# MEMORY-GUIDED SELECTIVE ATTENTION: AN INSTANCE THEORY OF AUTOMATIC ATTENTIONAL CONTROL

by

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This manuscript has been read and accepted for the Graduate Faculty in Psychology in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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# ABSTRACT


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Cognitive control enables flexible goal-directed behavior via attention and action selection processes that prioritize goal-relevant over irrelevant information. These processes allow us to behave flexibly in the face of contradicting or ambiguous information and update behavior in response to the changing environment. Furthermore, they are thought to be in direct opposition to learned, automatic processing in that they enable us to disregard learned behaviors when they are inconsistent with our current goals. The strict dichotomy between stimulus-driven and goal-driven influences, however, has downplayed the role of memory in guiding attention. The position forwarded in this thesis is that a memory-based framework is needed to fully understand attentional control. People often re-encounter similar objects, tasks, and environments that require similar cognitive control operations. A memory-retrieval process could shortcut the slow, effortful, and resource-demanding task of updating control settings by retrieving and reinstating the control procedures used in the past. The aim of the current thesis is to empirically test general principles of an instance theory of automatic attentional control using a converging operations approach. In Chapter 2, I examine the obligatory nature of memory encoding by investigating context-specific proportion congruent effects in a non-conflict selective attention task. In Chapter 3, I examine the assumption of long-term instance-based representation by investigating long-term single-trial effects in a context-cuing flanker paradigm. Finally, in Chapter 4 I examine how memory retrieval can influence context-specific attentional control in a context-specific proportion congruent task.

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# CHAPTER 1

# GENERAL INTRODUCTION

Cognitive control refers collectively to all the processes required to adjust goal-directed behavior via attention and action selection processes that prioritize goal-relevant over irrelevant information (e.g., Egner, 2017; Miller and Cohen, 2001). These processes allow us to behave flexibly in the face of contradicting or ambiguous information and update behavior in response to the changing environment. Control processes are often thought to be in direct opposition to learned, automatic processing in that they enable us to disregard learned behaviors when they are inconsistent with our current goals. While driving, for example, you have learned that when the traffic light turns yellow, you should hit the breaks to prepare for a stop. However, if you are already halfway through the intersection, then you will need to exert cognitive control to inhibit that learned response and ignore the yellow light.

Historically, cognitive control has fulfilled this supervisory role (e.g., Norman and Shallice, 1986) and is typically thought to be voluntary (e.g., Bugg and Crump, 2012), conscious (e.g., Dehaene and Naccache, 2001; Evans and Stanovich, 2013; Kunde, Reuss, and Kiesel, 2012), and domain-general (e.g., Kan et al., 2013). However, adopting a strict dichotomy between “controlled” and “automatic” processes (Posner and Snyder, 1975b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977) downplays the possibility that memory might play a role in guiding control processing (Awh, Belopolsky, and Theeuwes, 2012; Hutchinson and Turk-Browne, 2012). Likewise, a recent surge in evidence that control processes can be shaped by prior experiences in a seemingly stimulus-driven manner suggests that memory may play a larger role in directing attention than previously thought (Bugg and Crump, 2012; Egner, 2014).

The position forwarded in this thesis is that a memory-based framework is needed to fully understand attentional control. People often re-encounter similar objects, tasks, and environments that require similar cognitive control operations. A memory-retrieval process could shortcut the slow, effortful, and resource-demanding task of updating control by retrieving and reinstating the control procedures used in the past. The central aim of the thesis is to investigate general principles of an instance-based memory account of automatic attentional control.

Before moving on, it is worth outlining the general structure of the thesis. The goal of the introduction will be to motivate the need for examining attentional control from a memory perspective. I will first review theories of attention and demonstrate that almost all include a role for memory but typically do not elaborate on that role. I will argue that this demonstrates a need for a memory-based framework from a theoretical perspective. I will then review empirical evidence in the selective attention domain demonstrating that prior experiences influence selective attention performance in a stimulus-driven manner. I will argue that this demonstrates a need for a memory-based framework from an empirical perspective. Finally, I will outline the instance theory of automatic attentional control and review its theoretical foundations.

Chapter’s 2 and 3 are published in peer-reviewed journals. Chapter 4 is currently under review. Each chapter will begin with a short preface to link the empirical work with the central aims of the thesis. Finally, in the general discussion I will evaluate the empirical work as a whole and elaborate on the contribution of the thesis to furthering theories of attention and cognitive control.

## Does attention have a memory? Perspectives on automatic control

In the following sections I will review perspectives on attentional control. This review is not exhaustive, but provides a representative selection of attention theories. The aim of this review is to examine the role memory plays in theories of attention. I will argue that memory has always held a prominent role in theoretical treatments of attention. Although modern research tends to characterizes attentional theories as all-or-none dichotomies such as controlled versus automatic (also, at times framed as top-down/bottom-up and endogenous/exogenous), almost all theories of attention allow for an “automatic control” whereby attention is guided by an internal memory representation. I will first provide some historical context to better situate each of the perspectives before describing each theory in more detail.

**Some historical context**

Much of cognitive psychology is predicated on the assumption that responses to stimuli are mediated by an information processing system that is inherently limited in capacity and in need of an information selection mechanism (Allport, 1989, 1993). Attention is thought to fulfill this role: it allows the information processing system to prioritize further processing of relevant information, making efficient use of the available resources, and preventing the system from overloading. Bottleneck models of attention conceptualized this control process as a structural property of the information processing system situated somewhere along the processing chain. The debate about where the filter should be placed along the processing chain (i.e., the early versus late selection debate) largely dominated this era of attention research and produced a number of influential attention theories (Broadbent, 1958; Deutsch and Deutsch, 1963; Kahneman and Daniel, 1973; Norman, 1968, 1976; Treisman, 1960, 1964; A. M. Treisman, 1969; for a review, see Driver, 2001).

By the 1970s however, the bottleneck models of attention were largely overshadowed by capacity or resource models of attention (e.g., Kahneman and Daniel, 1973). Instead of a processing structure, attention was conceptualized as a resource that could be flexibly allocated at different stages of processing. Very early stages of processing, for instance, were thought to require little to no attentional resources and later stages required increasingly more attentional resources (Hasher and Zacks, 1979; Lavie, 1995, 2000; Lavie and Tsal, 1994). This work ushered in a new debate about controlled versus automatic processing (Logan, 1988b, 1992; Posner and Snyder, 1975b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977) that has dominated modern thinking of attentional control (e.g., Botvinick, Braver, Barch, Carter, and Cohen, 2001; Moors and De Houwer, 2006).

More recently, views of selective attention have reverted back to conceptualizing attention as a control process subsumed under the “cognitive control” umbrella (Miller, Galanter, and Pribram, 1960; Posner and Snyder, 1975b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977). Cognitive control refers to a collection of processes that allows us to flexibly process and act in accordance to our internal goals and adapt to changes in our environmental context. It includes, for example, the ability to filter out task-irrelevant information (selective attention), update our task set to accommodate changes in our goals (task-switching), and inhibit inappropriate behaviors (response inhibition). As a construct, cognitive control evolved out of the literature on attention and is arguably inseparable from theories of selective attention (e.g., Botvinick et al., 2001; Cohen, Dunbar, and McClelland, 1990; Egner, 2017); though, whether attention should be considered solely a function of control is questionable (e.g., Petersen and Posner, 2012).

Theories of cognitive control typically adopt the strict controlled/automatic dichotomy and include assumptions about capacity limitations (Egner, 2017). Over the last two decades, research has largely focused on how top-down control biases stimulus processing and response selection (e.g., Botvinick and Cohen, 2014). More recently, there has been increasing interest in how control is regulated over time and how attentional control changes with experience. This work tends to appeal to principles of associative learning to explain how experience can shape control processing in a seemingly automatic, stimulus-specific manner (Abrahamse, Braem, Notebaert, and Verguts, 2016; Chiu and Egner, 2019; Egner, 2014; Failing and Theeuwes, 2018; Jiang, 2018; Jiang and Sisk, 2019). Similarly, recent advances in the associative learning literature have demonstrated that learned “predictiveness” and “value” modulates both deliberate and automatic attention; much like in the cognitive control literature, they have proposed an automatic influence of learning on attention or “learned attentional priorities” (Kruschke, 2011; Le Pelley, Mitchell, Beesley, George, and Wills, 2016; Mackintosh, 1975).

### Treisman’s attenuation model of attention (1960; 1964; 1969)

Treisman’s model of attention (1960, 1964; 1969) was developed as a direct response to evidence that could not be accounted for by Broadbent’s influential filter model (1958). Broadbent’s filter theory was an extreme example of an ‘early selection’ attention model. He proposed two qualitatively different stages of processing: The first extracted physical properties (e.g., pitch, location) from incoming stimuli in a parallel manner; the second extracted more complex psychological characteristics like word meaning. The second stage was thought to be limited in capacity, unable to deal with all the incoming stimuli, and constrained to serial processing. The attentional filter was therefore placed between the first and second stage of processing as a means to protect the second stage from being overloaded. As a consequence, all unattended information, selected on the basis of physical properties, were blocked entirely from further processing.

Treisman proposed an alternative model to accommodate growing evidence that sometimes unattended information was processed deeper than predicted by an early selection model (Cherry, 1953; Moray, 1959; Morton, 1969; Oswald, Taylor, and Treisman, 1960; Shiffrin and Gardner, 1972; Treisman, 1960; A. M. Treisman and Riley, 1969). For instance, Moray (1959) found that participants would regularly notice their own name even when presented in the unattended channel (also see, Wood and Cowan, 1995) and Treisman found that words were recognized in the unattended channel when they were highly probable, predicted by the semantic context of the attended channel. To address these findings, Treisman made two revisions to the early filter model: First, she suggested that the selective filter does not 'block' incoming information completely, but instead 'attenuates' the signal. The second stage would now receive some input from the unattended stimuli albeit less than the attended stimuli. Second, she suggested that words have different thresholds for detection.

This second point is of particular importance for the present discussion on memory. Treisman posits a 'dictionary' or store of known words, each associated with different thresholds for detection. Important words like our own name, "help", or "fire" have permanently lowered thresholds and could be heard even if they were completely unattended (attenuated). Similarly, expectations and contextual information could temporarily lower thresholds allowing the detection of unattended, task-relevant information. According to Treisman, "if the three words 'I sang a' were heard, *the stored trace* of the word 'song' in the dictionary would have its threshold considerably lowered" (Treisman, 1960). This would explain why task-relevant words, supported by the immediate context, could be heard in the unattended channel.

Thus, according to this model, selection can occur automatically for unattended stimuli when they are associated with a lowered threshold. To accept this assumption however, requires that all stimuli, attenuated or not, are able to cue memory and retrieve associated thresholds prior to further selection. Treisman's model therefore, includes a memory-guided selection mechanism. However, how stimuli come to be associated with lowered thresholds and/or how attenuation influences memory encoding and retrieval was not explained.

**Norman's theory of memory and attention (1968)**

In contrast to the attenuation model, other 'late selection' models were also formulated to address the limitations of Broadbent's early selection model (e.g., Deutsch and Deutsch, 1963; Duncan, 1980; Norman, 1968; Posner, 1978). Late selection models also propose a distinction between the initial parallel processing stage and a second, capacity-limited processing stage. However, these models posit that all characteristics of a stimulus are extracted at the early stage, *before selection occurs* and the selection process has access to, and can make use of, all of this information (e.g., physical features, meaning, etc.). Selection however, still occurs before stimuli are encoded into long-term memory. The lack of awareness of unattended stimuli, according to this view, has less to do with perceptual processing and more to do with attentional selection occurring before explicit memory formation.

Norman (1968, 1976) provided one such late stage selection model (see also, Deutsch and Deutsch, 1963). The problem of selection, according to Norman, could be resolved if the initial analysis was performed automatically, without any need for complex cognitive processing. The proposed solution was to allow the initial stage of perceptual processing to have access to memory storage. He posited that sensory information automatically activates its associated memory representation without any intervening cognitive processing. Attention occurs after this initial stage of processing, selecting inputs that are deemed relevant based both on *stored attributes* of each input and physical attributes of each input.

Thus, there are two sources of input that determines selection: the sensory input and, what Norman refers to as, the 'pertinence' input. The pertinence input is the pre-specified importance or relevance of an input. The pertinence of an input can be set temporarily by the context, expectations, task-goals etc., but like Treisman's model, stimuli may also have associated levels of pertinence (e.g., your own name) – Presumably, the automatic activation of a memory representation also retrieves its associated level of pertinence. According to this model then, both the sensory and the pertinence inputs activate representations in memory storage simultaneously and those inputs having the highest activation levels are selected for further processing.

Although Norman refers to two sources of inputs, in my view, there are actually three sources of information that determine selection. The sensory input, or physical salience of the stimulus, is one source of information. But the pertinence input, I would argue, is actually two sources of information: First, top-down goals can pre-set pertinence levels of task-relevant features prior to stimulus presentation. For example, if I know that the target will be red, I can temporarily increase the pertinence of the color red in preparation. Second, pertinence levels are retrieved from memory. Thus, if I hear my name, this cues the retrieval of a high pertinence level. How these two sources of information are aggregated into the 'pertinence' input is not clear. Similarly, how memory encodes, stores, and retrieves 'pertinence' values was not explained. Norman's model however, clearly includes a mechanism for memory-guided attention and is actually very similar to modern explanations of 'selection history' effects (e.g., Awh et al., 2012; Failing and Theeuwes, 2018).

**Controlled, automatic and the "automatic attention response"**

Building on filter models of attention, which distinguished between a parallel and serial stage of processing (Broadbent, 1958; Deutsch and Deutsch, 1963; Norman, 1968; A. M. Treisman, 1969), Shiffrin and Schneider proposed an information processing model that defined selective attention in terms of control processes. In contrast to previous models that focused on where the filter occurred, Shiffrin and Schneider shifted the conversation toward the distinction between *controlled* and *automatic* processing (Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977; see also, Posner and Snyder, 1975b).

Information processing, under this view, is based on the activation of a sequence of nodes in long-term memory. A process is considered "automatic" if, given a particular input, a sequence of nodes are activated automatically “without the necessity of active control or attention” (Schneider and Shiffrin, 1977, p. 2). Automatic processes rely on a relatively permanent set of associative connections and require an appreciable amount of training to fully develop. Additionally, once an automatic process is learned, it is difficult to suppress or modify.

A process is considered “controlled” if a sequence of nodes are temporarily activated under “the control and attention of the subject” (Schneider and Shiffrin, 1977, p. 2). Controlled processes are capacity limited and only one sequence can be activated at a time without interference. Whereas automatic processes are considered fast and parallel, controlled processes are slow and serial. The trade-off however, is in flexibility: Automatic processes are rigid and ballistic, triggered by their associated inputs. Controlled processes can be flexibly set up, modified, and applied in novel situations.

Interestingly, Shiffrin and Schneider included a third kind of process called the “automatic attention response”. The automatic attention response was thought to be a special kind of automatic process. They proposed that training to recognize certain inputs as targets causes those inputs to automatically direct attention to the target, regardless of concurrent inputs or memory load. That is, those stimuli would acquire the ability to initiate an automatic attention response. They go on to suggest that automatic attention responses could be attached to individual stimulus features as well as categorical features and learning could occur simultaneously, perhaps at different rates, for different levels of features. Learning however, seems to be a matter of repeated pairing: the more often a controlled attentional response is paired with a particular input, the stronger the association and likelihood that the input will acquire the ability to direct attention automatically.

However, there does seem to be some confusion in how Shiffrin and Schneider use the terms “control” and “attention” (for a longer discussion of the opaque language, see Moors and De Houwer, 2006). For instance, they define automaticity as a process that does not need attention (e.g., “activated without the necessity of active control or attention by the subject”; Schneider and Shiffrin, 1977, p. 2), but then also claim that attention is “a controlled process” (Schneider and Shiffrin, 1977, p. 2) that can be directed automatically (i.e., the automatic attention response). Similarly, they refer to an "attention director" as a controlled process (Shiffrin and Schneider, 1977, p. 163). The unfortunate result of this ambiguous language is that "attention" can be directed automatically without the need for "attention".

On the one hand, they seem to be referring to "attention" in the traditional sense, as a selective filtering mechanism. That is, attention is the process that prioritizes the selection of perceptual stimuli. On the other hand, they also seem to be using "attention" to refer to higher-level, executive functions as well. One reasonable interpretation of the automatic attention response then, is that attentional selection and orienting can be triggered automatically without the need for higher-level executive functions.

In many ways, Shiffrin and Schneider's model is consistent with late-stage filtering models (e.g., Deutsch and Deutsch, 1963; Norman, 1968) in that all characteristics of the stimuli are fully processed and, like the Norman model (1968), early sensory processing has access to memory storage – prior to selection – enabling the automatic attention response. Unlike previous models however, where parallel and serial processing stages represent qualitatively different stages of a feed-forward processing system, this model assumes that an automatic attention response could be triggered at any level of stimulus processing.

**Attention from an associative learning perspective**

Attention has long-held a prominent role in theories of human and animal learning (Kruschke, 2001, 2011; Le Pelley, 2004; Le Pelley et al., 2016; Mackintosh, 1975). Typically, within these learning models, attention is thought to modulate how a cue is used during associative learning as indexed by a cue's *associability*. In the Rescorla and Wagner (1972) model, for instance, different cues are thought to be differentially attended resulting in varying degrees of associability and learning rates. Though, within this framework there was no theory for how learning rates were adjusted through experience. In Mackintosh's (1975) model, attention to specific cues could vary with with experience. Here, attention, again expressed through a cues associability, is thought to increase for cues that been predictive of some outcome, while decreasing for cues that have been non-predictive. Similarly, Pearce and Hall (1980) also present a model where attention changes with experience, though here attention increases toward cues when their outcomes are surprising (for a review, see Le Pelley, 2016).

Importantly, this work has traditionally focused on how learning influences attention, *as indexed by a cues associability*; a departure from the cognitive tradition. More recent work has begun exploring whether learning influences other aspects of stimulus processing that are traditionally taken as evidence of attentional changes or if it only influences the associability of cues (e.g., Le Pelley, 2004). For instance, there is evidence that knowledge of the predictiveness of cues can influence attention in a deliberate, top-down manner (Mitchell, Griffiths, Seetoo, and Lovibond, 2012). There is also evidence that learning about a cues predictiveness can influence attention in a more bottom-up, automatic manner. For instance, predictive stimuli were found to be less impaired than non-predictive stimuli in attentional blink paradigms (Glautier and Shih, 2015; Livesey, Harris, and Harris, 2009) and increasing the predictiveness of stimuli produced attentional biases in spatial cuing paradigms (Haselgrove et al., 2016; Le Pelley, Vadillo, and Luque, 2013). Finally, there is also a growing body of evidence demonstrating that the learned "value" of a stimulus can also influence attention across a variety of tasks (B. A. Anderson et al., 2011; Della Libera and Chelazzi, 2009; for a review, see Le Pelley et al., 2016).

Based on this evidence, Le Pelley (2016) has proposed that learning about a cue's predictiveness and value influences more than associability (e.g., Mackintosh, 1975). Instead, it has been proposed that learning has a more general effect on attentional selection and orienting, and as such, supports the notion that "attention can be learned just as other behavioral responses are learned" (Le Pelley, 2016, p. 1124).

### Associative learning from an attention perspective

The role of associative learning, although certainly referenced in attention models, has only recently taken a prominent role in the attention literature. For instance, there has been increasing interest in how prior experiences bias visual search (e.g., Chen and Hutchinson, 2018; Chun and Jiang, 1998, 2003; Hutchinson and Turk-Browne, 2012; Wolfe and Horowitz, 2017). Traditionally, this literature has distinguished between goal-driven and stimulus-driven attentional control. More recently some have argued for “selection history” as a third category of attentional control (Awh et al., 2012; Failing and Theeuwes, 2018; Wolfe and Horowitz, 2017). Attentional control via selection history occurs when prior experiences of attentional selection influence ongoing selection above and beyond goal-driven and stimulus-driven control.

This view consistent with a large body of evidence demonstrating that the history of attentional selection can and does bias attention in visual search (Chun and Jiang, 1998, 2003; Cosman and Vecera, 2013a, 2013b, 2014; Vecera, Cosman, Vatterott, and Roper, 2014; for a review, see Failing and Theeuwes, 2018). Selection history effects are often explained as the result of associative learning: through repeated attentional selection, stimuli become associated with a higher level of attentional priority (Awh et al., 2012; Failing and Theeuwes, 2018). Though, the mechanism by which this occurs has not been made clear. In some cases, it has been suggested that associated priority maps are retrieved and reinstated (Awh et al., 2012), others have suggested that the representation of the stimulus is somehow altered to include “positive” or “negative” priority (Failing and Theeuwes, 2018).

Similarly, the cognitive control and selective attention literature’s have also begun to invoke associative learning as a means to explain the influence of prior experience (Abrahamse et al., 2016; Braem and Egner, 2018; Egner, 2014). In particular, there is growing evidence that various control processes can become context-dependent (to be reviewed in more detail below). To explain such phenomena, some have suggested that associative learning extends beyond perceptual and/or motor representations to include goal representations. That is, perceptual, motor, and goal representations become bound together in an associative network to enable flexible, context-dependent control. Therefore, a particular stimulus feature (e.g., the color red) could become associated with a particular goal representation (e.g., inhibit) and the presentation of that stimulus would trigger the associated control process.

**A synthesis of automatic control perspectives**

Views on attentional control have changed quite dramatically over time and often these shifts in theoretical focus have occurred in an idiosyncratic, paradigm-specific manner (Allport, 1993; B. Anderson, 2011; Logan, 2004). Quite consistently however, theoretical accounts of attention have included an "automatic" mode of attentional control that does not fit neatly into the oft-cited dichotomies (e.g., top-down/bottom-up, exogenous/endogenous, controlled/automatic). To briefly synthesize these views: there is widespread consensus that attention can be directed in a voluntary, goal-directed manner and in an involuntary, stimulus-driven manner. Almost all theories, however, also include a third kind of control whereby stimuli are prioritized, not on the basis of current goals or the physical salience of the stimulus, but on the basis of prior experience with the stimulus.

To account for the influence of prior experience, these models assume that an internal memory representation can guide attentional selection. Though they use different terminology, all assume attentional control settings (e.g., thresholds, priorities, pertinence, attentional responses) are preserved in memory, retrieved, and used to guide attention. They also all tend to agree that the retrieval and influence of the internal representation is stimulus-driven and automatic (cued by the mere presence of the stimulus).

Equally consistent across models is a lack of theoretical framework for explaining how attentional control settings are stored and retrieved from memory. That is, all models make vague appeals to some sort of associative learning process whereby pairing an attentional control setting with a stimulus creates an associative link. These explanations, in my view, are unsatisfactory in that they do not make substantive predictions about how and when we should expect previously-experienced stimuli to cue attentional control. Furthermore, there is not even agreement about very general principles of learning.

For instance, some argue that creating an associative link requires extensive practice (Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977; A. M. Treisman, 1969) like the automatization of motor skills (Logan, 1988b, 1992). However, these claims were typically made because the data collected at the time suggested that practice was required, not because of any theoretical considerations. Similarly, these models do not explain how learning occurs other than referring to it as an associative process.

In contrast, late stage selection models like Norman's theory of attention and memory (Deutsch and Deutsch, 1963; Norman, 1968, 1969, 1976), do not make assumptions about practice. Under these views, automatic attention is the result of an obligatory memory retrieval process that occurs prior to selection. Presumably, whether an internal representation guides attention will be dependent on whether on what memories are retrieved. If a single prior experience is retrieved, it could theoretically guide attention – though again, Norman's model does not go into detail about how such a memory retrieval process would work. Shiffrin and Schneider (1977) interestingly, rule out Norman's model on the basis that it would predict single trial effects, which at the time, had not been demonstrated. Therefore, even on this basic principle of associative learning, there is no general theoretical (or empirical) agreement.

To sum, almost all theoretical models of attentional control make connections with and rely on a memory system to retrieve internal representations and guide attention. Yet none of the models provide a theoretical framework for how attentional control relates to memory encoding, storage, and retrieval processes. To fully understand how attentional control can be guided by prior experiences, it seems necessary, in my view, to examine attentional control using a memory-based framework.

## Empirical evidence for automatic attentional control

The aim of the current thesis is to evaluate general principles of a memory-based framework of attentional control. Above I argued that there is a need to elaborate on how memory influences attentional control from a theoretical perspective. In the following review, I will cover empirical evidence that prior experiences modulate selective attention demonstrating a need for a memory-based account of attentional control from an empirical perspective. The empirical chapters focus on selective attention in the context of congruency tasks, and therefore, this review will remain within that scope as well. The empirical focus was narrowed to congruency tasks like the flanker (Eriksen and Eriksen, 1974) and Stroop (1935) tasks because they have traditionally been the gold standard measures of selective attention (MacLeod, 1991). Similarly, congruency tasks are conventionally used to make more general inferences about cognitive control and finding support for a memory-based framework within these paradigms would lend evidence for a more general role of memory in modulating control processes. Although, I should note that major theoretical debate has focused on whether these trial-history effects are driven by processes that change attentional control settings (Mayr, Awh, and Laurey, 2003; Schmidt and Besner, 2008) and/or whether they reflect voluntary or automatic processes (Bugg, 2014; Bugg and Crump, 2012; Bugg, Jacoby, and Toth, 2008; Egner, 2007, 2014). However, these discussions will be elaborated on within each empirical chapter. This review is not meant to be exhaustive, but instead provide an overview of the kinds of paradigms used to make inferences about attentional control and how prior experiences have been shown to influence selective attention.

**Congruency tasks**

Congruency tasks have long been considered important models for evaluating selective attention and controlled processes more generally (Cohen et al., 1990; Cohen and Servan-Schreiber, 1992). Although they come in a variety of forms – Eriksen's flanker task (Eriksen and Eriksen, 1974), the Stroop task (1935), and the Simon task (Lu and Proctor, 1995; Simon, 1969) – they all typically involve bi-dimensional stimuli and measure target identification in the presence of potentially conflicting distractors. In the flanker task, for example, (Eriksen and Eriksen, 1974) participants identify the central letter flanked by two or three distractors (e.g., 'HHSHH'). Participants are typically faster and more accurate to identify a center letter when flanking letters are congruent (e.g., 'HHHHH') versus incongruent (e.g., 'FFHFF') with the response. Similarly, in the Stroop task (MacLeod, 1991; Stroop, 1935), stimuli comprise words printed in colored ink (e.g., "RED" in blue colored ink) and participants are tasked with identifying the color of the word while ignoring the word meaning. In both cases, the difference in performance between the incongruent and congruent conditions is called the congruency effect and modulations to the size of congruency effects are thought to index attentional priorities assigned to target and distractor dimensions. For example, target information is assumed to be prioritized over distractor information when smaller versus larger congruency effects are observed.

### Congruency sequence effects

Sequential modulation of congruency effects were first reported by Gratton, Coles, and Donchin (1992) using a flanker paradigm. Although they found the typical congruency effect (e.g., worse performance on incongruent trials), they also found an interaction between the current and previous trial congruency. Specifically, congruency effects on trial $n$ were found to be smaller when trial $n - 1$ contained an incongruent as compared to a congruent trial. That is, the influence of the distracting stimuli has on target processing is reduced on trials that follow an incongruent as compared to congruent trial demonstrating trial-to-trial shifts in attentional priorities (for reviews, see Duthoo, Abrahamse, Braem, and Notebaert, 2014; Egner, 2007). Congruency sequence effects demonstrate a short-term, transient influence of prior experiences on attentional control. Though typically explained as the result of control processing carrying-over from the previous trial (Botvinick et al., 2001), congruency sequence effects have been found to be dependent on the feature overlap between trials suggesting a memory component (Hommel, 1998; Hommel, Proctor, and Vu, 2004; Spapé and Hommel, 2008, 2014).

### List-wide proportion congruence effects

In the list-wide proportion congruent (LWPC) design, the proportion of congruent trials is manipulated across experimental blocks (Kane and Engle, 2003; Lindsay and Jacoby, 1994; Logan, 1980; Logan and Zbrodoff, 1979; Logan, Zbrodoff, and Williamson, 1984; Lowe and Mitterer, 1982). For instance, in a mostly congruent block of trials there are a higher percentage of congruent to incongruent trials (e.g., 75% congruent), while in a mostly incongruent block there are higher percentage of incongruent to congruent trials (e.g., 25%). Typically, a larger congruency effect is found for blocks that contain mostly congruent trials as compared to mostly incongruent blocks. Here, the frequency of experienced conflict is thought to influence the attentional priorities assigned each dimension. In the mostly incongruent block, the frequent conflict biases attention away from the irrelevant (conflicting) stimulus dimension, whereas in the mostly congruent – where the distracting dimension often predicts the correct response – attention is biased towards to irrelevant dimension. This result was originally interpreted as the consequence of strategic control (Logan, 1980; Logan and Zbrodoff, 1979): participants in the high proportion block become aware of the proportion manipulation and prepare to attend to the distracting information, while those in the low proportion block prepare to ignore the distracting information. However, more recent work suggests this may not be the case (e.g., Blais & Bunge, 2010; Bugg, Jacoby, & Toth, 2008).

