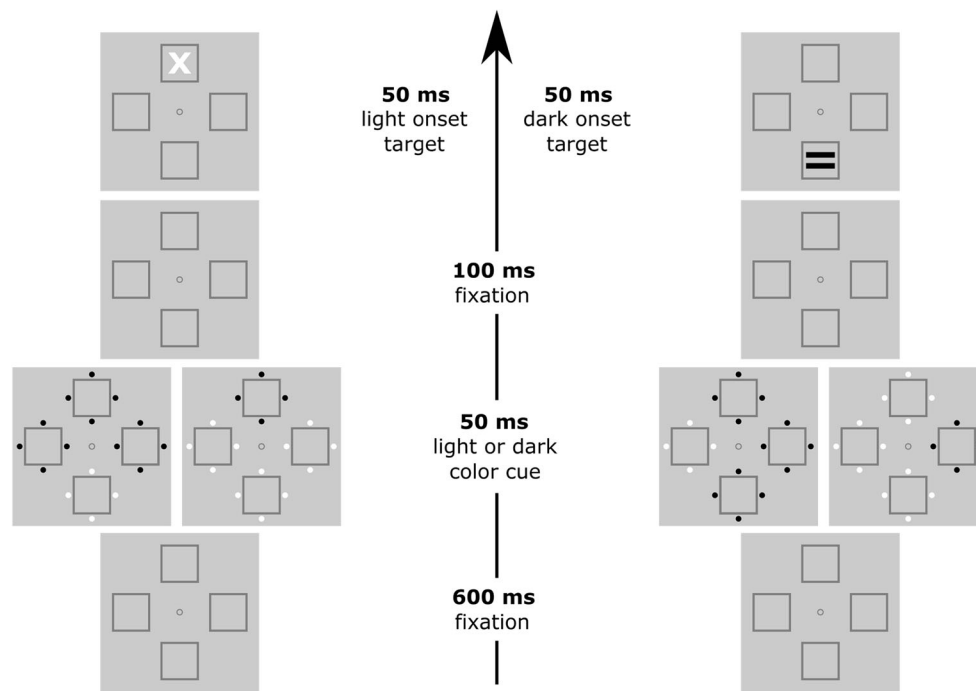


Fig. 7 Example sequences of displays in trials of Experiments 4a (left panel) and 4b (right panel). In Experiment 4a, participants had to identify a light onset target, and in Experiment 4b they

had to identify a dark onset target. In both experiments, the target was equiprobably preceded by a light or a dark color cue. The figure is not drawn to scale

suppression of the cues, if a nonmatching color cue was used. Together, these results cannot be explained by a set for luminance transients, because the strengths of the luminance changes were equated for the resulting contrasts in all conditions.

Additionally, the presence of a stronger cueing effect for top-down matching color cues than for nonmatching color cues throughout the RT distribution (from the fastest responses onward) is in line with a contingent-capture explanation. Again, however, as was noted already with respect to Experiment 1b, the presence of the nonmatching color cues might have invited the participants to select an attentional control setting for target colors instead of an attentional control setting for onsets. With an attentional control setting for colors, participants would have achieved two goals at the same time: They would have directed their attention to 100 % of the targets, and they would have ignored 50 % of the uninformative color cues—namely, the nonmatching cues with a color different than that of the targets.

Experiment 5

Taken together, Experiments 1–4 painted a relatively homogeneous picture: Cue–target color similarities contribute to attention capture by onset cues. However, in Experiments 2 and 3, it remained unclear what mechanism accounted for the residual cueing effects of the onset cues with a color different

from that of the onset targets. Following Experiment 2, we speculated that participants could have equally efficiently searched for the targets by their onsets or by a single target color. In general agreement with this possibility, recent findings have shown that participants base their inclusion of more than one feature into attentional control settings on the context—for instance, on the efficiency with which a feature can be used to find the targets (Mast & Frings, 2014).

To see whether context and efficiency affected the use of color or onsets in attentional control settings, and to get an idea of the origin of the residual cueing effects by onset cues with a target-dissimilar color in the present Experiments 2 and 3, we used two tasks in different blocks. In the “onset-target block” of Experiment 5, participants searched for two differently colored (red and white) onset targets simultaneously. In each trial, one of the two targets appeared randomly. Under these conditions, it was more efficient for the participants to search for onsets only than for colors, because it was possible to find all targets by searching for the single feature of onset. Hence, a top-down set for onsets was encouraged, and a top-down set for colors discouraged. If, under these conditions, participants searched for onsets only, we would expect to see similar cueing effects for both onset cues: cues with a target-similar color (e.g., red cues) and cues with a color dissimilar to all targets (blue cues, since the targets were red or white).

If, however, colors are an inherent property of any top-down set for onsets, participants should have continued to search for the particular target colors under the present

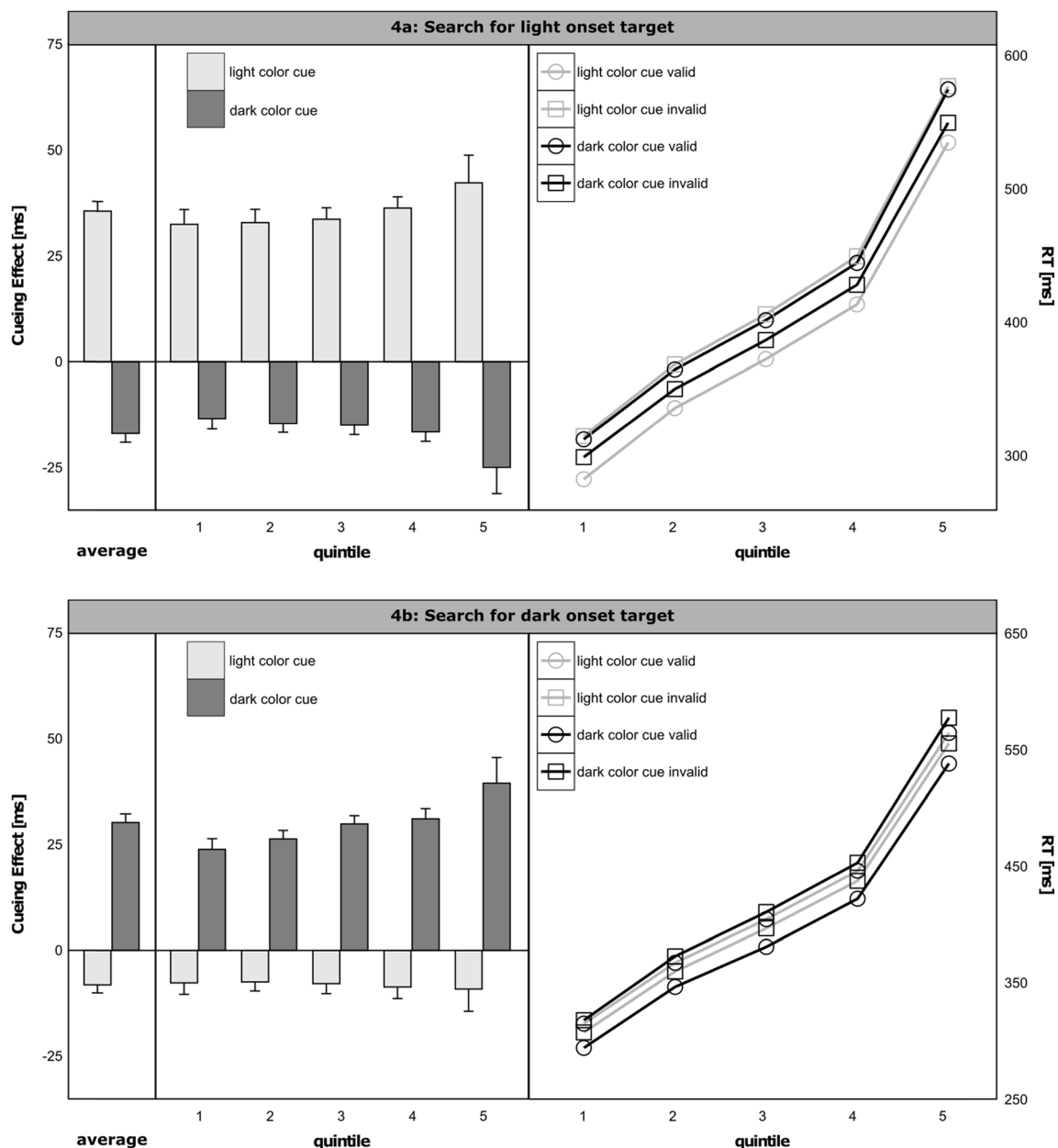


Fig. 8 The top panel depicts the results of Experiment 4a, and the bottom panel those of Experiment 4b

conditions, and we would have expected to observe a stronger cuing effect for onset cues of a target-similar color than for onset cues of a target-dissimilar color. In fact, if participants only searched for the two target colors and not at all for onsets, we would expect to find zero cuing effects by the cues of a target-dissimilar color. However, this is an unlikely outcome in light of the results of Experiments 2 and 3, in which onset cues of a target-dissimilar color captured attention at least to some degree.

To further explore the hypothesis that efficiency determined the inclusion of color in the top-down set, and to better understand the origin of the residual cuing effect by onset cues of a target-dissimilar color, we used a second blocked

task (the “white-target block”) with onset and color targets, in which a top-down set for color was encouraged. In this white-target block, participants searched for white onset targets and white color targets at the same time (because these two target types were presented in an unpredictable manner). If the efficiency of top-down search for onsets was responsible for the residual cuing effects of onset cues with a target-dissimilar color in Experiments 2 and 3, in the present situation, participants should have preferentially searched for the single color to find all of the targets, rather than keeping top-down sets for both onsets *and* colors. This prediction was tested by the use of two different onset cues in the second blocked condition: white onset cues with a target-similar color, and red onset cues

with a target-dissimilar color. If, under these conditions, participants abandoned their top-down sets for onsets entirely, we would find a cueing effect by onset cues with a target-similar color (i.e., the white onset cues), but a zero cueing effect by the onset cues with a target-dissimilar color (i.e., the red onset cues). Note how this prediction differs if the residual cueing effect of onset cues of a target-dissimilar color were due to stimulus-driven capture: If any onset cue captures attention to some degree, we would expect to see a (reduced but significant) cueing effect even for the onset cue with a target-dissimilar color.

The second block also allowed us to test yet another contribution to the residual cueing effect of the onset cues with a target-dissimilar color in Experiments 2 and 3: If the presence of an onset target in trial $n-1$ primed capture by an onset cue in the following trial n , we should find evidence of residual cueing effects of the onset cues with a target-dissimilar color, at least in those trials n that followed an onset target in $n-1$, but less so, or maybe not at all, in those trials n that followed a color target in $n-1$ (cf. Awh, Belopolsky, & Theeuwes, 2012).

Method

Participants, stimuli, and procedure Because the residual cueing effect of the onset cues with a target-dissimilar color was expected to further decrease (maybe to zero), in comparison to Experiments 2 and 3, the number of participants was increased to also increase the power of our test for the residual cueing effects. Twenty-six participants were tested (15 female, 11 male; $M_{\text{Age}} = 22.85$ years, $SD_{\text{Age}} = 4.15$). The stimuli were the same as in Experiments 1 and 2, except as noted. The experiment consisted of two different blocks (see also Fig. 9). In the onset-target block, each trial could feature either a red or a white onset target. Both targets were equally likely and randomly intermixed. The targets were preceded by either a red or a blue (25.8 cd/m^2 ; CIE Lab: 49.7/–98.5) onset cue. In the white-target block, we used white onset targets (that were presented in isolation) and white color targets (that were presented simultaneously with red distractors in the remaining placeholders). Targets were equally likely to be preceded by red or white onset cues.

Results

RTs Data handling was the same as in Experiment 1 and led to the exclusion of 6.91 % of all trials. We conducted separate analyses for the two experimental blocks. Each ANOVA contained the within-subjects variables RT quintile (1–5) and cue type (red vs. blue onset cue in Block 1; white vs. red onset cue in Block 2).

In the onset-target block, we only found a significant effect of RT quintile, $F(4, 100) = 17.17$, $p < .001$. The cueing effect increased across quintiles (17, 20, 24, 30, and 53 ms), but the

only significant differences were found between the fifth and all other quintile (all $ps < .001$). No other significant results were found, all $F_s < .21$, all $ps > .65$.

The white-target block yielded a different result. We found a main effect of cue type, $F(1, 25) = 130.66$, $p < .001$, indicating that the cueing effect elicited by white onset cues (30 ms) was larger than the cueing effect elicited by red onset cues (–5 ms). The cueing effect of the red onset cues was not different from zero, $t(25) = -1.80$, $p = .084$. Additionally, we found a main effect of quintile, $F(4, 100) = 17.44$, $p < .001$, indicating an increase of the cueing effect across RTs (3, 7, 12, 16, and 24 ms, respectively, from the 1st to the 5th quintile). The cueing effect in the first quintile was not different from zero, $t(25) = 1.39$, $p = .180$. The cueing effect in the fifth quintile was significantly larger than those in the first quintile ($p < .001$) and the second quintile ($p = .011$). Figure 10 illustrates the results.

Despite the facts that the color-dissimilar cue in the white-target block did not lead to significant cueing effects and that the conditions for a test of the origin of the residual cueing effect of these cues were not ideal, we conducted a repeated measurements ANOVA, with the cueing effect as a dependent variable and cue type, target type, and target type in trial $n-1$ as independent within-subjects variables. Besides the results reported above, we found an interaction between cue type and target type in trial $n-1$, $F(1, 25) = 4.96$, $p = .035$. In trials with red onset cues, there was a small cueing effect following a white onset target in $n-1$ (7 ms), but an inverted cueing effect after a white color target in $n-1$ (–8 ms), $t(25) = 2.31$, $p = .029$. This result supports the hypothesis that the residual cueing effect of red onset cues is due to intertrial priming of attention. Also of interest, in trials with white onset cues, the cueing effects were similar regardless of whether a white onset target (30 ms) or a white color target (34 ms), $t(25) = -0.72$, $p = .480$, had been shown in trial $n-1$. This finding points to independent origins of top-down contingent capture by color (here, of the white cues) and of capture by cues with a feature that was primed by a target in $n-1$.

ERs The arcsine-transformed ERs were subjected to a repeated measurements ANOVA, with the within-subjects variables validity, cue type, and experimental block. We only found a main effect of validity, $F(1, 25) = 18.97$, $p < .001$, with higher ERs with invalid (6.58 %) than with valid (5.61 %) cues. Additionally, a strong trend toward an effect of experimental block was apparent, $F(1, 25) = 4.20$, $p = .051$, with higher ERs in the white-target block (6.88 %) than in the onset-target block (5.30 %). No other effects were found, all $F_s < 3.49$, all $ps > .073$.

Discussion

In the onset-target block, during search for red and white onset targets, blue target-color-dissimilar cues and red target-color-similar onset cues captured attention with similar efficiency.

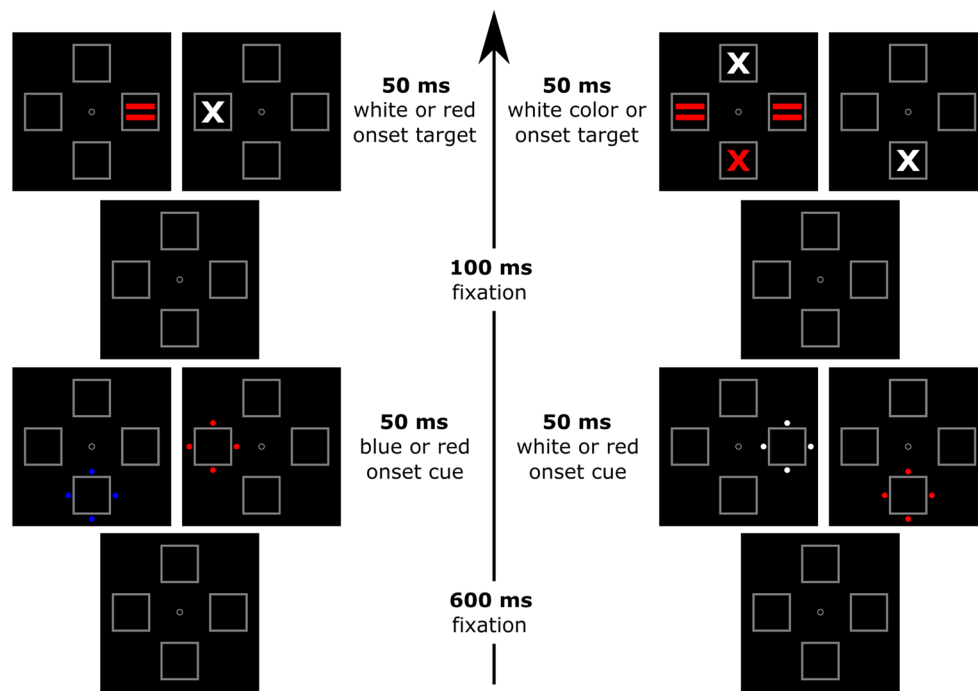


Fig. 9 Example sequences of displays in trials of Experiments 5. The sequence on the left depicts the onset-target block, in which participants searched for a red and a white onset target. These target displays were preceded by red or blue onset cues. The panel on the right depicts the

white-target block, in which participants searched for a white onset target and a white color target. Targets were preceded equiprobably by red or white onset cues. The figure is not drawn to scale

Cue–target color similarity therefore did not result in a stronger cueing effect. This indicates that participants probably adopted an attentional control setting for onsets rather than for different colors. The results are in line with a role of efficiency considerations among the participants when they were setting up their attentional control settings. If all targets could be efficiently found by a searching for onsets but if different target colors would imply searching for different colors, participants seemed to use onsets rather than colors to search for the targets. In the present experiment’s onset-target block, search for onsets was more efficient than search for colors, so the fact that cue–target color similarities did not play a role for the cueing effect supports a top-down explanation of color contributions to the cueing effect of onset cues.