The list-wide proportion congruent effect demonstrates a more sustained influence of prior experience on attentional control and suggests that the temporal or experimental context can become associated with an attentional control setting. This interpretation is supported by work by Bugg and colleagues showing list-wide effects transfer to frequency unbiased items (Bugg and Chanani, 2011; Bugg et al., 2008). For example, in one study they manipulated the list-wide proportion of congruent trials via a subset of items randomly intermixed with a stable set of frequency unbiased items. The congruency effect was found to be significantly smaller for the unbiased subset when embedded in a mostly incongruent list as compared to when embedded in a mostly congruent list. This result is consistent with the interpretation that the experimental context became associated with a general attentional control setting that generalized to other items presented within that context.

**Item-specific proportion congruence effects**

In the item-specific proportion congruency (ISPC) design, stimuli are separated into two non-overlapping item sets and the proportion of congruent trials is manipulated independently for each set (Bugg and Hutchison, 2013; Bugg et al., 2011; Jacoby, Lindsay, and Hessels, 2003). In a Stroop variant, item-specific designs assign one set of items (e.g., Red and Blue combinations) to a high proportion congruent condition, and another set (e.g., Green and Yellow combinations) to a low proportion congruent condition. Both item types are intermixed randomly, so subjects cannot accurately predict whether the next trial will be congruent or incongruent. In these designs, congruency effects are found to be larger for high versus low proportion congruent items. Of particular importance for these designs, participants are unable to predict the congruency of upcoming trials (unlike the LWPC design) and changes in the congruency effect must reflect, stimulus-driven automatic shifts in attentional control. The item-specific proportion congruent effect therefore, demonstrate that specific stimuli can become associated with an attentional control setting, which can be rapidly adjusted on a trial-to-trial basis.

**Context-specific proportion congruence effects**

Context-specific proportion congruent (CSPC) designs manipulate proportion congruent between two different contexts in which items can appear, again in a randomized, intermixed fashion. For example, Crump, Gong, and Milliken (Crump, Gong, and Milliken, 2006; see also, Corballis and Gratton, 2003) presented Stroop stimuli in one of two randomly chosen locations and manipulated the frequency of conflict associated with each location. One location was associated with a high frequency of conflict (25% congruent trials) and the other with a low frequency of conflict (75% congruent trials). Overall, the proportion of congruent trials was 50% and randomized such that the upcoming location could not be predicted. Even so, congruency effects were shown to be smaller for trials where the stimulus appeared in the high conflict location as compared to the low conflict location. This effect, known now as the context-specific proportion congruent effect (CSPC), has now been replicated using location (Brosowsky and Crump, 2016; Corballis and Gratton, 2003; Crump, 2016; Crump, Brosowsky, and Milliken, 2017; Crump et al., 2006; Hübner and Mishra, 2016; Weidler and Bugg, 2016), shape (Crump, Vaquero, and Milliken, 2008), color (Vietze and Wendt, 2009), social categories (Cañadas, Rodríguez-Bailón, Milliken, and Lupiáñez, 2013), and incidental semantic cues (Blais, Harris, Sinanian, and Bunge, 2015).

Critical evidence that CSPC effects reflect stimulus-driven, automatic control processes, rather than other non-control learning processes (e.g., Schmidt and Besner, 2008), comes from work showing that CSPC effects can transfer to frequency unbiased items (Crump Brosowsky 2017; Crump and Milliken, 2009; Weidler and Bugg, 2016; Weidler, Dey, and Bugg, 2018; though, see Hutcheon and Spieler, 2017). Crump and Milliken (2009), for example, divided Stroop items into two non-overlapping item sets (e.g., Red and Blue combinations; Green and Yellow combinations). One set was defined as the frequency biased set, and presented with 75% congruency in one location, and 25% congruency in the other. The second set however, was presented with 50% congruency in both locations. Nevertheless, they found smaller congruency effects for unbiased items presented in the high conflict location as compared to the low conflict location. The CSPC effects demonstrate that task-irrelevant contextual features can become associated with attentional control settings and generalizes to all items appearing within that context.

**Other related cognitive control phenomena**

Context-specific effects are not limited to congruency tasks. Other, equivalent effects have been demonstrated in other tasks that probe different components of cognitive control. For instance, negative priming refers generally to the finding that reaction times to identify a previously ignored target are slowed compared to a target that was not previously ignored (Tipper, 1985; for recent reviews, see D'Angelo, Thomson, Tipper, and Milliken, 2016; Frings, Schneider, and Fox, 2015). However, negative priming is sensitive to the match between probe and prime tasks, and that negative priming persists over the long-term, provided evidence suggesting a role for memory-based retrieval processes in negative priming (Neill, 1997; Neill, Valdes, Terry, and Gorfein, 1992).

Similarly, in task-switching paradigms performance is typically worse if the task set switches between trials rather than repeating (Monsell, 2003). This, 'switching cost', is thought to reflect the cost in overcoming interference from the previously used task set and re-configuring to the new task set (Rogers and Monsell, 1995). More recent work however, has demonstrated CSPC-like effects: smaller switch costs when items appeared in a context associated with a high switch-likelihood compared to items in a context associated with a low switch-likelihood (Chiu and Egner, 2017; Crump and Logan, 2010; Leboe, Wong, Crump, and Stobbe, 2008). Similar CSPC-like effects have also been observed in response inhibition tasks (Verbruggen and Logan, 2008), dual-task paradigms (Fischer, Gottschalk, and Dreisbach, 2014; Surrey, Dreisbach, and Fischer, 2017), Simon tasks (Hübner and Mishra, 2016), and attention capture (Crump, Milliken, Leboe-McGowan, Leboe-McGowan, and Gao, 2018).

Moreover, Waszak, Hommel, and Allport (2003) found long-term, item-specific effects in task-switching. Here, they observed switching costs for specific items re-presented after more than 100 intervening trials. Other long-term item-specific effects have also been observed in inhibition of return (Tipper, Grison, and Kessler, 2003), priming-of-pop out in visual search (Thomson and Milliken, 2012, 2013), and response inhibition in stop-signal tasks (Verbruggen and Logan, 2008).

### A summary of automatic attentional control phenomena

To summarize, there is widespread evidence that prior experiences can modulate selective attention as indexed by congruency effects. The influence of previous experiences on performance range in timescales from short, transient effects, to more long-term and sustained effects. Similarly, these effects range in their specificity: in some cases they are highly specific, requiring the re-presentation of the specific item. In other cases, these effects generalize across items that share contextual features like stimulus location.

This evidence supports the notion that attentional control settings can become associated with external cues, triggering automatic shifts in attentional control when re-presented. Moreover, this evidence demonstrates a need for a memory-based framework of attentional control. Although there is similar findings in other cognitive control paradigms, it remains unclear whether this collection of evidence points to a general role for memory retrieval of control operations linked with specific prior processing episodes to update and adjust control operations in the present moment.

## An instance theory of automatic attentional control

I have argued that there is a theoretical and empirical need to examine attentional control from a memory perspective. Here, I outline an instance-based memory theory of automatic attentional control which builds on Logan's theory of automaticity (1988a, 1988b, 1992) and Hintzman's exemplar model of memory (1984, 1986, 1988). The theory makes four main assumptions: (1) *obligatory* *memory encoding* – Encoding is an obligatory consequence of attending to an object or event; (2) *"instance" or "exemplar" memory representation* – A unique trace for an event is encoded into memory every time an individual attends to an object or event. Multiple exposures to the same stimulus will cause the creation of multiple unique memory traces, one for each exposure; (3) *obligatory memory retrieval* – Every time a stimulus is attended, a memory-retrieval process is initiated; and (4) *the preservation of cognitive processing details* (Kolers and Roediger, 1984) – How we attended during a given experience is encoded in memory, bound together, with other perceptual details of the experience.

The instance theory of automatic attentional control adopts particular theoretical views of memory and automaticity worth examining in more detail. I review these theoretical foundations, in brief below, before moving on to the consequences of adopting an instance theory and the potential explanatory power of an instance theory in the cognitive control domain.

**Theoretical foundations of the instance theory**

***A processing view of memory*** First, the instance theory adopts a processing view of memory (e.g., Baddeley, 1984; Craik and Jacoby, 1979; Hintzman, 1984, 1986; McKoon, Ratcliff, and Dell, 1986; Roediger, 1984; Whittlesea, Brooks, and Westcott, 1994).

There has been a long tradition in psychology to categorize different phenomenological experiences of memory and delineate between the types of underlying memory systems. Under the multiple memory systems view, memory is not a single, unitary system, but instead comprises multiple, interacting types of memory storage systems (e.g., Eichenbaum and Cohen, 2001; Gabrieli, 1998; Poldrack and Packard, 2003; Schacter and Tulving, 1994; Schacter, Wagner, and Buckner, 2000; Squire, Knowlton, and Musen, 1993; Tulving, 1985). Categories are determined by observed differences in memory performance. For instance, categories have been delineated by differences in how information is forgotten, differences in the types of information retrieved, and awareness of retrieval (e.g., Squire, 2004; Tulving, 1985). Memory performance in different tasks is dissociable then, because they depend on different underlying memory structures.

According to the multiple memory systems view, long-term memory is broadly divided into two systems, declarative (explicit) and non-declarative (implicit), distinguished by whether an individual has awareness of memory retrieval. Whereas retrieval from declarative memory occurs with awareness, retrieval from non-declarative memory occurs without awareness (Cohen and Squire, 1980; Squire and Zola-Morgan, 1988). These two categories are further sub-divided into sub-systems of memory: Declarative memory is thought to reflect the conscious influence of prior experiences and comprises episodic (knowledge of prior specific events) and semantic (general factual knowledge) forms of memory. Non-declarative is thought to reflect the unconscious, automatic influence of prior experiences on behavior and includes priming, conditioning, statistical learning, and habituation; though, what should or should not be included as 'type' of non-declarative memory are actively debated. The multiple memory systems view has inspired research programs investigating how different "types" of memory influence attention and some have attempted to categorize the memorial influences on attention in a similar manner (Chen and Hutchinson, 2018; Hutchinson and Turk-Browne, 2012).

The processing view of memory, in contrast, proposes a single, unified memory system serving multiple purposes and operating under a common set of principles (e.g., Benjamin, 2010; Curtis and Jamieson, 2018; Kinder and Shanks, 2001, 2003; Shanks and St. John, 1994; Surprenant and Neath, 2013; see also, Bussey and Saksida, 2007; Gaffan, 2002 for the same argument applied to nonhuman animals). By this view, dissociations arise because different processes are interacting with the memory system, not because of different underlying memory structures. Instance theories of memory are predicated on the idea that individual experiences (i.e., instances) represent the fundamental units of knowledge and learning, by this account, is the accumulation and deployment of instances from memory (Brooks, 1978, 1987; Hintzman, 1984, 1986, 1988; Logan, 1988b; Medin and Schaffer, 1978). Instance theories of memory are certainly more parsimonious than multiple memory systems theories. However, much of the evidence for instance-based representations come from computational work showing dissociations in memory and learning – commonly taken as evidence for multiple systems – can be produced with a single, instance-based memory system (e.g., memory phenomena: Arndt and Hirshman, 1998; Berry, Kessels, Wester, and Shanks, 2014; Clark, 1997; Curtis and Jamieson, 2018; Gillund and Shiffrin, 1984; Hintzman, 1988, classification and categorization phenomena: 1986; Jacoby and Brooks, 1984, 1984; Jamieson, Avery, Johns, and Jones, 2018; Jamieson, Mewhort, and Hockley, 2016; Raaijmakers and Shiffrin, 1981; Medin and Schaffer, 1978; Nosofsky, 1986, 1987, 1988, 1991; implicit and associative learning phenomena: Higham, Vokey, and Pritchard, 2000; Jamieson, Crump, and Hannah, 2012; Jamieson et al., 2010; Jamieson and Mewhort, 2009, 2010).

***Automaticity as single-step memory retrieval*** Second, the instance theory adopts a single-step memory retrieval view of automaticity (Jacoby, 1978; Logan, 1988b).

One traditional view defines automaticity relative to the amount of required attentional resources: a process was considered automatic to the extent it can operate independently of attentional resources. Automaticity is effortless because effort is proportional to the amount of resources required; fast because is not limited by the availability of resources; and obligatory because 'control' is defined as the allocation of resources which is not required (Hasher and Zacks, 1979; LaBerge and Samuels, 1974; Logan, 1979; Posner and Snyder, 1975a, 1975b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977). This view has been criticized heavily on multiple fronts – and largely beyond the scope of this discussion (Allport, 1989; Kahneman and Chajczyk, 1983; Navon and Gopher, 1979; Wickens, 1984; for reviews, see Logan, 1992; Moors and De Houwer, 2006). However, resource theories do not provide a mechanism for how automaticity is learned (Logan, 1988a, 1988b), and more seriously, cannot account for how controlled processes like attentional selection might become automatic since automaticity is by definition the absence of control and/or attention.

Instead, the view adopted here defines automatization as a shift from an algorithmic process to a single-step memory retrieval process (e.g., Jacoby, 1978; Logan, 1988b). Nonautomatic processes rely on a slow algorithmic process to produce an output. Once produced however, this output can be stored in memory. Future processing can then bypass the algorithmic computations and rely entirely on memory retrieval. According to Logan's model (1988; 1992), a race determines whether an algorithmic or memory-retrieval process is used. Memory-retrieval time is dependent on the number of exact stimulus-matches in memory and the algorithmic process races in parallel with the memory-retrieval process; whichever process wins determines the response.

For example, when a novel stimulus is first encountered, the algorithmic process wins because there are no memories to retrieve. After a single exposure, memory-retrieval may still be too inefficient to outrace the algorithm. However, after multiple exposures, the memory-retrieval process should bypass the algorithm and the response would become automatic. Thus, the development of automaticity requires practice. Logan's model however, uses an instance-based memory framework and could easily accommodate other retrieval mechanisms like physical and temporal similarity (for one such extension, see Palmeri, 1997).

***Procedures of the mind: Storage, retrieval, and reinstatement*** Finally, the instance theory assumes the preservation of cognitive processing (e.g., Kolers and Roediger, 1984; Estes, 1972).

This assumption is a natural extension of instance-based models of memory and theories of automaticity in that it treats attention like any other behavioral response: Attention procedures can be stored and retrieved from memory and can be automatically reinstated in the same way that other motor responses are automatically reinstated. As reviewed above, the storage and retrieval of attentional control processing is a deeply embedded assumption prevalent across many theoretical treatments of attention (Deutsch and Deutsch, 1963; Norman, 1968; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977; A. M. Treisman, 1969).

Additionally, the general idea that memory could represent details of prior processing has been proposed more generally elsewhere. Kolers and Roediger (1984) for example, argue that learning and memory should be viewed in terms of the cognitive procedures applied during an experience. They viewed the cognitive procedures as inseparable from the contents of our memory representations. Similarly, Estes (1972) proposed that stimuli become associated with 'control elements', allowing their cued reinstatement.

Returning to selective attention processes, there are two possible variants of this assumption: in one variant, an attentional control setting – essentially a set of instructions (e.g., prioritize the color and inhibit the word) – are preserved and, upon retrieval, sent to a specialized attention system to implement. This view is very much in spirit with filter models of attention (e.g., Deutsch and Deutsch, 1963; Norman, 1968; Treisman, 1960, 1964; A. M. Treisman, 1969). In a second variant attention is conceptualized as an action and stored as an action schema (e.g., Norman and Shallice, 1986). In this case, the action schema (e.g., move attention towards the color and away from the word), upon retrieval, is sent to a more generalized control system to implement. This view is more consistent with Shiffrin and Schneider's view of attention (1977) and modern views of cognitive control (e.g., Botvinick et al., 2001; De Pisapia and Braver, 2006). The instance theory makes no claims about the control systems required to implement automatic attention, only that processing details are preserved and reinstated.

**The (potential) explanatory power of an instance theory**

The instance theory offloads the difficult task of determining when and how to adjust attention to a memory-retrieval system. The benefit of adopting an instance-based theory is in its flexibility in determining which prior experiences influence on-going processing. For instance, it has the ability to produce highly specific, context-rich retrieval and more generalized, abstract retrieval. This was demonstrated by Hintzman (1984; 1986) who was able to model episodic-like and semantic-like memory retrieval using an instance-based memory system. In this model, when memory is probed all the stored traces that are similar to the probe are retrieved and aggregated into an "echo". If memory was probed with a specific cue very similar to only a small number of traces, then retrieval produced an echo that contained many specific details of those traces, much like a context-rich episodic memory. However, if memory was probed with a less specific cue, with low similarity to many traces in memory, then the retrieved echo did not contain many specific details of any one trace. Instead, the echo contained more generalized, abstracted, features common across traces. Therefore, Hintzman was able to show that contextual details can be preserved, much like episodic memory, and how contextual details can be lost, much like semantic memory.

Prior models of cognitive control have struggled to handle the varying levels of specificity observed across contextual cuing phenomena. One strategy has been to propose multiple control processes operating on different timescales (Botvinick et al., 2001; Braver, 2012; De Pisapia and Braver, 2006); although these too are unable to explain all the observed phenomena (Crump and Milliken, 2009; Egner, 2014; Verguts and Notebaert, 2008, 2009). The instance theory has the potential explain the full range of phenomena with one control process by offloading the selection of the attentional control setting to a memory retrieval system. By assuming memory retrieval is similarity-based, for instance, the instance theory would easily predict item-specific and context-specific proportion congruent effects. In the item-specific proportion design, items are separated into non-overlapping sets. Since item sets do not share any features, cuing memory on any given trial would be able to retrieve items from those sets. In contrast, the context-specific proportion congruent phenomena shows generalization across items that share contextual features. The instance theory would also predict generalization as items would be retrieved on the basis of the shared feature. By assuming that recent memories are retrieved more easily than distant memories, the instance theory could also predict congruency sequence effects (e.g., Egner 2014).

## Overview of empirical chapters

The aim of the current thesis is to test general principles of an instance theory of automatic attentional control using a converging operations approach. The instance theory makes four main assumptions: (1) *obligatory memory encoding*; (2) *”instance” or ”exemplar” memory representation*; (3) *obligatory memory retrieval*; and (4) *the preservation of cognitive processing details*. In Chapter 2, I will examine the obligatory nature of memory encoding by investigating context-specific proportion congruent effects in a non-conflict selective attention task. In Chapter 3, I will examine the assumption of long-term instance-based representation by investigating long-term single-trial effects in a context-cuing flanker paradigm. Finally, in Chapter 4 I will examine how memory-retrieval can influence context-specific attentional control in a context-specific proportion congruent task.

# CHAPTER 2

# INTENTIONAL CONTROL AS A PRE-REQUISITE FOR CONTEXTUAL CONTROL

## Preface

This chapter is reproduced from a published article in *Conciousness and Cognition* (for consistency, I have adjusted the format of the manuscript for the thesis). The full reference to the article:

**Brosowsky, N.P.**, & Crump, M.J.C. (2016), Context-specific attentional sampling: Intentional control as pre-requisite for contextual control. *Consciousness and Cognition*, 44, 146-160. https://doi.org/10.1016/j.concog.2016.07.001

Chapter 2 presents a set of four experiments extending prior work on proportion congruency phenomena using a novel bi-dimensional selective attention task. The aim of experiments 1 and 2 was to determine whether context-specific proportion congruent effects would generalize to a non-conflict selective attention task. Borrowing elements of Sperling's partial report task (Sperling, 1960) and the location-specific proportion congruent task (Crump, Gong & Milliken, 2006), target stimuli consisted of four letters superimposed over four colors were presented either above or below the fixation. After the stimulus was presented, participants were asked to identify either a letter or color at a particular position. Critically, the proportion of letter versus color identification trials were manipulated such that the locations were predictive of the identification dimension. Therefore, in our task, attentional priorities could be biased along one of two dimensions as predicted by the location of the target stimuli. However, in both experiments we found no evidence for context-specific effects.

The failure to find context-specific effects in experiments 1 and 2 was used as an opportunity to test general principles of the memory-based account and explore the role of intentions in producing contextual control. Memory-based theories explain contextual control by cue-driven retrieval and reinstatement of attentional priorities. One critical assumption then, is that participants possess memory traces that code for different attentional priorities for different contexts. Prior work had found that contextual control emerges regardless of awareness or intention.

We hypothesized that stimuli in conflict tasks (e.g., Stroop and flanker) may afford certain attentional priorities on the basis that one dimension either facilitates or impairs responding accurately (i.e., congruent versus incongruent stimuli). In those tasks, intentional control may not be required because the task itself produces experiences applying different attentional priorities in different contexts (e.g., down-weighting the irrelevent dimension on incongruent trials in the mostlyl incongruent location). In our task however, intentional control may be required because our stimuli do not cause participants to adopt differing attentional strategies (i.e., there is no congruent/incongruent). Experiments 3 and 4 examined this possibility and found that context-specific effects could be produced in our task but were dependent on prior experiences intentionally applying context-specific attentional control strategies.

**Abstract**

Recent work suggests that environmental cues associated with previous attentional control settings can rapidly and involuntarily adjust attentional priorities. The current study tests predictions from adaptive-learning and memory-based theories of contextual control about the role of intentions for setting attentional priorities. To extend the empirical boundaries of contextual control phenomena, and to determine whether theoretical principles of contextual control are generalizable we used a novel bi-dimensional stimulus sampling task. Subjects viewed briefly presented arrays of letters and colors presented above or below fixation, and identified specific stimuli according to a dimensional (letter or color) and positional cue. Location was predictive of the cued dimension, but not the position or identity. In contrast to previous findings, contextual control failed to develop through automatic, adaptive-learning processes. Instead, previous experience with intentionally changing attentional sampling priorities between different contexts was required for contextual control to develop.

## Introduction

Attentional control refers to processes that alter priorities for selecting relevant versus irrelevant information during task performance. Although attentional priorities are widely understood to be set in an effortful intentional fashion (Posner and Snyder, 1975b), they may also be set in a cue-driven fashion. For example, research across paradigms in attention suggests that contextual cues can trigger the automatic retrieval and reinstatement of attentional control settings previously used in those contexts in the past (for reviews, see Bugg and Crump, 2012; Cosman and Vecera, 2013b; Egner, 2008). The present experiments contribute to this body of work by showing new evidence of location-based contextual control over priorities for sampling from briefly presented bi-dimensional (e.g., letters and colors), multi-element displays. More important, across experiments we find that contextual control over sampling in this procedure depends on an intentional learning phase where subjects explicitly deploy different sampling strategies in different location contexts. These findings contrast with several existing demonstrations of contextual control that do not appear to depend on intentional processing, and they are also not well explained by accounts of contextual control that posit a role for learning processes that automatically adapt to the statistics of the environment. To set the stage for the present work, we briefly review the range of evidence for contextual control, what is known about the roles of awareness and intention in acquiring contextual control, and how these issues are treated among major theories of contextual control.

Demonstrations of contextual control over attentional priorities have been observed in procedures tapping different aspects of attention. For example, repeating the configuration of distractors in visual search facilitates target detection (Chun, 2000; Chun and Jiang, 1998). Stroop interference reflecting priorities for processing color versus word information is modulated by contextual cues (location, shape, font) associated with different proportions of congruent and incongruent items (Crump and Milliken, 2009; Crump et al., 2006, 2008). Flanker interference reflecting priorities for selecting a target in space from nearby distractors is also modulated by contexts associated with different proportions of congruent and incongruent items (Corballis and Gratton, 2003; Crump, 2016; King, Korb, and Egner, 2012). Similarly, task-switching costs reflecting task-specific attentional priorities can be modulated by contextual cues associated with specific tasks (Mayr and Bryck, 2007), and different proportions of switch and repeat trials (Crump and Logan, 2010). Attention capture by salient feature singletons (Cosman and Vecera, 2013b; see also, Le Pelley et al., 2013) can also be modulated by context cues (e.g., visual scenes) associated with differing attentional sets. Related findings showing cue-driven control over the setting of attentional priorities can be found in negative priming (Milliken, Thomson, Bleile, MacLellan, and Giammarco, 2012), priming of pop-out (Thomson and Milliken, 2013), and masked-priming (Heinemann, Kunde, and Kiesel, 2009; Panadero, Castellanos, and Tudela, 2015; Reuss, Desender, Kiesel, and Kunde, 2014). The present experiments borrow techniques from demonstrations of contextual control over congruency effects in classic selective attention procedures like Stroop or flanker; so, we discuss those demonstrations more closely as a venue for reviewing the roles of awareness and intention in contextual control.

Interference tasks like Stroop (1935) and flanker (Eriksen and Eriksen, 1974) require subjects to identify a target dimension while ignoring a distractor dimension. For example, in the Stroop task (for a review, see MacLeod, 1991) subjects identify the ink-color of a written color word, and performance is typically worse when the distracting word is incongruent (e.g., the word RED is printed in blue) than congruent (e.g., the word RED is printed in red) with the required response. The size of this difference, termed the congruency effect, is taken as an index of selective attention: larger differences show failures to prevent distractors from influencing performance, and smaller differences show success in preventing distractors from influencing performance. Proportion congruent manipulations modulate the size of congruency effects, and are a common tool for measuring control processes that set attentional priorities for target and distractor processing (for a review, see Bugg and Crump, 2012). Proportion congruent manipulations vary the relative proportion of congruent and incongruent items in a task. Generally, congruency effects are larger in blocks of trials that have a higher than lower proportion of congruent items (Logan and Zbrodoff, 1979).

Contextual control over congruency effects has been shown using context-specific proportion congruent manipulations. For example, Crump et al. (2006) presented Stroop items in a randomized intermixed fashion in one of two locations that were associated with a high or low proportion of congruent items. In this design, subjects were unable to predict whether an upcoming trial would be congruent or incongruent, or whether an item would appear in a location that was high or low proportion congruent. Nevertheless, larger congruency effects were found for the items in the high than low proportion congruent locations. This finding is consistent with contextual control over attentional sets, whereby rapid, online processing of location cues associated with different levels of proportion congruent trigger adjustments to priorities for filtering color and word dimensions of a current item. The CSPC effect has been reported several times in Stroop (Bugg et al., 2008; Crump and Milliken, 2009; Crump et al., 2008), and flanker tasks (Corballis and Gratton, 2003; Wendt, Kluwe, and Vietze, 2008; Wendt and Luna-Rodriguez, 2009).

The interpretation that CSPC effects reflect automatic contextual control depends on a subject's state of awareness and possible intention to set attentional priorities by context. We use awareness to refer to explicit knowledge of the proportion congruent manipulation, the source of conflict, or the presence of contextual cues. We use intention to refer to a deliberate route for setting attentional priorities. Subjects could become aware of the CSPC manipulation, that attentional requirements vary by context, and then deliberately set attentional priorities separately for each context. Intentional control could prepare two different strategies in advance, or rapidly shift attentional priorities in response to the presentation of a context cue. Either way, the CSPC effect would not provide clear evidence for an automatic cue-driven influence over attentional priorities. Additionally, subjects could be aware of the CSPC manipulation, but decide not to intentionally assign different attentional priorities between contexts. In this case, intentional control would not explain CSPC effects, although awareness could play a role in learning about predictive cues. Subjects could also be unaware of the CSPC manipulation, and presumably for that reason would not intentionally set attentional priorities in a context-specific fashion that would produce consistent CSPC effects. Here, CSPC effects would be more consistent with an automatic, cue-driven influence over the setting of attentional priorities.

Awareness of the CSPC manipulation has been assessed by post-experimental questionnaires.

All of the studies measuring awareness showed that subjects could not accurately report the relative proportions of congruent items between contexts (Crump et al., 2006; Crump and Logan, 2010; Gough, Garcia, Torres-Quesada, and Milliken, 2014; King et al., 2012; Sarmiento, Shore, Milliken, and Sanabria, 2012). A few studies have also manipulated awareness. Crump et al. (2008) for example, tested whether awareness of the CSPC manipulation would be sufficient for producing contextual control using shape cues, which were previously found to be an ineffective cue for observing CSPC effects (Crump et al., 2006). Subjects were informed about the CSPC manipulation, encouraged to use shape-specific attentional control strategies, and signed a statement acknowledging they understood the instructions. CSPC effects for shape cues were not observed, and subjects were unable to accurately complete the post-experiment awareness questionnaire. Awareness of conflict between the relevant and irrelevant dimensions has also been assessed as a pre-requisite for CSPC effects. For example, CSPC effects can be produced in masked-prime procedures where subjects are not aware of the source of response conflict (Heinemann et al., 2009; Panadero et al., 2015; Reuss et al., 2014; but see, Schouppe, Ferrerre, Van Opstal, Braem, and Notebaert, 2014). Reuss et al. (2014) embedded the contextual cue within the masked prime and found CSPC effects even when the prime was below a perceptible threshold. Taken together, these studies suggest that awareness of the CSPC manipulation, source of response conflict, and in one case the presence of a contextual cue, are not prerequisites for contextual control; they also suggest that CSPC effects are not driven by intentional means.

Process theories of CSPC effects and contextual control phenomena generally do not invoke awareness or intention as pre-requisites for acquiring or displaying cue-driven control after learning. We review two general classes of theories termed adaptive learning and memory-based accounts.

Adaptive learning theories explain contextual control in terms of automatic learning processes sensitive to the statistics of the environment, and have a long history in attention (Moray and Fitter, 1973) and associative learning (Mackintosh, 1975) theory. Generally speaking, adaptive learning processes update attentional priorities in response to errors, such that future errors are minimized (Kruschke, 1992, 2001, 2003, 2011). Similar approaches using response conflict signals (Botvinick et al., 2001) have modeled item-specific proportion congruent effects (Blais, Harris, Guerrero, and Bunge, 2012), and could account for CSPC effects if items in each context were represented individually. Verguts and Notebaert (2008, 2009) also showed that a Hebbian learning rule could further control how conflict signals update attentional priorities. More generally, experienced conflict, actual errors, and error estimates all represent learning signals by which performance could potentially be optimized Verguts and Notebaert (2008). According to these models, the prerequisites for acquiring contextual control include the presence of a learning signal, and the presence of statistical regularities among environment cues that can direct optimization. Awareness of environmental regularities, or intentional control of attentional prioritization are not required for contextual control.

Memory-based theories explain contextual control by cue-driven retrieval and reinstatement of prior attentional priorities (Crump, 2016; Crump and Milliken, 2009; Crump et al., 2006, 2008). Memory traces store the perceptual details of specific experiences and the attentional priorities for information processing assigned during those experiences. In this way, cues in the present moment can retrieve similar instances from memory and reinstate the attentional priorities used in the past to update attentional priorities in the present. Memory-driven theories can be flexible with respect to the roles of awareness and intention. The critical assumption of the memory account is that subjects possess memory traces that code different attentional priorities for different contexts. The theory suggests that context-specific attentional priorities could be obtained through various means, including adaptive learning, or intentional control at the level of items or contexts. First, adaptive learning processes could work in concert with memory and provide the mechanism for changing attentional priorities in a context-specific fashion. Second, memory processes could produce contextual control through generalization of attentional priorities for specific items that vary between contexts. For example, subjects may intentionally modify attentional priorities for specific items to maximize speed and accuracy, and item-specific priorities associated to and retrieved by context cues could generalize to other items appearing in those contexts. Finally, memory processes could rely on initial awareness of context-specific differences in attentional requirements, and subsequent intentional control to assign different attentional priorities between contexts. In this way, memory would be populated with instances that code context-specific attentional priorities, which could then be retrieved in an automatic cue-driven fashion.

Taking stock, research into contextual control has generated numerous empirical demonstrations and some process theories with generalizable principles for explaining how cues acquire the ability to adjust attentional priorities during performance. A broad aim of the present experiments was to evaluate the generalizability of predictions from adaptive-learning and memory-based theories, especially with respect to intentional control. Our approach was to examine the acquisition of contextual control in a novel task that required subjects to prioritize sampling from one of two dimensions (letters or colors) in briefly presented multi-element displays. The use of a novel task had the dual benefits of assessing whether theoretical predictions are paradigm-specific or paradigm-general, as well as identifying new empirical boundaries for contextual control phenomena. The major finding across experiments is that contextual control in our task depends on intentional control. That is, previous experience with intentionally changing attentional sampling priorities between different contexts is a pre-requisite for contextual control over sampling from briefly presented displays.

## Experiment 1

Our task was similar to Sperling's (1960) partial report paradigm where complex visual displays are presented followed by instructions indicating target selection criterion. We used bi-dimensional displays containing letter and colors, which allow for independent, selective processing (Bundesen, Kyllingsbæk, and Larsen, 2003; Kyllingsbæk and Bundesen, 2007). Each display contained a row of four different letters inside four uniquely colored squares. Displays were briefly presented (300 ms) followed by a task cue to report the identity of the letter or color that appeared in one of the squares (see Figure 2.1).

Borrowing from the location-based CSPC manipulation, our displays appeared in one of two locations above or below the fixation, and location was predictive of the identification task (color vs. letter), but not the position or identity of the items in each display. Specifically, one location involved 75% color and 25% letter identification trials, and the other location involved 75% letter and

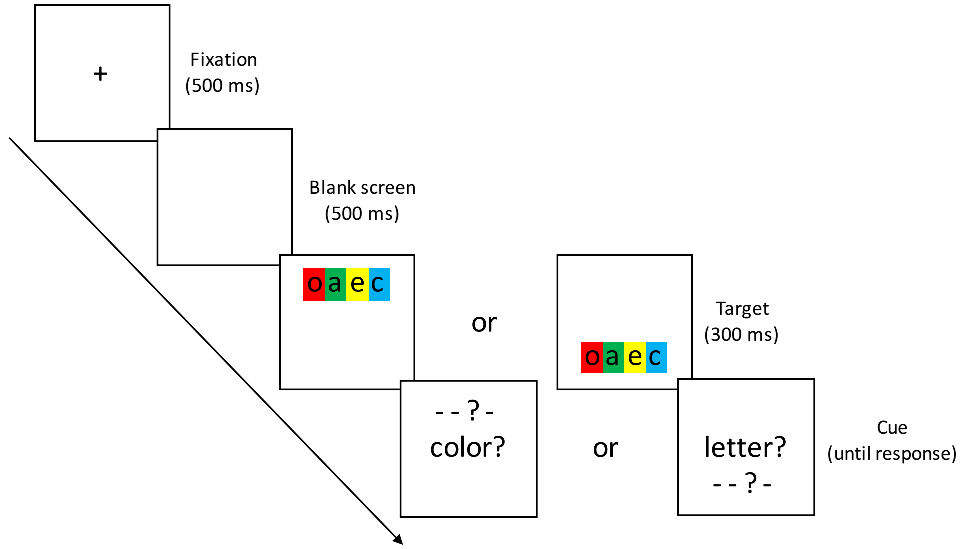


**Figure 2.1:** Illustration of the trial sequence for all experiments.

Note that the target could appear above or below the fixation. The identity cue (”Color?” or ”Letter?”) always appeared in the center of the screen while the position cue (”- - ? -”) always appeared in the same location as the target. Participants were instructed to report either the letter or color in the cued position.

25% color identification trials, so the location predicted the current task with 75% validity. We use the term valid to refer to trials where each identification task appeared in its likely location (75%), and the term invalid to refer to trials where each identification task appeared in its unlikely location (25%).

Thus, we created experimental conditions that have produced contextual control over congruency effects in Stroop and flanker. We assumed these conditions would also produce location-based control over attentional priorities for sampling from distinct dimensions in briefly presented visual displays. Specifically, we expected that identification accuracy would be higher for both letter and color targets when displayed in their respective valid than invalid locations. We did not inform subjects that location was predictive of the identification task, and we expected that subjects would not become aware of the manipulation. From the perspective of adaptive learning theories of contextual control, we assumed that attentional priorities would shift automatically in response to errors or conflict.

## Methods

***Subjects*** All subjects were Brooklyn College undergraduate students (approximately ages 18-22) who participated for course credit. Twenty subjects completed Experiment 1 and all were included in the analysis.

***Apparatus & Stimuli*** All experiments were programmed using LiveCode 7.0. The target stimuli consisted of four lower-case letters (o, e, c, and a) superimposed on four colored squares (red, green, blue, and yellow). The relative positions of letters and colors on each trial were randomly chosen from all possible letter/color permutations. The background was black and stimuli were presented either above or below the fixation on a dark gray rectangle.

The keyboard was labeled to indicate the four letters and four color responses required. The keys A, S, D, and F were relabeled to A, O, E, and C respectively; and, the keys H, J, K, and L were relabeled as red, blue, green, and yellow, respectively.

***Design*** Experiment 1 used a 2x2 within-subjects design with cue validity (75% vs. 25%) and task (color identification vs. letter identification) as factors. There were 480 trials in Experiment 1. One location (randomly assigned as above or below the fixation) consisted of 75% color identification trials (160 trials) and 25% letter identification trials (80 trials) while the other location consisted of 75% letter identification trials and 25% color identification trials. The trial sequence was randomized and presented in an intermixed fashion for each subject.

***Procedure*** Each subject read a brief overview about the stimuli they would be presented and the types of responses required before signing a consent form. Subjects were instructed to remember the locations of both the colors and letters. Immediately following the target stimulus, a cue would indicate the to-be-identified target, which could be a letter or a color, located in any of the four positions.

Each trial began with a white fixation-cross presented in the center of the screen for 500 ms followed by a blank screen for 500 ms. Next, the target stimulus containing the four letters and four colors appeared for 300 ms followed immediately by the instructions for which stimulus to identify.

The dimension of the to-be-identified stimulus was indicated by the words “Letter?” or “Color?” presented in the center of the screen and its location was indicated by three dashes and a question mark (i.e., “- - ? -”) presented in the same location as the target stimulus (see Figure 2.1). For example, “Letter?” and “? - - -” would indicate the letter located in the first position. No accuracy feedback was given following a response and the next trial began automatically. A mandatory 30-second break was given every 120 trials.

### Results

Overall identification accuracy was fairly low with an average accuracy of 63%, though subjects were well above chance (25%). Given the low accuracy scores, a response time analysis was inappropriate as nearly 40% of trials would have been removed from the analysis resulting in insufficient cell sizes. Furthermore, accuracy was well below ceiling levels of performance and therefore, would be sensitive to improvements in performance. Mean RTs and accuracy scores for all experiments are displayed in Table 2.1.

Mean accuracy scores for each subject in each condition were submitted to a repeated-measures ANOVA with cue validity (75% vs. 25%) and task (color identification vs. letter identification) as factors. Mean accuracy rates collapsed across subjects are presented in Figure 2.2.

Both main effects for cue validity, $F(1, 19) = 0.21$, $MSE = 19.70, p = .654$, $\hat{\eta}_p^2 = .011$, and task $F(1, 19) = 0.49$, $MSE = 152.59, p = .492$, $\hat{\eta}_p^2 = .025$, were non-significant. Additionally, the two-way interaction between cue validity and task was also non-significant, $F(1, 19) = 0.44$, $MSE = 28.19, p = .517$, $\hat{\eta}_p^2 = .022$. Accuracy was not significantly better for targets presented in their valid versus invalid locations.

### Discussion

Experiment 1 failed to demonstrate context-specific control over priorities for sampling from color vs. letter dimensions in briefly presented displays. Specifically, identification performance did not vary as a function of location cue validity. The absence of contextual control occurred despite the

**Table 2.1:** Mean correct color and letter identification response latencies, standard errors, and accuracy rates for all experiments.

| | Task | | Training Phase | | Mixed Phase | | | |
|---|---|---|---|---|---|---|---|---|
| | | | 100% | | 75% | | 25% | |
| | | | M | SE | M | SE | M | SE |
| **Exp. 1** | | | | | | | | |
| | Color | ACC | - | - | 64.56 | 3.58 | 63.32 | 3.63 |
| | | RT | - | - | 1611 | 70 | 1606 | 68 |
| | Letter | ACC | - | - | 61.84 | 3.56 | 62.17 | 3.35 |
| | | RT | - | - | 1824 | 83 | 1858 | 90 |
| **Exp. 2** | | | | | | | | |
| | Color | ACC | 75.02 | 3.55 | 64.81 | 4.36 | 63.36 | 4.71 |
| | | RT | 1152 | 47 | 1510 | 59 | 1556 | 67 |
| | Letter | ACC | 77.11 | 3.23 | 58.31 | 3.4 | 59.09 | 3.88 |
| | | RT | 1309 | 61 | 1707 | 80 | 1711 | 83 |
| **Exp. 3** | | | | | | | | |
| | Color | ACC | 79.83 | 3.08 | 67.59 | 4.65 | 56.62 | 4.91 |
| | | RT | 1318 | 61 | 1595 | 58 | 1687 | 62 |
| | Letter | ACC | 81.99 | 2.54 | 65.56 | 3.55 | 56.62 | 5.13 |
| | | RT | 1560 | 96 | 1751 | 64 | 1858 | 87 |
| **Exp. 4** | | | | | | | | |
| Trained Context | Color | ACC | 67.71 | 4 | 58.4 | 4.34 | 45 | 4.91 |
| | | RT | 1519 | 80 | 1524 | 52 | 1596 | 52 |
| | Letter | ACC | 77.81 | 4.14 | 56.04 | 4.52 | 48.12 | 5.39 |
| | | RT | 1686 | 90 | 1625 | 43 | 1620 | 74 |
| Reversed Context | Color | ACC | - | - | 56.53 | 4.93 | 51.88 | 4.96 |
| | | RT | - | - | 1489 | 31 | 1526 | 61 |
| | Letter | ACC | - | - | 52.01 | 4.46 | 47.08 | 5.17 |
| | | RT | - | - | 1607 | 36 | 1616 | 60 |

*Note*: RT = Reaction Time (ms); ACC = Accuracy (%); M = Mean; SE = Standard Error; 100%\75%\25% = Cue Validity.

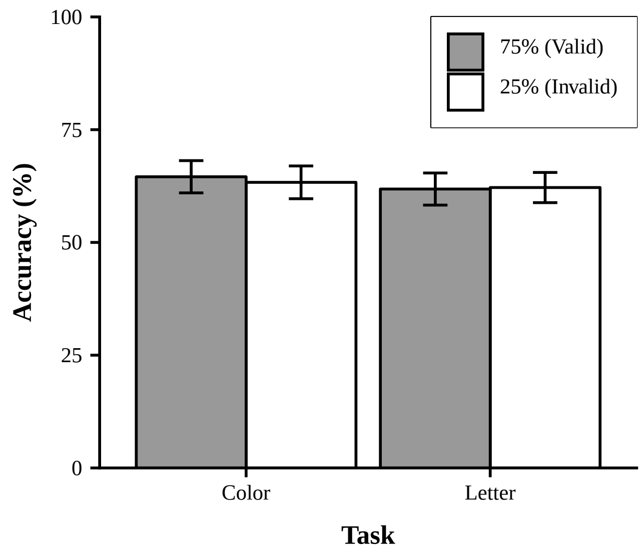


**Figure 2.2:** Results from Experiment 1

Accuracy scores (%) for color and letter identification trials as a function of location cue validity.

significant amount of errors made in all locations. So, although there was an opportunity for adaptive learning processes to modify attentional priorities based on error signals (in this case, the subjective appraisals of performance), those processes did not appear to influence performance. We cannot rule out whether or not additional practice was necessary for contextual control to develop; however, it clearly did not develop with amounts of practice sufficient to produce contextual control in related procedures.

One possibility is that subjects did not attempt to intentionally change attentional priorities for sampling from color and letter dimensions between location contexts, and instead adopted a "sample-everything" strategy. This strategy would not prioritize one dimension over another, but would instead involve attempting to register as many details about both dimensions as possible. If subjects were employing such an experiment-wide indiscriminate sampling strategy, then their memory record would not be populated with instances preserving different attentional priorities between contexts. Experiment 2 was designed to populate the memory record with traces where attentional priorities were modified between contexts.

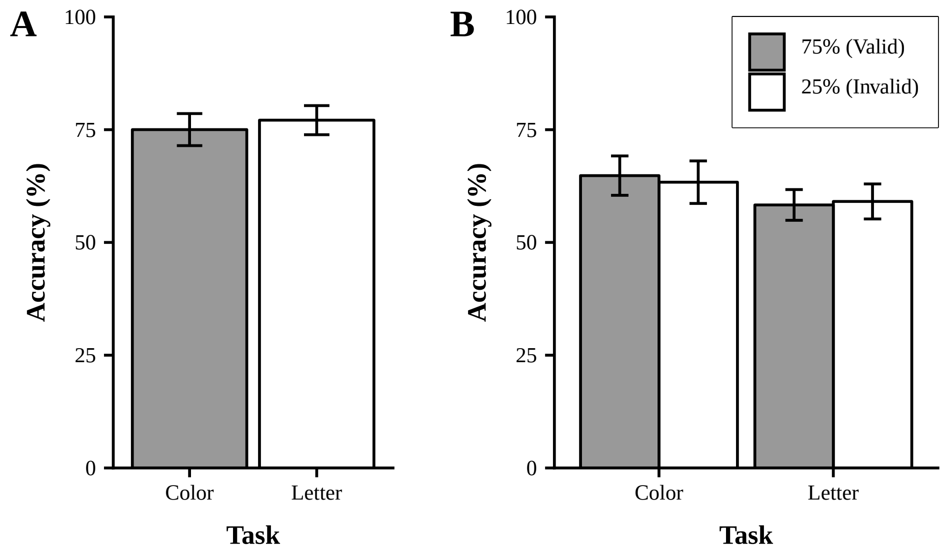


**Figure 2.3:** Results from Experiment 2

Results for the Training Phase (left) and Mixed Phase (right) from Experiment 2. Accuracy scores (%) for color and letter identification trials as a function of location cue validity.

### Experiment 2

The memory-driven account proposes that attentional processing details are stored in individual memory traces. Contextual control is then the result of the cue-driven retrieval and reinstatement of prior attentional priorities (Crump, 2016; Crump and Milliken, 2009; Crump et al., 2006, 2008). One interpretation of the absence of contextual control in experiment one was the possibility that subjects were adopting the very same "sample everything" strategy in both locations. As a result, the locations may still be operating as effective cues, but they may be cuing the very same attentional control settings in both locations. The purpose of Experiment two was to establish a history of differential attentional processing in each location. This was achieved by including a blocked practice phase prior to the mixed trial phase. The practice phase consisted of a block of trials where a single identification task (e.g., color) was paired consistently with one location, followed by another block where the other identification task (e.g., letter) was paired consistently with the other location. Subjects were informed about the blocked practice phase and mixed phase experimental structure, but were not informed about the proportion manipulations.

Proportion congruent designs sometimes include a blocked practice phase to achieve context-specific attentional control. For example, Lehle and Hübner (2008) could only demonstrate contextual control over flanker effects when subjects received blocked practice first (see also Crump, 2016). This is consistent with the memory-driven account which requires a history of experiences where different attentional priorities were deployed in different situations. The blocked practice phase would allow subjects to adopt dimension-specific sampling strategies, and the mixed phase would allow us to test whether this training is required for producing contextual control.

## Methods

***Subjects*** All subjects were Brooklyn College undergraduate students (approximately ages 18-22) who participated in this study for course credit. Twenty-one subjects completed Experiment 2.

***Apparatus & Stimuli*** The apparatus and stimuli were identical to Experiment 1.

***Design*** The design was similar to Experiment 1 except that subjects completed a blocked practice phase prior to the mixed phase. Experiment 2, therefore involved a one-way within-subjects practice phase with task as a factor (color identification vs. letter identification) and a separate 2x2 within-subjects design for the mixed phase with cue validity (75% vs. 25%) and task (color identification vs. letter identification) as factors. The high-proportion tasks assigned to each location (above or below the fixation) were randomly assigned across subjects. The locations assigned to each task in the blocked practice phase were kept consistent with the validity manipulation in the mixed phase. For example, if the above location was assigned to color during the block phase, the same location was assigned to be 75% color identification in the mixed phase. Whether the first practice block involved the color or letter location was randomly assigned across subjects.

There were a total of 512 trials. The first practice block included 128 trials, with all stimuli appearing in one location and requiring only one identification task. The second block repeated this procedure with the other location and identification task. The last two blocks (the mixed phase) consisted of 128 trials each, with 50% color and letter identification trials occurring with equal probability above or below the fixation. As with Experiment 1, one location (randomly assigned as above or below the fixation) consisted of 75% color identification trials (96 trials) and 25% letter identification trials (32 trials) while the other location consisted of 75% letter identification trials and 25% color identification trials. The trial sequence was randomized for each subject.

***Procedure*** The procedure was identical to Experiment 1.

**Results**

Mean accuracy scores for each subject in each condition are displayed in Figure 2.3. Mean RTs and accuracy scores for all experiments are displayed in Table 1.

***Training Phase.*** Mean accuracy scores for each subject were submitted to a repeated-measures ANOVA with task (color identification vs. letter identification) as the sole factor. There was no significant difference between mean accuracy in the color identification block (M = 74.6%) and the letter identification block (M = 76.6%), $F(1, 20) = 0.29$, $MSE = 159.68$, $p = .597$, $\hat{\eta}^2_p = .014$.

***Mixed Phase.*** Mean accuracy scores for each subject were submitted to a repeated-measures ANOVA with cue validity (75% vs. 25%) and task (color identification vs. letter identification) as factors. Both main effects for cue validity, $F(1, 20) = 0.04$, $MSE = 0.01$, $p = .847$, $\hat{\eta}^2_p = .002$ and task, $F(1, 20) = 1.96$, $MSE = 0.03$, $p = .177$, $\hat{\eta}^2_p = .089$ were non-significant. The two-way interaction between cue validity and task was also non-significant, $F(1, 20) = 0.46$, $MSE = 0.01$, $p = .505$, $\hat{\eta}^2_p = .023$. Subjects performed equally well on the valid and invalidly cued trials.

How performance compared between the mixed and training phases was also of interest. To address this question, overall accuracy scores from the mixed and training phases were submitted to a pairwise t-test and found that subjects performed significantly better in the Training Phase (M = 76%) than the Mixed Phase (M = 61%), $t(20) = -5.38$, $p < .001$.

**Discussion**

Experiment 2 again failed to demonstrate context-specific control over attentional priorities for sampling from color and letter dimensions in briefly presented displays. As with Experiment 1, in the mixed phase targets were not better identified on valid versus invalid trials.

However, accuracy was substantially better in the blocked practice phase than the mixed phase. One interpretation of this finding is that attentional priorities for sampling from letter vs. color dimensions can be set in a preparatory fashion, when subjects know in advance which dimension will be cued. On this view, our blocked phase would have successfully created a memory record that would be suitable for producing contextual control. That is, each subject would have a history of experiences where they prioritized the color dimension in one location and the letter dimension in the other. Yet, there was no evidence of contextual control in the mixed phase.

There are several possibilities for the absence of context-specific effects. Increased accuracy in the blocked phase may not reflect changes to attention, but could instead reflect general differences in task difficulty between blocked and mixed phases. Perhaps contextual control phenomena do not generalize to our bi-dimensional sampling task. Finally, it is possible that contextual control can be interfered with by intentional control. More specifically, even though subjects may have learned to assign different attentional priorities in each location during the blocked practice phase, when they were confronted with the trial-to-trial uncertainty of the upcoming identification task in the mixed phase, they may have intentionally deployed a "sample-everything" strategy, which could have superseded any contextual influences over setting attentional priorities.

## Experiment 3

Experiment 3 investigated the role of intentions in producing contextual control. Subjects were made aware of the location-specific proportion manipulation and explicitly instructed to adopt and maintain differential attentional strategies in each location. Specifically, subjects were instructed to attend more to the colors in the color location and more to the letters in the letter location, as indicated by the training phase. Importantly, subjects were instructed to maintain the location-specific attentional priorities in the mixed phase. In this way, we tested whether or not context-specific differences in attentional priorities could be set by intentional means

**Methods**

***Subjects*** All subjects were Brooklyn College undergraduate students (approximately ages 18-22) who participated in this study for course credit. A total of 20 subjects completed Experiment 3.

***Apparatus & Stimuli*** The apparatus and stimuli were identical to the previous experiments.

***Design*** The design was identical to Experiment 2.

***Procedure*** The procedure was similar to Experiment 2, however in Experiment 3 subjects were made aware of the proportion manipulation and instructed to follow explicit strategies consistent with the predictiveness of the location cue. Specifically, they were instructed to maintain the strategy of "attend more to the letters in the letter location" and "attend more to the colors in the color location" as defined by their practice phase. In addition, they were told to do there best when they were asked to identify a dimension that was inconsistent with the strategy, but to continue the strategy.

**Results**

One concern with adopting this set of instructions was that subjects would ignore the task cues and always respond with the high-proportion task response set. For example, when the target display occurred in the 75% letter location, subjects may ignore the cue that says "Color?" and respond with a letter every trial. To determine which subjects may have adopted this strategy, we calculated the task accuracy for each subject in each condition. The task accuracy reflects the proportion of trials where a subject responded with one of the four appropriate response keys (letters when asked for a letter and colors when asked for a color) regardless of whether it was the correct response. Subjects with less than 25% task accuracy in any condition were not included in the analysis. This criterion was applied to all remaining analyses. For Experiment 3, this eliminated three subjects. Mean task accuracy for the remaining subjects was 96%.

Mean accuracy scores for each subject in each condition are displayed in Figure 2.4. Mean RTs and accuracy scores for all experiments are displayed in Table 1.

***Training Phase*** There was no significant difference between the mean accuracy in the color identification block (M = 79.8%) and the letter identification block (M = 81.9%), $F(1, 16) = 0.61$, $MSE = 64.60$, $p = .445$, $\hat{\eta}_p^2 = .037$.

***Mixed Phase*** Mean accuracy scores for each subject were submitted to a repeated-measures ANOVA with cue validity (75% vs. 25%) and task (color identification vs. letter identification) as factors. The critical main effect of cue validity was significant $F(1, 16) = 14.34$, $MSE = 0.01$, $p = .002$, $\hat{\eta}_p^2 = .473$. Additionally, the main effect for task was non-significant $F(1, 16) = 0.06$, $MSE = 0.03$, $p = .808$, $\hat{\eta}_p^2 = .004$, and the two-way interaction between cue validity and task was non-significant $F(1, 16) = 0.28$, $MSE = 0.01$, $p = .605$, $\hat{\eta}_p^2 = .017$. Subjects therefore performed better for both letter and color targets on valid than invalid trials.

### Discussion

Experiment 3 successfully demonstrated that subjects can adjust attentional priorities for sampling from letter versus color dimensions between contexts in the mixed phase. It is possible that the adjustment of attentional priorities reflected contextual control processes, but they could also reflect rapid intentional control. That is, subjects could simply be following task instructions to deliberately change their attentional priorities in response to the location context in which a display occurs.

## Experiment 4

Experiment 4 used a process-dissociation type logic (Jacoby, 1991) to isolate potential automatic and voluntary influences driving the context-specific effects found in Experiment 3. Like Experiments 2 and 3, Experiment 4 included a blocked practice and a mixed phase. However, the

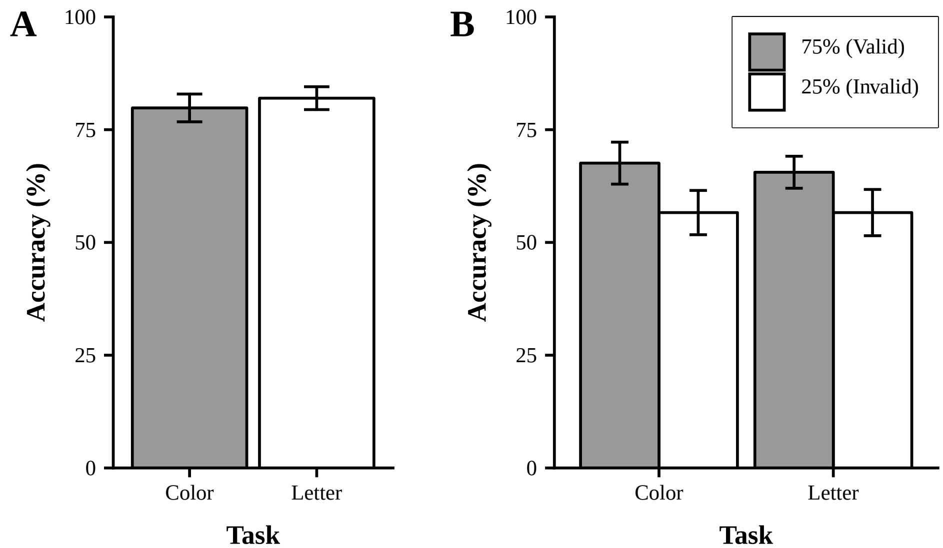


**Figure 2.4:** Results from Experiment 3

Results for the Training Phase (left) and Mixed Phase (right) from Experiment 3. Accuracy scores (%) for color and letter identification trials as a function of location cue validity.

mixed phase contained two kinds of blocks that were either consistent or inconsistent with training. In the consistent blocks, the valid (75%) locations for color and letter tasks were consistent with training. In the inconsistent blocks, the valid locations were reversed from those used in training. Additionally, subjects were instructed throughout the experiment about which dimension should be attended to in each location. Subjects were always told which location was valid for both tasks, and were always instructed to prioritize each task in its respective predicted location for the current block. If the effects from Experiment 3 are due solely to volition, then subjects should be able to adjust their strategies appropriately even when those strategies are assigned to locations that were inconsistent with training. If on the other hand, subjects did form associations between location cues and intentionally set attentional priorities, then reversing the high-proportion tasks assigned to location contexts should interfere with context-specific effects by acting against intentional influences.