In contrast to the onset-target block, the white-target block of the present experiment showed that participants adopted an attentional control setting for color. This attentional control setting for color was apparently very effective in eliminating the residual cueing effect of the onset cues with a target-dissimilar color: On average, in the white-target block we found no cueing effect of the red onset cues, although half of the trials did contain a (white) onset target. We think that the absence of an average cueing effect of the red onset cues, which contrasts with the residual cueing effect of the same cues in Experiment 2a, is due to the fact that an attentional control setting for one color was necessary and sufficient to search for all targets of Experiment 5.

On closer inspection, however, we found that a small cueing effect of the red onset cues might have been obtained in target-color blocks in which an onset target in the preceding trial primed attention capture by an onset cue in the immediately following trial. This finding is in line with the observation that intertrial priming of attention by feature repetitions (Maljkovic & Nakayama, 1994) is one source of cueing effects (Belopolsky, Schreij, & Theeuwes, 2010). Additionally, the same intertrial priming effects were not found with the white onset cues. It seems that intertrial priming of attention could not account for the cueing effect of the onset cues with a target-similar color. Instead, these cues probably captured attention in a top-down way—that is, due to their color’s match to the top-down control settings set up to search for the white targets, and regardless of whether these features were primed by target features in $n-1$.

General discussion

In the present research, we set out to study a simple question: Are cueing effects in onset-target conditions of contingent-capture experiments due to attentional control settings directed to abrupt onsets, or are they due to control settings directed to target colors? The results clearly supported a role of attentional control settings for colors. When participants were searching for a white onset target, we found a cueing effect

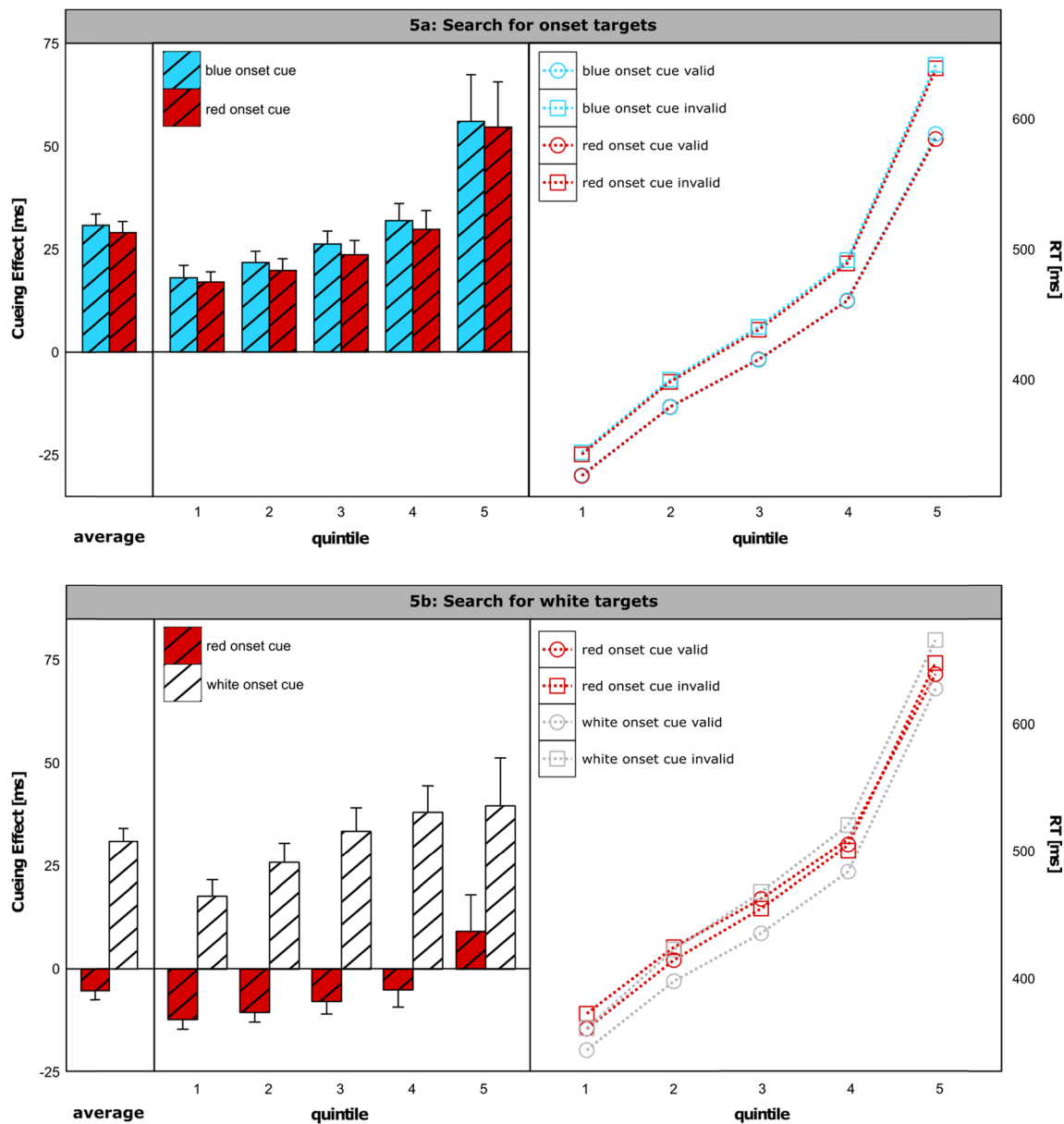


Fig. 10 The panel at the top depicts performance in the onset-target block of Experiment 5, and the bottom panel depicts performance in the white-target block of Experiment 5

of white onset cues as well as of white color cues, although the white color cues were presented together with red color distractors (in Exps. 1a and 1b) or with dark color distractors (in Exp. 4a), so that it was impossible for these color cues to capture attention by their onsets alone. In contrast to the white color cues, red color cues (in Exp. 1b) or black color cues (in Exp. 4a) did not induce cueing effects during search for white onset targets. Thereby, explanations in terms of a stimulus-driven capture effect by just any pop-out stimulus (e.g., Theeuwes, 2010), or in terms of a contingent-capture effect based on an attentional control setting for such pop-out singletons (e.g., Bacon & Egeth, 1994), were ruled out as potential alternative explanations. Also, in control conditions with

dark onset targets, the pattern of results was reversed, so that standard cueing effects were restricted to dark color cues and absent for light color cues (Exp. 4b).

In addition, we also found stronger cueing effects of onset cues that had a color similar to the searched-for onset target. This stronger cueing effect was found relative to onset cues with a color dissimilar to that of the onset target. For example, when the participants searched for a red onset target, we observed stronger cueing effects of red than of white onset cues, a pattern that reversed during search for white onset targets (Exp. 2). Analogous results were found during search for light versus dark onset targets (Exp. 3). These experiments showed that participants might have used top-down control settings

directed to onset targets (at least on some of the trials), but that top-down control settings directed to target colors were probably also at work.

This interpretation was supported in Experiment 5. In Experiment 5, participants either searched for two onset targets of different colors or for two white targets, half of which were onset targets. Under these conditions, attentional control settings for onsets and for the color white, respectively, were encouraged. In line with the view that residual capture by target-color-dissimilar onset cues in Experiments 2 and 3 were due to control settings for onsets, in Experiment 5 color-dissimilar onset cues did not capture attention during search for white onset and color targets, but they did capture as much attention as color-similar onset cues during search for red and white onset targets. Together, these results suggest that even the residual cueing effect of the target-dissimilar cues in Experiments 2 and 3 was contingent on the participants' use of an appropriate top-down attentional control setting—a conclusion that has also been reached on prior research (Folk et al., 1992).

Furthermore, the stronger cueing effects of cues with a color matching that of the searched-for targets than of cues with a color different from the targets were present from the fastest responses onward. This observation is in line with the theory of top-down contingent capture because, according to this theory, participants would set up their attentional control settings in advance, so that each stimulus that matched the setting would have a chance of capturing attention more efficiently from the fastest responses onward (Ansorge & Horstmann, 2007; Burnham, 2013). In the present study, we found no direct support for the deallocation theory, which predicts an increase of the cueing-effect difference between matching and nonmatching cues across time. Of course, it is possible that the cue–target interval was simply too long for this predicted pattern of deallocation. For example, on the basis of the data of Kim and Cave (1999), one would expect that deallocation could have taken effect within 100 ms—a time considerably shorter than the 150-ms interval between cue and target that was used here. Thus, theoretically, deallocation might have already taken place even before the fastest responses in the present study. Note, however, that other studies that have varied the cue–target interval showed contingent-capture effects—stronger cueing effects of top-down matching than of nonmatching cues—with much shorter intervals (Ansorge & Heumann, 2003; Ansorge & Horstmann, 2007; Remington, Folk, & McLean, 2001). The conclusion that deallocation does not play a prominent role is also supported by event-related potential studies. In such studies, attentional capture by the cue can be continuously tracked with millisecond precision from cue onset onward, and generally, no evidence of early capture by nonmatching color cues can be observed in such studies (Ansorge, Kiss, Worschech, & Eimer, 2011; Eimer & Kiss, 2008; see also McDonald, Green, Jannati, & Di Lollo, 2013).

One might also question whether the stronger cueing effect of the top-down matching color cues reflected (only) contingent capture. It might equally well be that intertrial priming of capture contributed to the cueing effect (Awh et al., 2012). According to this theory, the allocation of attention to a target in a preceding trial $n-1$ might have boosted attention capture by a similarly colored top-down matching cue in the current trial n (cf. Maljkovic & Nakayama, 1994; Theeuwes, 2013). Note that a corresponding priming of capture could not have taken effect with those nonmatching cues that were always of a color different from that of the preceding target. In Experiments 1–4 of the present study, it was impossible to test for intertrial priming because only one target was used, and therefore the same cue types were primed in each and every trial. On the basis of the existing literature, however, it is clear that the intertrial priming of capture does not account for the full strength of the contingent-capture effect (see Ansorge & Horstmann, 2007; Folk & Remington, 2010; Worschech & Ansorge, 2012; for a review, see Lamy & Kristjánsson, 2013). Also, as the results of Experiment 5 show, during search for onset targets, the residual cueing effect of onset cues with a nonmatching color can be attributed to intertrial priming, but not that of the onset cues with a matching color.

The results of Experiment 5 also suggest one other consideration. It has been suggested that onsets (transient features) elicit stimulus-driven capture of attention, whereas color elicits top-down contingent capture (Liao & Yeh, 2013). The results of Experiments 2 and 3 would be in line with this explanation. Onsets could have captured attention automatically, and color would have worked on top of this automatic capture. However, as Experiment 5 showed, the residual capture effect of the onset cues in the present study was a consequence of top-down sets. If a target-color search was suggested by its greater economy (as in Exp. 5's white-target block), no capture by onset cues of a nonmatching color was found, unless an onset target in the preceding trial had primed capture by an onset cue in the immediately following trial. Yet it is possible that more evidence for a stimulus-driven cueing effect of onset cues with a target-dissimilar color not matching the control settings could be found with a shorter cue–target interval than was used here.

In a broader perspective, our results can be regarded as further evidence that top-down attentional control settings are not as simple as has previously been assumed. For example, recently the idea has been advocated that control settings might be directed to combinations of features, such as color and position (Adamo et al., 2008) or different colors (Irons et al., 2012), or even that semantic categories could be used to selectively attend to particular classes of stimuli (Wyble, Folk, & Potter, 2013). Along similar lines, it has been shown that control settings vary as a function of the task context—that is, color could be included flexibly within control settings, depending on how helpful color is for telling apart targets from distractors (Mast & Frings, 2014), and even control settings

for nonmatching cues seem to be flexibly used by participants when recent cue utility suggests doing so (Goller & Ansorge, 2015). These findings were generally supported by the present research, in which we showed that attentional capture can depend on both onset and color information (Exps. 2 and 3), and that it seems to be a question of how efficiently the targets can be selected that determines which features are incorporated into top-down attentional control settings (Exp. 5).

Conclusion

The present study shows that cue–target color similarity has a boosting effect on attentional capture by onset cues during search for onset-defined targets. A contingent-capture effect based on a match of the cues' onsets to the top-down control settings directed to target onsets was therefore overestimated in past studies that had used the classic contingent-capture protocol of Folk et al. (1992). Future studies should avoid confounding influences of color matches on top-down contingent-capture effects based on stimulus onsets, in order to provide an appropriate estimate of these effects. Furthermore, our results add to the growing body of evidence showing that top-down attentional control settings are more complex than hitherto has been thought, that they can comprise multiple features, and that the control setting depends on the task context.

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FG and UA developed the study concept and the study design. FG programmed and conducted the experiments, data analysis and interpretation. FG drafted the manuscript, UA added to the manuscript and provided critical revisions. Both authors approved the final version of the manuscript for submission.

There is more to trial history than priming in attentional capture experiments

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Abstract We used contingent attentional capture to investigate whether capture in a given trial n was affected by the cue–target position relations in a preceding trial $n-1$. Typically, attentional capture by a cue facilitates reaction times for targets in valid conditions (with the cue and target at the same position) relative to invalid conditions (with the cue and target at different positions). Also, this validity effect holds for cues with a feature similar to the searched-for target features (i.e., matching cues), but not for cues dissimilar to the searched-for target features (i.e., nonmatching cues), a pattern termed *contingent capture* because capture is assumed to be contingent on the match between the cue and top-down control settings. Here, we replicated this contingent-capture pattern with cues that were nonpredictive of the target position. In addition, we showed that during search for white onset targets, red nonmatching color cues also created a validity effect if the same nonmatching cue had been used as a valid cue in trial $n-1$ (Exps. 1 and 2). This intertrial contingency of the nonmatching cue’s validity effect was also found if the cues and targets both changed their positions from trial to trial, rendering position priming unlikely (Exp. 2). A similar intertrial contingency was found for nonmatching white onset cues, but not for matching red color cues during search for red color targets (Exp. 3). These results are discussed in light of explanations of the contingent-capture effect and of intertrial contingencies.

Keywords Cognitive and attentional control · Attention · Attentional capture

According to contingent attentional capture theory, visual cues attract attention to the degree that they match to the top-down attentional control settings (Folk, Remington, & Johnston, 1992). Using cues that were not predictive of the target position, Folk et al. found that only top-down matching cues captured attention: A valid cue at the same position as the target facilitated finding the target, as compared to an invalid cue presented away from the target. This validity effect was attributed to the capture of attention by the cue (Posner, 1980). Importantly, the validity effect was absent with cues that did not match the top-down settings. For example, a validity effect of white onset cues was found only if participants searched for white onset targets, but the validity effect was absent if participants searched for red color targets. Folk et al. reasoned that the participants must have set up top-down control settings to search for the target features, and that the cue must have matched these settings to capture attention. In line with this theory, these authors also found the complementary result pattern of a validity effect of red color cues only if participants searched for red color targets, but not if they searched for white onset targets.

An alternative theory proposes that selection history or intertrial priming of attentional capture might contribute to this contingent-capture effect (Awh, Belopolsky, & Theeuwes, 2012; Theeuwes, 2010; but see Ansorge & Horstmann, 2007; Ansorge, Kiss, Worschech, & Eimer, 2009; Eimer & Kiss, 2008; Folk & Remington, 2008). For example, attentional capture by a red target in trial $n-1$ might prime capture by a matching red cue in a subsequent trial n by way of cue–target color similarity (cf. Maljkovic & Nakayama, 1994). Clearly, a similar priming of attention would not be possible

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with the nonmatching cues, because their color would always differ from that of the target in trial $n-1$.

Here we set out to test a different influence of selection history: the cue–target position relations in trial $n-1$. A number of studies have suggested that attentional capture by a cue (or a distractor) could be stronger after preceding valid trials (or preceding trials without a distractor) than after preceding invalid trials (Jongen & Smulders, 2007; Mordkoff, Halterman, & Chen, 2008; Müller, Geyer, Zehetleitner, & Krummenacher, 2009; Qian, Shinomori, & Song, 2012). The corresponding *intertrial contingency* of the validity effect could be due to a number of different processes (Mayr, Awh, & Laurey, 2003; Schlaghecken & Martini, 2012; Schmidt, 2013). For example, it could reflect the participants' expectancy of the usefulness of the cue (cf. Gratton, Coles, & Donchin, 1992). Alternatively, it might also be due to the active suppression of a stimulus that led to conflict (here: of an invalid cue) in $n-1$ (Botvinick, Braver, Barch, Carter, & Cohen, 2001). Whatever its exact origin, this intertrial contingency has never been studied in contingent-capture experiments. The present study closes this gap.

Experiment 1

We were interested in the effect of the cue–target position relations in $n-1$ on the validity effects from average nonpredictive cues in trial n . These intertrial contingencies of the validity effect were assessed for matching and nonmatching cues. For example, intertrial contingencies of validity effects could be characteristic of all forms of cueing, such as cueing by both matching and nonmatching cues. Alternatively, some forms of cueing, such as by nonmatching cues, might be particularly prone to intertrial contingencies. These were the questions that we addressed using search for onset targets (see Folk et al., 1992).

Method

Participants A total of 28 participants (19 females, nine males; $M_{Age} = 22.93$ years, $SD_{Age} = 3.22$) were recruited from among the psychology students of the University of Vienna. Here and in all following experiments, participants received partial course credit, had normal or corrected visual acuity and normal color vision. Prior to the experiments, informed consent was obtained from all participants. In Experiment 1, one participant was excluded because of an error rate (ER) exceeding 20 %. The ERs of the remaining sample ranged between 1.43 % and 15.89 %.