## Methods

***Subjects*** All subjects were Brooklyn College undergraduate students (approximately ages 18-22) who participated in this study for course credit. Twenty-six subjects completed Experiment 4.

***Apparatus & Stimuli*** The apparatus and stimuli were identical to the previous experiments.

***Design*** The design was similar to Experiment 3 except that subjects completed blocks of trials where the high-proportion locations were reversed. This involved a practice phase with task as a factor (color identification vs. letter identification), and a separate 2x2x2 within-subjects design for the mixed phase with cued validity (75% vs. 25%), task (color identification vs. letter identification) and test blocks (trained context vs. reversed context) as factors. The high-proportion tasks assigned to each location (above or below the fixation) were randomly assigned across subjects. The locations assigned to each high-proportion task in the blocked practice phase were consistent with the trained context blocks and reversed in the reversed context blocks. Additionally, whether the first practice block involved the high-proportion color or high-proportion letter location was also randomly assigned across subjects.

There were 480 trials. The first practice block included 48 trials, with all stimuli appearing in one location and requiring only one identification task. The second block repeated this procedure with the other location and other identification task. The mixed phase involved four blocks of 96 trials with 50% color and letter identification trials occurring with equal probability above or below the fixation. One location (randomly assigned to above or below the fixation) consisted of 75% color identification trials and 25% letter identification trials while the other location consisted of 75% letter identification trials and 25% color identification trials. The first and third blocks were trained context blocks, in that the locations for the valid trials were consistent with those during training. The second and fourth blocks were reversed context blocks, in that the locations for the valid trials were reversed from those in the training phase. The trial sequence was randomized for each subject.

***Procedure*** The procedure was similar to Experiment 3, in that subjects were made aware of the proportion manipulation and instructed to follow explicit strategies to account for the predictiveness of the location cue. However, every 96 trials, the prompt would instruct them to reverse their strategies.

**Results**

As with Experiment 3, task accuracy was calculated for each subject and those with less than 25% were not included in the analysis. Accordingly, six subjects were eliminated from the following analysis. Mean task accuracy for the remaining subjects was 92%. Mean accuracy scores for each subject in each condition are displayed in Figure 2.5. Mean RTs and accuracy scores for all experiments are displayed in Table 1.

***Training phase*** Accuracy was significantly better in the letter identification task (M = 77.8%) than the letter identification block (M = 67.7%), $F(1, 19) = 10.48$, $MSE = 97.38$, $p = .004$, $\hat{\eta}^2_p = .356$.

***Mixed phase*** Mean accuracy scores for each subject were submitted to a repeated-measures ANOVA with cue validity (75% vs. 25%), task (color identification vs. letter identification) and test blocks (trained context vs. reversed context) as factors.

The three-way interaction between cue validity, task, and test blocks was non-significant, $F(1, 19) = 1.94$, $MSE = 0.00$, $p = .180$, $\hat{\eta}^2_p = .093$. However, the critical two-way interaction between cue validity and test blocks was significant $F(1, 19) = 5.25$, $MSE = 0.01$, $p = .034$, $\hat{\eta}^2_p = .217$, showing that the validity effect was larger during trained context test blocks compared to reversed context test blocks. To further analyze the significant two-way interaction, task was collapsed over and the reversed and trained context conditions were analyzed separately. A separate analysis of the trained context revealed significantly higher accuracy on valid (M = 57%) than invalid (M = 47%) trials, $t(19) = 3.34$, $p = .003$. The analysis of the reversed context also showed higher accuracy on valid (M = 54%) than invalid (M = 50%) trials, $t(19) = 2.09$, $p = .050$.

For the sake of completeness: The two-way interaction between cue validity and task $F(1, 19) = 1.00$, $MSE = 0.01$, $p = .329$, $\hat{\eta}^2_p = .050$, and task and test blocks, , were both non-significant. Finally, the main effect for cue validity was significant, $F(1, 19) = 9.85$, $MSE = 0.02$, $p = .005$, $\hat{\eta}^2_p = .341$, and the main effects for both task $F(1, 19) = 0.98$, $MSE = 0.02$, $p = .335$,

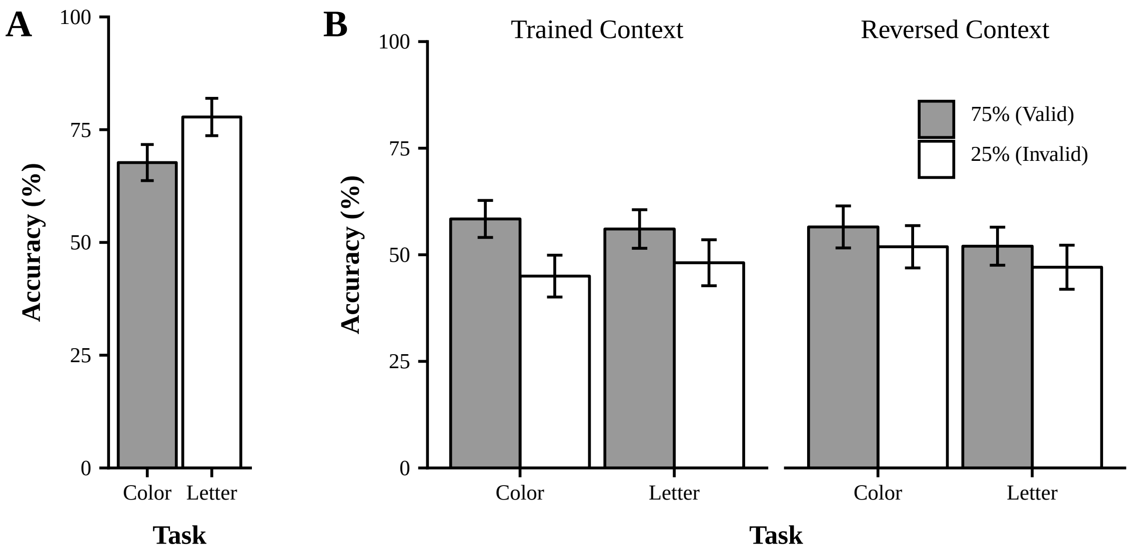


**Figure 2.5:** Results from Experiment 4.

Results for the Training Phase (left) and Mixed Phase (right) from Experiment 4. Accuracy scores (%) for color and letter identification trials as a function of location cue validity.

$\hat{\eta}_p^2 = .049$, and test blocks, $F(1, 19) = 0.00$, $MSE = 0.01, p = .991, \hat{\eta}_p^2 = .000$, were non-significant.

***Block analysis*** Also of interest was whether the validity effects changed over the course of the experiment. We divided the test trials in half, examining the validity effects in the first half of the test trials (comprised of the first trained and reversed context blocks) and the second half (comprised of the second trained and reversed context blocks). A visual inspection of the results displayed in Figure 2.6 suggest a validity effect for the trained context block but not the reversed context block in the first half, and no validity effects for either the trained or reversed context blocks in the second half. This result was confirmed in the following statistical analyses. For the sake of brevity, only the critical tests are reported.

Mean accuracy scores were submitted to a repeated-measures ANOVA with test half (first half vs. second half), cue validity (75% vs. 25%), task (color identification vs. letter identification), and test block (trained context vs. reversed context) as factors. The four-way interaction was non-significant, $F(1, 19) = 0.01$, $MSE = 0.01, p = .926, \hat{\eta}_p^2 = .000$. However, the critical three-way interaction between test half, test block, and cue validity was significant, $F(1,19) = 7.55$, $MSE = 0.02$, $p = .013$, $\hat{\eta}_p^2 = .284$.

To probe the three-way interaction, each test half (first and second) was analyzed separately, collapsing over task. Mean accuracy scores for each test half were submitted to a repeated measures ANOVA with cue validity (75% vs. 25%) and test block (trained context vs. reversed context) as factors.

First, we analyzed the first half and found a significant two-way interaction between cue validity and test block, $F(1,19) = 18.27$, $MSE = 0.01$, $p < .001$, $\hat{\eta}_p^2 = .490$. Further analyses of the simple effects revealed significantly higher accuracy in the 75% (valid) trials (M = 57%) as compared to the 25% (invalid) trials (M = 37%) for the trained context, $t(19) = 4.62$, $p < .001$, and no significant difference between accuracy scores in the 75% (valid) and 25% (invalid) trials for the reversed context, $t(19) = 1.62$, $p = .121$.

An analysis of the second half revealed no significant two-way interaction between cue validity and test block, . The main effect of cue validity was also non-significant, $F(1,19) = 2.23$, $MSE = 0.01$, $p = .152$, $\hat{\eta}_p^2 = .105$. The main effect of test block however, was significant, $F(1,19) = 8.63$, $MSE = 0.01$, $p = .008$, $\hat{\eta}_p^2 = .312$.

To summarize, we analyzed the first and second halves of the test trials separately and found a significant validity effect for the trained context and no validity effect for the reversed context in the first half of the test trials. For the second half we found no significant validity effects across both the trained and reverse context test blocks.

**Discussion**

The results of Experiment 4 replicate the general findings of Experiment 3 showing that identification accuracy was higher on valid than invalid trials. The critical finding was that the validity effect depended on the nature of the test block. The validity effect was larger on trained context blocks compared to reversed context blocks. Trained context blocks assigned the 75% color and 75% letter task locations to the same locations used for each task during blocked practice. The reversed

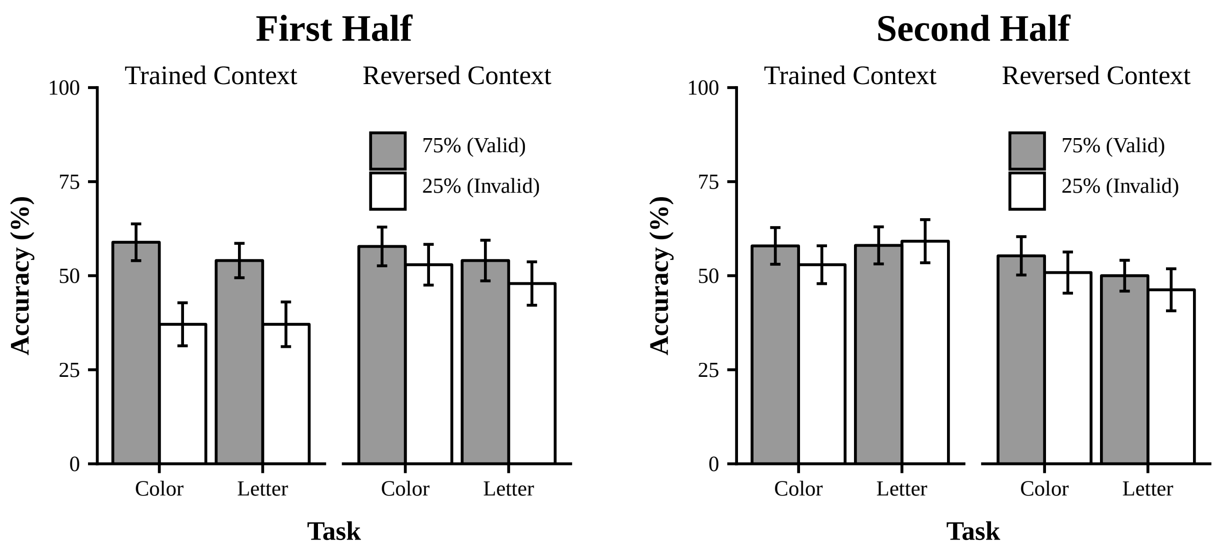


**Figure 2.6:** Results from Experiment 4: Block Analysis.

Results from Experiment 4 for the first (left) and second (right) halves of the Mixed Phase. Accuracy scores (%) as a function of task (color vs. letter), location cue validity (75% vs. 25%) and test blocks (trained context vs. reversed context).

context blocks flipped the assignment with respect to blocked practice. As a reminder, prior to and during each test block subjects were always told which location predicted each task. They were also instructed to attend more to color in the location that predicted the color task, and more to letters in the location that predicted the letter task. If the validity effect was entirely driven by a flexible intentional control process, then we expected that process to produce equivalent validity effects regardless of whether the test block was consistent or reversed from training. Instead, we found smaller validity effects in the reversed context test blocks. One interpretation of this finding is that the associations between contextual cues and attentional strategies formed during training interfered with the deployment of intentional strategies during the reverse context test blocks. On this view, the results from Experiments 3 and 4 are not due solely to voluntary shifts in attentional prioritization, but also reflect some contribution of contextual control over setting of attentional priorities.

We also found significant changes in the validity effects from the first to second half of the test blocks. Specifically, there was only a validity effect in the first block of the trained context trials and there were no significant validity effects for the reversed context trials in either block. Our purpose in running Experiment 4 was to determine whether the results from Experiment 3 could be accounted for solely on the basis of volitional shifts in attentional prioritization. The block analysis then provides even less support for this idea that subjects could flexibly shift attentional priorities in the reversed context blocks because there was no evidence for any validity effect within each reversed block when analyzed separately. However, our design consisted of a fixed order of test blocks (trained, reversed, trained, reversed) and we only found validity effects in the first block. One possibility is that subjects were unable to continually deploy instructed strategies. Perhaps subjects lacked motivation to continue applying the instructed strategies or they were motivated but only had available resources to apply the effortful strategies for a short period of time. On this view, subjects could voluntarily shift attentional priorities but chose not to or were unable to maintain voluntary control over longer periods. However, we note that accuracy in general increased across blocks, which suggests that subjects became more motivated to perform well in the task.

Alternatively, the finding that the validity effects were only evident in the first test blocks could also reflect some contribution of contextual control over setting of attentional priorities. It is clear that in the first test block subjects did adopt the trained attentional strategy, but then failed to adopt the reversed strategy in the second block. One interpretation is that training provided the needed experiential support for enacting the strategy, and when that support was missing (for the reversed context blocks) strategic control was not possible. Similarly, the disappearance of the validity effect could be due to the fact that associations formed during training were extinguished during the mixed phase which involved many new trial types that were inconsistent with training. This interpretation fits with previous findings that effectively applying an attentional strategy for ignoring a distractor requires experiential learning, or practice with ignoring the distractor (Vecera et al., 2014). For example, Cunningham and Egeth (2016) examined whether subjects could make use of a pre-cue signaling the identity of an upcoming distractor for the purposes of ignoring the irrelevant feature when it appeared. They found that subjects learned to ignore distractors that were preceded by a consistent cue that always signaled the same distracting feature across trials. However, when the pre-cue signaled different distracting features across trials participants failed to benefit from the pre-cue for the purpose of ignoring the signaled distractor. Our results are similar in that subjects were able to transfer their learning from the practice blocks to the first test block which was mostly consistent with training. However, across test blocks attentional sampling demands became increasingly inconsistent with practice and prevented transfer of learning from the practice phase to test block performance.

## General Discussion

In experiment one, one location involved 75% color and 25% letter identification trials and the other involved 75% letter and 25% color identification trials. Despite the fact that location predicted the likely task on each trial, we found no evidence of contextual control. Specifically, identification performance on validly cued trials was not different from invalidly cued trials. The automatic adaptation accounts posit learning processes sensitive to error- or conflict-driven signals; however, no evidence of contextual control was found in Experiment 1 despite the poor accuracy that should allow for such learning processes to operate. One explanation of the absence of contextual control was the suggestion that subjects adopted a "sample everything" strategy on every trial, and learned associations between location contexts and the attentional priorities assigned by the "sample everything" strategy.

In experiment two, the mixed phase was preceded by a blocked training phase where each identification task was performed consistently in a particular location. The blocked phase was included to ensure that subjects had experiences with deploying different attentional priorities between contexts. We found better identification performance in the blocked than the mixed phases. However, there was no validity effect in the mixed phase, indicating no evidence of contextual control. Although there was evidence that subjects did learn to assign different attentional priorities in the different contexts during the blocked practice, this learning apparently failed to transfer to control setting of attentional priorities in the mixed phase. One reason for the absence of contextual control in the mixed phase was that subjects again decided to adopt the "sample everything" strategy which could have overridden the ability of contextual cues to set attentional priorities.

In experiment three, we repeated the same design as experiment two, but made subjects aware of the validity manipulation and gave them instructions to attend more to color information in the 75% color location, and more to letter information in the 75% letter location. The major finding was the presence of a validity effect in the mixed phase, indicating that subjects were capable of assigning different attentional priorities between locations. However, it remained unclear whether this effect was mediated by cue-driven or intentional influences over setting of attentional priorities.

In experiment four, we included new test blocks in the mixed phase where the valid locations for the color and letter tasks were consistent or reversed from training. The critical finding was a larger validity effect when the locations were consistent rather than reversed from training. Here, the maintenance of voluntary attentional strategies was impaired when the strategies were inconsistent with the attentional priorities cued by location contexts established during training. This finding suggests that the validity effect in the mixed phase was not entirely driven by a flexible intentional process capable of setting attentional priorities according to instructions, but also reflects a contribution from contextual influences that control the setting of attentional priorities in a cue-driven fashion.

One general aim of the present work was to test guiding principles of contextual control by using a novel bi-dimensional sampling task. We found some cases where contextual control was not acquired or expressed in performance, and some cases where contextual control appeared to depend on previous experience with deploying different strategies for setting attentional priorities between contexts. We now turn to a discussion of the roles of awareness and intention in promoting contextual control, as well as a number of task differences between our procedure and previous ones that may explain the presence and absence of contextual control across our experiments.

Our findings depart from previous work showing that contextual control develops without awareness and intention. We consider two perspectives about how our findings relate to previous work. First, we may have found an exceptional case where contextual control does depend on experiences with intentionally adopting different attentional priorities in different contexts. On this view, contextual control can develop with or without intention, and whether or not contextual control relies on intention would be task-dependent.

Second, we consider the possibility that prior demonstrations of CSPC effects did depend on experiences with intentionally adopting different attentional priorities between contexts. A common finding in Stroop and flanker variants is that subjects are not aware of the CSPC manipulation. One interpretation of this finding is that subjects who were not aware of the manipulation did not intentionally use contextual cues to adopt different attentional priorities. However, we assume that subjects were always intentionally controlling attention priorities at the level of specific items, and that individual stimuli provoke subjects to adopt particular attentional priorities appropriate to the stimulus and task at hand. Consider the item-specific strategies adopted by a subject in CSPC Stroop task who is unaware of the CSPC manipulation, but who is following instructions to respond to a color dimension as quickly and accurately as possible. When a congruent item is presented, subjects may actively assign more priority to the word dimension because it matches with the correct color response. When an incongruent item is presented, subjects may actively assign less priority to the word dimensions because it does not match the correct color response. In this way, subjects may have intentions for setting attentional priorities at the item-specific level that can be exercised on each trial regardless of whether they are aware of the context-specific proportion congruent manipulation. As a result, the attentional priorities resulting from intentional control at the item-level could become associated with the contexts in which items frequently occur, thereby leading to the development of contextual control.

In order to entertain the view that intentional control does influence the acquisition of contextual control across tasks, we also need to consider discrepancies in the effectiveness of instructional manipulations. We show that subjects can follow instructions to adopt context-specific attentional priorities in the bi-dimensional sampling task. However, similar instructional manipulations were not effective in a CSPC Stroop task (Crump et al., 2008). Here, subjects were encouraged to adopt context-specific attentional priorities in response to the shape context of a target stimulus. Subjects were aware of the CSPC manipulation and were instructed to adopt context-specific attentional priorities; nevertheless, no evidence of contextual control was obtained. One interpretation of this finding is that intentional control is not sufficient for producing contextual control. However, it remains unclear whether those subjects could follow the instructions. For example, in their prime-probe Stroop task the subject was briefly presented with a word that disappeared before the target color patch was displayed in one of two shapes. The instruction was to use the shape cue to rely more or less on the previous word to help with responding to the color of the target. It is not clear how subjects would attempt to retrospectively ignore the influence of an already presented word. Therefore, intentional control may have failed to influence the acquisition of contextual control in those tasks because subjects were never engaging in context-specific intentional control to begin with. By contrast, in the current task the instruction to attend more to the colors versus the letters may be easier to communicate, more readily understood, and easier to execute than instructions employed in prior tasks.

Finally, there are critical differences between our task and those that have previously demonstrated contextual control. Though the current study suggests that intentional control may be a pre-requisite for the acquisition of contextual control, these task differences could potentially limit the generalizability of our findings and warrant further discussion.

Contextual control has been observed in several interference tasks where there is response conflict between target and distractor dimensions. In contrast, there was no apparent response conflict between our color and letter dimensions. Some theories of contextual control assume that the presence of this conflict drives learning and mediates the acquisition of contextual control (Blais et al., 2012; Botvinick et al., 2001; Verguts and Notebaert, 2008, 2009). For example, Crump et al. (2008) showed no CSPC effects in a Stroop task where subjects named words rather than colors. If contextual control depends on the presence of response conflict between target and distractor dimension, then the absence of contextual control in experiments one and two could be due to the absence of similar kinds of conflict in our task.

At the same time, we assume that our displays prompted ubiquitous response competition on each trial. Target displays consisted of four unique letters and colors and therefore on any given trial, eight potential responses. Additionally, the total amount of response competition could have been reduced substantially (from eight to four) to the extent that contextual cues signaled selective processing of the dimension that was usually probed in that location. On this view, if the acquisition of contextual control is strongly related to the size of the conflict signal, we would have expected rapid learning and strong evidence for contextual control in our first experiments.

Alternatively, it is possible that the high degree of response conflict prevented the acquisition of contextual control. Prior work has demonstrated an absence of contextual control when subjects failed to attend to contextual information. For example, Crump, Gong, and Milliken (2006) could only find evidence for contextual control using shape cues when subjects were given a secondary task that required them to explicitly attend to the shapes. This suggests that contextual information may need to be attended to and integrated with target information in order for contextual control to develop. The high degree of response conflict in our task may have limited the amount of attention subjects could direct to the contextual features or caused a failure in the stimulus-context integration process; both of which could explain the absence of contextual control in our experiments.

The time-course of stimulus presentation relative to the contextual cues may also influence the acquisition of contextual control. In our experiments we always presented the target display simultaneously with the contextual cue. Simultaneous presentation has been effective in demonstrating contextual control in Stroop and flanker tasks (Bugg and Crump, 2012). In general, prior work has not systematically explored how the time-course of contextual cueing influences the effectiveness of the cues. However, Reuss et al. (2014) presented the contextual cue either prior to or simultaneously with the target and found contextual control in both cases and Fischer et al. (2014) found contextual control developed more rapidly when the location-context was cued prior to the target. We could speculate however, that the effectiveness of the cue changes as a function of the timing relationships and it is possible that these changes are task-dependent. For example, simultaneous presentations may be effective in a Stroop or flanker task, but ineffective in a bi-dimensional sampling task such as ours. For this reason, it is possible that the absence of contextual control in our experiments was due to the fact that we used simultaneous presentations rather than advance presentations. In general, systematic investigation of time-course issues in any demonstration of contextual control is an important topic for future work.

Numerous studies show that attentional priorities for processing stimulus dimensions can be modulated by contextual cues (e.g., Corballis and Gratton, 2003; Crump, 2016; Crump et al., 2006, 2008; Gough et al., 2014; King et al., 2012). Furthermore, a growing body of literature has demonstrated that awareness of experimental manipulations such as dimensional-conflict, proportion of trial-types, and contextual cues, are not required to produce such effects (e.g., Heinemann et al., 2009; King et al., 2012; Panadero et al., 2015; Reuss et al., 2014; Sarmiento et al., 2012). This lack of awareness has suggested that contextual control can occur independent of a subject's intent. Our findings add to this body of work and show one situation where intentional setting of attentional priorities appears to be a pre-requisite for context-specific control. Our experiments also show that principles from theories of contextual control such as automatic error-driven learning, and automatic retrieval of prior instances do not necessarily generalize across tasks in a straightforward manner.

# CHAPTER 3

# SINGLE EXPERIENCES WITH CONFLICT HAVE LONG-LASTING EFFECTS ON COGNITIVE CONTROL

## Preface

This chapter is reproduced from a published article in *Journal of Experimental Psychology: General* (for consistency, I have adjusted the format of the manuscript for the thesis). The full reference to the article:

**Brosowsky, N.P.** & Crump, M.J.C. (2018), Memory-guided selective attention: Single experiences with conflict have long-lasting effects on cognitive control. Journal of Experimental Psychology: General , 147(8), 1134-1153. http://dx.doi.org/10.1037/xge0000431

Chapter 3 presents a set of three experiments testing general principles of the instance-based memory account. Two key assumptions that fall out of the memory-based account are that (1) processing details from individual experiences are stored in memory traces and (2) these memory traces are stored for the long-term. Although the context-specific proportion congruent phenomena are often taken as evidence for a memory-based retrieval process, because a small set of items repeat throughout the experiment there is no way to trace the changes in performance back to any single prior experience. Here, we examined whether single experiences can have long-term (4 to 319 intervening trials) on selective attention.

Long-term item-specific effects have been demonstrated across a variety of different cognitive control paradigms like negative priming and task-switching, but has never been examined within the context of more traditional selective attention paradigms like Stroop and flanker tasks. However, the congruency sequence effect, a very common Stroop and flanker phenonemon, demonstrates short-term single-trial effects, but is not typically explained as the result of a memory-retrieval process. Here, we used a large set of unique context items to determine whether a single experience applying attentional control on an item can influence a second presentation after many intervening trials. If so, we predicted we would see a pattern of results similar to the congruency sequence effect.

Across three experiments we found evidence that single experiences can influence selective attention after many intervening trials (4 to 319). However, we also note some boundary conditions, failing to find such effects when the repeated items differed in superficial details (color).

**Abstract**

Adjustments in cognitive control, as measured by congruency sequence effects, are thought to be influenced by both external stimuli and internal goals. However, this dichotomy has often over-shadowed the potential contribution of past experience stored in memory. Here, we examine the role of long-term episodic memory in guiding selective attention. Our aim was to demonstrate new evidence that selective attention can be modulated by long-term retrieval of stimulus-specific attentional control settings. All the experiments used a modified flanker task involving multiple unique stimuli. Critically, each stimulus was only presented twice during the experiment: first as a prime, and second as a probe. Experiments 1 and 2 varied the number of intervening trials between prime and probe and manipulated the amount of conflict using a secondary task. Experiment 3 ensured that specific colors assigned to prime stimuli were not repeated when presented as probes. Across both experiments 1 and 2, we consistently found smaller congruency effects on probe trials when its associated prime trial was incongruent compared to congruent, demonstrating long-term congruency sequence effects. However, experiment 3 showed no evidence for long-term effects. These findings suggest long-term preservation of selective attention processing at the episodic level, and implicate a role for memory in updating cognitive control.

## Introduction

Cognitive control enables flexible goal-directed behavior via attention and action selection processes that prioritize goal-relevant over irrelevant information. Attention is known to be strongly influenced by both external stimuli and internal goals. However, the strict dichotomy between stimulus-driven and goal-driven influences (Posner and Snyder, 1975b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977) has downplayed the role of memory in guiding attention (Awh et al., 2012; Hutchinson and Turk-Browne, 2012). People often re-encounter similar objects, tasks, and environments that require similar cognitive control operations. A memory-retrieval process could shortcut the slow, effortful, and resource-demanding task of updating control settings by retrieving and reinstating the control procedures used in the past. Here we examine the role of long-term episodic memory in guiding selective attention.

Evidence for long-term, cue-driven retrieval of control operations has been reported in multiple attention paradigms, suggesting a general phenomenon. However, evidence within paradigms is limited to a small number of reports, and remains absent in conventional selective attention tasks, such as Stroop (1935) and Flanker (Eriksen and Eriksen, 1974), commonly used to make inferences about cognitive control processes. Our aim was to demonstrate new evidence that selective attention can be modulated by long-term retrieval of stimulus-specific attentional control settings, and then discuss implications of these findings for theories of cognitive control.

### Long-term retrieval of control settings

Early evidence for long-term, cue-driven retrieval of attentional control settings was developed in the negative priming literature (for recent reviews, see D'Angelo et al., 2016; Frings et al., 2015). Negative priming refers generally to the finding that reaction times to identify a previously ignored target are slowed compared to a target that was not previously ignored (Tipper, 1985). In a typical design, a prime display might include a to-be-named green target word (e.g., TRUCK) interleaved with a to-be-ignored red distractor word (e.g., PIANO). An immediately following probe display then presents a target/distractor pair, involving a target that was previously attended (attended repetition: TRUCK), previously ignored (ignored repetition: PIANO), or a word that was not attended or ignored (control: MOCHA). Negative priming is observed when ignored repetition reaction times are slower than control trials. Early explanations of negative priming invoked a short-term, transient inhibitory process: ignoring a stimulus causes it to be briefly inhibited, and negative priming reflects the extra time needed to recover from inhibition during responding (Tipper, 1985; Tipper and Driver, 1988). However, two classes of findings were difficult to reconcile with the short-term inhibition explanation, and were formative for the idea that long-term, cue-driven memory processes may play a role in re-instating prior attentional control settings.

First, negative priming is sensitive to the match between probe and prime tasks, and can disappear when the probe task does not require selective attention to the target. The above task description involves selection in both prime and probe trials, as both trials present an interleaved target/distractor pair. If negative priming reflects carry-over of inhibition from the ignored distractor on the prime trial, then that inhibition ought to be detected on a following probe trial that presented the ignored distractor alone, as a single target. In this case, the probe trials do not require selection because only a single target is displayed. However, several experiments showed that negative priming is abolished when the probe display contains a single target (Lowe, 1979; Milliken, Joordens, Merikle, and Seiffert, 1998; Moore, 1994; Tipper and Cranston, 1985).

Second, negative priming can persist for long temporal intervals between a prime and probe trial. DeSchepper and Treisman (1996) demonstrated that negative priming in a shape discrimination task is observed up to 30 days between a prime trial (including a target and distractor shape), and a probe trial (including the previously ignored shape as the target). We are aware of only two other investigations of long-term negative priming. Lowe (1998) demonstrated negative priming persisting for 5 minutes, and Grison, Tipper, and Hewitt (2005), showed negative priming persisting over 54 intervening trials between a prime and probe.

Taken together, the findings that negative priming is sensitive to the match between probe and prime tasks, and that negative priming persists over the long-term, provided evidence suggesting a role for memory-based retrieval processes in negative priming. For example, inspired by instance-theories of memory (Hintzman, 1984; Logan, 1988b), Neill and colleagues (Neill, 1997; Neill et al., 1992) proposed an episodic retrieval account of negative priming. Here, an ignored distractor presented during a prime trial is tagged with a "do-not-respond" control operation. If the ignored distractor is presented as a target on the following probe trial, it could then retrieve its associated "do-not-respond" control operation, which would interfere with responding to that stimulus on the probe trial. Furthermore, because control operations associated with prime processing are preserved in an instance-based memory, they could be available (under the appropriate retrieval conditions) over the long-term.

Evidence for long-term retrieval of attention control settings, like those observed in negative priming, has been shown in a few different attention paradigms. These include long-term inhibition of return (Tipper et al., 2003), long-term retrieval of task-sets in task-switching (Waszak et al., 2003), long-term priming-of-pop out in visual search (Thomson and Milliken, 2012, 2013), and long-term response inhibition in stop-signal tasks (Verbruggen and Logan, 2008). It remains unclear whether this collection of evidence points to a general role for memory retrieval of control operations linked with specific prior processing episodes to update and adjust control operations in the present moment.

However, evidence for long-term retrieval of attention control settings has not been established in classic selective attention paradigms, such as Stroop and Flanker, commonly used to make inferences about cognitive control processes. A demonstration would be useful in its own right to further establish the generality of the phenomena and would test theories of control processes used to explain modulations to congruency effects. We outline theoretical implications for explanations of $n - 1$ congruency sequence effects, and proportion congruent effects; and, then overview the procedures we adopted to measure long-term memory based control of attention.

**Congruency effects**

Congruency tasks measure target identification in the presence of potentially conflicting distractors. For example, in the Flanker task (Eriksen and Eriksen, 1974) participants are faster and more accurate to identify a center letter (e.g., ‘HHFHH’) when flanking letters are congruent (e.g., ‘HHHHH’) versus incongruent (e.g., ‘FFHFF’) with the response. Modulations to the size of congruency effects can index the gain of attentional control assigned to target and distractor dimensions. For example, target information is assumed to be prioritized over distractor information when smaller versus larger congruency effects are observed.

Importantly, congruency effects are modulated by the history of previously experienced conflict. Congruency effects are reduced immediately following an incongruent trial, and when the proportion of incongruent trials is greater than the proportion of congruent trials. It is possible that both trial history effects could be explained by common principles, and some existing accounts have forwarded unified theories (Abrahamse et al., 2016; Egner, 2014; Verguts and Notebaert, 2008). We consider whether common principles invoked by the notion of long-term, cue-driven retrieval of attention control settings could explain congruency sequence and proportion congruent effects. Alternatively, memory-driven control could reflect a distinct influence that clarifies how different processes acting over the long and short-term use prior experience with conflict to update control settings.

***Congruency sequence effects.*** Congruency effects on trial $n$ are smaller when trial $n - 1$ contains an incongruent versus congruent trial (Egner, 2007; for a review, see Gratton et al., 1992). Early explanations invoked voluntary control (Gratton et al., 1992), but recent findings suggest volition is not necessary. For example, congruency sequence effects can be produced despite contradictory expectations about the likelihood of conflict on the next trial (Jiménez and Méndez, 2013, 2014) and in the absence of awareness (Desender, Van Lierde, and Van den Bussche, 2013). Congruency sequence effects also occur over short timescales, persisting only for one or two trials (Akçay and Hazeltine, 2008; Mayr et al., 2003), quickly decaying with increased inter-stimulus or response-to-stimulus intervals, and eliminated all-together after three- to seven-second intervals (Duthoo et al., 2014; Egner, Ely, and Grinband, 2010).

All accounts of congruency sequence effects assume that influences from a recent trial on current trial performance are transient and decay rapidly. Debate focuses on whether or not congruency sequence effects are driven by processes that change attentional control settings. Rapid decay is assumed by non-control accounts based on feature integration or event-binding processes (Hommel, 1998; Hommel, Müsseler, Aschersleben, and Prinz, 2001; Hommel et al., 2004), repetition priming (Mayr et al., 2003), and sequential contingency biases (Schmidt and De Houwer, 2011). Rapid decay is also assumed by control accounts based on conflict-monitoring theory (Botvinick et al., 2001). Here, a conflict-monitoring unit registers a transient conflict signal that triggers adjustments to attentional control settings which carry-forward to influence performance on the next trial.

There are notable parallels between the congruency sequence effect and negative priming. Like the congruency sequence, negative priming was assumed to operate on a transient, short-term basis. Although the congruency sequence can dissipate over the short-term, it remains unclear whether experiencing conflict on one trial can have long-term influences over congruency effects on future trials. There is some evidence that congruency sequence effects can accumulate in strength as a function of the number of preceding incongruent trials (Aben, Verguts, and Van den Bussche, 2017; Jiménez and Méndez, 2013; Rey-Mermet and Meier, 2017). However, there is no evidence, akin to long-term negative priming, showing that control operations applied on a single trial to a specific stimulus can be retrieved on a long-term basis to influence control operations to similar stimuli in the future. Another parallel is that congruency-sequence effects, like negative priming, can depend on the match between tasks performed on trial $n-1$ and trial n. For example, conflict experienced on trial $n-1$ in one interference task does not always cause modulations to congruency effects for a different task presented on trial $n$ (for a review, see Braem, Abrahamse, Duthoo, and Notebaert, 2014).

These parallels motivated us to determine whether congruency sequence-like effects could extend across many intervening trials well beyond trial n-1. On the one hand, a finding of this nature could identify a memory-based attentional control process that is distinctly different from other short-term processes also capable of producing congruency sequence effects. On the other hand, perhaps memory-based retrieval of attention control settings could explain the short-term $n - 1$ congruency sequence effect, especially if temporal similarity, along with item and context features are assumed to act as retrieval cues to apply control settings from recent trials (for similar perspectives, see Egner, 2014; Spapé and Hommel, 2008, 2014).

***Proportion congruent effects.*** Proportion congruent effects show larger congruency effects for conditions associated with high rather than low proportions of congruent trials (for a review, see Bugg and Crump, 2012), and are demonstrated in list-wide, item-specific, and context-specific designs. In a Stroop variant, item-specific designs assign one set of items (e.g., Red and Blue combinations) to a high proportion congruent condition, and another set (e.g., Green and Yellow combinations) to a low proportion congruent condition. Both item types are intermixed randomly, so subjects cannot accurately predict whether the next trial will be congruent or incongruent. In these designs, congruency effects are found to be larger for high versus low proportion congruent item. Similarly, Context-specific proportion congruent (CSPC) designs manipulate proportion congruent between two different contexts in which items can appear, again in a randomized, intermixed fashion. CSPC effects have been shown using location (Brosowsky and Crump, 2016; Corballis and Gratton, 2003; Crump, 2016; Crump et al., 2017, 2006; Hübner and Mishra, 2016; Weidler and Bugg, 2016), shape (Crump et al., 2008), color (Vietze and Wendt, 2009), social categories (Cañadas et al., 2013), and incidental semantic cues (Blais et al., 2015). Again, congruency effects are larger for items appearing in high than low proportion congruent contexts. These trial history effects imply that item and context-specific cues become associated with attentional control settings, and that changes to attentional control can be triggered in a cue-driven manner.

We roughly group theories of item and context-specific proportion congruent effects into memory-based and conflict-monitoring accounts. Memory-based accounts invoke instance-based, long-term, cue-driven retrieval processes (Logan, 1988b). Some proportion congruent designs are confounded by item-frequency, and may be explained simply by an event-learning process sensitive to the frequency of events (Schmidt, 2013; Schmidt and Besner, 2008). At the same time other designs show evidence that cues associated with proportion congruent can bias congruency effects even for frequency unbiased items (Crump et al., 2017; Crump and Milliken, 2009; though, see Hutcheon and Spieler, 2017). Here, memory-based accounts argue that attentional control settings are encoded during each processing experience, and are retrieved to update ongoing control operations in the present moment (Bugg and Hutchison, 2013; Crump, 2016; Crump et al., 2008). Conflict-monitoring accounts can explain item-specific proportion congruent effects by assuming that conflict-signals trigger adjustments to attentional control settings on an item-specific basis (Blais, Robidoux, Risko, and Besner, 2007; Verguts and Notebaert, 2008), and this kind of account could in principle be extended to explain context-specific proportion congruent effects.

There are clear parallels between early item-specific proportion congruent designs (Jacoby et al., 2003), and negative priming designs manipulating the application of attentional control sets on an item-specific basis (Milliken, Lupianez, Debner, and Abello, 1999). Indeed, the idea from negative priming that episodic retrieval processes are used to retrieve and reinstate prior attentional control sets was borrowed to explain proportion congruent effects. In the proportion congruent literature however, there is no direct evidence supporting the core assumption of episodic retrieval theories that control operations from single-trials are stored in traces, or that single-traces could be retrieved to influence control operations for specific items on a long-term basis. For example, most proportion congruent designs use a small number of stimuli that are repeatedly presented over an experiment. It is unknown whether cues retrieve a single instance from among the available item repetitions or multiple instances that are aggregated during retrieval.

A demonstration that congruency effects could be modulated by the long-term retrieval of item-specific attention control settings has theoretical implications for proportion congruent effects. A positive demonstration would corroborate predictions from memory-based accounts, and challenge conflict-monitoring accounts that aggregate over item-specific control settings (Botvinick et al., 2001; Braver, 2012; De Pisapia and Braver, 2006; Jiang, Heller, and Egner, 2014).

**Overview of present studies**

Our experiments test whether a single experience with applying attentional control to a unique stimulus can be retrieved on a long-term basis to influence how attentional control is applied when the same stimulus is re-presented later. We reasoned that if the single prior experience is retrieved, it will influence performance on the current trial in a manner similar to the $n-1$ congruency sequence effect where smaller congruency effects are found following an incongruent as compared to congruent trial. In other words, we asked whether a congruency sequence-like effect could be observed on a long-term basis, when there are many intervening trials between a first and second experience with a unique stimulus.

All the experiments used a modified flanker task involving multiple unique stimuli. The designs were inspired by long-term negative priming where a unique target/distractor pair could be presented once as a prime stimulus, and once as a probe stimulus after any number of intervening trials. We created unique stimuli using a large bank of natural objects that could be displayed in different colors (Brady, Konkle, Gill, Oliva, and Alvarez, 2013). Each trial involved a row of objects, and the task was to identify the color of the central object as quickly and accurately as possible. Like other context-specific designs, the object feature dimension was irrelevant to the color-identification task. Each object was only presented once as a prime, either in a congruent or incongruent format, and once as a probe, either in a congruent or incongruent format. Across experiments we varied the number of intervening trials between prime and probe presentations. Our design allowed us to determine whether congruency effects for probe stimuli would vary as a function of prime congruency, indicating a long-term congruency sequence-like effect. Specifically, we measured whether the congruency effect for probe stimuli preceded by incongruent primes would be smaller than the congruency effect for probe stimuli preceded by congruent primes.

Experiments 1a, b, and c varied the number of intervening trials between prime and probe by five to eleven trials and manipulated the amount of conflict using a secondary task. Experiments 2a and b increased the number of intervening trials to an average of 160 trials. To foreshadow our results, we found clear evidence of a long-term congruency-sequence-like effect. Congruency effects for probes preceded by incongruent primes were smaller than congruency effects for probes preceded by congruent primes. Experiments 3a and 3b were conducted to test a long-term feature integration account, and ensured that specific colors assigned to prime stimuli were not repeated when presented as probes. These experiments showed no evidence of long-term congruency sequence-like effects.

## Experiment 1A, 1B, and 1C

For experiment 1, we report three replications of the same experimental design (see Figure 3.1). In all three experiments, the primary task was to identify the color of a central image (either blue or green) flanked on the left and right by the same image presented in either the same (congruent) or alternate color (incongruent). Each image was only presented twice during the experiment: once as a prime stimulus, and once as a probe stimulus. The trial order was constructed such that the distance between any given prime and probe stimulus always ranged from 5 to 11 trials (8 trials, on average). We chose to use a color flanker task so that congruency could be manipulated independently of the image representing the target and flanker stimuli such that we could repeat contextual images while alternating congruency.

The amount of conflict has been shown to influence the size of the $n - 1$ congruency sequence effect (Forster, Carter, Cohen, and Cho, 2011; Wendt, Kiesel, Geringswald, Purmann, and Fischer, 2015; though, see Weissman and Carp, 2013). It was unclear however, whether the amount of conflict would influence our ability to detect long-term influences. For experiment 1A, we used the basic design described above. For experiments 1B and 1C, we included a secondary task to increase conflict and potentially improve our ability to detect the presence of long-term sequence effects. For the secondary task, we required participants to press the spacebar if the identity of the center image differed from the flankers. We reasoned that having participants continuously monitor for differing flanker and target images would cause them to attend more to the flanking images throughout the experiment and increase the overall level of conflict. This alternative task was randomly presented once for every 8 normal trials.

Experiment 1C was a replication of experiment 1B. A Monte-Carlo simulation analysis of the results from experiment 1A suggested that doubling our trial count from 216 to 432 and increasing our subject count to 50 would increase our power to detect the long-term sequence effect from an estimated .7 to .95 (for a complete description of this procedure, see Crump et al., 2017). Therefore, for experiment 1C the trial count was doubled, and we collected data until we had 50 participants who completed all trials and maintained an error rate less than 20%.

## Methods

***Participants.*** All participants were recruited from Amazon Mechanical Turk (AMT) and compensated $1.00 (experiment 1A & 1B) or $3.00 (experiment 1C) for participating. The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hour. For each experiment the number of HITs (Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study. Participants were included in the study if they completed all trials and each experiment consisted of unique participants. For experiment 1A, 40 HITs were posted, and 40 participants completed all trials. For experiment 1B, 40 HITs were posted, and 39 participants completed all trials, and for experiment 1C, 55 HITs were posted, and 54 participants completed all trials.

***Apparatus & Stimuli.*** The experiments were programmed using JavaScript, CSS and HTML. The program allowed participants to complete task only if they were running Safari, Google Chrome, or Firefox web browsers. Flanker stimuli were constructed using the 540 images created by Brady, Konkle, Gill, Oliva, and Alvarez (2013). Images were color rotated to either blue or green (for a more detailed description see Brady et al., 2013) and presented at 200 x 200 pixels. Each experiment ran as a pop-up window that filled the entire screen. The background was white, and stimuli were presented in the center of the screen.

***Design.*** Experiment 1 used a 2x2x3 mixed design with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as within-subject factors, and experiment (1A, 1B, and 1C) as the between-subject factor.

Experiments 1A, 1B, and 1C were all constructed using the same general method (see 3.1). Every block of 16 trials was divided into four sub-blocks, each consisting of four trials (referred to as the Prime A, Prime B, Probe A, and Probe B sub-blocks). The images presented in the Prime A sub-block were then repeated in the Probe A sub-block and images presented in the Prime B sub-block, repeated in the Probe B sub-block. The trial order of each sub-block was randomized. The use of the interleaved A/B sub-blocks ensured that the distance between any probe (trial n) and prime stimulus pair ranged from $n - 5$ to $n - 11$. Importantly, the congruency of each prime/probe pair was randomized and counterbalanced across each block with an equal number of each congruency combination (i.e., Con - Con, Con - Inc, Inc - Con, and Inc - Inc), and an equal number of response repetition and alternation prime/probe pairs. Additionally, images were randomly selected for every participant from the total 540 images (Brady et al., 2013) and randomly assigned a color and condition. Each image was only presented twice during the experiment: once in a prime block and once in a probe block.

Experiment 1A consisted of 192 trials constructed using this basic method. Experiment 1B used the same general design but included a secondary task where participants were instructed to press the spacebar if the center image differed in identity to the flanking images. This alternate task occurred once for every 8 flanker trials, bringing the total trials to 216. Experiment 1C was identical to experiment 1B except the number of trials was doubled, bringing the total to 432 trials.

***Procedure.*** All participants were AMT workers who found the experiment using the AMT system. The participant recruitment procedure and tasks were approved by the Brooklyn College Institutional Review Board. Each participant read a short description of the task and gave consent by pressing a button acknowledging they had read the displayed consent form. Participants then completed a short demographic survey, and proceeded to the main task, which was displayed as a pop-up window. Participants were instructed to identify the color of the center image on each trial as quickly and accurately as possible by pressing ‘g’ if the image was green, and ‘b’ if the image was blue. For experiments 1B and 1C, participants were further instructed to press the spacebar if the identity of the center image differed from the identity of the flanking images. Throughout the course of the experiment the upper left corner of the display indicated the number of completed and remaining trials,

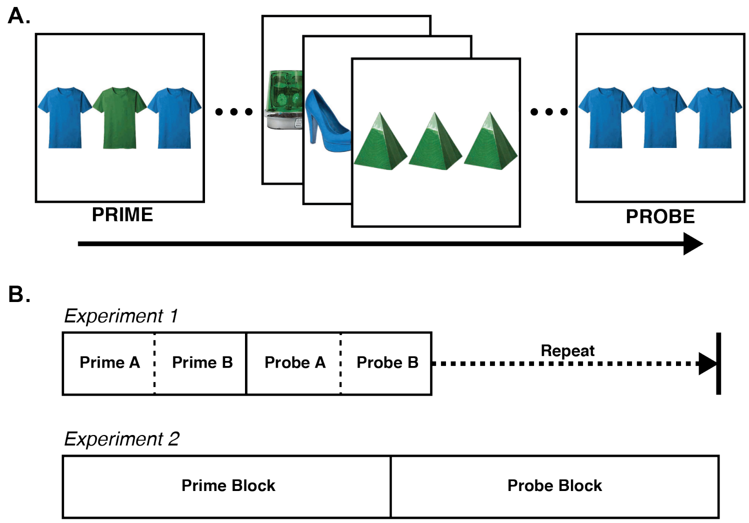


**Figure 3.1:** Illustration of the stimuli and prime/probe structure used in all experiments.

Figure 3.1A shows examples of the stimuli and basic prime/probe structure used in all experiments. Figure 3.1B shows the trial block structures from Experiments 1 and 2. In Experiment 1, every block of 16 trials was divided into four sub-blocks, each consisting of four trials (referred to as the Prime A, Prime B, Probe A, and Probe B sub-blocks). The images presented in the Prime A sub-block were then repeated in the Probe A sub-block and images presented in the Prime B sub-block, repeated in the Probe B sub-block. In Experiment 2, there were two blocks of trials, each consisting of 160 trials. The images presented in the Prime block were then repeated in the Probe block.

as well as an instruction reminder button that displayed the instructions in a new pop-up window.

Each trial began with a fixation cross presented in the center of the screen for 1,000 ms, followed by a blank inter-stimulus interval (ISI) of 250 ms. Next, the flanker stimulus appeared in the center of screen, and remained on screen until a response was made. Following a response, feedback indicating whether the response was correct or incorrect was presented above the target stimulus for 500 ms. For experiments 1B and 1C, if the participant failed to press the spacebar on a secondary task trial, a message appeared below the target stimulus reminding the participant of the secondary task instructions. A response automatically triggered the next trial.

Halfway through experiments 1A (96 trials) and 1B (108 trials), participants were instructed to take a short break, and to press the button on-screen when they were ready to continue. In experiment 1C they received this message three times, each after they had completed 108 trials.

**Results**

Participants with mean error rates greater than 20% were excluded from the analyses. For experiment 1A, this eliminated five participants, for 1B this eliminated seven participants, and for 1C this eliminated four participants. For all remaining participants, the RTs from correct trials in each condition were submitted to an outlier removal procedure (the non-recursive procedure; Van Selst & Jolicoeur, 1994) that eliminated an average of 3.58%, 3.53%, and 3.11% of the observations from experiments 1A, 1B, and 1C respectively.

***Long-term congruency sequence effects.*** The primary question of interest was whether the repetition of unique stimuli after a single presentation (trial $n-5$ to $n-11$) would produce sequential-like effects. To address this question, mean RTs from correct responses on the probe trials and error rates were submitted to a mixed analysis of variance (ANOVA) with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as within-subject factors, and experiment (1A, 1B, and 1C) as the between-subject factor.

The results of the RT analysis revealed a significant two-way interaction between prime congruency and probe congruency,$F(1,114) = 10.05$, $MSE = 1,508.82$, $p = .002$, $\hat{\eta}_p^2 = .081$, 90% CI $[0.02, 0.17]$, demonstrating a smaller congruency effect when the prime stimulus was incongruent rather than congruent. Furthermore, the three-way interaction between prime congruency, probe congruency, and experiment, was non-significant,$F(2,114) = 0.11$, $MSE = 1,508.82$, $p = .897$, $\hat{\eta}_p^2 = .002$, 90% CI $[0, 0.01]$, showing no significant difference between the size or direction of the long-term sequence effects across experiments.

The results of the error analysis revealed no significant effects of interest. The three-way interaction between experiment, prime congruency, and probe congruency was non-significant, $F(1,114) = 1.39$, $MSE = 11.17$, $p = .240$, $\hat{\eta}_p^2 = .012$, 90% CI $[0, 0.06]$, and the two-way interaction between prime congruency and probe congruency was non-significant, $F(2,114) = 0.48$, $MSE = 11.17$, $p = .621$, $\hat{\eta}_p^2 = .008$, 90% CI $[0, 0.04]$. Average error rates from experiments 1A, 1B, and 1C (probe trials only), were 4.38%, 3.29%, and 2.9% respectively.

***n-1 congruency sequence effects.*** In our experimental design, specific stimuli never repeated trial-to-trial. Another question of interest was whether this design would still produce $n-1$ sequence effects when using non-repeating stimuli. Some previous work has demonstrated that sequence effects were eliminated when contextual features alternate rather than repeat (Spapé & Hommel, 2008) whereas other studies using non-repeating stimuli have successfully produced sequential effects (Egner, 2010; King, Korb, & Egner, 2012). To address this question, mean RTs from correct responses and error rates were submitted to a mixed analysis of variance (ANOVA) with trial $n-1$ congruency (congruent vs. incongruent) and trial $n$ congruency (congruent vs. incongruent) as within-subject factors, and experiment (1A, 1B, and 1C) as the between-subject factor (see 3.2).

Two results of the RT analysis are of particular interest. First, the two-way interaction between trial $n$ congruency and experiment was significant, $F(2,114) = 3.34$, $MSE = 1,743.37$, $p = .039$, $\hat{\eta}_p^2 = .055$, 90% CI $[0, 0.13]$, suggesting the size of the congruency effect differed across experiments. Specifically, the congruency effect was smallest in experiment 1A (M = 33 ms), then experiment 1C (M = 50 ms), and largest in experiment 1B (M = 60 ms).

Second, the critical two-way interaction between trial $n-1$ congruency and trial $n$ congruency was significant, $F(1,114) = 11.00$, $MSE = 1,023.99$, $p = .001$, $\hat{\eta}_p^2 = .088$, 90% CI $[0.02, 0.18]$ showing a smaller congruency effect when trial $n-1$ was incongruent compared to congruent. However, this interaction was qualified by a significant three-way interaction between trial $n-1$ congruency, trial $n$ congruency, and experiment, $F(2,114) = 3.65$, $MSE = 1,023.99$, $p = .029$, $\hat{\eta}_p^2 = .060$, 90% CI $[0, 0.13]$.

To further probe the three-way interaction, we analyzed each of the experiments separately. The analysis of experiment 1A resulted in no significant interaction between trial $n-1$ congruency and trial $n$ congruency, $F(1,34) = 0.07$, $MSE = 1,364.48$, $p = .793$, $\hat{\eta}_p^2 = .002$, 90% CI $[0, 0.07]$, suggesting no sequence effects. However, there were significant two-way interactions between trial $n-1$ congruency and trial $n$ congruency for both experiment 1B, $F(1,31) = 8.56$, $MSE = 1,226.60$, $p = .006$, $\hat{\eta}_p^2 = .216$, 90% CI $[0.04, 0.4]$, and experiment 1C, $F(1,49) = 13.87$, $MSE = 659.55$, $p = .001$, $\hat{\eta}_p^2 = .221$, 90% CI $[0.07, 0.37]$, showing a smaller congruency effect following

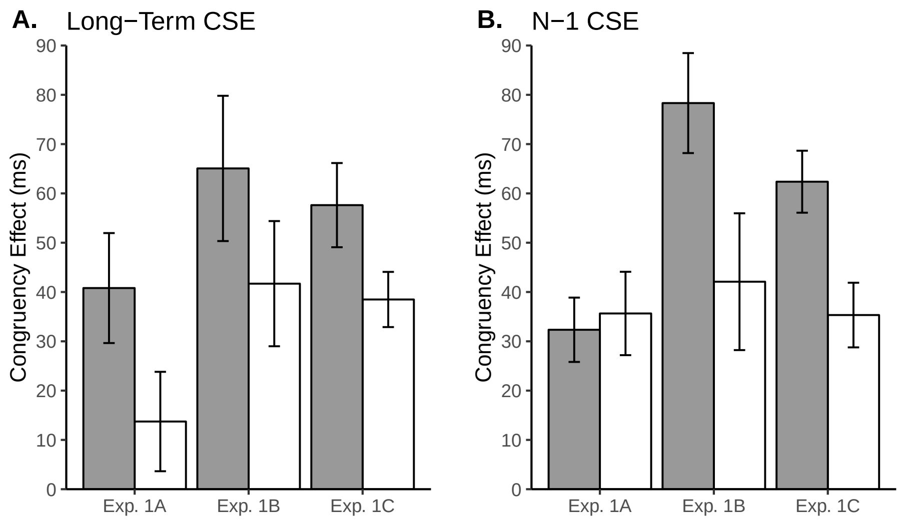


**Figure 3.2:** Results from Experiment 1

Figure 3.2A shows congruency effects in reaction times as a function of Prime Congruency (congruent and incongruent) and Experiment (A, B, and C). Figure 3.2B shows congruency effects in reaction times as a function of Trial $n - 1$ Congruency (congruent and incongruent) and Experiment (A, B, and C). Error bars represent the Standard Error of the Mean (SEM)

incongruent rather than congruent trials.

The results of the error analysis revealed no significant effects of interest. The three-way interaction between experiment, prime congruency, and probe congruency was non-significant, $F(2, 114) = 0.97$, $MSE = 7.10$, $p = .383$, $\hat{\eta}_p^2 = .017$, 90% CI $[0, 0.06]$, and the two-way interaction between trial $n - 1$ congruency and trial $n$ congruency was non-significant, $F(1, 114) = 0.91$, $MSE = 7.10$, $p = .341$, $\hat{\eta}_p^2 = .008$, 90% CI $[0, 0.06]$. Average error rates from experiments 1A, 1B, and 1C, were 3.62%, 3.18%, and 2.67% respectively.

## Discussion

Across three replications, we found that a single experience with a unique stimulus could influence performance 5 to 11 trials after the initial presentation. Specifically, we consistently found smaller congruency effects for the probe when the first prime presentation was incongruent as compared to congruent, demonstrating a long-term congruency sequence effect. This result is consistent with the instance-based memory account suggesting that contextual features (image identity) could cue the rapid adjustment of attentional priorities after only a single prior presentation.