Apparatus Stimuli were presented on a 19-in. LCD Monitor (Acer B 193) with an aspect ratio of 4:3 and a resolution of 1,024 × 768 pixels at a vertical refresh rate of 75 Hz. The

viewing distance was kept stable at 57 cm by a chin-and-forehead rest. Manual responses were recorded as keypresses of the left and right index fingers on a keyboard. The experiment was programmed and controlled using MATLAB 7.7.0 (The MathWorks Inc., Massachusetts, USA) and the Psychophysics Toolbox (Brainard, 1997).

Stimuli All stimuli were presented against a black (CIE Lab: 0.8 cd/m²; 0.1/–0.1) background. Four gray (CIE Lab: 75.5 cd/m²; –5.3/–18.8) square-shaped outline placeholders (1.5° × 1.5°) were presented at the corners of an imaginary diamond around a gray central fixation point (0.5°). The center-to-center distance between the fixation point and the placeholders was 5.0°. On each trial, a white (CIE Lab: 137.7 cd/m²; –7.8/–28.4) onset target was shown at the center of one of the placeholders. This target could equally likely be an “X” or an “=” (1.33° × 1.33°). The cues were realized as four filled dots (diameter: 0.25°) centered on the placeholders, with the dots being 0.13° away from the placeholders' edges. In the case of the color cues, these dots were red (CIE Lab: 73.9 cd/m²; 73.9/98.7) for one placeholder and white for the remaining three placeholders. In the case of the onset cues, the dots appeared in white color around one of the four placeholders only.

Procedure Figure 1 illustrates the sequence of events in a trial. After fixation (600 ms), the cueing display was presented for 50 ms, followed by another fixation display for 100 ms. Next, the target display was shown for 50 ms. After the offset of the target, a blank screen was presented until participants responded or the timeout was reached (see below). The stimulus–response mappings were kept constant for each participant but counterbalanced across participants. Reaction times (RTs) were measured to the nearest millisecond as the time intervals between target onset and a keypress. If no keypress was registered within 1,550 ms, participants received an on-screen instruction to react faster, and the trial was counted as an error. In total, the experiment consisted of a minimum of 1,536 trials, but since error trials were repeated at the end of the experiment, the exact duration of the experiment varied slightly across participants.

Half of all cues matched the search settings for the target feature (i.e., the white onset cues), whereas the other half did not (i.e., the red color cues). From trial to trial, the cue positions, target positions, and target identities varied randomly, but across trials, both cues and targets were counterbalanced across all four locations. The cue and target positions were 25 % valid, 75 % invalid, and uncorrelated across trials. Invalid cues appeared randomly but equally often at each of three positions away from the target. Cues were also not informative about the target's identity. In addition, we presented each of the two possible targets equally often. Every 384 trials (25 % of all trials), there was a self-paced break. Before the start of the experiment, participants received 20 practice trials

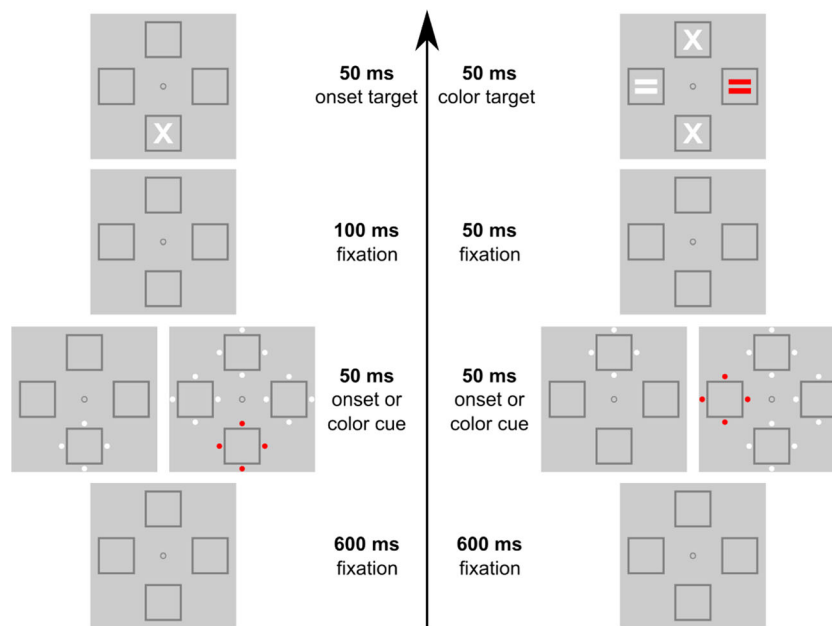


Fig. 1 Example sequences of Experiments 1 and 2 (left side) and Experiment 3 (right side). In each experiment, both color and onset cues were used, whereas the target was of a constant feature throughout

the experiment (onset targets in Exps. 1 and 2, and color targets in Exp. 3). The figure is not drawn to scale and in both experiments, a black background color was used

that were chosen randomly from the pool of experimental trials. During practice, participants received on-screen feedback about their answer.

Unlike in Folk et al. (1992), participants only searched for an onset target; there was no second block with a color target. The search for a color target was realized in a separate experiment with different participants (see Exp. 3).

Results

Reaction times We based the analysis of correct RTs on trials following a correct trial to ensure that participants had applied target-search settings in $n-1$ as intended. Out of all correct trials, 7.51 % that following an error were excluded. Furthermore, RTs below 150 ms were excluded (an additional 0.04 %). We computed validity effects as the mean correct RTs in invalid minus the mean correct RTs in valid trials, separately for each participant and condition. Unless otherwise stated, all validity effects (or their reversals; see below) are significantly different from zero (all $ps < .05$). All results in this article are reported in this way. Validity effects were subjected to a repeated measures analysis of variance (ANOVA), with $n-1$ cue type ($n-1$ matching, $n-1$ nonmatching), $n-1$ validity ($n-1$ valid, $n-1$ invalid), and n cue type (n -matching, n -nonmatching) as within-subjects variables. Table 1 (top rows) provides a detailed overview of the RTs and ERs of Experiment 1.

The validity effect was stronger following an $n-1$ valid trial (23 ms) than following an $n-1$ invalid trial (11 ms), indicated by a main effect of $n-1$ validity, $F(1, 26) = 10.61, p < .01$. A

main effect of n cue type, $F(1, 26) = 105.24, p < .01$, reflected a validity effect that was only present with n -matching cues (36 ms), but absent with n -nonmatching cues [−2 ms; t test against zero: $t(26) = -1.02, p = .32$]. These results correspond to the classic finding of contingent capture. Furthermore, we found an interaction between $n-1$ validity and n cue type, $F(1, 26) = 11.77, p < .01$, which is best considered in light of the also-significant interaction between all variables, $F(1, 26) = 6.15, p = .02$, which we will discuss next (see also Fig. 2). All other effects did not reach statistical significance, all nonsignificant F s < 2.30 , all $ps > .14$.

We conducted separate follow-up ANOVAs split up for the levels of n cue type to analyze more closely how n -matching and n -nonmatching trials were influenced by $n-1$ cue validity and $n-1$ cue type. In n -matching trials, we found no evidence of an intertrial contingency, but only an interaction between $n-1$ cue type and $n-1$ validity, $F(1, 26) = 4.23, p = .05$. Post-hoc comparison of the validity effects after an $n-1$ valid versus an $n-1$ invalid trial (separately for the levels of $n-1$ matching) revealed no significant differences (all $ps > .20$). No other effects were found in the follow-up analysis of the n -matching trials, all nonsignificant F s < 0.44 , all $ps > .51$.

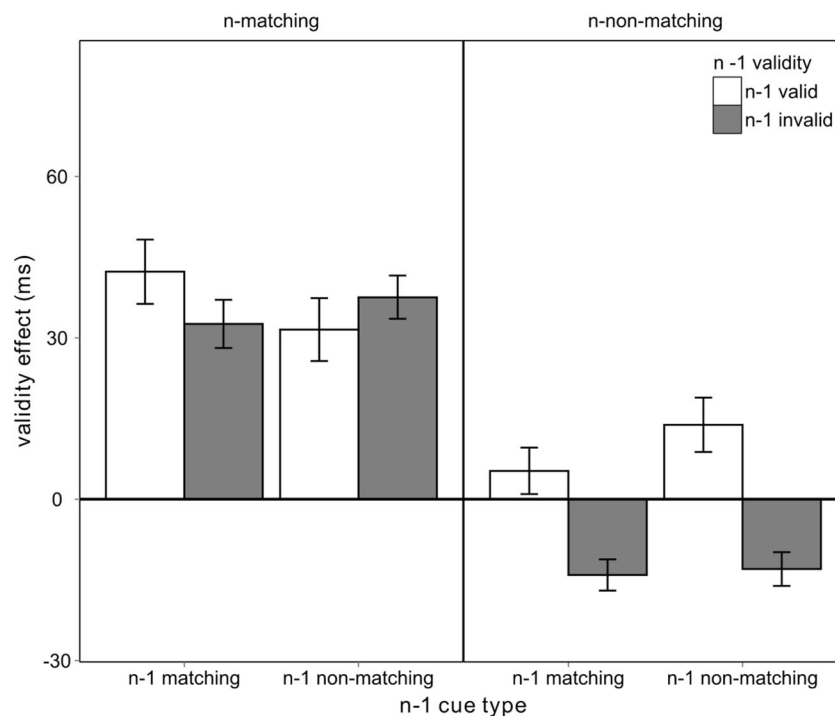
Looking at n -nonmatching trials, we found an effect of $n-1$ validity, $F(1, 26) = 31.46, p < .01$, reflecting an intertrial contingency: The validity effect was present following $n-1$ valid trials (10 ms) but was reversed following $n-1$ invalid trials (−13 ms). No other effects were found in the follow-up analysis of the n -nonmatching trials, all nonsignificant F s < 1.31 , all $ps > .26$.

Table 1 Reaction times (RTs; in milliseconds) and error rates (ERs; in %) as a function of cue type (matching vs. nonmatching) and cue validity (valid vs. invalid) for both trials n and $n-1$ in Experiment 1 (upper rows), Experiment 2 (middle rows), and Experiment 3 (lower rows)

			Trial n							
			Matching Cue				Nonmatching Cue			
			Valid		Invalid		Valid		Invalid	
	Trial $n-1$		RT	ER	RT	ER	RT	ER	RT	ER
Experiment 1	Matching cue	Valid	449	7.80	491	7.41	466	6.92	471	7.53
		Invalid	451	7.46	483	7.10	481	6.98	467	6.54
	Nonmatching cue	Valid	454	9.66	485	7.68	458	6.89	472	7.56
		Invalid	452	6.07	489	6.61	480	7.86	467	6.11
Experiment 2	Matching cue	Valid	433	5.87	470	8.01	452	5.85	448	6.56
		Invalid	435	6.48	456	8.24	450	6.15	437	6.93
	Nonmatching cue	Valid	440	6.35	470	7.61	437	5.92	457	7.62
		Invalid	431	7.70	455	8.07	450	6.81	442	7.79
Experiment 3	Matching cue type	Valid	450	5.45	522	6.37	472	5.10	482	4.11
		Invalid	427	5.61	506	7.93	479	4.26	477	3.78
	Nonmatching cue	Valid	436	4.71	513	8.27	462	4.33	485	2.39
		Invalid	427	5.47	501	7.93	478	4.22	471	3.58

Error rates Validity effects were computed from the arcsine-transformed ERs and analyzed analogously to the RTs. For better transparency, the means of the untransformed ERs are reported. An interaction between $n-1$ validity and n cue type, $F(1, 26) = 7.57, p = .01$, showed that for n -matching trials, the

validity effects following an $n-1$ valid trial (1.55 %) and following an $n-1$ invalid trial (0.73 %) were not different from zero (both $ps > .28$). For n -nonmatching trials, we found a strong trend toward a validity effect following an $n-1$ valid trial [2.01 %; t test against zero: $t(26) = 1.98, p = .06$], whereas

**Fig. 2** Validity effects (n -invalid RT – n -valid RT) as a function of n cue type (n -matching, left panel; n -nonmatching, right panel), $n-1$ validity ($n-1$ valid, white bars; $n-1$ invalid, gray bars), and $n-1$ cue type (x-axis) in Experiment 1 (onset search). Error bars represent the standard errors of the means (SEMs)

no such trend was found following $n-1$ invalid trials [0.65 %; t test against zero: $t(26) = -1.36$, $p = .18$]. No other significant effects were found, all nonsignificant F s < 2.23, all p s > .15.

Discussion

Experiment 1 yielded two main findings. First, a larger validity effect in top-down matching than in nonmatching trials reflected contingent capture. Interestingly, this effect was only present on average. A closer look revealed that in the n -nonmatching conditions, the null effect was due to averaging across standard and inverted validity effects. Second, a larger validity effect following $n-1$ valid than $n-1$ invalid trials corresponded to an intertrial contingency of the validity effect. Follow-up analyses of the four-way interaction between all variables demonstrated that the intertrial contingency was restricted to the n -nonmatching conditions. Here, we only found a validity effect following $n-1$ valid trials, whereas this effect reversed after $n-1$ invalid trials. For the n -matching conditions, we found a tendency toward such an intertrial contingency, but no significant pairwise comparisons. Furthermore, in the ERs, we found a trend toward a validity effect after $n-1$ valid trials, but only in the n -nonmatching condition. No such trend was found in the n -matching condition. In addition to the validity effects in RTs, this result might be regarded as further weak evidence for intertrial contingencies of nonmatching cues.

These results show that more evidence for attentional capture can be found in nonmatching conditions when one scrutinizes the influence of the trial history with respect to factors beyond color priming. The influence of more or less conflict between relevant targets and irrelevant cues in trial $n-1$ on validity effects can be attributed to one of several possible sources. For example, according to the influential conflict-monitoring hypothesis of Botvinick et al. (2001; Carter & van Veen, 2004), participants would try to shield themselves more from irrelevant information that has been harmful to their performance than in a situation in which this information has been helpful. According to this explanation, active suppression of the nonmatching (irrelevant) cue following an invalid trial would imply that a target at the cued position could also be affected by (partial) suppression of this position. As a consequence, the validly cued target on trial n would be processed at slightly higher costs than an invalidly cued target (cf. Anderson & Folk, 2012; Belopolsky, Schreij, & Theeuwes, 2010; Carmel & Lamy, 2014). This is exactly the pattern that we observed for the n -nonmatching cues following an $n-1$ invalid trial.

But why was no similar intertrial contingency found with the matching cues? One reason could be that suppression was easier with the nonmatching cues. For example, active deallocation could be one of the means by which conflict is regulated. According to Theeuwes, Atchley, and Kramer

(2000), participants are better at deallocating their attention away from nonmatching cues than from matching cues. Thus, the stronger validity effects with n -matching than with n -nonmatching cues could be due to a faster and more efficient deallocation of attention with an n -nonmatching than with an n -matching cue (Theeuwes et al., 2000). In the n -matching conditions, too little deallocation could have prevented successful conflict control after an $n-1$ invalid trial.

Alternatively, active suppression could have nothing to do with attentional capture (Carmel & Lamy, 2014) or might occur in advance of attentional capture (Anderson & Folk, 2012). According to the latter view, it is also possible that following an $n-1$ invalid trial, participants suppressed the nonmatching cues more efficiently right from the outset of the displays. Targets at the position of the nonmatching cues would therefore have been subject to this attention-preceding suppression in trial n .

Another possibility is that participants might have learned to attend to the target and/or to suppress the cue in invalid trials more efficiently (cf. Verguts & Notebaert, 2008). This contingency-learning theory could also explain why intertrial contingencies were restricted to the n -nonmatching conditions: The salient color of the nonmatching cue would be associated only with the irrelevant cue, whereas the single white abrupt onset of the matching cue would also be associated with the target. Consequently, discriminative learning of selectively attending to targets versus cues could have been stronger with n -nonmatching than with n -matching cues.

Experiment 2

Experiment 2 was a replication of Experiment 1, except for the intention to rule out position priming (Maljkovic & Nakayama, 1996; Mayr et al., 2003; see also Hommel, 1998; Hommel, Proctor, & Vu, 2004, for a related idea) as a factor generating the stronger validity effect of the nonmatching cues following $n-1$ valid conditions. The intertrial contingency of n -nonmatching conditions in Experiment 1 could have been based on the exact repetitions of cue positions from trials $n-1$ to n . For example, maybe participants encoded the full episode of a trial $n-1$, consisting of the cue and target colors and their locations. Retrieval of this encoded episode in a subsequent trial n could have facilitated target responses when trial n was a full repetition of the episode in $n-1$ (cf. Huang, Holcombe, & Pashler, 2004). Because facilitation by full repetitions would have occurred only in (1) $n-1$ valid/ n valid and (2) in $n-1$ invalid/ n invalid sequences, full repetitions would have boosted the validity effect (calculated as n -invalid RT minus n -valid RT) following $n-1$ valid trials, and decreased it following $n-1$ invalid trials. To test this possibility, Experiment 2 included no cue or target position repetitions across consecutive trials. Under these conditions, no

trial n was a full replication of its immediately preceding trial $n-1$.

Our predictions were straightforward. If an influence, such as the strategic use of knowledge about the cue's utility (Gratton et al., 1992) or conflict adaptation (Müller et al., 2009), accounted for the intertrial contingencies of the validity effect for the nonmatching cues, we expected to replicate the findings of Experiment 1, because the potential influences of these factors were not affected by our manipulation. If, however, position priming was crucial for an increased validity effect with nonmatching cues following $n-1$ valid trials, we expected to see no such intertrial contingency in the present experiment. The fact that cue and target positions were not repeated from trial to trial also implies that more contingency learning of the specific target position in $n-1$ invalid than in $n-1$ valid trials could not be responsible for the any intertrial contingencies that we might observe.