An unlikely, but alternative interpretation is that the decaying control signal carried forward over trials from the first presentation to influence the second. $n-1$ congruency sequence effects are often interpreted as the result of control settings or conflict signals from trial $n-1$ carrying forward to influence trial n. Various studies have shown that sequence effects, given the right conditions, can persist longer than one trial, from two to four trials (e.g., Jiménez and Méndez, 2013; Mayr et al., 2003), and up to five seconds (Egner et al., 2010) after the initial presentation. On the one hand, this interpretation seems unlikely given the intervening length in our experiments was much longer than previous demonstrations. On the other hand, the rate of decay is not well understood, and certainly conflict-monitoring models are flexible in terms of the speed of decay (e.g., Botvinick et al., 2001; Braver, 2012). Additionally, there is evidence that under some conditions the rate of decay could be slowed. For example, one study demonstrated that the use of proactive strategies could prevent the sequence effect from decaying as rapidly as previously demonstrated (Duthoo et al., 2014). It is possible that the use of contextual cues combined with the frequency and regularity by which they repeated created some expectation for when contextual cues would repeat. This may have promoted the use of proactive strategies that slowed the decay rate long enough to produce our long-term sequence effect.

An additional consideration is whether the secondary task influenced performance in experiments 1B and 1C. The secondary task had participants monitor the flanking images and press the spacebar when the flanking images differed in identity to the target image. The use of contextual cues in attention tasks is often thought to develop automatically (e.g., Chun and Jiang, 1998). However, there have been demonstrations using proportion congruent designs where context-dependency fails to develop without specific task instructions to engage in specific strategies (e.g., Brosowsky and Crump, 2016; Crump et al., 2008). The nature of our secondary task could have caused participants to attend more to the identities of the images and encouraged the use of contextual cues. Regardless, the long-term congruency sequence effect was also found in experiment 1A where participants did not have the secondary task. So, although it may be possible that the secondary task contributed to the effects in experiments 1B and 1C, removing the secondary task was not sufficient for eliminating the long-term sequence effect.

Finally, experiments 1B and 1C included a secondary task to increase the amount of conflict, as measured by the congruency effect. Consistent with that manipulation, we found a smaller congruency effect in experiment 1A as compared to 1B and 1C. However, the long-term sequence effect appeared to be insensitive to the conflict manipulation as we found no significant differences in the size of the long-term sequence effect across experiments. In contrast, we only found $n-1$ congruency sequence effects in experiments 1B and 1C. These findings are consistent with prior work demonstrating the $n-1$ sequence effect despite the use of non-repeating stimuli (Egner et al., 2010; King et al., 2012), and consistent with prior work showing a sensitivity to the amount of conflict (Forster et al., 2011; Wendt et al., 2015).

## Experiment 2A and 2B

In experiment one, across three replications, we found long-term congruency sequence effects when there were 5 to 11 intervening trials between the first and second presentation of a unique stimulus. The goals of experiment two were to conceptually replicate and extend the findings from experiment one by increasing the number of intervening trials between the prime and probe pairs, increasing the variability in the frequency of stimulus repetition, and including an alternate conflict manipulation.

For both experiments 2A and 2B, the primary task was the same as experiment one which involved identifying the color of a central image (either blue or green) flanked on the left and right by the same image presented in either the same (congruent) or the alternate color (incongruent). Each image was only presented once as a prime stimulus, and once as a probe stimulus. Importantly, the experiment consisted of two blocks of 160 trials; A prime block followed by a probe block. Each block was randomized such that the distance between any given prime and probe stimulus ranged from 1 to 319 trials (160 trials, on average). To increase conflict in experiment 2B, the flanking images preceded the target image by 100 ms, a manipulation known to increase the congruency effect

**Table 3.1:** Long-Term Congruency Sequence Effects for Experiments 1-3

| | Probe | | | | Congruency Effect | Long-term CSE |
|---|---|---|---|---|---|---|
| | Con | | Inc | | $(I - C)$ | $(C_{(I-C)} - I_{(I-C)})$ |
| Prime | RT | ER | RT | ER | RT | RT |
| **Exp. 1A** | | | | | | |
| Con | 623 (23) | 5 (0.8) | 664 (24) | 5.12 (0.71) | 41 (11) | 27 (13) |
| Inc | 632 (22) | 3.69 (0.74) | 646 (22) | 3.69 (0.68) | 14 (10) | |
| **Exp. 1B** | | | | | | |
| Con | 766 (22) | 3.78 (0.71) | 831 (25) | 2.21 (0.53) | 65 (15) | 23 (16) |
| Inc | 779 (27) | 3.65 (0.69) | 821 (29) | 3.52 (0.9) | 42 (13) | |
| **Exp. 1C** | | | | | | |
| Con | 774 (26) | 2.88 (0.44) | 831 (28) | 2.21 (0.42) | 58 (9) | 19 (9) |
| Inc | 773 (26) | 3.12 (0.47) | 812 (27) | 3.38 (0.51) | 38 (6) | |
| **Exp. 2A** | | | | | | |
| Con | 566 (20) | 2.99 (0.55) | 605 (21) | 4.17 (0.68) | 39 (6) | 21 (9) |
| Inc | 575 (21) | 2.64 (0.43) | 593 (19) | 4.31 (0.55) | 18 (7) | |
| **Exp. 2B** | | | | | | |
| Con | 589 (22) | 1.97 (0.38) | 647 (21) | 4.34 (0.66) | 58 (8) | 17 (11) |
| Inc | 589 (23) | 2.43 (0.38) | 630 (19) | 3.95 (0.68) | 40 (11) | |
| **Exp. 3A** | | | | | | |
| Con | 842 (21) | 2.37 (0.5) | 880 (22) | 3.03 (0.63) | 38 (8) | 13 (14) |
| Inc | 846 (24) | 2.84 (0.55) | 871 (21) | 2.94 (0.57) | 25 (11) | |
| **Exp. 3B** | | | | | | |
| Con | 837 (22) | 2.73 (0.45) | 867 (20) | 2.5 (0.48) | 30 (7) | 0 (10) |
| Inc | 840 (22) | 1.92 (0.39) | 870 (23) | 2.15 (0.47) | 30 (7) | |

*Note*: RT = Reaction Time (ms); ER = Error Rates (%);
Con/C congruent; Inc/I incongruent; standard errors are presented in parentheses.

**Table 3.2:** $N-1$ Congruency Sequence Effects for Experiments 1-3

| | Trial *n* | | | | Congruency Effect | $N-1$ CSE |
|---|---|---|---|---|---|---|
| | Con | | Inc | | $(I-C)$ | $(C_{(I-C)} - I_{(I-C)})$ |
| Trial *n*-1 | RT | ER | RT | ER | RT | RT |
| **Exp. 1A** | | | | | | |
| Con | 626 (22) | 2.97 (0.55) | 658 (22) | 3.48 (0.48) | 32 (7) | -3 (12) |
| Inc | 635 (21) | 3.55 (0.46) | 671 (25) | 4.48 (0.71) | 36 (8) | |
| **Exp. 1B** | | | | | | |
| Con | 753 (21) | 2.4 (0.51) | 832 (25) | 3.48 (0.53) | 78 (10) | 36 (12) |
| Inc | 791 (24) | 3.58 (0.54) | 833 (25) | 3.27 (0.71) | 42 (14) | |
| **Exp. 1C** | | | | | | |
| Con | 771 (27) | 2.46 (0.37) | 834 (27) | 2.66 (0.39) | 62 (6) | 27 (7) |
| Inc | 794 (25) | 2.91 (0.4) | 829 (26) | 2.65 (0.37) | 35 (7) | |
| **Exp. 2A** | | | | | | |
| Con | 557 (17) | 2.36 (0.38) | 593 (16) | 4.36 (0.57) | 36 (6) | 8 (8) |
| Inc | 576 (18) | 3.59 (0.46) | 605 (19) | 3.8 (0.6) | 28 (6) | |
| **Exp. 2B** | | | | | | |
| Con | 568 (20) | 1.91 (0.34) | 638 (20) | 4.67 (0.61) | 70 (5) | 33 (6) |
| Inc | 606 (24) | 2.83 (0.41) | 643 (23) | 3.78 (0.58) | 37 (7) | |
| **Exp. 3A** | | | | | | |
| Con | 837 (24) | 2.18 (0.44) | 879 (21) | 2.65 (0.41) | 43 (8) | 22 (12) |
| Inc | 855 (21) | 2.83 (0.4) | 876 (22) | 2.85 (0.56) | 20 (8) | |
| **Exp. 3B** | | | | | | |
| Con | 860 (23) | 2.54 (0.38) | 889 (24) | 2.38 (0.42) | 30 (7) | 13 (10) |
| Inc | 873 (24) | 2.28 (0.36) | 889 (23) | 2.43 (0.35) | 16 (8) | |

*Note*: RT = Reaction Time (ms); ER = Error Rates (%);
Con/C congruent; Inc/I incongruent; standard errors are presented in parentheses.

(Wendt et al., 2015).

**Methods**

***Participants.*** All participants were recruited from Amazon Mechanical Turk (AMT) and compensated $2.00 for participating. The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hour. For each experiment the number of HITs (Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study and each experiment consisted of unique participants. Participants were included in the study if they completed all trials. For experiment 2A, 40 HITs were posted, and 39 participants completed all trials. For experiment 2B, 40 HITs were posted, and 40 participants completed all trials.

***Apparatus & Stimuli.*** The apparatus and stimuli were identical to those used in experiment 1. Design. Experiment 2 used a 2x2x2 mixed design with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as within-subject factors, and experiment (2A and 2B) as the between-subject factor.

Experiments 2A and 2B were both constructed using the same general method. Both experiments consisted of 320 total trials divided into two halves, a prime block and probe block. The prime block was constructed using 160 unique images randomly selected for each participant from the total 540 images (Brady et al., 2013). The images presented in the prime block were then repeated in the probe block. The trial order for each block was randomized, so the distance between any given probe (trial n) and prime stimulus paired ranged from $n - 1$ to $n - 319$. Each experiment consisted of 50% congruent/incongruent trials, an equal number of each congruency combination between prime/probe pairs (i.e., Con - Con, Con - Inc, Inc - Inc, and Inc - Con), and an equal number of response repetition and response alternation prime/probe pairs.

***Procedure.*** All participants were AMT workers who found the experiment using the AMT system. The participant recruitment procedure and tasks were approved by the Brooklyn College Institutional Review Board. Each participant read a short description of the task and gave consent by pressing a button acknowledging they had read the displayed consent form. Participants then completed a short demographic survey, and proceeded to the main task, which was displayed as a pop-up window. Participants were instructed to identify the color of the center image on each trial as quickly and accurately as possible by pressing ‘g’ if the image was green, and ‘b’ if the image was blue. Throughout the course of the experiment the upper left corner of the display indicated the number of completed and remaining trials, as well as an instruction reminder button that displayed the instructions in a new pop-up window.

For experiment 2A, each trial began with a fixation cross presented in the center of the screen for 1,000 ms, followed by a blank ISI of 250 ms. Next, the flanker stimulus appeared in the center of screen, and remained on screen until a response was made. Feedback indicating whether the answer was correct or incorrect was presented above the target stimulus following a response and remained on-screen for 500 ms which automatically triggered the next trial. For experiment 2B, each trial began with a fixation cross presented in the center of the screen for 1,000 ms, followed by a blank ISI of 250 ms. Next, the flanking images appeared for 100 ms followed by the presentation of the center image. All images remained on screen until a response was given. Feedback indicating whether the answer was correct or incorrect was presented above the target stimulus following a response and remained on-screen for 500 ms which automatically triggered the next trial. In both experiments, after every 80 trials, a message appeared on-screen that instructed participants to take a short break and to press the button when they were ready to continue.

**Results**

Participants with mean error rates greater than 20% were excluded from the analyses. For experiment 2A, this eliminated three participants and for 2B this eliminated two participants. For all remaining participants, the RTs from correct trials in each condition were submitted to an outlier removal procedure (the non-recursive procedure; Van Selst & Jolicoeur, 1994) that eliminated an average of 3.2% and 3.3% of the observations from experiments 2A and 2B, respectively.

***Long-term congruency sequence effects.*** Mean RTs from correct responses on probe trials and error rates were submitted to a mixed analysis of variance (ANOVA) with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as within-subject factors, and experiment (2A and 2B) as the between-subject factor (see Figure 3.3).

The results of the RT analysis revealed a significant two-way interaction between prime congruency and probe congruency, $F(1, 72) = 6.99$, $MSE = 980.13$, $p = .010$, $\hat{\eta}^2_p = .089$, 90% CI $[0.01, 0.2]$, demonstrating a smaller congruency effect when the prime stimulus was incongruent rather than congruent. Additionally, the three-way interaction between prime congruency, probe congruency, and experiment, was non-significant, $F(1, 72) = 0.09$, $MSE = 980.13$, $p = .768$, $\hat{\eta}^2_p = .001$, 90% CI $[0, 0.04]$, showing no difference between the size or direction of the long-term sequence effects across experiments.

The results of the error analysis revealed no significant effects of interest. The three-way interaction between experiment, prime congruency, and probe congruency was non-significant,$F(1, 72) = 1.23$, $MSE = 6.77$, $p = .272$, $\hat{\eta}^2_p = .017$, 90% CI $[0, 0.09]$, and the two-way interaction between prime congruency and probe congruency was non-significant, $F(1, 72) = 0.09$, $MSE = 6.77$, $p = .761$, $\hat{\eta}^2_p = .001$, 90% CI $[0, 0.04]$. Average error rates from experiments 2A and 2C (probe trials only), were 3.52% and 3.17% respectively.

***n-1 congruency sequence effects.*** Mean RTs from correct responses and mean error rates were submitted to a mixed analysis of variance (ANOVA) with trial $n - 1$ congruency (congruent vs. incongruent) and trial $n$ congruency (congruent vs. incongruent) as within-subject factors, and experiment (2A and 2B) as the between-subject factor (see Figure 3.3).

The RT analysis resulted in a significant two-way interaction between trial $n$ congruency and experiment, $F(1, 72) = 9.90$, $MSE = 851.61$, $p = .002$, $\hat{\eta}^2_p = .121$, 90% CI $[0.03, 0.24]$; The size of the congruency effect was significantly smaller in experiment 2A (M = 33 ms), as compared to experiment 2B (M = 54 ms).

The critical two-way interaction between trial $n - 1$ congruency and trial $n$ congruency was also significant, $F(1, 72) = 15.92$, $MSE = 481.26$, $p < .001$, $\hat{\eta}^2_p = .181$, 90% CI $[0.06, 0.31]$, demonstrating a smaller congruency effect when trial $n-1$ was incongruent rather than congruent. However, this was qualified by a three-way interaction between trial $n-1$ congruency, trial $n$ congruency, and experiment, $F(1,72) = 5.99$, $MSE = 481.26$, $p = .017$, $\hat{\eta}_p^2 = .077$, 90% CI $[0.01, 0.19]$.

A separate analysis of experiment 2A showed no significant interaction between trial $n-1$ congruency and trial $n$ congruency, $F(1,35) = 0.91$, $MSE = 614.66$, $p = .347$, $\hat{\eta}_p^2 = .025$, 90% CI $[0, 0.15]$. However, the analysis of experiment 2B showed a significant two-way interaction, $F(1,37) = 28.87$, $MSE = 355.08$, $p < .001$, $\hat{\eta}_p^2 = .438$, 90% CI $[0.23, 0.58]$, with a smaller congruency effect when trial $n-1$ was incongruent rather than congruent.

The results of the error analysis revealed a significant two-way interaction between trial $n-1$ congruency and trial $n$ congruency,$F(1,72) = 13.69$, $MSE = 4.35$, $p < .001$, $\hat{\eta}_p^2 = .160$, 90% CI $[0.05, 0.28]$, showing a larger congruency effect following a congruent (M = 2.37%), as compared to an incongruent trial (M = 0.57%). However, the three-way interaction between experiment, trial $n-1$ congruency, and trial $n$ congruency was non-significant, $F(1,72) < 0.01$, $MSE = 4.35$, $p = .978$, $\hat{\eta}_p^2 < .001$, 90% CI $[0, 1]$. Average error rates from experiments 2A and 2C, were 3.53% and 3.3% respectively.

**Discussion**

The critical result in experiment two was that congruency effects were significantly smaller on probe trials paired with an incongruent as compared to congruent prime trial. Experiment 2 therefore conceptually replicates experiment 1 and demonstrates long-term congruency sequence effects with 1 to 319 intervening trials, increased variability in the frequency of stimulus repetition, and an alternate conflict manipulation.

Additionally, the level of conflict was manipulated across experiments. Consistent with our manipulation, the congruency effect was significantly larger in experiment 2B as compared to 2A. However, this manipulation did not modulate the size or direction of the long-term congruency sequence effect. In contrast, we only found $n-1$ congruency sequence effects in experiment 2B, sug-

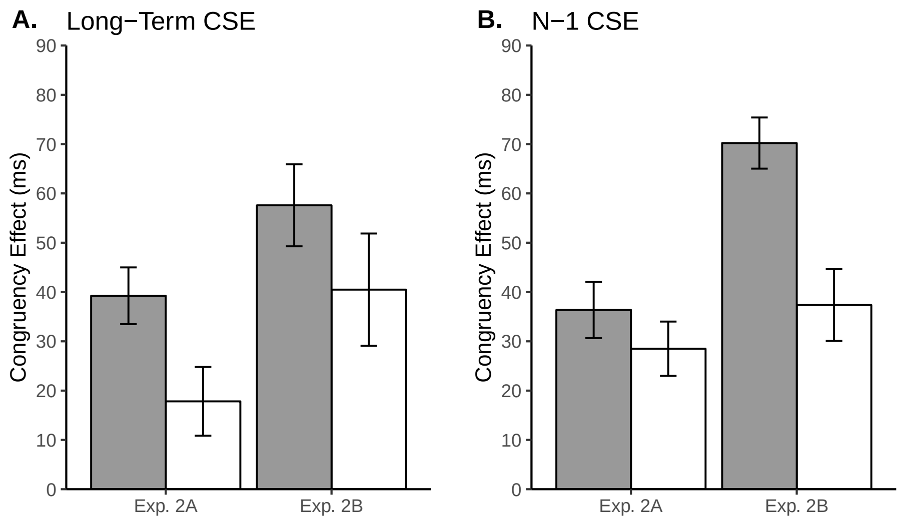


**Figure 3.3:** Results from Experiment 2

Figure 3.3A shows congruency effects in reaction times as a function of Prime Congruency (congruent and incongruent) and Experiment (A and B). Figure 3.3B shows congruency effects in reaction times as a function of Trial $n-1$ Congruency (congruent and incongruent) and Experiment (A and B). Error bars represent the Standard Error of the Mean (SEM).

gesting a sensitivity to the level of conflict, and replicating the results of experiment 1.

### Experiment 3A and 3B

Across experiments 1 and 2, we have demonstrated long-term congruency sequence effects with as many as 160 intervening trials between the first and second presentation of a unique stimulus. However, both experiments used a 2-choice flanker task resulting in some feature-overlap between the prime and probe trial. Feature integration accounts have proposed that differences in match between features presented on trial $n-1$ and trial $n$ could account for congruency sequence effects by way of event files and a memory retrieval process (Hommel, 1998; Hommel et al., 2001, 2004). This issue will be discussed in greater detail in the general discussion, however, the goal of experiment 3 was to test whether the long-term congruency effect would persist when the prime and probe trials consist entirely of non-overlapping color features.

For both experiments 3A and 3B, the primary task was the same as experiments 1 and 2 identifying the color of a central image flanked on the left and right by the same image presented in either the same (congruent) or the alternate color (incongruent). Each image was only presented once as a prime stimulus, and once as a probe stimulus. However, in contrast to experiments 1 and 2, images could appear in one of four colors (red, blue, green, or yellow). For each participant, colors were randomly assigned to two mutually exclusive color sets. Each prime/probe stimulus pair used both color sets ensuring that colors did not overlap between the prime and probe trial.

Except for the image colors, experiment 3A followed the same methods as experiment 1A such that the distance between any given prime and probe stimulus ranged from 5 to 11 trials (8 trials, on average). Similarly, experiment 3B followed the same methods as experiment 2A such that the distance between any given prime and probe stimulus ranged from 1 to 319 (160 trials, on average).

### Methods

***Participants.*** All participants were recruited from Amazon Mechanical Turk (AMT) and compensated $1.00 for participating. The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hour. For each experiment the number of HITs (Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study and each experiment consisted of unique participants. Participants were included in the study if they completed all trials. For experiment 3A, 50 HITs were posted, and 50 participants completed all trials. For experiment 3B, 50 HITs were posted, and 47 participants completed all trials.

***Apparatus & Stimuli.*** The apparatus and stimuli were identical to those used in experiments 1 and 2.

***Design.*** Experiment 3 used a 2x2 within-subjects design with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as factors.

Experiment 3A was constructed using the methods as described in experiment 1A. Therefore, experiment 3A consisted of 192 total trials with the distance between each prime and probe stimulus pair ranging from $n-5$ to $n-11$. Experiment 3B was constructed using the methods as described in experiment 2. Therefore, experiment 3B consisted of 320 total trials with the distance between each prime and probe stimulus pair ranged from $n - 1$ to $n - 319$. Each experiment consisted of 50% congruent/incongruent trials, an equal number of each congruency combination between prime/probe pairs (i.e., Con - Con, Con - Inc, Inc - Inc, and Inc - Con). Images were randomly selected for every participant from the total 540 images (Brady et al., 2013) and randomly assigned a color and condition. Each image was only presented twice during the experiment: once in a prime block and once in a probe block.

The colors of the images however, differed from experiments 1 and 2. For experiment 3, images could appear in one of four colors: blue, green, red, or yellow. For each participant, the four colors were randomly assigned to two color sets (e.g., blue/green, red/yellow), such that colors in differing sets were never presented together on a single trial (e.g., green/yellow never appeared together). Additionally, each prime/probe pair always consisted of colors from both sets to ensure that colors did not repeat from the prime to probe trial. The assignment of colors to prime/probe trials was counterbalanced for each participant. Therefore, on 50% of trials, color set 1 was assigned to the prime stimuli and color set 2 to the corresponding probe, and on the other half, color set 2 was assigned to the prime and color set 1 to the probe.

***Procedure.*** The procedure was identical to experiments 1 and 2. However, because of the use of four colors, participants were instructed to identify the color of the center image on each trial as quickly and accurately as possible by pressing ‘b’ if the image was blue, ‘g’ if the image was green, ‘r’ if the image was red, and ‘y’ if the image was yellow.

## Results

Participants with mean error rates greater than 20% were excluded from the analyses. For experiment 3A, this eliminated six participants and for 3B this eliminated four participants. For all remaining participants, the RTs from correct trials in each condition were submitted to an outlier removal procedure (the non-recursive procedure; Van Selst & Jolicoeur, 1994) that eliminated an average of 3.19% and 2.89% of the observations from experiments 3A and 3B, respectively.

## Experiment 3A: *n*-8 trials

***Long-term congruency sequence effects.*** Mean RTs from correct responses and error rates were submitted to a repeated measures analysis of variance (ANOVA) with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as factors. As a result, the two-way interaction between prime congruency and probe congruency was non-significant, $F(1, 43) = 0.84$, $MSE = 2,173.47$, $p = .365$, $\hat{\eta}_p^2 = .019$, 90% CI $[0, 0.13]$, showing no differences between the congruency effects when the prime was congruent versus incongruent.

The results of the error analysis also revealed no significant effects of interest. The two-way interaction between prime congruency and probe congruency was non-significant, $F(1, 43) = 0.36$, $MSE = 9.81$, $p = .551$, $\hat{\eta}_p^2 = .008$, 90% CI $[0, 0.1]$.

***n-1 congruency sequence effects.*** Mean RTs from correct responses and mean error rates were submitted to a repeated measures analysis of variance (ANOVA) with trial $n - 1$ congruency (congruent vs. incongruent) and trial $n$ congruency (congruent vs. incongruent) as within-subject factors. As a result, the two-way interaction between trial $n - 1$ and trial $n$ congruency was marginal, though non-significant, $F(1, 43) = 3.57$, $MSE = 1,546.60$, $p = .066$, $\hat{\eta}_p^2 = .077$, 90% CI $[0, 0.22]$, showing no differences between the congruency effects when trial $n - 1$ was congruent versus incongruent.

The results of the error analysis also revealed no significant effects of interest. The two-way interaction between trial $n - 1$ congruency and trial $n$ congruency was non-significant, $F(1, 43) = 0.46$, $MSE = 4.80$, $p = .503$, $\hat{\eta}_p^2 = .011$, 90% CI $[0, 0.11]$. Average error rates were 2.36%.

## Experiment 3B: *n*-160 trials

***Long-term congruency sequence effects.*** Mean RTs from correct responses and mean error rates from probe trials were submitted to a repeated measures analysis of variance (ANOVA) with prime congruency (congruent vs. incongruent) and probe congruency (congruent vs. incongruent) as factors (see Figure 3.4). As a result, the two-way interaction between prime congruency and probe congruency was non-significant, $F(1, 42) < 0.01$, $MSE = 973.11$, $p = .961$, $\hat{\eta}_p^2 < .001$, 90% CI $[0, 1]$, showing no differences between the congruency effects when the prime was congruent versus incongruent.

The results of the error analysis also revealed no significant effects of interest. The two-way interaction between prime congruency and probe congruency was non-significant, $F(1, 42) = 0.27$, $MSE = 8.50$, $p = .604$, $\hat{\eta}_p^2 = .006$, 90% CI $[0, 0.09]$.

***n-1 congruency sequence effects.*** Mean RTs from correct responses and mean error rates were submitted to a repeated measures analysis of variance (ANOVA) with trial $n - 1$ congruency (congruent vs. incongruent) and trial $n$ congruency (congruent vs. incongruent) as within-subject factors. As a result, the two-way interaction between trial $n - 1$ and trial $n$ congruency was non-significant,$F(1, 42) = 1.58$, $MSE = 1,178.33$, $p = .215$, $\hat{\eta}_p^2 = .036$, 90% CI $[0, 0.16]$, showing no differences between the congruency effects when trial $n - 1$ was congruent versus incongruent.

Similarly, the results of the error analysis also revealed no significant effects of interest. The two-way interaction between trial $n - 1$ congruency and trial $n$ congruency was non-significant,$F(1, 42) = 0.23$, $MSE = 4.51$, $p = .636$, $\hat{\eta}_p^2 = .005$, 90% CI $[0, 0.09]$. Average error rates were 2.58%.

**Discussion**

The critical result of experiment three was the failure to find long-term congruency sequence effects. Experiment 3, therefore failed to replicate experiments 1 and 2 when color features did not overlap between prime and probe stimuli. A positive finding would have convincingly ruled out a potential long-term feature integration account of the findings from experiments 1 and 2. However, the failure to find the effect is more ambiguous. Any theory that relies on memory-retrieval might make the prediction that decreasing the similarity between the prime and probe could diminish or eliminate the effect because the probe is no longer an adequate retrieval cue. Therefore, the finding in experiment 3 does not provide direct evidence for a long-term feature integration account though it does fail to rule out such a possibility. These issues are discussed in greater detail in the general

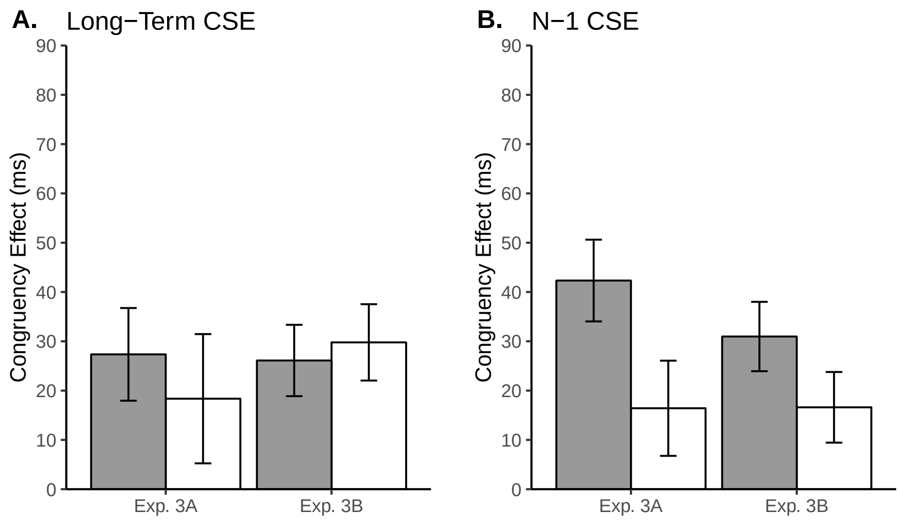


**Figure 3.4:** Results from Experiment 3

Figure 3.4A shows congruency effects in reaction times as a function of Prime Congruency (congruent and incongruent) and Experiment (A and B). Figure 3.4B shows congruency effects in reaction times as a function of Trial $n - 1$ Congruency (congruent and incongruent) and Experiment (A and B). Error bars represent the Standard Error of the Mean (SEM). In both experiments, the interaction was non-significant ($p > .05$).

discussion.