Method

Participants A group of 21 new participants (nine female, 12 male; $M_{Age} = 23.53$ years, $SD_{Age} = 3.37$) was tested. Two participants were excluded because they made more than 20 % errors. The ERs of the remaining sample ranged from 0.45 % to 13.22 %.

Procedure The procedure was the same as in Experiment 1, with the notable exception that the positions of targets and cues were never repeated in subsequent trials. For example, if the cue and target were presented on the left in an $n-1$ valid trial, an n valid trial could have contained the cue and target on the right, at the bottom, or at the top, but never on the left. Accordingly, in the case of $n-1$ valid trials with the cue and target on the left, an n invalid trial could have contained a cue at the top and a target at the right or the bottom, a cue at the right and a target at the top or the bottom, or a cue at the bottom and a target at the top or the right, but neither the cue nor target would have appeared on the left.

Results

Reaction times We removed trials following errors (9.30 %) and with RTs below 150 ms (additional 0.03 %). Unless otherwise stated, all validity effects (or their reversals; see below) are significantly different from zero (all $ps < .05$). Table 1 (middle rows) provides a detailed overview of the RTs and ERs for Experiment 2 (see also Fig. 3). A main effect of $n-1$ validity, $F(1, 18) = 8.24$, $p = .01$, reflected an n -validity effect following $n-1$ valid trials (21 ms) that was absent following $n-1$ invalid trials [6 ms; t test against zero: $t(18) = 1.69$, $p = .11$]. Furthermore, the validity effect was only different from zero in n -matching trials (28 ms) but not in n -nonmatching trials [-2 ms; t test against zero: $t(18) = -0.43$, $p = .67$], as was

indicated by a main effect of n cue type, $F(1, 18) = 23.75$, $p < .01$. A borderline significant main effect of $n-1$ cue type, $F(1, 18) = 3.38$, $p = .08$, and an interaction between $n-1$ cue type and n cue type, $F(1, 18) = 7.84$, $p = .01$, will be resolved together with the analysis of the also-significant interaction between all variables, $F(1, 18) = 7.09$, $p = .02$. No other significant effects were found, all nonsignificant F s $< .94$, all $ps > .34$.

The follow-up analysis of the n -matching trials revealed no significant effects, all F s < 2.10 , all $ps > .16$.

For n -nonmatching trials, we found effects of $n-1$ cue type, $F(1, 18) = 13.47$, $p < .01$, and of $n-1$ validity, $F(1, 18) = 7.98$, $p = .01$. These two significant effects were qualified by their interaction, $F(1, 18) = 9.53$, $p < .01$. If the preceding trial had used a nonmatching cue, the validity effect was only present following $n-1$ valid trials (19 ms) but absent following $n-1$ invalid trials [-8 ms; t test against zero: $t(18) = -1.50$, $p = .15$]. If $n-1$ had used a matching cue, the validity effect was inverted following $n-1$ invalid trials (-13 ms) and absent following $n-1$ valid trials [-4 ms; t test against zero: $t(18) = -0.73$, $p = .48$].

Error rates The main effect of $n-1$ validity came closest to significance, $F(1, 18) = 3.09$, $p = .10$, with a tendency toward a larger validity effect after $n-1$ valid trials (2.67 %) than after $n-1$ invalid trials (0.77 %). No other significant effects were found, all nonsignificant F s < 1.82 , all $ps > .19$.

Discussion

The important results of Experiment 2 closely resembled those of Experiment 1. Again, we found that validity in trial $n-1$ had no impact on the validity effect of n -matching cues, but only on the validity effect of n -nonmatching cues. A validity effect of the n -nonmatching cues was only found if the cue in trial $n-1$ had been both *valid* and *nonmatching*. In addition, evidence of active suppression of the nonmatching cue (reflected in an inverted validity effect) was found only if the preceding trial had used a *matching* and *invalid* cue. If the preceding trial had used a *matching*-*valid* or a *nonmatching*-*invalid* cue, there was no validity effect of the nonmatching cue.

These intertrial contingencies of the validity effect in the n -nonmatching conditions were found even though the cue and target in trials $n-1$ and n were never presented at the same positions. Due to this manipulation, the number of possible target positions was effectively reduced, meaning that search for the targets was easier and that new forms of predictability of the target (and cue) were possible. However, position priming and contingency learning of specific target positions were ruled out as factors that could be responsible for the intertrial contingency. This fact in turn makes it likely that factors such as expectancy changes or conflict adaptation lowered the

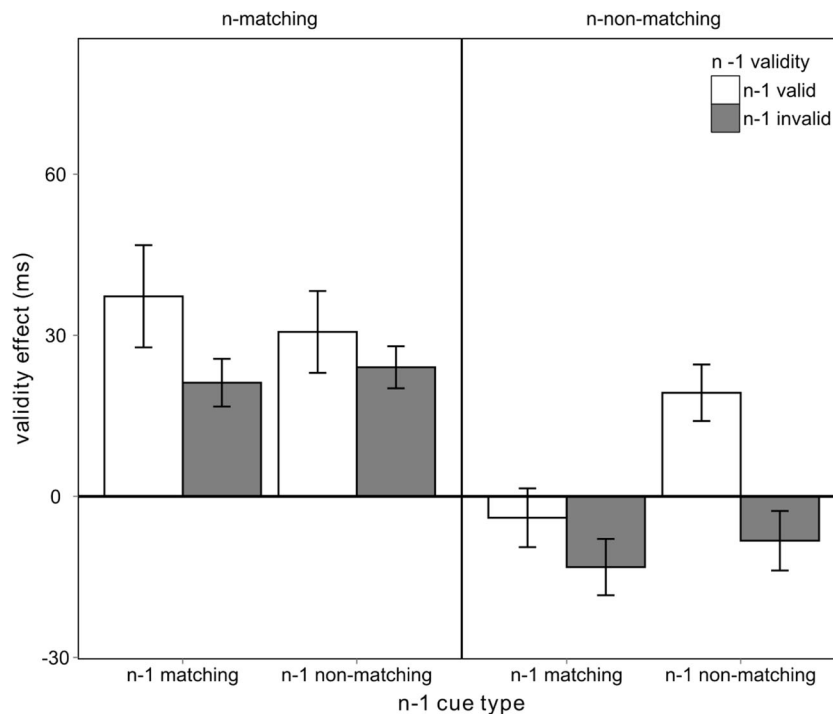


Fig. 3 Validity effects (n -invalid RT – n -valid RT) as a function of n cue type (n -matching, left panel; n -nonmatching, right panel), $n-1$ validity ($n-1$ valid, white bars; $n-1$ invalid, gray bars), and $n-1$ cue type (x -

axis) in Experiment 2 (onset search without position repetition between trials $n-1$ and n). Error bars represent the standard errors of the means (SEMs)

validity effects of n -nonmatching cues that were preceded by $n-1$ invalid cues.

However, there were also differences from Experiment 1. In the present experiment, preventing position priming brought to light a cue-color repetition effect in the nonmatching conditions: In Experiment 2, comparing $n-1$ valid versus $n-1$ invalid trials reflected a validity effect versus no validity effect if the nonmatching cue was shown in both trials $n-1$ and n , whereas it reflected no validity effect versus an inverted validity effect if a matching cue in $n-1$ preceded a nonmatching cue in trial n . This result suggests that attentional capture of nonmatching cues benefited from cue-color repetitions across subsequent trials. This finding would be consistent with an episodic retrieval account, in so far as not only the last attended-to color (that of the $n-1$ target) accounted for a cue's capture in the subsequent trial. The fact that this form of color priming was only found in the present but not the previous experiment might have been due to the cue positions never repeating across trials of Experiment 2, since participants might have focused more on cue colors than on cue positions during the encoding and retrieval of trial episodes. In Experiment 1, cue-position repetitions might have discouraged this kind of color-directed encoding and retrieval strategy.

In addition, several other key findings of Experiment 1 and of the existent literature were replicated. Most importantly, we again found clear evidence for contingent capture, although, as in Experiment 1, it was possible that the selective presence

of the validity effect for n -matching trials reflected more intertrial priming of cue colors in n -matching than in n -nonmatching conditions. Crucially, however, as in Experiment 1, intertrial contingency was prevented in the matching conditions, indicating a lower vulnerability of attentional capture in these conditions to the influence of validity in a preceding trial.

Experiment 3

Here, we tested whether $n-1$ validity affected validity effects when participants searched for a color-defined red target. Under these conditions, the red color cues and the white onset cues would change their roles: When participants searched for a red color target, a red color cue would match the top-down control settings, and a white onset cue would not match the top-down control settings. This allowed us to unconfound the roles of colors and of top-down matching (vs. nonmatching) for the intertrial contingencies. If the intertrial contingencies of the validity effects in Experiments 1 and 2 were due to the use of red cues, we expected to find an intertrial contingency of the validity effect of the matching cues in Experiment 3. Instead, if the intertrial contingencies in Experiments 1 and 2 were based on the lower task relevance of the nonmatching cues, we expected to see no such intertrial contingency of the validity effect of the red cues, because in Experiment 3 the red cue matched the control settings. In addition, if intertrial

contingencies are generally found with nonmatching cues, we expected to observe an intertrial contingency with the white onset cues (here: the nonmatching cues).

Method

Participants A total of 22 (16 female, six male; $M_{Age} = 24.50$ years, $SD_{Age} = 7.12$) new participants were tested. The ERs ranged from 0.26 % to 13.73 %.

Procedure Participants searched for a red color target that was presented simultaneously with three white distractors at the remaining placeholders (see also Fig. 1, right panel). Most importantly, we used an SOA of 100 ms, instead of 150 ms as in Experiment 1 and 2. In a pretest color-search experiment, we used the standard SOA of 150 ms but did not obtain any intertrial contingencies (see the Appendix). We think that color search is more difficult than onset search and that due to the higher RTs in the color-search conditions, we might have missed out on parts of a rather short-lived nonmatching validity effect. By shortening the SOA, we thus aimed at compensating for the RT delay. This increased the sensitivity of our method for nonmatching validity effects (see Liao & Yeh, 2013) and their intertrial contingencies.

Results

Reaction times Trials following an error trial (6.33 %) and trials with RTs below 150 ms (additional 0.01 %) were removed from the analysis. Unless otherwise noted, all validity effects are significantly different from zero (all $ps < .05$). Table 1 (lower rows) provides a detailed overview of the RTs and ERs for Experiment 3.

The validity effect was larger in n -matching trials (75 ms) than in n -nonmatching trials (6 ms), $F(1, 21) = 232.40$, $p < .01$. In the n -nonmatching trials, the validity effect was not different from zero, $t(21) = 1.62$, $p = .12$. Furthermore, the validity effect was larger after $n-1$ valid (45 ms) than after $n-1$ invalid (36 ms) trials, $F(1, 21) = 4.28$, $p = .05$, indicating an intertrial contingency of the validity effect. Importantly, this intertrial contingency was modulated by n -matching, $F(1, 21) = 8.49$, $p = .01$. The validity effects in n -matching trials were not different following $n-1$ invalid (76 ms) and $n-1$ valid (74 ms) trials, $t(21) = 0.32$, $p = .75$. Only in n -nonmatching trials was the validity effect larger following $n-1$ valid (16 ms) than $n-1$ invalid (−4 ms) conditions. Note that the latter validity effect (in $n-1$ invalid and n -nonmatching trials) was not significantly different from zero, $t(21) = -1.27$, $p = .22$. No other effects were found, all nonsignificant F s < 3.13 , all $ps > .09$. Figure 4 summarizes the results of Experiment 3.

Error rates The main effect of $n-1$ cue type came closest to significance, $F(1, 21) = 3.17$, $p = .09$, with a tendency toward

a larger validity effect after $n-1$ nonmatching trials (1.64 %) than after $n-1$ matching trials (0.84 %). No other effects were found, all nonsignificant F s < 2.77 , all $ps > .11$.

Discussion

Experiment 3 showed that the intertrial contingencies of validity effects for nonmatching cues can also be found if participants search for color-defined targets. With a white nonmatching onset cue, the validity was stronger following $n-1$ valid than following $n-1$ invalid trials. With matching cues, no such intertrial contingencies were found. This resembled the findings of Experiments 1 and 2. However, we did not find an inverted validity effect of the n -nonmatching cues following an $n-1$ invalid trial. This could be explained in terms of suppression difficulty: The onset cue was of a higher luminance than the red color cue, and might therefore be harder to suppress.

General discussion

In the present study, we tested whether the attentional capture effects of matching cues, nonmatching cues, or both could be higher following $n-1$ valid trials than following $n-1$ invalid trials. Using the well-established contingent capture paradigm of Folk et al. (1992), we did find an intertrial contingency of attentional capture, in the form of a stronger validity effect following $n-1$ valid trials than following $n-1$ invalid trials. However, on closer inspection this intertrial contingency effect was restricted to n -nonmatching conditions.

This finding could be due to conflict adaptation or expectancies, but theoretically it could also point to supportive roles of position priming and maybe even cue-color repetitions for attentional capture by nonmatching cues (cf. Huang et al., 2004; Maljkovic & Nakayama, 1996). However, in Experiment 2, in which cue and target positions were never repeated across trials, we still found an intertrial contingency of the validity effect for the nonmatching cues. Therefore, we can conclude that the intertrial contingency of attention capture depends neither on the learning of the exact target positions, nor on the repetition of the exact cue–target position relations, nor on position priming alone.

In addition, in the n -nonmatching conditions, a differently colored and attended-to target was presented in-between the cues in trials $n-1$ and n . Participants could have been slightly more inclined to redirect their attention to the nonmatching cue in trial n if its color repeated from trial $n-1$ to n (see Huang et al., 2004, for discussion of a possible principle). However, color priming per se would have been strongest in n -matching conditions, in which the color of the cue in trial n was always a repetition of the color of the target in $n-1$. Yet, in Experiments 1–3, we found no significant intertrial contingency of the

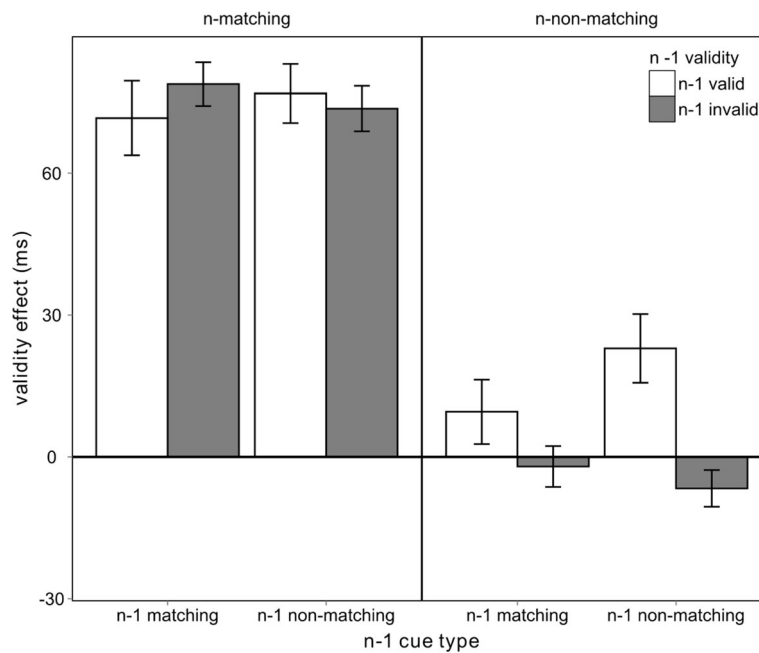


Fig. 4 Validity effects (n -invalid RT – n -valid RT) as a function of n cue type (n -matching, left panel; n -nonmatching, right panel), $n-1$ validity ($n-1$ valid, white bars; $n-1$ invalid, gray bars), and $n-1$ cue type (x -

axis) in Experiment 3 (color search). Error bars represent the standard errors of the means (SEMs)

validity effect with matching cues. Some authors have argued that stronger capture by matching than by nonmatching cues could be due to more intertrial priming of attention capture by the matching cues in trial n through the similarly colored targets in trial $n-1$ (Awh et al., 2012; Theeuwes, 2013). Although a number of studies have confirmed that more capture by top-down matching cues than by nonmatching cues can be found with changes from an $n-1$ target color to an n cue color (Ansorge & Horstmann, 2007; Ansorge, Kiss, & Eimer, 2009; Folk & Remington, 2008; Worschech & Ansorge, 2012; for a review, see Lamy & Kristjánsson, 2013), in the original experimental protocol of Folk et al. (see also Folk & Remington, 1998), participants searched for only one target color. This implies that all top-down matching cues were also potentially intertrial color primed by the preceding target. According to this line of argument, very strong intertrial priming of color could have prevented the influence of $n-1$ cue–target validity on the validity effects in the matching conditions. For example, a very strong influence of intertrial color priming on attention might overcome a smaller effect based on expecting a helpful (after an $n-1$ valid trial) or less helpful (after an $n-1$ invalid trial) cue.

Instead, the intertrial contingency for the nonmatching cues must have reflected an influence such as the expectancy-based strategic use of cues after $n-1$ valid trials but not after $n-1$ invalid trials (Gratton et al., 1992), or more adaptation of control with the nonmatching cues (Botvinick et al., 2001). In line with active suppression (cf. Botvinick et al., 2001), we found inverted validity effects of the n -nonmatching cues after $n-1$ invalid trials in Experiments 1 and 2. If the $n-1$

nonmatching cues were harmful (i.e., invalid), they were actively suppressed in trial n . For example, in Experiments 1 and 2, participants might have actively suppressed the red color cue from the start of a display. However, this strategy might have been more difficult with the higher-luminance nonmatching onset cues. In line with this argument, we found no inverted validity effect of the nonmatching onset cue following an $n-1$ invalid trial in Experiment 3.

This brings us to the limitations of the present study. Although we have selected a widely popular paradigm for our study and did not change its main characteristics, future studies would potentially benefit from doing so. For example, similar to Folk et al. (1992) we used a color red with a luminance that was lower than that of the white stimuli. Although it could be that participants relied on chromaticity to find the relevant red targets, it remains possible that the latencies of luminance processing also influence the time that it takes to find the target or to process the cue. Future studies could therefore use luminance-equated red and white colors or could parametrically vary the luminances of the cues to systematically investigate the influence of this variable on intertrial contingencies. Likewise, future studies could also use more than one target color to test whether different degrees of intertrial contingency could also be found for matching cues that are not primed versus cues that are primed by the color of the target in $n-1$.

Conclusion

The most interesting result of the present study was that nonmatching cues captured attention, provided that a valid

cue had been used in the previous trial $n-1$. This finding holds true even though the cues were not predictive of the target position. The zero capture effects in nonmatching conditions could be the result of averaging across standard validity effects and inverted validity effects; both capture and suppression of capture could contribute to the performance in the nonmatching conditions. This finding also indicates that contingent-capture effects (defined as validity effect differences between matching and nonmatching cues) reflect more than capture by preexperimentally implemented search settings, and more than combined intertrial feature and position priming alone.

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Appendix

This was our first experiment in which we tested for intertrial contingencies when participants had to search for a color-defined red target. We used the same procedure as in Experiment 1, with the difference that participants searched for a red color target that was concomitantly presented with three white distractors.

Method

Participants A group of 26 new participants (18 female, eight male; $M_{\text{Age}} = 23.48$ years, $SD_{\text{Age}} = 3.44$) were tested. One participant was excluded because of more than 20 % error trials. The ERs of this sample ranged between 1.82 % and 15.89 %.

Procedure The procedure was the same as in Experiment 1. This time, we used one red color target that was presented with three white nontargets per target display, and the participants had to search for the red target and report its identity (see also Fig. 1, right panel, but note the exception that the duration of the fixation display between cue and target was 100 ms, and not 50 ms as depicted).

Results

Reaction times All trials following an error trial (6.92 %), as well as all trials with RTs below 150 ms (an additional 0.09 %) were removed from the analysis. Mean correct RTs and ERs are listed in Table 2.

The analysis yielded a significant main effect of n -cue type, $F(1, 24) = 269.33$, $p < .01$. This pattern of results conforms to the predictions of the contingent capture view, suggesting that

Table 2 Summary of reaction times (RTs; in milliseconds) and error rates (ERs; in %)

		Trial n							
		Matching Cue Type				Nonmatching Cue Type			
						Valid		Invalid	
		Valid		Invalid		Valid		Invalid	
Trial $n-1$		RT	ER	RT	ER	RT	ER	RT	ER
Matching cue type	Valid	500	5.07	563	8.60	527	4.60	529	4.07
	Invalid	487	4.89	562	8.85	523	4.62	527	5.81
Nonmatching cue type	Valid	498	5.03	564	8.64	520	5.38	522	5.43
	Invalid	489	5.91	567	8.81	526	4.80	523	5.21

The table is split by the steps of the variables cue type (matching vs. nonmatching) and cue validity (valid vs. invalid) for both trials n and $n-1$.

only n -matching cues led to a validity effect of 70 ms, whereas n -nonmatching cues created no n validity effect [1 ms; t test against zero: $t(24) = 0.45$, $p = .66$]. Close to significance came an interaction between $n-1$ validity and n cue type, $F(1, 24) = 3.52$, $p = .07$. For n -matching trials, the validity effect was slightly larger after $n-1$ valid than $n-1$ invalid trials (76 vs. 64 ms), $t(24) = -2.03$, $p = .05$. No such trend was found for n -nonmatching trials (0 vs. 2 ms), $t(24) = 0.45$, $p = .66$. No other significant effects were found, all F s < 2.69 , all p s $> .15$.

Error rates We found a main effect of n cue type, $F(1, 24) = 13.78$, $p < .01$. The validity effect was slightly larger in n -matching than in n -nonmatching trials (1.94 % vs. 1.64%). The best evidence for a Gratton effect was a main effect of $n-1$ validity, $F(1, 24) = 5.11$, $p = .03$: The validity effect was larger after $n-1$ valid than after $n-1$ invalid trials (2.77 % vs. 0.81 %). No other significant effects were found, all F s < 1.29 , all p s $> .27$.

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The good, the bad,
and the misattributed:
The facilitating influence of
nonconscious affect-origins on
misattributions of affect and the
congruence sequence effect

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Author contributions:

FG and UA developed the study concept and the study design. FG programmed and conducted the experiments, data analysis and interpretation. FG drafted the manuscript, SK and UA added to the manuscript and provided critical revisions. All authors approved the final version of the manuscript for submission.

Running head: Nonconscious Processing and Affect-Misattributions

The Good, the Bad, and the Misattributed: The Facilitating Influence of
Nonconscious Affect-Origins on Misattributions of Affect and the Congruence
Sequence Effect

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Abstract

Congruence sequence effects – stronger congruence effects in a current trial n following a preceding incongruent trial $n-1$ than following a congruent trial $n-1$ – are known to be stronger after conscious than nonconscious congruence relations. However, affect misattributions – evaluations of neutral objects as more positive than negative following congruent as compared to incongruent trials – are stronger following nonconscious than conscious relations. Because congruence sequence effects depend on affective evaluations of the congruence relations and more affect-misattributions are found following nonconscious than conscious affect-origins, we hypothesized that the two phenomena could coincide. In two experiments, we were able to confirm this assumption. We showed that nonconscious conditions lead to weaker congruence sequence effects and to higher affect-misattribution rates, and that conscious conditions lead to stronger congruence sequence effects and to lower affect-misattributions rates. These findings are in line with the assumption that affective responses to and evaluations of congruent versus incongruent conditions as the true affect origins could be critical for the congruence sequence effect: Because affective responses are misattributed away from nonconscious congruent versus incongruent conditions as their true origins, congruence sequence effects in such conditions could be relatively weak.

Keywords: flanker task; masking; nonconscious processing; misattributions of affect; affective priming; congruence sequence effect

Introduction

Although it is important to know how labor is divided between conscious and nonconscious vision for answering questions, such as what the unique role of consciousness is, until today, this division of labor is not entirely understood. It is clear that there are differences between conscious and nonconscious vision: There is more reactive top-down control of processing in response to conscious than to nonconscious visual stimuli (Greenwald, Draine, & Abrams, 1996; Kunde, 2003), and there are more affect-misattributions following nonconscious than conscious visual stimulation (Murphy & Zajonc, 1993). Furthermore, because reactive top-down control could be due to the proper evaluation of the stimulus that triggers this control and because affect misattributions could dilute or replace such proper stimulus evaluation, the two phenomena of less control and more misattributions could coincide, but the connection between these effects has never been tested. The current study therefore set out to investigate a potential connection between these two differences. The theoretical reasons for this test will be explained in more detail next.

To start with, even nonconscious visual input can be processed in a top-down dependent way, if top-down control is exerted by action plans or task sets that are set up in advance of the nonconscious visual input (for a review see Ansorge, Kunde, & Kiefer, 2014). For example, Kunde, Kiesel and Hoffmann (2003) asked their participants to respond to specified visible number targets. In this situation, presenting one of these numbers as a nonconscious prime (i.e., a prime of which the

participant remains unaware) before the visible number target led to a congruence effect, with faster responses when both prime and target indicated the same response than when they indicated different responses. This was the case although the participants could not see the primes, demonstrating nonconscious priming. Critically, nonconscious primes with numbers outside the top-down set of predefined defined target numbers had no such modulating effect on target responses, even though these primes semantically also fitted more or less well to the target numbers. These and similar results with words (e.g., Klinger, Burton, & Pitts, 2000) and figures (e.g., Ansorge & Neumann, 2004) make it clear that advance specification of top-down task sets has a modulating effect on the processing of nonconscious stimuli.

Yet, when it comes to *reactive* top-down control of processing in response to visual input, the usual finding is that this sort of top-down control is more difficult to exert following nonconscious than conscious visual input. For instance, if conscious primes are used, participants show weaker congruence effects in the current trial n of an experiment, following preceding ($n-1$) incongruent trials than following $n-1$ congruent trials (Greewald, Draine, & Abrams, 1996; Kunde, 2003). Such *congruence sequence effects* can have a number of different origins (Mayr, Awh, & Laurey, 2004; Schlaghecken & Martini, 2010), but critically they could at least partly be due to expectancies (Gratton, Coles, & Donchin, 1992) and conflict regulation (Botvinick, Braver, Barch, Carter, & Cohen, 2001). According to these interpretations, participants would notice that a congruent prime in $n-1$ was helpful and based on

this most recent experience would allow processing the prime in the next trial (Gratton et al, 1992). This has the inevitable consequence of a substantial congruence effect in trial n . However, participants would also notice that an incongruent prime in $n-1$ has harmed their performance, and would, thus, try to shield processing from the prime in the subsequent trial (Botvinick et al., 2001). This would lead to a smaller congruence effect in trial n . Critically, the evidence for such forms of reactive top-down control (i.e., for the congruence sequence effect) is much weaker in the case of nonconscious than in the case of conscious priming (Ansorge, Fuchs, Khalid, & Kunde, 2012; Boy, Husain, & Sumner, 2010; Greenwald et al., 1996; Kunde, 2003). This could be due to the fact that a participant would not attribute response slowing (and other forms of lower fluency) as readily to a (incongruent) nonconscious prime as its true origin as s/he would attribute it to a conscious prime (Desender, Van Opstal, & Van den Bussche, 2014). As a consequence of these attribution differences between conscious and nonconscious visual inputs, participants might exert more reactive control over processing following conscious than nonconscious priming trials.

Intriguingly, there is indeed evidence for more *misattributions of affect* following nonconscious than following conscious stimuli that could be decisive for congruence sequence effects (Bornstein & D'Agostino, 1992; Murphy & Zajonc, 1993). Affect-misattributions are attributions of an affective response away from one stimulus as its true origin and towards a neutral object in temporal vicinity. For example, Murphy and Zajonc's participants evaluated affectively neutral Chinese

symbols following pleasant or unpleasant photos as irrelevant primes. Participants judged the symbols as more positive following positive than negative primes.

Critically, in line with a supportive role of the participants' unawareness of the primes as the true affect-origins, suboptimal (aka nonconscious) presentation of affective primes for a very limited duration led to much stronger misattributions of affect to the Chinese symbols than optimal (aka conscious) presentation of affective primes.

Importantly, such affect-misattributions could be decisive for reactive top-down control in the form of the congruence sequence effect because negative feelings elicited by incongruent trials could be necessary for conflict control (van Steenbergen, Band, & Hommel, 2009, 2010). To understand this, consider the study of van Steenbergen et al. (2010). These authors induced a positive or negative mood in their participants, and then measured a congruence effect. In line with a critical role of affect for conflict regulation, they found more evidence of congruence sequence effects when their participants were in a negative mood than when they were in a positive mood. Van Steenbergen and colleagues (2010) argued that it is the negative evaluation of the incongruent trial that leads to more conflict regulation.

Consequently, a less liberal response criterion is used and the relevant target in a subsequent trial is selected more carefully. This negative evaluation was more compatible with the negative mood than with the positive mood and, therefore, the preceding incongruent trials had a greater effect in the former than in the latter

condition. In agreement with this explanation, incongruent conditions elicit more negative feelings than congruent conditions (Fritz & Dreisbach, 2013).

Against this background, we wanted to test if nonconscious target-congruent versus target-incongruent primes can indeed lead to more affect misattributions than conscious primes. Such affect misattributions have the potential to dilute or replace the attribution of the same affective responses to the congruence versus incongruence between nonconscious primes and targets as the true affect-origins, with the potential to weaken the congruence-sequence effect. Therefore, at the same time, we wanted to confirm that the typical congruence-sequence effect is also weaker following nonconscious than conscious priming under otherwise similar conditions.

Experiment 1

To manipulate affect-misattributions, we varied conscious and nonconscious target-prime congruence. We used a flanker task (Eriksen & Eriksen, 1974) because congruence effects with nonconscious (masked) primes are well established in this task (Schwarz & Mecklinger, 1995; Tapia, Breitmeyer, & Shooner, 2010), and less controversial than with other target-prime congruence tasks (e.g., Stroop tasks, see Kouider & Dupoux, 2004). In addition, a role of affect has been suggested for congruence sequence effects in the flanker task (Van Steenbergen et al., 2010). Our participants discriminated between two possible targets (square versus diamond). The target was flanked by irrelevant primes that were either congruent (e.g., a square flanked by two squares) or incongruent (e.g., a square flanked by two diamonds). Under these conditions, congruent primes create less conflict than incongruent

primes, as reflected in congruence effects: faster responses under congruent than incongruent conditions (Gratton, Coles, Sirevaag, Eriksen, & Donchin, 1988). We expected a congruence effect for both conscious and nonconscious primes.

Crucially, at the end of some trials, participants additionally rated another object, a neutral Chinese symbol, as positive or negative. If target-prime congruence leads to more positive affective responses than incongruence and if these affective responses are (sometimes) misattributed, we expected less negative evaluations of the symbols in congruent than incongruent trials. Furthermore, a failure to perceive the true affect-origin could increase the likelihood of such misattributions (Bornstein & D'Agostino, 1992), resulting in more frequent affect-misattributions following nonconscious than conscious primes.

In addition to these effects of high importance, we also expected more evidence of a congruence-sequence effect in the conscious than in the nonconscious priming conditions, and evidence of less awareness of the participants of the nonconscious primes than of the conscious primes.

Methods

Participants.

Twenty-eight right-handed participants (15 female, 13 male, $M_{Age} = 24.05$, $SD_{Age} = 8.64$) recruited among Psychology students of the University of Vienna were tested. Two participants were post-hoc excluded due to more than 20% errors in the flanker task. Here and in Experiment 2, all participants received course credit, had normal or corrected-to-normal vision and reported no prior experience with Chinese symbols.

Informed consent was obtained from all participants and a full debriefing followed the experiment. The experiment fulfilled the World Medical Association Declaration of Helsinki and was conducted in accordance with the ethical guidelines of the University of Vienna. Based on a moderate expected effect size, the sample size was estimated using G*Power (Faul, Erdfelder, Buchner, & Lang, 2009), which indicated that 25 subjects were required for a 95% power of detecting a statistically significant difference. We stopped data collection after 28 participants to compensate for a few expected drop-outs.

Apparatus and Stimuli.

The experiment was programmed and controlled using Matlab 7.7.0 (The MathWorks inc., Massachusetts, USA) and the Psychophysics Toolbox (Brainard, 1997; Pelli, 1997). Viewing distance was stable at 57 cm, supported by a chin and forehead rest. Responses were manual key presses with the left versus right index fingers. Targets and primes ($1.5^\circ \times 1.5^\circ$) were squares and diamonds (Klotz & Neumann, 1999). To decrease participants' awareness of the primes, in the nonconscious condition, we used circular metacontrast masks that were neutral with respect to the response-relevant angular target shapes ($2^\circ \times 2^\circ$). A fixation-cross ($0.5^\circ \times 0.5^\circ$) was displayed at screen center. Targets appeared always on the screen's vertical meridian, 6.2° above or below the fixation-cross. Primes and masks were placed equally distant (2.2°) left and right of the target. In half of all trials, target and primes were of the same shape (congruent trials). In the other half of all trials, target and primes were of different shapes (incongruent trials). All stimuli were black (CIE

Lab: 0.9/-0.3; 0.8 cd/m²), presented against a grey background (CIE Lab: 0.9/-8.4; 31.1 cd/m²).

A third of all trials was followed by a rating task with a black Chinese symbol ($4^\circ \times 4^\circ$) at screen center. The symbols were randomly chosen from an online English-Chinese dictionary (<http://www.mdbg.net/chindict/chindict.php>). In a prestudy, fresh participants rated a total set of 700 symbols on valence, arousal, and complexity. To rule out a priori effects of the used symbols, we selected symbols with moderate ratings on these dimensions. Also, by randomization of the symbols across trials, each specific symbol was equally likely used in congruent and incongruent trials.

Procedure.

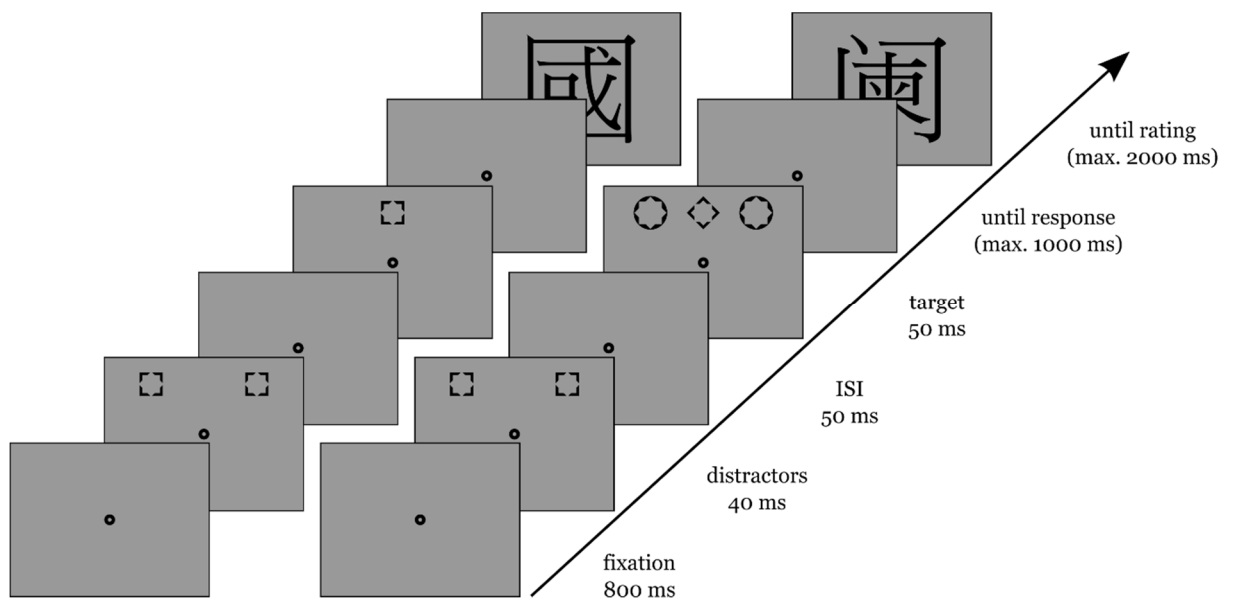


Figure 1: Example trials of Experiments 1 and 2, with time flowing from bottom to top. The left side depicts a congruent conscious trial, the right side depicts an incongruent nonconscious trial. Note that only in a third of all trials, participants had to rate a Chinese symbol. The figures are not drawn to scale.