## Stimulus-Response Repetition Analyses

Previous work has demonstrated that stimulus-response repetition biases confounded with congruency manipulations may contribute to, or account entirely for, sequential modulations of congruency effects (e.g., Mayr et al., 2003). This issue will be discussed in more detail in the general discussion, however to determine the contribution of repetition biases, we compared response repeat to response change trials for both the long-term and $n - 1$ congruency sequence effects.

### Long-Term Congruency Sequence Effects.

Collapsing across experiments 1 and 2, mean RTs from correct probe trials were submitted to a repeated-measures analysis of variance (ANOVA) with response (repeat vs. change), prime congruency (congruent vs. incongruent), and probe congruency (congruent vs. incongruent) as factors (see Figure 3.5).

There was a significant main effect of response showing speeded responses for response repeat versus change trials, $F(1, 190) = 44.85$, $MSE = 5,394.67$, $p < .001$, $\hat{\eta}_p^2 = .191$, 90% CI $[0.11, 0.27]$. The two-way interaction between prime congruency and probe congruency was also significant showing a smaller congruency effect when the prime trial was incongruent as compared to congruent, $F(1, 190) = 11.32$, $MSE = 2,394.87$, $p = .001$, $\hat{\eta}_p^2 = .056$, 90% CI $[0.01, 0.12]$. Critically, three-way interaction between response, prime congruency, and probe congruency was non-significant, $F(1, 190) = 0.14$, $MSE = 3,119.34$, $p = .709$, $\hat{\eta}_p^2 = .001$, 90% CI $[0, 0.02]$.

Therefore, although we found an overall long-term response priming effect, we found no significant difference between the long-term congruency sequence effects for response repeat versus change trials.

#### *n*-1 Congruency Sequence Effects.

Collapsing across experiments 1 and 2, mean RTs from correct trials were submitted to a repeated measures analysis of variance (ANOVA) with response (repeat vs. change), trial $n-1$ congruency (congruent vs. incongruent), and trial $n$ congruency (congruent vs. incongruent) as factors (see Figure 3.5). One participant was removed prior to the analysis due to missing data in one condition.

There was a significant main effect of response showing speeded responses for response repeat versus change trials, $F(1, 189) = 25.57$, $MSE = 5,370.18$, $p < .001$, $\hat{\eta}_p^2 = .119$, 90% CI $[0.06, 0.19]$. The two-way interaction between trial $n-1$ congruency and trial $n$ congruency was significant showing a smaller congruency effect when the prime trial was incongruent as compared to congruent, $F(1, 189) = 27.67$, $MSE = 2,140.76$, $p < .001$, $\hat{\eta}_p^2 = .128$, 90% CI $[0.06, 0.2]$. However, the critical three-way interaction between response, trial $n-1$ congruency, and trial $n$ congruency was also significant, $F(1, 189) = 30.18$, $MSE = 2,940.63$, $p < .001$, $\hat{\eta}_p^2 = .138$, 90% CI $[0.07, 0.21]$.

To probe the three-way interaction, the response change and repeat trials were analyzed separately. The analysis of the change trials revealed no significant interaction between trial $n-1$ congruency and trial $n$ congruency, $F(1, 189) = 0.52$, $MSE = 2,883.72$, $p = .474$, $\hat{\eta}_p^2 = .003$, 90% CI

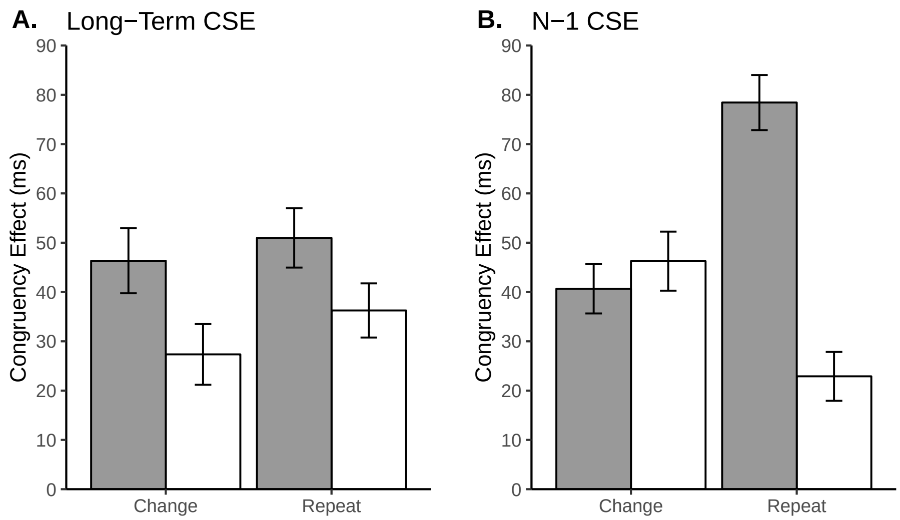


**Figure 3.5:** Results of the stimulus-response repetition analyses

Figure 3.5A shows congruency effects in reaction times collapsed across all experiments as a function of Prime Congruency (congruent and incongruent) and Response (change and repeat). Figure 3.5B shows congruency effects in reaction times collapsed across all experiments as a function of Trial $n-1$ Congruency (congruent and incongruent) and Response (change and repeat). Error bars represent the Standard Error of the Mean (SEM).

$[0, 0.03]$. However, the analysis of the repeat trials revealed a significant interaction with a smaller congruency effect when trial $n-1$ was incongruent as compared to congruent, $F(1, 189) = 66.66$, $MSE = 2,197.67, p < .001, \hat{\eta}_p^2 = .261$, 90% CI $[0.18, 0.34]$. Therefore, the $n-1$ congruency sequence effect was only found for response repeat and not for response change trials.

## Short- and Long-Term Comparison Analysis

The results thus far suggest that the short- and long-term congruency effects are the result of independent processes. However, to directly test their independence, we analyzed the congruency effects on probe trials as a function of the previous trial congruency. We collapsed across experiments 1 and 2 and submitted the mean RTs from correct probe trials to a repeated-measures analysis of variance (ANOVA) with prime congruency (congruent vs. incongruent), trial $n-1$ congruency (congruent vs. incongruent), and trial $n$ (probe) congruency (congruent vs. incongruent) as factors. Two participants were removed prior to the analysis due to missing data.

The critical three-way interaction, was non-significant, $F(1, 188) = 1.55$, $MSE = 4,320.43$, $p = .214$, $\hat{\eta}^2_p = .008$, 90% CI $[0, 0.04]$. Furthermore, the two-way interaction between trial $n-1$ and trial $n$ (probe) congruency was significant, $F(1, 188) = 8.64$, $MSE = 4,566.08$, $p = .004$, $\hat{\eta}^2_p = .044$, 90% CI $[0.01, 0.1]$, and the two-way interaction between prime and trial $n$ (probe) congruency was also significant, $F(1, 188) = 6.62$, $MSE = 3,742.42$, $p = .011$, $\hat{\eta}^2_p = .034$, 90% CI $[0, 0.09]$. Therefore, the presence of the long-term congruency sequence effect did not depend on the previous trial congruency.

## General Discussion

The current study investigated whether congruency sequence effects could be observed on a long-term basis. Across experiments 1 and 2, congruency effects were significantly smaller on probe trials when the prime was incongruent versus congruent. Long-term congruency sequence effects were demonstrated with an average of eight intervening trials (experiment one), and 160 intervening trials (experiment two); and were observed in both response-repeat and response-change trials. In experiment 3, when specific colors assigned to prime stimuli were not repeated when presented as probes, we failed to find long-term congruency sequence effects.

In addition to the long-term congruency sequence effect we also found the traditional $n - 1$ congruency sequence effect. However, the $n - 1$ sequence effect was only observed in experiments including a high-conflict manipulation. Last, the $n - 1$ congruency sequence effect was only found for response repeat trials and not for response change trials, suggesting that these effects could be explained entirely by stimulus-response repetitions.

The finding that a single presentation of a unique stimulus can influence performance on the second, much later presentation, is consistent with the instance-based memory account. Per this account, the attentional priorities adopted in the presence of unique stimulus features on the prime trial were retrieved and reinstated when those features were presented again. This work contributes to a small, but growing body of evidence suggesting that adjustments in cognitive control processes like attention can be guided by memory representations (Awh et al., 2012; Egner, 2014; Hutchinson and Turk-Browne, 2012). Memory influences on cognitive control have largely been studied in the context of negative priming (D'Angelo and Milliken, 2012; Frings et al., 2015) and visual spatial attention using visual search tasks (e.g., Chun and Jiang, 1998, 2003; Hutchinson and Turk-Browne, 2012). Our findings complement and extend this prior work demonstrating long-term memory can influence selective attention in a conflict task.

Although the instance-based memory account provides one explanation for the current results, we now discuss other accounts of the congruency sequence and proportion congruent effects and the implication of our findings. Whether modulations of congruency effects indexes adjustments in cognitive control or instead, emerges from lower level learning and memory processes, remains one major point of disagreement. We have organized our discussion around these two perspectives.

## Control perspectives

***Expectation and voluntary control accounts.*** The expectation account postulates that participants develop expectations about the congruency of upcoming trials and engage in compensatory voluntary control strategies (Gratton et al., 1992; Logan and Zbrodoff, 1979). For example, the $n-1$ congruency sequence effect could reflect deliberate controlled adjustments following participants' expectation that trial $n$ will be the same congruency as trial $n-1$ (Gratton et al., 1992). In proportion congruent designs, participants could become aware that the trials are mostly congruent or mostly incongruent and then engage in global control strategies in anticipation of the more likely item type (Logan & Zbrodoff, 1979).

It is possible that setting voluntary attentional priorities could explain the long-term congruency effect. For example, when a stimulus is presented, participants might expect the stimulus will repeat and prepare a strategy for the next time they see that stimulus. When that stimulus is presented a second time, they would then voluntarily adopt the prepared strategy. However, this proposal relies on several assumptions that make such an explanation unlikely.

First, we must assume participants had become aware that each prime stimulus would be repeated as a probe. Although awareness was not measured in the current experiments, participants in experiment 1 could easily have noticed that primes are repeated as probes every five to eleven trials. However, in experiment 2 all the prime trials were presented first, followed by all the probe trials. Therefore, during the prime block, participants have no reason to expect stimulus repetition, and during the probe block, once stimuli start repeating, it would be too late to rely on previously unprepared stimulus-specific voluntary strategies. Second, participants would have to actively maintain multiple stimulus-specific strategies simultaneously. In experiment 1, they would have to maintain 5 to 11 at any given point and in experiment 2 they would need to maintain 160. Although it is unknown how many stimulus- or context-specific voluntary strategies can be maintained simultaneously, 160 seems implausible. Finally, participants would have to rapidly adjust attentional control in a voluntary manner at the time of stimulus onset. However, voluntary control is traditionally thought of as slow and effortful (Shiffrin and Schneider, 1977). Taken together, the expectation or voluntary control account is not a viable explanation of the long-term congruency sequence effects.

***Conflict-monitoring accounts.*** According to the conflict-monitoring model, modulations of congruency effects reflect conflict-driven adjustments in cognitive control (Botvinick et al., 2001). That is, the detection of response-conflict – the simultaneous activation of competing responses – triggers an up-regulation of cognitive control which biases attentional priority towards the target dimension and away from the distractor dimension. Thus, the influence of the distractor dimension is reduced following a high-conflict, incongruent trial producing the congruency sequence effect.

The conflict-monitoring model and many of its variants (e.g., Botvinick, 2007; Braver, 2012; Dreisbach and Fischer, 2012; Egner, 2008; Hazeltine, Lightman, Schwarb, and Schumacher, 2011; Jiang et al., 2014) are incapable of explaining the long-term congruency effects for the same reasons that they have difficulties explaining item- and context-specific proportion congruent effects. Namely, adjustments in control operate on task-level representations, pre-specified by the model as relevant versus irrelevant. For example, in a Stroop task, the detection of conflict would trigger an attentional bias toward the task-relevant dimension of color, regardless of the item presented. Of course, item- and context-specific proportion congruent effects, and now the long-term congruency sequence effect, demonstrate that control can be adjusted differentially for specific items. Traditional conflict-monitoring models cannot discriminate between items while detecting conflict or adjusting control, and therefore, cannot produce item-specific effects.

The adaptation-by-binding account proposes one remedy: aggregate conflict-driven learning at the level of item features (Blais et al., 2007; Verguts and Notebaert, 2008, 2009). Like other conflict-monitoring accounts, the detection of response conflict provides a signal that task-relevant connections should be strengthened. In contrast to the previous models however, this is accomplished through a Hebbian learning rule that only strengthens connections between active representations. Representations consist of item-level features (e.g., the color red, the word 'BLUE'), and considered active if they are task-relevant and currently presented. Therefore, item-specific features can selectively become associated to the current task representation if it is frequently paired with conflict. Such computational models have been successful in simulating item-specific proportion congruent effects as well as more generalized forms of the congruency sequence effects (Blais et al., 2007; Verguts and Notebaert, 2008, 2009).

The adaptation-by-binding account may be able to produce the long-term congruency sequence effects, but doing so may stretch the model beyond its plausible limits. For example, it is not clear whether the model could produce single-trial, long-term learning, or whether repeated presentations are required to produce measurable changes in performance. Additionally, the irrelevant contextual features that defined the unique stimuli would need to be considered 'task-relevant' by the model for them to be active during learning. Each unique stimulus would also need to receive its own input layer in which case the model would require at most 160 input layers. The stimulus-set is never specified prior to the experiment so we would also have to assume that each new stimulus presented creates the new required input layers. If we accept these assumptions, it is possible that the model could produce the long-term congruency sequence effects. However, it is not clear that this model is compatible with these assumptions. Furthermore, once these additional assumptions are made, it is not clear how different this account is from the instance-based memory account.

***Conflict-monitoring with memory selection.*** We propose a new alternative conflict-monitoring model that could account for the long-term congruency sequence effect. The congruency task literature has had difficulty explaining why cognitive control adjustments have been demonstrated to be at times, specific, failing to generalize (e.g., item-specific), and at other times, nonspecific, successfully generalizing across stimuli (e.g., Abrahamse et al., 2016; Braem et al., 2014; Egner, 2014). The conflict-monitor, as specified by traditional accounts (Botvinick et al., 2001; Braver, 2012) can detect response conflict and aggregate recent or frequent conflict within a conflict-signal, but lacks the ability to select what experiences are aggregated or the ability to preserve multiple conflict signals. To incorporate the ability to select and store multiple conflict signals into the conflict monitor is problematic because it would require the monitor to know in advance, what items should be aggregated over and which signals preserved (Egner, 2008).

Instead, we suggest a memory-retrieval process could provide a mechanism by which prior experiences are selected and then aggregated over by a conflict-monitor. For example, the word 'RED' in blue ink, would cue the retrieval of any other similar experiences: any trial containing the word 'RED' or the color blue. The conflict-monitor then detects and aggregates the conflict across the retrieved item-set and adjusts control accordingly. By offloading the selection to a memory system, the conflict-monitor does not need to distinguish between items and can bias attentional priority along task-dimensions, as originally specified (Botvinick et al., 2001). However, by allowing memory to select what prior experiences are evaluated by the conflict-monitor, the model becomes extremely flexible in determining when control should be adjusted.

Such an account for example, can easily explain item- and context-specific proportion congruent effects. In a typical item-specific design (e.g., Jacoby et al., 2003), items are organized into two distinct sets associated with different proportions of congruency (e.g., the words 'RED' and 'BLUE' could be high proportion congruent, and the words 'GREEN' and 'YELLOW' could be low proportion congruent). Importantly, the individual features do not overlap between item sets (though, see Bugg and Hutchison, 2013; Bugg et al., 2011). For example, if the word 'RED' is the high proportion item set and the word 'GREEN' in the low proportion, then the word and/or color red will never be presented with the word and/or color green. Presenting 'RED' in blue will then only cue the retrieval of items from the high proportion item set. Similarly, items that appear in one context will be associated to items that have also appeared in that context by virtue of their shared contextual features (e.g., same location). Therefore, context-specific effects (Crump et al., 2006), including generalization to frequency unbiased items (Crump et al., 2017; Crump and Milliken, 2009; Weidler and Bugg, 2016) would also be predicted by such an account. Similarly, memory retrieval could contribute to traditional congruency sequence effects if we accept that that more recent memories are more easily retrievable than distant memories (e.g., Egner, 2014).

There are however, some potential remaining issues. For example, memory-based theories beg questions about the active features and/or dimensions controlling memory retrieval. Prior work in the item-specific proportion congruent literature has suggested that single features, rather than conjunctions of features, drive proportion congruent effects (e.g., Bugg and Hutchison, 2013; Jacoby et al., 2003). Similarly, context-specific transfer effects suggest that single, context-features can also drive memory retrieval (e.g., Crump and Milliken, 2009; Crump et al., 2017; Weidler and Bugg, 2016). In the current study, we found long-term congruency sequence effects when stimuli shared contextual features. We only found this effect however, when stimuli appeared in the same color set (experiments 1 and 2). When the prime and probe appeared in different colors (experiment 3) we failed to find evidence for the long-term congruency effect. These findings suggests that the conjunction of features may have served as a retrieval cue in the current study. A task for future work is to clarify the conditions that enable a feature or conjunction of features to drive retrieval.

**Non-control perspectives**

Although the control perspective remains popular, several accounts have challenged the underlying premise that modulations of the congruency effect index cognitive control adjustments. Instead, some have argued that learning and memory processes could produce the same effects without the need for notions of conflict-driven control (Mayr et al., 2003; Schmidt, 2013; Schmidt and Besner, 2008). For example, many congruency task designs contain item- or feature-repetition biases that are confounded with congruency manipulations. These confounds have produced alternative explanations of congruency phenomena, two of which are of importance to the current study.

***Contingency learning.*** First, the contingency learning account suggests that the frequency of item presentation can produce predictive relationships between item features and responses. Responses are thought to be speeded for stimuli that contain features that are highly predictive of a response regardless of congruency. For example, if the word 'BLUE' is most often presented in red ink, then the word 'BLUE' becomes predictive of the red response, and responses would be quicker relative to non-predictive items. In many proportion congruent designs, the frequency of item presentation is confounded with the proportion congruent manipulations. Therefore, the contingency learning account has been sufficient for explaining many proportion congruent effects, although still unable to explain transfer to frequency unbiased items (Crump et al., 2017; Weidler and Bugg, 2016). In the current study, however, we used a two-choice flanker task and all potential contingencies were held constant. That is, there were no predictive relationships between stimulus features and responses that could explain our results.

***Stimulus-specific repetition priming and feature integration.*** The stimulus-specific repetition priming account was proposed to explain congruency sequence effects. Mayr, Awh, and Laurey (2003) noted that the frequency of complete stimulus-response repetitions in trial-to-trial transitions are unbalanced in two-choice congruency tasks. Specifically, some congruent-to-congruent and incongruent-to-incongruent transitions contain complete stimulus-response repetitions which could speed responses selectively for those conditions (Hommel, 1998; Pashler and Baylis, 1991). Consistent with this proposition, Mayr et al. found that the congruency sequence effect disappeared when response repetition trials were either removed from the analysis, or prevented from occurring in the trial sequence. Though, many studies have now demonstrated congruency sequence effects while controlling for stimulus-response repetitions suggesting stimulus-response repetitions cannot entirely account for sequential effects (Akçay and Hazeltine, 2008; Kerns et al., 2004; Kunde and Wühr, 2006; Ullsperger, Bylsma, and Botvinick, 2005; Weissman, Jiang, and Egner, 2014).

To determine whether stimulus-response repetitions played a role in producing the current result we compared response repeat to response change trials. We found an overall long-term response repetition effect, in that performance was facilitated when responses repeated from prime to probe trials. However, the size of the long-term congruency sequence effect did not differ between response repeat and change trials suggesting that the current result could not be explained by stimulus-response repetitions.

The feature integration account makes a similar proposal. According to this account, stimuli and responses that co-occur in time are bound together in a common episodic memory representation called an event file (Hommel, 1998; Hommel et al., 2001, 2004); a more general form of the ‘object file’ proposed by Kahneman, Treisman, and Gibbs (1992). The subsequent re-occurrence of any features automatically retrieves the entire event file which could either help or hinder performance depending on the match between the currently presented features and the features contained in the event file. Across two consecutive trials, features are either completely matched, partially matched, or completely mismatched.

Critically, an effortful ‘unbinding’ process is necessary whenever features are partially matched. That is, feature representations must be unbound from the associated event file so that they can be re-used in the creation of a new event file. Therefore, performance would be predicted to be slowed on partial match trials relative to complete match or complete mismatch trials. In many congruency task designs, feature overlap is confounded with congruency sequences and as such, feature integration can explain trial-to-trial effects in many cases (for reviews, see Egner, 2007, 2014). Similarly, experiments 1 and 2, we utilized a two-choice flanker task that contains these same feature overlap confounds.

Event files are typically referred to as ‘transient’ or ‘temporary’ memory structures (Hommel, 1998; Hommel et al., 2001, 2004), and are generally invoked to explain short-term, trial-to-trial effects (Egner, 2007, 2014). However, event files are based on instance-based memory theories (Hintzman, 1986; Logan, 1988b, 1990), and typically, the timescale is not explicitly defined. If we assume that event files are stable episodic memory structures, then feature integration, like the other memory-retrieval accounts proposed above, could also explain long-term congruency sequence effects. The evidence for long-term feature integration across our experiments however, is mixed and largely inconclusive.

On the one hand, across experiments one and two we found long-term congruency sequence effects for response-change trials. This result is generally inconsistent with feature integration theories. One possibility, as others have suggested, is that event files may not be limited to stimulus-response associations. That is, other aspects of an experience like perceived conflict and control processes may also be encoded in the event file (Bugg and Hutchison, 2013; Spapé and Hommel, 2008). We could speculate that the added contextual features could have provided additional support for event file retrieval, even in the absence of response repetitions (for a similar proposal, see Spapé & Hommel, 2008), or perhaps response outcomes are forgotten more rapidly than degree of conflict. On the other hand, in experiment three we failed to find long-term congruency sequence effects when colors did not repeat from prime to probe trials. This result is consistent with feature integration theory. Although our speculation about why we found long-term effects in response-change trials could have also applied here, so these two results are at odds. Furthermore, any memory-based explanation might predict that altering the similarity between the prime and probe trials would influence the long-term effects. Therefore, the failure to find long-term effects when colors do not repeat, does not allow us to discriminate between any of the memory-based theories we have proposed. Finally, to the extent that you allow event files to be permanent memory representations and allow them to encode many aspects of our experience like stimulus and context features, responses, perceived conflict, and control processing, it is not clear how different feature integration theories are from instance-based memory theories.

***Perceptual learning and attentional control.*** Finally, we propose an alternative non-control account. Perceptual learning refers to experience-dependent changes in perception and is thought to reflect perceptual or neural plasticity in visual representations (Goldstone, 1998; Lu, Hua, Huang, Zhou, and Dosher, 2011; Roelfsema, Ooyen, and Watanabe, 2010; Sasaki, Nanez, and Watanabe, 2010). Perceptual learning has been demonstrated across a wide variety of perceptual tasks including the discrimination and detection of stimulus orientation (Dosher and Lu, 1998; Shiu and Pashler, 1992; Vogels and Orban, 1985), motion direction (Ball and Sekuler, 1987; Ball, Sekuler, and Machamer, 1983) , and object recognition (Furmanski and Engel, 2000), to name a few (for a review, see Watanabe and Sasaki, 2015). Importantly, across tasks, selective attention has been shown to influence perceptual learning, such that learning is enhanced for task-relevant, or attended features as compared to irrelevant, unattended features (Ahissar and Hochstein, 1993; Gutnisky, Hansen, Iliescu, and Dragoi, 2009; Shiu and Pashler, 1992; Szpiro and Carrasco, 2015).

One possible explanation of the long-term congruency sequence effect is that selective attention influences perceptual learning on the first presentation, which in-turn, influences how the stimulus is attended on the second presentation. For example, when presented with an incongruent stimulus, attention is shifted towards the target and away from the flankers facilitating perceptual learning of the target features relative to the flanker features. On the second presentation, the altered visual representation and enhanced perceptual processing of the target could cue attention towards the target, facilitating performance if the second presentation is incongruent.

There is some evidence that increased selective attention demands from incongruent stimuli may enhance target representations on a long-term basis. As noted earlier, in perceptual learning tasks, learning is enhanced for attended versus unattended features (Ahissar and Hochstein, 1993; Gutnisky et al., 2009; Shiu and Pashler, 1992; Szpiro and Carrasco, 2015). However, recognition memory has also been shown to be improved for items previously presented with incongruent versus congruent distractors. Here, the increased need for cognitive control is thought to facilitate target encoding at the time of study improving later memory recognition (Krebs, Boehler, De Belder, and Egner, 2013; Rosner et al., 2015; Rosner and Milliken, 2015).

Perceptual learning could provide an important mechanism for informing how attention changes through experience and learning. Perceptual learning has been shown to be highly stimulus-specific and produces long-lasting effects (Watanabe & Sasaki, 2015). Though in contrast to the immediate effects in our study, measuring changes in performance on perception tasks often requires extensive training (Dosher and Lu, 1999). Therefore, changes in perception alone could not account for the current results. Instead, we are suggesting that small changes in perceptual representations could help guide attention, causing more immediate and measurable effects in attention tasks.

**Short- and long-term congruency sequence effects: Single or multiple processes?**

In the current experiments, we found both short- (n-1) and long-term congruency sequence effects. All current accounts of the short-term congruency sequence effects posit rapidly decaying representations and are generally incapable of accounting for the long-term congruency sequence effects (Egner, 2007). The alternative accounts we have proposed above however, could accommodate both long- and short-term effects. For example, if memory retrieval mediates shifts in attentional control then we might expect that similarity in temporal context could cue retrieval (e.g., Egner, 2014). One possibility is that both short- and long-term effects are produced via a single, memory-driven process. Alternatively, we might speculate the contribution from two independent processes: A memory-driven process and a short-term, conflict-driven process.

On the one hand, there were some clear differences between the short- and long-term effects found in the current study. First, the long-term effects were insensitive to increased response conflict, whereas the short-term effects were only present in the high conflict experiments. This might be expected, if we assume that the short-term effect reflects changes in the conflict-signal which dissipates over time (e.g., Botvinick, et al., 2001). Second, the long-term effects were present for both response change and response repeat trials while the short-term effects were only present for response repeat trials. As such, the short-term effect could be explained entirely by stimulus-response repetitions (e.g., Mayr et al., 2003), while the long-term effect cannot. Third, and most importantly, we found no interaction between the short- and long-term effects. Taken together, these differences suggest that the two phenomena measured in the current study do not reflect the same underlying process.

On the other hand, several other studies have reported $n-1$ congruency sequence effects while controlling for repetition biases (e.g., Kunde and Wühr, 2006; Weissman et al., 2014), and others have found $n-1$ congruency sequence effects to be insensitive to changes in the degree of conflict (e.g., Weissman and Carp, 2013). One possible resolution to these inconsistencies, is that a memory-driven process can, under the right circumstances, contribute to short-term congruency effects. That is, if the stimuli presented on trial $n$ provides an effect retrieval cue for trial $n-1$, then we might expect that a memory-driven process could influence performance on trial n. Of course, if trial $n$ is a poor retrieval cue for trial $n-1$, then we might expect no influence from the memory-retrieval process, leaving only the short-term, conflict-driven effect. In the current study, we alternated the context trial-to-trial such that the same contextual cue never repeated from trial $n-1$ to trial n. Therefore, it could be the case that trial n, while an effective retrieval cue for the prime trial (e.g., $n-8$, $n-160$), was a poor retrieval cue for trial n-1. As such, the short-term effects observed in the current study reflect only the influence of a short-term, conflict-driven process, which was sensitive to the degree of conflict, and response repetition.

Consistent with this interpretation, Spapé and Hommel (2008) found that the $n-1$ congruency sequence effect was eliminated on trials that alternated contextual cues, but preserved when contextual cues repeated. The authors suggested that the alternation of contextual cues selectively disrupted episodic retrieval on those trials. However, others have found $n-1$ congruency sequence effects with non-repeating contextual cues (e.g., Egner et al., 2010; King et al., 2012). One possibility is that the combination of making a single prior experience (the prime trial) distinctly similar to trial $n$ and the non-repeating contextual cues, could have disrupted memory retrieval of the $n-1$ trial. This would also suggest that memory retrieval in this context, is a competitive process, whereby only the most similar experiences are retrieved. What constitutes an effective versus ineffective retrieval cue however, remains unclear. Furthermore, whether only a single, most-similar previous experience is retrieved, or if a collection of similar experiences is aggregated over and used to guide attention remains an open question.