After fixation alone (800 ms), primes were added (40 ms) and with an inter-stimulus interval (ISI) of 50 ms, a target was shown for 100 ms (see also Figure 1). Participants were first tested in the nonconscious block, and afterwards in the conscious block. The order of blocks was kept constant to ensure minimal awareness of the nonconscious primes. Participants pressed the left or right key, with each key mapped to a specific shape (counterbalanced across participants). An on-screen feedback informed about too slow (> 1 s) or erroneous responses. In a third of all trials, following target discrimination, participants rated the valence of a Chinese symbol as negative (left key) or positive (right key). This mapping was kept constant across participants. An on-screen feedback warned if the rating time exceeded 2 s.

In a separate block at the end, participants' awareness of the primes was assessed. Participants were informed about the presence and identities of the primes, and their task was to discriminate and report prime identities. Stimulus-to-response mapping varied randomly across trials (via an onscreen mapping rule).

Results

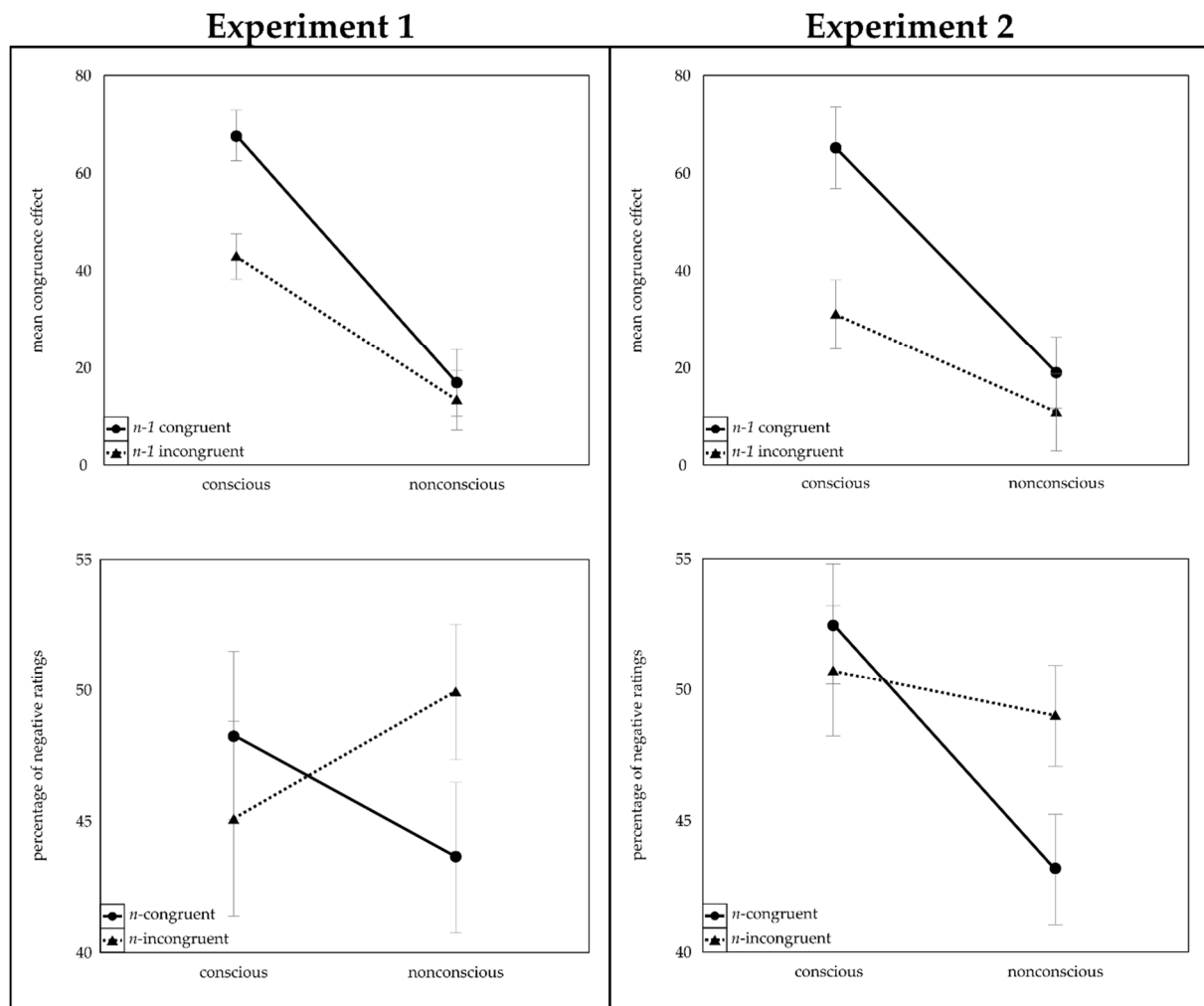


Figure 2: Mean congruence effects (RT difference between incongruent and congruent trials; top row) to target shapes and percentage of negative ratings (bottom row) of the Chinese symbols for Experiments 1 (left panel) and 2 (right panel). Error bars represent *SEM*.

Reaction Times (RTs).

Trials with an error (8.80%) or an RT below 150 ms (< 0.1%) were excluded. We computed RT congruence effects as the mean correct RTs in invalid minus the mean correct RTs in valid trials, separately for each participant and condition. The results of both experiments are reported in this way. RT congruence effects were

subjected to a repeated-measurements analysis of variance (ANOVA), with the within-participant variables consciousness (nonconscious; conscious) and $n-1$ congruence ($n-1$ congruent; $n-1$ incongruent). Where appropriate, degrees of freedom were Greenhouse-Geisser corrected. For better transparency, the uncorrected degrees of freedom but the corrected p values are reported. Figure 2 (upper left panel) illustrates the results. Besides main effects of consciousness, $F(1, 25) = 64.59, p < .001, \eta_p^2 = .72$, and $n-1$ congruence, $F(1, 25) = 7.35, p = .012, \eta_p^2 = .23$, we found an interaction, $F(1, 25) = 4.77, p = .038, \eta_p^2 = .16$. In the conscious block, the RT congruence effect was smaller after $n-1$ incongruent ($M = 43$ ms, 95% CI = [34, 52]) compared to $n-1$ congruent trials ($M = 68$ ms, 95% CI = [58, 78]), $t(25) = 5.24, p < .001, d = 1.45$. In the nonconscious block, the RT congruence effect was of similar size in $n-1$ incongruent ($M = 13$ ms, 95% CI = [1, 25]) and $n-1$ congruent ($M = 17$ ms, 95% CI = [3, 30]) trials, $t(25) = .40, p > .249, d = .11$. An ANOVA, in which trials following $n-1$ trials with evaluations of Chinese pictographs were excluded, confirmed the results.

Error rates (ERs).

From the arcsine transformed ERs, congruence effects were computed analogous to the RTs. A repeated measurements ANOVA with the same factors as before yielded a main effect of consciousness, $F(1, 25) = 36.57, p < .001, \eta_p^2 = .59$. In the conscious block, the ER congruence effect was larger ($M = 3.12\%$, 95% CI = [1.64, 4.82]) than in nonconscious block ($M = .76\%$, 95% CI = [.35, 1.16]). No other effects were found, all $F < 2.09$, all $p > .161$, all $\eta_p^2 < .08$.

Ratings of the Chinese symbols.

From of all correct flanker-task trials, we computed the probabilities of negative ratings, separately for each participant and combination of the steps of the variables consciousness and congruence. First, we analyzed whether the mean probability of negative ratings (collapsed across all conditions and participants) was equal to 50% because a mean probability above or below 50% would indicate a bias towards a positive or negative mean evaluation of the Chinese pictographs. A *t*-test against 50% revealed no such bias ($M = 46.74\%$, 95% CI = [40.94, 52.53]), $t(25) = -1.27$, $p = .215$, $d = .35$. This was also true for separate analyses of nonconscious ($M = 46.79\%$, 95% CI = [41.65, 51.92]), $t(25) = -1.02$, $p > .249$, $d = .28$, and conscious ($M = 46.69\%$, 95% CI = [40.21, 53.17]) blocks, $t(25) = -1.25$, $p = .223$, $d = .35$.

We next analyzed the probability of negative symbol ratings as a function of consciousness and congruence. Figure 2 (lower left panel) illustrates the results. A repeated-measurements ANOVA revealed an interaction between congruence and consciousness, $F(1, 25) = 10.50$, $p = .003$, $\eta_p^2 = .30$. Only in the nonconscious block, participants rated the neutral symbols less often negative in congruent ($M = 43.65\%$, 95% CI = [37.94, 49.35]) than incongruent ($M = 50.00\%$, 95% CI = [44.77, 55.12]) trials, $t(25) = 3.46$, $p = .002$, $d = .96$. In contrast, the probability of negative ratings in the conscious block was not different between congruent ($M = 48.27\%$, 95% CI = [41.83, 54.71]) and incongruent ($M = 45.09\%$, 95% CI = [37.67, 52.52]) trials, $t(25) = 1.27$, $p = .216$, $d = .35$. No other effects were found, all $F < 0.92$, all $p > .249$, all $\eta_p^2 < .04$.

Prime awareness.

Awareness of the primes was assessed by d' . Diamonds counted as signals, squares as noise. Performance was above chance in the conscious condition ($M = 1.05$, 95% CI = [0.66, 1.45]), t -test against zero: $t(25) = 5.32$, $p < .001$, $d = 1.47$, and not different from chance in the nonconscious condition ($M = 0.08$, 95% CI = [-0.12, 0.28]), t -test against zero: $t(25) = 0.78$, $p > .249$, $d = 0.22$.

Correlations between prime awareness and congruence effect.

To additionally test if the RT congruence effect in the nonconscious blocks depended on the participants' awareness of the primes, we computed a Pearson correlation between the mean values of d' and the z -transformed means of the RT congruence effect. No correlation was found in the nonconscious block, $r(26) = .18$, $p > .249$.

Discussion

Participants were not aware of the nonconscious primes, and therefore substantial affect misattributions and low congruence sequence effects were expected for these primes. In line with the first expectation, neutral symbols were judged less often as negative following a nonconscious congruent than following a nonconscious incongruent prime. Additionally, a lacking correlation between participants' awareness of the nonconscious primes and on the one hand, and RT congruence effects on the other hand supported the conclusions that these effects reflected nonconscious processing.

Crucially, in line with a supportive role of unawareness of the true affect-origins for affect-misattributions, no affect-misattributions were found following

conscious primes: With conscious congruent and incongruent primes, equal proportions of negative and positive judgments were observed, although conscious primes produced a stronger RT congruence effect than nonconscious primes (for the latter finding see also Tapia et al., 2010).

In line with the second prediction and a role of affect-misattributions away from target-prime congruence and towards other (neutral) symbols for the lacking congruence sequence effect, in the nonconscious condition, we found no evidence of a congruence-sequence effect but a high affect-misattribution rate. In contrast, in the conscious condition, a congruence sequence effect was found, together with a low affect-misattribution rate. Experiment 1 is the first time that the two phenomena of (1) more affect misattributions and (2) less congruence-sequence effects following nonconscious than conscious visual input have been demonstrated to coincide under the same experimental conditions.

Besides these predicted effects, there was also an unexpected finding. Whereas a stronger misattribution effect with nonconscious than conscious primes was in line with our predictions, an entire lack of affect-misattributions following conscious primes was not. This null finding is at variance with affect-misattributions following conscious nogo Stroop trials, in which participants only judged a symbol following the Stroop stimuli but did not respond to the Stroop stimuli themselves (Fritz & Dreisbach, 2013). Maybe the former study's use of nogo trials was critical because this was in contrast to the present experiment. Our participants responded to each target, and this might have prevented affect-misattributions with the conscious

primes. Responding correctly to targets in incongruent trials can change the participant's evaluation of incongruence from negative to more positive (Schouppe et al., 2015). Therefore, our participants could have felt more positive about their successful response to a target following a conscious incongruent prime than the participants of Fritz and Dreisbach. Furthermore, our participants might have felt less positive about their correct responses to nonconscious incongruent primes simply because participants would have failed to register the nonconscious incongruent prime as a challenge in the first place. Consequently, only the present conscious incongruent primes but not the nonconscious incongruent primes might have prompted positive feelings of success that counteracted the negative affective responses (and their misattributions to the symbols). To clarify this issue, we conducted Experiment 2, in which our participants rated the Chinese symbols in nogo trials, too.

Experiment 2

We used a go/no-go flanker task, in which a no-go prime is incongruent to the go target and accordingly delays the go response (for congruence effects with nonconscious primes in the go/no-go task see Experiment 5 of Ansorge, 2004). We expected an RT congruence effect in the go trials, and affect-misattributions based on congruence versus incongruence in go and no-go trials.

If the participants' unawareness of the primes facilitated affect-misattributions, we expected more misattributions following nonconscious than conscious primes. However, if in Experiment 1 overt correct responses to conscious

incongruent primes changed the evaluations and prevented a misattribution effect, we expected more misattributions following conscious primes in the present no-go than go trials.

Methods

Participants.

Twenty-eight right-handed participants were tested. Two participants were post-hoc excluded due to more than 20% errors in the flanker task. The final sample consisted of 17 females and 9 males ($M_{Age} = 23.46$, $SD_{Age} = 3.59$).

Apparatus and Stimuli.

These were as in Experiment 1, with two exceptions. To reduce visual crowding and task difficulty, we reduced prime and target eccentricities to 4.5° (6.2° in Experiment 1), and slightly increased target-prime distances to 2.5° (2.0° in Experiment 1).

Procedure.

The sequence of events in a trial was as in Experiment 1 (see Figure 1), but the task was changed. If the target was a diamond (or a square, counterbalanced across participants), participants pressed the spacebar. Otherwise, they had to withhold their response and to wait until the next trial started. There was a fixed time window of 1 s for participants to give their answer (in go trials) or to wait (in no-go trials).

Results.

Reaction Times (RTs).

Go trials that were also preceded by a go trial were subjected to an ANOVA as in Experiment 1. From these trials, errors (8.05%) and RTs below 150 ms ($< 0.1\%$) were excluded. RT congruence effects were computed as in Experiment 1. Figure 2 (upper right panel) illustrates the results. We found significant main effects of consciousness, $F(1, 25) = 16.42, p < .001, \eta_p^2 = .40$, $n-1$ congruence, $F(1, 25) = 11.14, p = .003, \eta_p^2 = .31$, and an interaction, $F(1, 25) = 4.76, p = .039, \eta_p^2 = .16$. In the conscious block, RT congruence effects were larger following $n-1$ congruent ($M = 65$ ms, 95% CI = [49, 82]) than $n-1$ incongruent ($M = 31$ ms, 95% CI = [17, 45]) trials, $t(25) = 3.95, p = .001, d = 1.30$. In the nonconscious block, RT congruence effects following $n-1$ congruent ($M = 19$ ms, 95% CI = [5, 33]) and $n-1$ incongruent ($M = 11$ ms, 95% CI = [-5, 26]) trials were similar, $t(25) = .93, p > .249, d = .26$.

Error Rates (ERs).

From the arcsine transformed mean ERs, we computed congruence effects and subjected them to an ANOVA similar to the ERs in Experiment 1 with the additional factor of task (go; nogo). This ANOVA yielded main effects of task, $F(1,25) = 10.90, p = .003, \eta_p^2 = .30$, and consciousness, $F(1,25) = 4.63, p = .041, \eta_p^2 = .16$, as well as interactions between task and consciousness, $F(1,25) = 8.39, p = .008, \eta_p^2 = .25$, and consciousness and $n-1$ congruence, $F(1,25) = 4.97, p = .035, \eta_p^2 = .17$. In the go trials, the congruence effect was larger in the conscious ($M = 5.82\%$, 95% CI = [3.44, 8.21]) than in the nonconscious block ($M = 1.95\%$, 95% CI = [.83, 3.08]), $t(25) = 3.02, p = .006, d = 0.84$. In the nogo trials, no difference between the levels of consciousness were found, $t(25) = 0.12, p = .903, d = 0.03$. In the conscious block, the congruence effect was

smaller following *n-1* incongruent ($M = 4.29\%$, 95% CI = [1.87, 6.72]) than *n-1* congruent trials ($M = 2.25\%$, 95% CI = [.71, 3.77]), $t(25) = 2.25$, $p = .033$, $d = 0.62$. No such difference was found in the nonconscious block ($M = 1.68\%$, 95% CI = [.80, 2.55] versus $M = .81\%$, 95% CI = [.45, 1.17]), $t(25) = .72$, $p = .478$, $d = .20$. Furthermore, we found a borderline significant interaction between task and *n-1* congruence, $F(1,25) = 3.83$, $p = .062$, $\eta_p^2 = .13$. In the go trials, no difference between the levels of *n-1* congruence was found, $t(25) = .49$, $p = .626$, $d = 0.14$. In the nogo trials however, following an *n-1* congruent trial, the congruence effect was slightly larger ($M = 1.51\%$, 95% CI = [.49, 2.54]) than following an *n-1* incongruent trial ($M = 1.13\%$, 95% CI = [.28, 1.98]), $t(25) = 2.15$, $p = .041$, $d = 0.60$. No other effects were found, all $F < 1.13$, all $p > .249$, all $\eta_p^2 < .04$.

Ratings of the Chinese symbols.

The analysis was based on correct flanker-task trials. No overall bias towards negative evaluations was found ($M = 48.84\%$, 95% CI = [44.05, 53.62]), $t(25) = -0.49$, $p > .249$, $d = .13$. The same held true if tests were conducted separately for nonconscious ($M = 46.06\%$, 95% CI = [41.60, 50.53]), $t(25) = -1.76$, $p = .090$, $d = .49$, and conscious ($M = 51.61\%$, 95% CI = [45.43, 57.79]) blocks, $t(25) = -0.52$, $p > .249$, $d = .14$. A repeated-measurements ANOVA, with the same variables as in the ERs, revealed a main effect of consciousness, $F(1, 25) = 4.98$, $p = .035$, $\eta_p^2 = .17$, and an interaction, $F(1, 25) = 8.78$, $p = .007$, $\eta_p^2 = .26$. Again, in the nonconscious block, participants rated the symbols significantly less often as negative in congruent ($M = 43.13\%$, 95% CI = [38.22, 48.04]) than incongruent ($M = 49.00\%$, 95% CI = [44.06, 53.93]) trials, $t(25) = 2.82$, $p = .009$, $d =$

.78. In contrast, in the conscious block, there was no significant difference between congruent ($M = 52.51\%$, 95% CI = [46.27, 58.74]) and incongruent ($M = 50.71\%$, 95% CI = [44.32, 57.10]) trials, $t(25) = 1.39$, $p = .175$, $d = .39$. Furthermore, we found a strong trend towards a main effect of task, $F(1, 25) = 4.06$, $p = .055$, $\eta_p^2 = .14$. Ratings were more often negative in no-go trials ($M = 50.51\%$, 95% CI = [44.23, 56.80]) than go trials ($M = 47.16\%$, 95% CI = [40.75, 53.57]). All non-significant tests, including the interactions with the variable response, were $F < 3.15$, all $p > .088$, all $\eta_p^2 < .11$. Figure 2 (lower right panel) illustrates the results.

Prime awareness.

Prime discrimination was above chance in the conscious condition ($M = 1.32$, 95% CI = [.94, 1.71]), $t(25) = 6.96$, $p < .001$, $d = 1.93$, and not different from chance in the nonconscious condition ($M = .07$, 95% CI = [-0.15, 0.29]), $t(25) = .64$, $p > .249$, $d = .18$.

Correlations between prime awareness and congruence effect.

For the nonconscious condition, we computed a Pearson correlation between the mean values of d' and the z-transformed mean of the congruence effect. No correlation was found in the nonconscious block, $r(26) = .26$, $p = .198$.

Discussion

Experiment 2 confirmed that affect-misattributions were restricted to the nonconscious primes. In contrast, with conscious primes, congruence-elicited affect-misattributions were missing in go and no-go trials. The results are in line with a facilitating role of unawareness for affect-misattributions to the neutral symbols. In

contrast, successful responses in incongruent conscious trials and just prior to a symbol's evaluation were not responsible for lacking affect-misattributions following conscious primes.

Again, we also found congruence sequence effects in the conscious go conditions, but not in the nonconscious go conditions. Therefore, the pattern is the same as in Experiment 1 and as predicted: The conditions showing more evidence of a congruence sequence effect show less evidence of affect-misattribution and the conditions showing less evidence of a congruence sequence effect show more evidence of affect-misattribution.

In the present experiment, we also observed another congruence-independent type of affect-misattributions to neutral symbols that was based on events of which the participants were aware: more negative ratings of the symbols in no-go than go trials. These affective responses were probably due to "distractor devaluation", which denotes that (no-go) stimuli requiring to be ignored or that are associated with (response) inhibition are evaluated more negatively than (go) targets (Fenske, Raymond, Kessler, Westoby, & Tipper, 2005; Raymond, Fenske, & Tavassoli, 2003). Because this effect was due to the no-go targets but measured in the evaluations of the symbols, devaluation must have been due to affect-misattribution. Yet, participants were aware of the no-go status of the targets as indicated by low error rates in no-go trials, and, thus, participants must have been aware of the true affect-origins. Therefore, even in the present study not all affect-misattributions depended

on the participants' unawareness of the true affect-origins (see also Fritz & Dreisbach, 2013; Payne et al., 2005).

General Discussion

We found more affect-misattributions following nonconscious primes than conscious primes. Only following nonconscious congruent primes but not after conscious congruent primes, the frequency of negative ratings of an otherwise neutral Chinese symbol dropped. This affect-misattribution was likely due to the participants' lacking awareness of the nonconscious primes as the true affect-origins, as indicated by the participants' unawareness of the nonconscious primes. The findings are in line with prior findings (e.g., Bornstein & D'Agostino, 1992; Murphy & Zajonc, 1993), but in advancement of these prior studies, the present experiments showed that the effects go hand in hand with a weaker congruence-sequence effect. We found no congruence-sequence effect and more affect-misattributions following nonconscious primes, and we found a congruence-sequence effect and no affect misattributions following conscious primes. Assuming that more misattributions of affect were possible in nonconscious than conscious priming conditions because target-prime congruence was not recognized as the true affect-origin in the former but not in the latter case, the pattern of results would explain why congruence-sequence effects with nonconscious primes are weaker: Because the processing of the primes in a trial n could be the result of the evaluation of the prime's influence in the preceding trial $n-1$ (Van Steenbergen et al., 2009, 2010), preconditions for these inter-trial contingencies would better be met in the conscious priming conditions. In the

conscious priming conditions, participants probably attributed the affective responses that were triggered by the target-prime pairs only to these stimuli as their true-affect origins. Because such affective responses are critical for the exertion of control over processing of the primes in a subsequent trial, with the conscious primes a congruence-sequence effect was observed.

In contrast, in the nonconscious priming conditions, participants sometimes misattributed their affective responses to also the Chinese symbols. Such misattributions could have either diluted the attribution of the affective responses to the target-prime pairs, or the misattributions could have even replaced the attributions of affective responses to their true origins. In the current study, we cannot tell which of these two possible explanations for the findings in the nonconscious conditions is more likely because we have not measured to what extent the evaluation of the prime-target pairs as the true affect-origins was influenced by prime visibility.

The corresponding target (or target-prime) evaluations could maybe be directly investigated in future nonconscious priming studies, but then it would be good to maybe use different targets in nonconscious congruent versus incongruent trials. With the current procedure, all that the participants clearly see in the nonconscious priming conditions are the targets. However, because the same two target identities (squares and diamonds) were used repeatedly (and equally often) in congruent and incongruent trials, even if the participants would have felt more or less negative due to congruent versus incongruent target-prime pairings, the danger

would be that judgments of the targets would also be influenced by (1) the higher fluency of the processing of all repeated target shapes (Bornstein & D'Agostino, 1992) and (2) a lack of a covariation between the affective responses to congruence versus incongruence (as more or less positive) and the perceived identities of the clearly visible targets (Kelley, 1967).

The lacking misattribution with conscious primes.

To our surprise, however, we found no evidence for affect-misattributions following conscious primes, although a prior Stroop study found affect-misattributions with conscious Stroop stimuli (Fritz & Dreisbach, 2013). Several reasons for this difference are conceivable. Maybe Stroop effects reflected congruence-effect sources that were not present in the flanker task. For example, only in the Stroop but not in the flanker task, congruent and incongruent trials could have differed in terms of the attentional demands during memory retrieval (Keele, 1972; MacLeod, 1991) or the degrees of task conflict (Goldfarb & Henik, 2007; MacLeod & MacDonald, 2000). If such Stroop-specific conflict sources depended on prime (or distractor) awareness, the absence of the same conflict sources in the flanker task could have prevented some conflict-dependent affect-misattributions based on the present study's conscious primes.

Relation to response priming.

One issue that deserves a brief discussion is the role of response priming in the present study. Here, we have described the evaluation of the Chinese symbols as being due to affect-misattributions. However, from the priming literature it is known

that a prime could also directly impact on a required response, without a mediating influence on a symbol in-between prime and response (for a discussion and test of the different influences of response priming vs. object-mediated priming effects, see Ansorge, Kiefer, Khalid, Grassl, & König, 2010). In the current study, this means that theoretically the target-prime relations could have served as primes themselves and might have directly influenced the evaluations as the responses, without first affecting the Chinese symbols via misattributions in-between the target-prime pair and the evaluation response. We think, however, that this is an unlikely alternative explanation of the current findings because under this perspective, there would be no obvious reason why affect-misattributions should have been stronger with nonconscious than conscious primes. In fact, in the present study response priming in the flanker task was stronger with conscious than nonconscious primes so that a simple response priming effect of the target-prime pair on overt evaluation responses would have been possible in conscious priming conditions, too. However, more future research is necessary to finally decide upon this open question.

Conclusion.

For many applications, such as for rational decisions based on evaluations and for attitudes towards objects and persons, it is important to understand the favorable side conditions of correct and incorrect affect-attributions. This is so important because the affective responses towards objects and persons shape and trigger human actions, such as how much effort we invest into getting an object or how much another person is pitied and helped. Here, we have identified conscious

processing and fewer affect misattributions as favorable side conditions for such rational decisions in yet another domain: the reactive adjustment of the participant's processing strategy to the changing advantages and disadvantages offered by accessory prime stimuli. This finding is in line with the assumption that sufficient awareness of the true affect-origins could indeed be helpful for correct affect-attributions and, thus, could be a precondition for rational human action (see also Topolinski & Strack, 2009).

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Author Contributions

FG and UA developed the study concept and the study design. FG performed the data collection, data analysis and interpretation under the supervision UA. FG drafted the manuscript, SK and UA added to the manuscript and provided many critical revisions. All authors approved the final version of the manuscript for submission.

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General Discussion

5.1 Summary

In the three studies of this cumulative thesis, I showed that top-down sets can be more complex than hitherto thought, and tested some correlates of the preconditions for a change/adaptation of these top-down sets. According to my results, prior experience and the economy of the search set could both have a substantial influence on attention capture. In detail, the results of the studies can be summed up as follows:

Chapter 2 studied the role of colour information during search for onset targets. The results showed that an onset cue that matches the colour of an onset target elicits a larger cueing effect than an onset cue with a non-matching colour. This was the case although all targets were onset targets, so that it was possible for the participants to search for the targets by their abrupt onsets alone. Therefore, it seems as if during search for onsets participants also take colour information into account. It is noteworthy that a few trivial explanations of the colour-based effects were ruled out: Pop-out search and differences in contrast and luminance transients between target-similar and target-dissimilar colour cues were not responsible for these results. Additionally, rapid disengagement of attention from colour non-matching onset cues was (at least on face validity, see discussion below) very unlikely and probably not responsible for my results. In the final experiment of Chapter 2, participants searched either for two onset targets of different colours (thus encouraging a top-down search sets for onsets) or for two white targets, half of which were onset targets and half of which were colour targets (thus encouraging a top-down search sets for colour). Here, colour non-matching onset cues did only capture attention during search for onsets, but not during search for colour. These results strengthen the notion that top-down control

settings vary as a function of the task context, depending on how helpful a feature is for discriminating targets from distractors.

Chapter 3 examined the role of inter-trial contingencies in attention capture experiments. During search for white onset targets, I found that non-matching cues created a cueing effect in the current trial n , if the same non-matching cue was valid on the previous trial $n-1$. In case of a previous non-matching invalid cue, I found suppression of non-matching cues in trial n . Furthermore, position priming was ruled out as an explanation for the inter-trial contingencies. The classical zero capture effect of non-matching cues in contingent capture experiments, could therefore be the result of averaging across standard cueing effects and inverted cueing effects. This means that capture and suppression of capture could both contribute to the performance in the non-matching conditions. This finding also indicates that top-down search sets reflect more than pre-experimentally implemented templates and inter-trial priming, but are also highly adaptive to task context and previous experience.

Chapter 4 investigated some preconditions of such inter-trial contingencies. In most studies, participants remained mostly unaware of whether primes (or cues) in the previous trial were helpful or not, and should therefore be processed or suppressed in the current trial. Under such conditions, participants do not show much strategic changes of their processing of the primes as a consequence of their utility. I reasoned that affective evaluations of more or less fluent processing of the prime-target pairs could be misattributed to a third object in temporal vicinity. By using a flanker task, I studied how conflict (in the form of incongruent compared to congruent prime-target pairs) influenced the evaluation of an otherwise neutral symbol that was unrelated to the task. Previously it was hypothesised that adaptation to the utility of a prime in $n-1$ is a reaction to negative affect that is evoked by incongruent primes (Dreisbach & Fischer, 2012; Fritz & Dreisbach, 2013; van Steenbergen et al., 2009; van Steenbergen, Band, & Hommel, 2010). Indeed, I found smaller inter-trial contingencies (i.e., less adaptation to

prime utility) and more affect-misattributions following nonconscious primes of which the participants were unaware, and larger inter-trial contingencies and less affect misattributions following conscious primes of which the participants were aware. Assuming that prime-target congruence and, hence, prime utility was not recognized as the true affect origin in the nonconscious condition, these results would explain why inter-trial contingencies with nonconscious primes are generally weaker (Gratton et al., 1992): Processing of the primes in a current trial n could be the result of the correct (affective) evaluation of only the prime's influence in the preceding trial $n-1$. This precondition would only be fulfilled if the participants were aware of the prime – that is, if it was consciously perceived. In case of nonconscious primes, the affect cannot be attributed to only its proper origin, but is partly also misattributed to a neutral symbol.

5.2 Limitations and open questions

In addition to the discussions in the preceding chapters, several more general limitations apply to the present studies of this thesis. A selection of these limitations will be outlined below. Additionally, further research questions that might shed more light on the complex origins of top-down control of selective visual attention will be discussed. Despite seeming unorthodox, I will mainly focus on open questions that stem from the experiments in the previous chapters and I will not discuss implications for theories of visual attention. Such a discussion can be found in the previous chapters. Furthermore, considerations about concrete experiments and research questions seem to be more fruitful at this point in time.

5.2.1 Automatic attention capture by special features

A particular promising approach in testing the adaptation of the currently pertaining top-down sets to the occurrence of further (redundant) features that allow finding relevant and ignore irrelevant information would be frequency information. For example, if the probability with which a feature would predict a

relevant stimulus should directly affect its perceived utility and, thus, the likelihood of its incorporation into the top-down set. Roughly speaking, the higher the probability of a feature is to predict the target, the higher its perceived utility and its integration into the top-down set should be. By systematically varying the probability of different target features, it should be possible to demonstrate different degrees of adaptation of the top-down sets. However, if feature or stimulus frequencies are manipulated, one should also consider the possibility that infrequently presented, unexpected, novel or surprising stimuli could capture attention automatically – that is, despite their lack of information for the targets, at least if certain further preconditions are met (for a review see Horstmann, 2015). For example, it has been shown that infrequently presented onset cues capture attention, even though a top-down set for colour had been firmly established (Folk & Remington, 2015). These results do not seem to hold for an infrequently presented colour cue during the search for onset targets, so that Folk & Remington (2015) concluded that infrequent onsets could be a special class of stimuli that capture attention automatically, thereby overriding top-down search sets. This argumentation would be in line with neural mechanisms that quickly detect events or stimuli that are highly relevant for survival (Ranganath & Rainer, 2003; Sokolov & Vinogradova, 1975). Yet not only survival, but also learning and memorising in general may depend upon the orientation towards novel stimuli as a critical precondition for any alteration of the top-down sets. Sokolov (1963) proposed that humans orient their attention towards stimuli that indicate changes in the external world. These stimuli make it necessary to update one's internal representation of the external world and to incorporate this novel information into the internal representation. Once the initially novel (or deviating) stimulus becomes predictable and common, its ability to capture attention automatically is lost. A similar argumentation was put forward to explain inhibition of return (Klein, 2000), a phenomenon that describes the inhibition a previously visited spatial location (see general introduction). The visual system seems to deliberately facilitate the orientation towards novel locations (or stimuli).

Given that colour information moderates attention capture by onset stimuli (see chapter 2, Experiments 1 to 4), the question arises how an additional match between less frequent onsets or colours of ‘novel’ stimuli would affect target-directed capture of attention. For instance, using high frequency of colour-defined targets and an only occasional (infrequent) usage of a similarly coloured onset targets could actually increase the contribution of an automatic capture by the targets. To test if such factors contributed to the performance in the current Chapter 2, one would have to devise entirely new paradigms, for instance, target-only trials in which a difficult search among distractors should be easier where the target has an infrequent onset on top of its relevant and always present top-down matching colour. A similar question can be asked with respect to more or less helpful cues that were valid in some of the preceding trials $n-1$ (see Chapter 3). Attention capture by these cues could also benefit from a relative low frequency of such cues, at least if the rare feature is an abrupt onset. It has never been tested if an on average (seemingly) null cueing effect of non-matching cues is in fact a joint function of (1) its utility in trial $n-1$ and (2) the rareness of the cue’s overall feature.

Partly related questions can be raised by jointly looking at Chapters 3 and 4. In Chapter 3, I have seen that the experienced utility of a cue in $n-1$ was responsible for its capture of attention in trial n . In Chapter 4, I have demonstrated that this could at least partly be due to a correct attribution of affect to these cues. This raises the question whether a cue that is helpful also elicits positive affect and could therefore be positively connoted and used in trial n . This has never been tested but some recent findings suggest that this mechanism could work. First, in general adaptation to the utility of accessory stimuli, such as primes, is fostered by congruent affective states (van Steenbergen et al., 2009). Second, affective stimuli are thought to capture attention, regardless of whether their affective value stems from emotional content or a history of reward (Kennedy & Most, 2015). Following the results from Chapter 4, attention capture by non-matching but previously helpful cues in Chapter 3, might also be driven by positive affect rather than by

strategic use of cue information. Further evidence for this assumption comes from studies showing that stimuli associated with reward capture attention automatically (see for example Theeuwes & Belopolsky, 2012). The subsection 5.2.4 will point towards possible ideas as to how to test whether attention capture in such cases is driven by affect, reward, or utility alone. Based on the studies in this thesis, I cannot distinguish between these two possibilities.

By using a percentile analysis of the reaction times in Chapter 2, I argued that rapid disengagement of attention probably played no role in these experiments. More efficient capture of attention by colour matching onset stimuli was present from the fastest reactions onwards. These results are, however, not entirely conclusive, because disengagement of attention might have occurred even prior to the first reaction time percentile. By using EEG, a much more time-sensitive method could be used to further elucidate this question. Moreover, one could also analyse if colour non-matching onset cues elicit event-related potentials (ERPs) that mark attentional engagement, such as the N2pc (Eimer, 1996). If they do not elicit such an ERP, the notion that onsets capture attention automatically would no longer hold. In fact, some recent literature suggests that the advantage of onset stimuli does not (always) reflect capture of attention but might sometimes stem from a different source (Wu, Remington, & Folk, 2014). These considerations lead to the questions about the time course of attention capture which I will discuss in the next subsection.

5.2.2 Time course and temporal stability

The analysis in Chapter 3 is based on cue utility in the previous trial. Although I obtained significant results with this ‘one-trial-back’ analysis, the question what would happen if the (especially the non-matching) cues were not only valid in trial $n-1$, but also in trial $n-2$, and so on was left open. Based on the priming literature, one might expect that the utility-based effects are further boosted with additional repetitions confirming the utility of the cues (cf. Maljkovic

& Nakayama, 1994, 1996). Even more interestingly, would be to ask what happens after the first encounter of an 'invalid colour' (or feature) non-matching cue. Do participants immediately abandon the non-matching cue as a source of helpful information? Or would they pay attention to such cues nevertheless because they proved useful in a majority of the last seen trials. How many trials would then have to be harmful to their performance until the participants decide to suppress active usage of the non-matching cues? It has been suggested that between 32 and 64 trials (Liao & Yeh, 2011) are necessary to construct a context from which information can be collected and used strategically for the control of attention. By employing longer sequences of valid and invalid non-matching cues, the temporal stability of inter-trial contingencies in attention capture experiments could be studied more closely.

Another group of interesting questions concerns how long target displays are presented to the participants, because it denotes how much information can be extracted from such a display. In general, one would expect that picking up of additional features of either cues or targets that are helpful for searching for the relevant stimuli is a function of the time that is available for the processing of the features and the stimuli. Therefore, the top-down search templates would be even more efficient where sufficient time allows registering and weighing all available features in cue and target displays. However, the length of the target displays can have also adverse influences on the top-down control of attention. For example, Kiss, Grubert, Petersen, and Eimer (2012) showed bottom-up capture by irrelevant colour singletons if participants had a longer time to search through the target display. Under conditions where the target display was only presented for a limited amount of time (200 ms), rapid suppression of salient, but non-matching stimuli was apparent. But, similarly, the temporal task demands may also moderate the influence of memory on attention capture. Under time pressure, the use of memory information may be more restricted than if more time is allowed to actually draw on memory (Dalvit & Eimer, 2011). One interpretation of the

influence of time on top-down contingent capture is that without time pressure, cognitive control is loosened, thus, enabling bottom-up processes to exert more control on attention. Under this perspective, bottom-up processes could be regarded as a default mode at work when the human mind is not preoccupied with a different task. An interesting consequence of this thought concerns serial versus parallel deployment of attention. Under high task demands, serial processing may be more feasible, because more focussed information (and thus suppression of irrelevant information) is needed to identify the target (Lavie, 1995; Zhang & Luck, 2015). Temporal demands play a role in the parallel deployment of attention to multiple target objects within the visual field (Eimer & Grubert, 2014). This also true if the target attribute changes unpredictably and search cannot be guided by a single target defining feature (Grubert & Eimer, 2015). However, as I see it, the seemingly bottom-up capture of attention under less restricted display length could also reflect the participants' active search for new features that might allow predicting the targets even better. Under this perspective, the longer target displays could lose their power as favourable side conditions of capture by apparently irrelevant features or stimuli with more and more experience with the displays in question.

5.2.3 The role of instruction and redundant features

Another question that I had to leave open is how search templates for a target carrying multiple features are represented by the nervous system. Are there two separate templates (e.g., one for colour, one for onsets) or is there one combined template including both features? Experimental evidence favours the notion that multiple templates work in parallel and interact or even compete with each other. If participants search for two colours simultaneously, increasing activation in the search template of one colour decreases the activation in the search template of the other colour (Olivers, Peters, Houtkamp, & Roelfsema, 2011). Along similar lines, Kiss, Grubert, and Eimer (2013) found that if a target is defined by multiple features, attention is initially captured by all stimuli that

match the target in at least one of these features, and that only afterwards attention was quickly withdrawn from the stimuli with features that only partially matched the target. The instruction to the participants may play an important role here, at least in the beginning of an experiment. A simple verbal instruction ('look for the red item and report it') or a more visual instruction ('decide whether you have seen an '=' or an 'X') could form top-down sets of varying strengths. Recent results from electrophysiological studies suggested that attentional templates are acquired quickly upon the first presentation, and resemble visually rich, analogous representations of real-world objects (Nako, Smith, & Eimer, 2015). Whether the pre-experimental task instruction would influence such results, remains an open question.

Chapter 2 as well as other studies (e.g. Mast & Frings, 2014) found that top-down search sets can be comprised of multiple features that can even stem from different modalities (see also Matusz & Eimer, 2013). The question arises whether *redundancy gains* (Miller, 1982) can be obtained of cues carrying multiple features of the target. Redundancy gains are usually observed in visual search tasks and describe the phenomenon that multiple target defining features (such as a red, vertically aligned bar) speed up the target identification, although one feature would be sufficient to discriminate targets from distractors (e.g., because all distractor stimuli are green). Redundancy gains have been found in reaction times (Krummenacher, Müller, & Heller, 2001, 2002) as well as in ERPs (Grubert, Krummenacher, & Eimer, 2011; Töllner, Zehetleitner, Krummenacher, & Müller, 2011). It would be interesting to study if colour-matching onset cues (as in Chapter 3) would elicit redundancy gains if compared to an onset-only matching cue during search for an onset target. A nice comparison could be between colour-only matching cue's ERP effects in pure colour-target blocks and in colour-plus-onset target blocks. Here, every tested cueing display could be exactly the same and only the features of the target context would vary. If with such a procedure a redundancy gain in the attentional ERPs could be established, this would be

further evidence for the inclusion of additional features into the set of top-features beyond the instructed features.

5.2.4 Attribution of affect and alternative measurements

In the current Chapter 4, many questions were left open. Chapter 4 argued that nonconscious primes lead to the misattribution of affect towards a different object. However, does this also mean that affect was attributed away from the prime-target congruency as the true affect origin? And is the situation concerning the evaluation of the prime-target congruency different with conscious primes? The most straightforward prediction would be that in conscious priming conditions, the affect is correctly attributed to the prime-target pairs. A study that lets participants evaluate the prime-target pairs of the flanker task under conscious and unconscious priming conditions would clarify this issue. Of course a major caveat would be that the prime-target pairs are (regarding their visual complexity) much simpler than the Chinese pictographs. Moreover, and in contrast to the pictographs which were only used once in either a congruent or an incongruent trial, neither prime nor target identity would consistently co-vary with only one affective response. Thus, participants may be more hesitant to give an intuitive judgment of varying affective content. Furthermore, as primes and targets are repeated multiple times, positive affect stemming from mere exposure could overshadow any remaining influences of prime-target congruency on the evaluative judgments (Murphy & Zajonc, 1993; Zajonc, 1968). One possible way to circumvent this problem would be the use of different measurements of affect than directly prompted evaluations. As methodical monotony can be considered as one major caveat of Chapter 4 and related research, other measurements would allow for a more subtle approach to study the effect of affect on inter-trial contingencies.

For example, pupil dilation has previously been used as a marker of conflict adaptation (van Steenbergen & Band, 2013). In a Simon task trials that induced more or less response conflict as one type of congruency, these authors found that

pupil dilation mirrored the pattern of the congruence sequence effect in reaction times: Decreased pupil dilation in an incongruent trial n was found, if it followed an $n-1$ incongruent trial. Pupil responses are an increasingly favoured measurement to study (nonconscious) cognitive processes (Laeng, Sirois, & Gredebäck, 2012), but many issues would remain. Most importantly, pupil dilation may reflect multiple cognitive processes such as motor preparation (Cohen, Botvinick, & Carter, 2000), task demands (Ariel & Castel, 2014; Kahneman & Beatty, 1966) and arousal (R. Henderson, Bradley, & Lang, 2014), and may, therefore, also not be ideal to clarify the evaluation of congruent versus incongruent trials and the origin of the adaptation effects found in Chapters 3 and 4. A combined measurement of pupil dilation and electro-dermal activity (Krupinski & Locher, 1988; Lang, Greenwald, Bradley, & Hamm, 1993) could be more promising and might be of more help for clarifying whether affect is the driving factor here. Event-related potentials could also be used to study in a more exhaustive way whether nonconscious conflict in Chapter 4 elicits conflict adaptation. It has been shown that the error-related negativity (ERN) is sensitive to congruency and conflict, and can even dissociate between stronger and weaker degrees of conflict (for a review see Larson, Clayson, & Clawson, 2014). The ERN as evidence for adaptation effects might be observed even in such situations where the overt behaviour does not show these adaptation effects.

5.3 Perspectives

As can be seen by the discussion above, this thesis answers some questions regarding the interplay of time, prior experience, and the top-down control of attention capture, while at the same time posing many new questions. The astonishingly complex interplay between top-down, bottom-up, and other processes, such as memory and implicit task demands, paints the picture of a Gordian knot. From our own everyday experience, we know how dependent our attention and perception is on situational factors. To disentangle all these factors

and to provide a holistic model of selective visual attention that accounts for all these factors is far beyond the scope of this thesis. A lot of new research questions arise from this thesis that at least promise to advance our understanding of the operation of visual attention in the more controlled situations of the contingent-capture protocol and related experimental procedures.

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Zusammenfassung

Jeden Moment erreicht unsere Augen eine unglaubliche Fülle an visueller Information. Menschen selektieren aus dieser Information aber nur einen kleinen Teil zur Weiterverarbeitung, während der restliche Teil ignoriert wird. Diese Selektivität ist stark von Vorerfahrungen und Umgebungsfaktoren abhängig. Die vorliegende Doktorarbeit diskutiert drei dieser Faktoren näher. Studie 1 untersucht den Einfluss der Aufgabe in einem Experiment auf die Lenkung selektiver visueller Aufmerksamkeit durch plötzlich auftauchende Reize und durch farb-definierte Reize, während nach plötzlich auftauchenden Reizen gesucht wird. Versuchspersonen können ihr Wissen über die Effektivität bestimmter Eigenschaften von Reizen strategisch einsetzen um Zielreize von Störreizen zu unterscheiden. In acht Experimenten wird demonstriert, dass Versuchspersonen Farbinformationen selbst während der Suche nach plötzlich auftauchenden Reizen nutzen, wenn sich diese Farbinformation als nützlich erweist. Studie 2 untersucht in wie weit Versuchspersonen ihre Aufmerksamkeit Reizen zuwenden, die sich in der Vergangenheit (etwa im letzten Durchgang) als nützlich erwiesen haben und in wie weit diese Nützlichkeit vom instruierten Suchset abweichen kann. In insgesamt drei Experimenten wird gezeigt, dass diese Nützlichkeit einen großen Einfluss auf die Lenkung unserer Aufmerksamkeit hat. Das gilt sogar für Reizeigenschaften die irrelevant für die eigentliche Suche sind. Studie 3 beschäftigt sich mit dem Ausbleiben solcher Effekte, wenn Versuchspersonen solche Reize subliminal dargeboten werden. In zwei Experimenten wird demonstriert, dass Versuchspersonen die Nützlichkeit (oder Schädlichkeit) solcher Reize unter subliminalen Bedingungen auf ein anderes Objekt attribuieren. Diese Fehlattribution könnte erklären, warum subliminal dargebotene Reize keine Verhaltensanpassung zur Folge haben. Diese Ideen werden in der vorliegenden Doktorarbeit weiter ausgeführt werden und mit aktueller Forschung in Bezug gesetzt werden.

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Bachelor studies in German Language Studies and Media Sciences 10|2006 – 10|2007
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Publications

- [1] **Goller, F.**, Ditye, T., & Ansorge, U. (2016). The contribution of color to attention capture effects during search for onset targets. *Attention, Perception, & Psychophysics*, 1-19. <http://doi.org/10.3758/s13414-015-1053-8>
- [2] **Goller, F.**, & Ansorge, U. (2015). There is more to trial history than priming in attentional capture experiments. *Attention, Perception, & Psychophysics*, 77(5), 1574-1584. <http://doi.org/10.3758/s13414-015-0896-3>

Manuscripts

- [3] **Goller, F.**, Khalid, S., & Ansorge (2016). The good, the bad, and the misattributed: The facilitating influence of nonconscious affect-origins on misattributions of affect. *Manuscript submitted for publication.*
- [4] Ditye, T., Huber-Huber, C., **Goller, F.**, & Ansorge, U. (2016). *Low-level versus high-level processing in visual attention.* Manuscript submitted for publication.
- [5] Ansorge, U., Ditye, T., & **Goller, F.** (2016). *Investigating the contribution of spatial attention shifts to sequential modulations of a spatial stimulus-response congruence effect.* Manuscript submitted for publication.

Conference Presentations

— 2015 —

- [1] **Goller F.**, Lee, D., Ansorge, U & Choi, S. (2015). How I speak is what I see – About differences in gaze behaviour between Korean and German speakers. Poster presented at the *ECCEM (European Conference on Eye Movements)*, Vienna, Austria, August 16–21, 2015.
- [2] **Goller F.**, & Ansorge, U. (2015). Old Dog – New Tricks? New perspectives on contingent attentional capture. Poster presented at the *A-Dok Workshop for Doctoral Students in Cognitive Psychology*, Jena, Germany, June 19–21, 2015.
- [3] **Goller F.**, & Ansorge, U. (2015). Inter-Trial Contingencies in Contingent-Capture Experiments. Poster presented at the *Vision Sciences Society 15th Annual Meeting*, St Pete Beach, FL, USA, May 15–20, 2015.
- [4] **Goller, F.**, & Ansorge, U. (2015). Is conflict the root of all evil? An approach to clarify the effect of conflict priming on target valence. Poster presented at the *TeaP 2015 (Tagung experimentell arbeitender Psychologen/Conference of Experimental Psychologists)*, Hildesheim, Germany, March 08-11, 2015.

— 2014 —

- [5] **Goller, F.**, & Ansorge, U. (2014). The role of colour in the contingent capture paradigm. Poster presented at the *HExKop (Autumn Meeting of Experimental Psychologists)*, Trier, Germany, November 06-09, 2014.
- [6] **Goller, F.**, Valuch, C., & Ansorge, U. (2014). Follow me! A spatial compatibility effect of perspective changes on viewing directions in cinematic cuts. Poster presented at the *ECVP (European Conference on Visual Perception)*, Belgrade, Serbia, August 24-28, 2014.
- [7] **Goller, F.**, Valuch, C., & Ansorge, U. (2014). „Links, Rechts, Mitte, Nahe oder Totale“ – Über den Einfluss der Kameraperspektive auf das Blickverhalten beim Betrachten

von Filmen. Poster presented at the 11th Conference of the *Austrian Society of Psychology (Oesterreichische Gesellschaft fuer Psychologie)*, Vienna, Austria, April 24-26, 2014.

- [8] **Goller, F., & Ansorge, U.** (2015). There is more to contingent capture than feature search. Talk presented at the *TeaP 2014 (Tagung experimentell arbeitender Psychologen/Conference of Experimental Psychologists)*, Gießen, Germany, March 30 - April 02, 2014.

Teaching Experience

Undergraduate courses

Seminar on scientific reading	2015
Bachelor thesis writing seminar	2014 & 2016
Cognitive Principles of Perception and Behavior	2013 - 2016
Lab visit "Cognitive Psychology" with an introduction to eye-tracking and EEG	2013 - 2016

Professional Activities

Spokesman of the 'Young Scientists' for the Austrian Society of Psychology (ÖGP)	since 2015
Representative of the 'Young Scientists' at the Faculty of Psychology, University of Vienna	2013 - 2016

Memberships in Organizations

Austrian Society of Psychology (ÖGP)	Member since 2015
Vision Sciences Society (VSS)	2015

Conference Co-Organization

ECCEM: European Conference On Eye Movements, University of Vienna	Conference date August 16-21, 2015
Poster Session of the 'Young Scientists Psychology', University of Vienna	June 24, 2014
Visual Attention Meeting, University of Vienna	July 10-12, 2013
TeaP: Conference of Experimental Psychologists, University of Vienna	March 24-27, 2013