## Broader implications

A global aim of this research program is to determine how memory for specific prior experiences guides performance in the present moment. Previous work has focused on evidence that contextual cues can rapidly modify cognitive control settings. The instance-based memory account of contextual control (Crump, 2016) is the general hypothesis that memory not only preserves a record of the details of specific experiences (Hintzman, 1986; Jacoby and Brooks, 1984; Logan, 1988b), but also preserves a record of the control procedures involved in processing those experiences (Kolers and Roediger, 1984). Our aim here was to supply evidence showing that attentional control in the present moment can be modified on a long-term basis by memories of specific prior processing experiences. Beyond the implications of this finding for theories of cognitive control from the congruency literature, we are optimistic the idea behind our results will spur more work into the memorial basis of cognitive control. In everyday life, we expect that memory for prior cognitive control operations routinely optimizes performance in familiar environments. In these situations, people gain the benefit of applying memory-based procedures for regulating information without the normal cost of effortful deliberation. We also expect that problems in regulating the flow of information can result from the inappropriate use of, or failure to rely on memory. For example, deliberate control may need to override memory based control when situations act as strong cues for prior memory procedures that may not be appropriate for the present moment. Or, when memory fails to encode or retrieve cognitive control procedures, people may be forced to rely on taxing voluntary control processes to supply the control they normally receive from memory for free. The present results show that some aspects of the attentional control procedures used during a fleeting encounter with a unique stimulus in a flanker task have long-term influences on responding to that stimulus in the future. Everyday life presents many more fleeting and meaningful experiences, and the capacity of memory to preserve and reinstate past control procedures to regulate cognition, behavior, and performance points to a healthy avenue for future work.

# CHAPTER 4

# CONTEXTUAL RECRUITMENT OF SELECTIVE ATTENTION CAN BE UPDATED VIA CHANGES IN TASK-RELEVANCE

## Preface

This chapter is reproduced from a manuscript currently under review (for consistency, I have adjusted the format of the manuscript for the thesis). The full reference to the article:

**Brosowsky, N.P.**, & Crump, M.J.C. (2019, January 15). Contextual recruitment of selective attention can be updated via changes in task-relevance. https://doi.org/10.31234/osf.io/43tj7

Chapter 4 presents an experiment that examines the role of task-relevance in producing and updating context-specific control. Context-specific attentional phenomena demonstrate that prior experiences and context cues can automatically trigger adjustments in attentional control independent of awareness and intentions. Although this finding seems plausible in a laboratory task where there is only a single context cue (e.g., location), it is unclear how such a process would operate in more complex, real-world environments that contain a multitude of contextual features. That is, what determines which context features in my environment are used to guide attention? In this experiment, we asked whether task-relevant context cues will trigger adjustments to attention in the presence of competing irrelevant context cues. And whether changes in the relative task-relevance will cause changes contextual control.

We adopted a paradigm similar to the context-specific proportion congruent (CSPC) transfer design. However, unlike previous CSPC designs where contexts were defined by a single discriminating feature (e.g., upper vs. lower screen locations), contexts were defined by two feature dimensions (see Figure 4.1A) and critically, the frequency unbiased context (50% congruent) always shared a feature with each of the frequency-biased contexts (0% and 100% congruent). To manipulate which overlapping feature was task-relevant, we used a secondary counting task. Halfway through the experiment participants received new instructions, changing which feature was task-relevant. Across three different stimulus sets, we found that changing the task-relevance caused predictable changes in the congruency effects. This result implicates an important role for task-relevance in producing and manipulating context-dependency in complex environments.

**Abstract**

Evidence across a wide variety of attention paradigms shows that environmental cues can trigger adjustments to ongoing priorities for attending to relevant and irrelevant information. This context-specific control over attention suggests that cognitive control can be both automatic and flexible. For instance, in selective attention tasks, congruency effects are larger for items that appear in a context associated with infrequent conflict than in a context associated with frequent conflict. Since the to-be-presented context cannot be predicted or prepared for in advance, attention is assumed to be rapidly updated on-the-fly, triggered by the currently presented context. Context-specific control exemplifies how learning and memory processes can influence attention to enable cognitive flexibility. However, what determines the use of previously learned associations still remains unclear. In the current study, we examined whether task-relevance would influence the learning and use of context cues in a flanker task. Using a secondary counting task, context dimensions associated with differing levels of conflict were made task-relevant or -irrelevant across the experiment. In short, we found that making new contextual information task-relevant caused participants to ignore a previously learned context-attention association and adopt a new context-specific control strategy; all without changing the experimental stimuli. This result suggests that task-relevance is a key determinant of context-specific control.

## Introduction

Selective attention is commonly investigated using interference paradigms such as the Stroop (1935) and flanker (Eriksen and Eriksen, 1974) tasks, where participants identify a target while ignoring a response-congruent or -incongruent distractor. Performance is typically better on congruent versus incongruent trials and the difference—the congruency effect—taken as an index of attentional priorities. Large congruency effects are thought to reflect ineffective filtering of the distracting stimuli whereas small congruency effects are thought to reflect effective filtering. By probing factors that systematically alter congruency effects, we can then make inferences about processes that control attentional filtering. For example, manipulating the frequency of conflict via the proportion of congruent versus incongruent trials has shown to influence the size of the congruency effect. Typically, a high proportion congruent experiment produces large congruency effects, whereas a low proportion congruent experiment produces small congruency effects (Logan and Zbrodoff, 1979; Lowe and Mitterer, 1982; West and Baylis, 1998). This result is usually explained as strategic control, where participants increase attentional control under high-conflict demands and relax attentional control under low-conflict demands (Logan, 1980; Logan and Zbrodoff, 1979; Logan et al., 1984; Lowe and Mitterer, 1982). Recent work however, has demonstrated that attentional control is not only adjusted by top-down regulation, but can also be triggered automatically by environmental cues (Brosowsky and Crump, 2018; Bugg and Crump, 2012; Egner, 2014; Fischer and Dreisbach, 2015; King et al., 2012; Mayr and Bryck, 2007).

For example, Crump, Gong, and Milliken (Crump et al., 2006; see also, Corballis and Gratton, 2003) presented Stroop stimuli in one of two randomly chosen locations and manipulated the frequency of conflict associated with each location. One location was associated with a high frequency of conflict (25% congruent trials) and the other with a low frequency of conflict (75% congruent trials). Overall, the proportion of congruent trials was 50% and randomized such that the upcoming location could not be predicted. Even so, congruency effects were shown to be smaller for trials where the stimulus appeared in the high conflict location as compared to the low conflict location.

This effect, known now as the context-specific proportion congruent effect (CSPC), has now been replicated in a number of different selective attention paradigms (e.g., Alards-Tomalin, Brosowsky, and Mondor, 2017; Blais et al., 2015; Bugg, 2014; Crump, 2016; Crump et al., 2018; Fischer et al., 2014; Hübner and Mishra, 2016).

Critical evidence however, that CSPC effects reflect context-specific control rather than other non-control learning processes (e.g., Schmidt and Besner, 2008), comes from work showing that CSPC effects can transfer to frequency unbiased items (Brosowsky and Crump, 2016; Crump and Milliken, 2009; Weidler and Bugg, 2016; Weidler et al., 2018; though, see Hutcheon and Spieler, 2017). Crump and Milliken (2009), for example, divided Stroop items into two mutually exclusive sets (e.g., RED/GREEN and BLUE/YELLOW). One set was defined as the frequency biased set, and presented with 75% congruency in one location, and 25% congruency in the other. The second set however, was presented with 50% congruency in both locations. Nevertheless, they found smaller congruency effects for unbiased items presented in the high conflict location as compared to the low conflict location.

One explanation for CSPC effects is that the repeated application of attentional priorities in a particular context creates an associative link in episodic memory between the attentional control procedures and the contextual information (Abrahamse et al., 2016; Brosowsky and Crump, 2018; Crump, 2016; Egner, 2014). Once the associative link is established, processing the context is assumed to trigger the retrieval of previous experiences, automatically reinstating associated attentional priorities. As a result, automatic memory-based retrieval allows attentional priorities to be rapidly adjusted in a context-appropriate fashion. However, the obligatory nature of memory-based attentional control remains unclear, in particular in situations when multiple contextual features could be used as cues for retrieval.

For example, real-world environments offer a plethora of context cues. Consider writing an article in a coffee shop. The coffee shop is a complex environment with numerous context cues (e.g., pictures on the wall, chairs, coffee, laptop), and associated attentional priorities. Although a laptop might cue attentional processing helpful for writing, other features of the coffee shop might cue attentional processing related to other experiences like socializing and people-watching. Given that all of these cues are available to set attentional priorities, what determines which associative relationships will be used to guide attention? On the one hand, context-based retrieval of attentional priorities might be completely obligatory and a writer's attention would be at the mercy of the environment. On the other hand, there is some evidence that contextual cuing of attention is not entirely obligatory and can be constrained by task goals, allowing the laptop to take priority as a contextual cue over other contextual cues that are irrelevant to the task at hand.

Prior work shows that task-relevance is necessary for establishing and using context-attention associations. First, the most commonly used context cue is location (Brosowsky and Crump, 2016; Corballis and Gratton, 2003; Crump, 2016; Crump et al., 2006, 2008; Weidler and Bugg, 2016; Weidler et al., 2018). Although in some sense, the location is irrelevant for identifying the color of a word, it seems disingenuous to consider it completely task-irrelevant. In order to identify the target, the participant must first identify and orient to its location on the screen. In this is sense, it is relevant to their ability to complete the task and furthermore, location information may receive priority during encoding (Logan, 1998; Mayr, 1996). Similar arguments could be made about other context features that are inherent to processing the target stimulus, like font-type (Bugg et al., 2008) and color (Vietze and Wendt, 2009).

Second, we are aware of only one study that has looked at task-relevance within the CSPC literature. Crump, Vaquero, & Milliken (2008) used a Stroop prime-probe task where the distractor word is presented first followed by a color patch probe. Critically, they used the shape of the color patch probe, either a square or a circle, as the contextual dimension; one associated with a high frequency of conflict, the other with a low frequency. In one experiment participants were made aware of the shape-proportion contingencies but given no additional instructions. In the second, participants were given a secondary task to count the number of trials that contained a square. The first experiment failed to find evidence for context-specificity, whereas the second did. Although not testing task-relevance directly, Cañadas et al. (2013)] found similar effects. In this study they used images of male and female faces as context cues in a flanker task. When given instructions to think of the faces as members of the gender categories they found CSPC effects, but when given instructions to think of the faces as individuals they did not. These results suggest that making a context dimension relevant to the on-going task is important for producing CSPC effects.

## The Current Study

Prior work suggests that the task-relevance of contextual information is important for establishing new associations between context cues and attentional priorities. Simply put, CSPC effects emerge when context cues are somehow made relevant to the task, but do not when made irrelevant (Cañadas et al., 2013; Crump et al., 2006, 2008). However, it still remains unclear how task-relevance may or may not inform the use of previously learned associations. In the current study, we examined whether manipulating task-relevance would cause participants to ignore a previously learned association and adopt a new context-specific control strategy.

A flanker task was used to measure attentional control in an adapted CSPC design (Crump et al., 2017; Crump and Milliken, 2009). Similar to previous studies, flanker stimuli were presented in contexts associated with differing levels of conflict (0%, 50%, or 100% congruent). However, unlike previous CSPC designs where contexts were defined by a single discriminating feature (e.g., upper vs. lower screen locations), contexts were defined by two feature dimensions (see Figure 4.1A). In one condition, for example, contexts were defined by object identity (hat or chair) and color (blue or green). Only three out of the four possible feature combinations were presented, each associated with a different proportion of congruent trials (0%, 50%, or 100% congruent). Critically, the frequency unbiased context (50% congruent) always shared a feature with each of the frequency-biased contexts (0% and 100% congruent). The frequency biased contexts (0% and 100% congruent) however, did not share any features (see Figure 4.1C for an example).

A secondary counting task was used to manipulate the task-relevance of the context dimensions (Crump and Milliken, 2009) which critically, switched halfway through the experiment. Participants were instructed to keep a running count of one of the overlapping features (e.g., “count whenever a hat is presented”) which was associated with either a high or low frequency of conflict.

Halfway through the experiment they received new instructions to count the other overlapping feature (e.g., “count whenever a green item is presented”). Critically, the set of context images remained the same throughout the experiment (see Figure 4.1C for an example) and the task-relevance of the context dimensions was the only aspect of the experiment that changed from the first to second phases.

The critical measure of interest is the congruency effect produced in the frequency unbiased context. If, on the one hand, task-relevance has no impact on the use of context-attention associations we would expect no differences between the congruency effects when the high-conflict context dimension is made task-relevant or the low-conflict context dimension is made task-relevant. This could occur because all three contexts are treated as individual contexts throughout the whole experiment (e.g., Cañadas et al., 2013). Or it could occur because associations formed in the first block interfere with learning new associations in the second (e.g., Brosowsky and Crump, 2016). On the other hand, task-relevance might not only dictate how associations are formed, but also whether learned associations are used. In this case, we would expect smaller congruency effects when the high-conflict context dimension is made task-relevant as compared to when the low-conflict context dimension is made task-relevant. This would demonstrate that participants were able to ignore a previously learned association and adopt a new context-specific control strategy.

A secondary goal was to conceptually replicate previous findings and test the generalizability of any task-relevance effects. Therefore, we included three conditions that differed only in the kinds of stimuli used to create context cues. In one condition we used face images where context dimensions were defined by social categories, similar to Cañadas et al. (2013). However, they found that social categorization occurred spontaneously and it was unclear what impact that would have on the current results. Therefore, we included another condition that used images of simple objects where context dimensions were defined by object features. Finally, Cañadas et al. (2013) also found that context effects generalized to novel face images. In our third condition we used non-repeating face exemplars to examine whether any task-relevance effects would also generalize in our design or if learning would be image-specific.

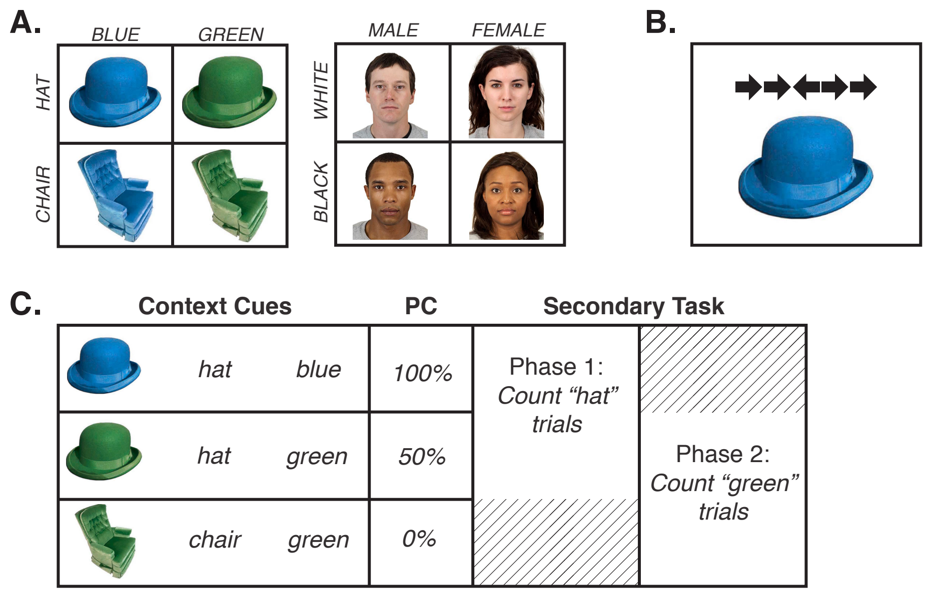


**Figure 4.1:** Illustration of the stimuli and trial construction.

Illustration of the stimuli and trial construction. Figure 4.1A shows the feature dimensions for each of the conditions. Figure 4.1B shows an example trial stimulus containing both the context and flanker images. Figure 4.1C shows an example of how feature dimensions could have been assigned to each proportion congruency (PC) condition, and an example of the secondary task assignments. The secondary task order, as well as the feature and congruency assignments were all randomized for each participant.

## Methods

### Participants

All participants were recruited from Amazon Mechanical Turk (AMT) and compensated $2.00 for participating. The amount compensated was calculated by estimating the maximum amount of time required to complete each experiment and multiplying by $6.00 per hour. For each experiment the number of HITs (Human intelligence tasks, an Amazon term for a work-unit) refers to the number of participants who initiated the study. Participants were included in the study if they completed all trials. We posted 150 HITs and 144 participants completed all trials.

### Apparatus & Stimuli

The experiments were programmed using JavaScript, CSS and HTML. The program allowed participants to complete task only if they were running Safari, Google Chrome, or Firefox web browsers. Flanker stimuli consisted of images of five arrows pointed left or right presented at 250 x 50 pixels (each arrow was 50 x 50 pixels). Context stimuli were constructed using images selected from Brady et al. (2013) color-rotated to blue and green, and face images from the Chicago Face Database (Ma, Correll, and Wittenbrink, 2015), supplemented with the NimStim Set of Facial Expressions (Tottenham et al., 2009). The object images were displayed at 250 x 250 pixels, while the face images were displayed at 250 x 313 pixels. The experiment ran as a pop-up window that filled the entire screen. The background was white, and stimuli were presented in the center of the screen.

### Design

Experiment 1 used a 2x2x3 mixed design with task-relevant context (0% and 100%) and unbiased-item congruency (congruent and incongruent) as within-subject factors, and context-type (object, social, and social/non-repeating) as the between-subjects factor. All three conditions were constructed using the same general method. The experiment was divided into two phases. Each phase consisted of 144 flanker trials (48 trials per context), and 13 count response trials for a total of 314 trials. On the count response trials, participants indicated how many trials they had counted until that point. The count response trials occurred once for every 12 flanker trials and was randomly inserted between trial 6 and 12 of each 12-trial block. Each phase ended with one additional count response trial.

On every flanker trial, participants were presented with flanker stimuli paired with one of three contexts. Each context was associated with a different proportion congruency such that two cues were associated with a biased frequency (0% and 100% proportion congruency), while one was associated with an unbiased frequency (50% proportion congruency). The feature dimensions and corresponding context images assigned to each of the biased and unbiased item sets were randomly determined for each participant. However, context images used for the frequency biased trials never shared features, while the frequency unbiased context image always shared a feature with each of the frequency biased context images (see Figure 4.1). Additionally, the feature assignments remained the same throughout phases 1 and 2, and critically, the only change to the task was which feature the participant was instructed to count (see Figure 4.1C for an example).

All critical aspects of the task were randomized between participants. This includes the three chosen context images, the features assigned to proportion levels, the features assigned to each counting condition, the secondary task order, and the order of trials.

**Procedure**

All participants were AMT workers who found the experiment using the AMT system. The participant recruitment procedure and tasks were approved by the Brooklyn College Institutional Review Board. Each participant read a short description of the task and gave consent by pressing a button acknowledging they had read the displayed consent form. Participants then completed a short demographic survey, and proceeded to the main task, which was displayed as a pop-up window. Participants were instructed to identify the direction of the center arrow on each trial as quickly and accurately as possible by pressing ‘z’ if the arrow pointed left, and ‘m’ if the arrow pointed right. Additionally, they were instructed to silently keep count of the number of trials that contained a feature. In the object context condition, they were asked to count trials that contained a certain color (blue or green) or object-identity (hat or chair) and in the social context conditions they were asked to count the number of trials that contained appeared a certain gender (male or female) or race (black or white). Periodically throughout the experiment, participants were asked to report how many trials they had counted until that point and to restart their count from 0. Halfway through the experiment participants received new instructions about which feature to count (see Figure 4.1C).

Each trial began with a blank inter-stimulus interval (ISI) of 400 ms, followed by a fixation cross presented in the center of the screen for 200 ms, then a second blank ISI of 400 ms. Next, the flanker and context stimuli appeared in the center of screen (the flanker above the context image;

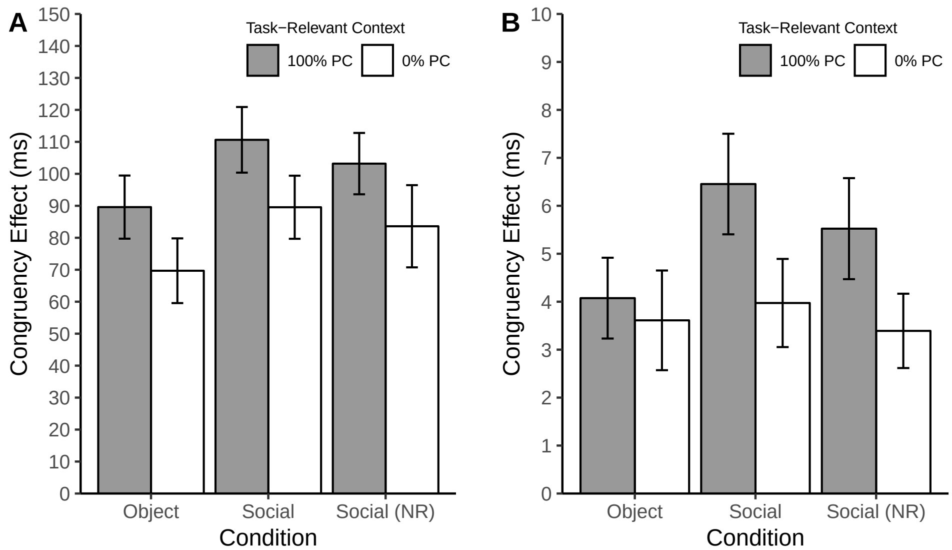


**Figure 4.2:** Results from Experiment 1.

Results from Experiment 1 showing congruency effects (incongruent - congruent) for frequency unbiased items in reaction times (left) and error rates (right) as a function of the task-relevant context (100% proportion congruent versus 0% proportion congruent).

see Figure 4.1B) and remained on screen until a response was made. Following a response, accuracy feedback was presented for 1000 ms. A response automatically triggered the next trial.

Halfway through the experiment (157 trials), participants received new instructions about which feature to count and to press the button on-screen when they were ready to continue.

## Results

Participants with mean error rates greater than 25% were excluded from the analyses, eliminating 13 participants. For all remaining participants, the correct RTs from frequency unbiased trials in each condition were submitted to an outlier removal procedure. The non-recursive Van Selst and Jolicoeur outlier removal procedure was applied after removing response times greater than 3000 ms (Van Selst and Jolicoeur, 1994). This procedure removed 3.39% of the total observations.

The primary question of interest was whether the task-relevance of context features associated with different levels of conflict would influence the size of the congruency effect for frequency unbiased items (see Table 4.1). To that end, mean correct RTs and mean error rates from frequency unbiased trials were submitted to a mixed analysis of variance (ANOVA) with task-relevant context (0% and 100% PC) and unbiased-item congruency (congruent and incongruent) as within-subject factors, and context-type (object, social, and social/non-repeating) as the between-subjects factor.

The results of the RT analysis revealed the critical, two-way interaction between task-relevant context and unbiased-item congruency to be significant, $F(1, 128) = 8.30$, $MSE = 1,608.54$, $p = .005$, $\hat{\eta}_p^2 = .061$, demonstrating smaller congruency effects when the context dimension associated with high conflict was made task-relevant. Additionally, the three-way interaction between the task-relevant context, unbiased-item congruency, and context-type was non-significant, $F(2, 128) = 0.00$, $MSE = 1,608.54$, $p = .996$, $\hat{\eta}_p^2 = .000$, showing no evidence for differences between the CSPC effects across context-type manipulations (see Figure 4.2).

Similarly, the corresponding error analysis also resulted in a significant two-way interaction between the task-relevant context and unbiased-item congruency, $F(1, 128) = 6.72$, $MSE = 13.93$, $p = .011$, $\hat{\eta}_p^2 = .050$, and a non-significant three-way interaction between task-relevant context, unbiased-item congruency, and context-type, $F(2, 128) = 0.92$, $MSE = 13.93$, $p = .400$, $\hat{\eta}_p^2 = .014$. Therefore, the congruency effects in error rates were also smaller when the context dimension associated with high conflict was made task-relevant, corroborating the results of the reaction time analysis.

## General Discussion

The aim of the current study was to determine whether task-relevance plays a role in the contextual recruitment of selective attention. Specifically, we tested whether manipulating the relative task-relevance of context cues could cause participants to ignore a previously learned context-association and apply a new association. We created a frequency unbiased context cue that shared features with two frequency biased contexts and used a feature-counting task to manipulate the task-relevance of context dimensions across two blocks of trials. Critically, halfway through the experiment participants received new instructions changing the task-relevant feature from one frequency biased cue to the other.

**Table 4.1:** Reaction times and error rates from Experiment 1.

| | | | Congruent | | Incongruent | |
|---|---|---|---|---|---|---|
| Task-Relevant Context | Condition | PC | RT | ER | RT | ER |
| 100% PC | Object | 0% | - | - | 693 (26) | 4.17 (0.73) |
| | | 50% | 585 (23) | 0.74 (0.27) | 675 (28) | 4.81 (0.82) |
| | | 100% | 587 (25) | 0.69 (0.23) | - | - |
| | Social | 0% | - | - | 745 (25) | 5.86 (0.77) |
| | | 50% | 612 (21) | 0.39 (0.19) | 722 (26) | 6.78 (1.04) |
| | | 100% | 616 (20) | 0.63 (0.2) | - | - |
| | Social (NR) | 0% | - | - | 813 (25) | 5.04 (0.74) |
| | | 50% | 689 (23) | 0.19 (0.14) | 793 (27) | 5.72 (1.06) |
| | | 100% | 689 (25) | 0.48 (0.17) | - | - |
| 0% PC | Object | 0% | - | - | 649 (16) | 3.84 (0.6) |
| | | 50% | 587 (17) | 0.56 (0.21) | 657 (17) | 4.17 (1.02) |
| | | 100% | 634 (18) | 2.69 (0.58) | - | - |
| | Social | 0% | - | - | 722 (23) | 5.77 (0.92) |
| | | 50% | 633 (20) | 0.78 (0.5) | 722 (25) | 4.75 (0.9) |
| | | 100% | 673 (21) | 1.84 (0.47) | - | - |
| | Social (NR) | 0% | - | - | 807 (26) | 4.55 (0.8) |
| | | 50% | 735 (28) | 0.87 (0.36) | 818 (30) | 4.26 (0.8) |
| | | 100% | 779 (34) | 2.03 (0.4) | - | - |

*Note*: RT = Reaction Time (ms); ER = Error Rates (%); PC = Proportion Congruent; NR = Non-Repeating; Standard Errors are presented in parantheses.

The key finding was that the congruency effects for the frequency unbiased items were significantly larger when the low-conflict context was made task-relevant as compared to when the high-conflict context was made task-relevant. This result is consistent with prior CSPC effects and, like the previous work, suggests that the context cues triggered rapid adjustments to attentional control (Crump and Milliken, 2009). However, unlike prior studies, we were able to experimentally manipulate the CSPC effect across blocks of trials without changing any of the physical properties of the stimuli. This novel finding demonstrates that participants were able to learn and apply one context-attention association in the first phase, and subsequently ignore that association to learn a new association in the second phase.

This result implicates an important role for task-relevance in producing CSPC phenomena. Crump et al. (2008) used shapes as context cues in a prime-probe Stroop task and did not find CSPC effects until the context cues were made task-relevant. Similarly, Cañadas et al. (2013) eliminated the CSPC effect by making the contextual cue effectively unrelated to the task. These studies suggest that task-relevance plays an important role in establishing associations between contextual information and attentional priorities to produce CSPC effects. Our finding extends this work in two important ways. First, we show that changing the task-relevance of the presented cues corresponded with a change in attentional control in the predicted direction. This demonstrates that task-relevance is also a key determinant of whether a previously learned attention-context association will be used or ignored in favor of a new association. Second, we show that task-relevance allowed participants resolve competition between two competing contextual cues, responding on the basis of one at the expense of the other. To our knowledge, this is the first demonstration that CSPC effects can be produced when there are multiple, overlapping contextual cues available.

In light of prior work, we take this as evidence that the contextual recruitment of selective attention, although likely implicit, is not obligatory (e.g., Brosowsky and Crump, 2016), requiring that environmental information be incorporated into the task representation. Similarly, there appears to be flexibility in which environmental features are selected and used to guide attention, which can be rapidly updated depending on the task-relevance of those features. Such a result lends some insight into how context-specific control might operate in more complex, real-world environments, where there is an over-abundance of environmental features that afford many different learned associations. From a theoretical perspective, this result is consistent memory-based accounts of CSPC phenomena (Brosowsky and Crump, 2018; Bugg and Hutchison, 2013; Crump et al., 2017, 2018; Crump and Milliken, 2009). Under this view, a memory process encodes attentional priorities in the representation of individual experiences and, as a result, become associated with the environment where they were used. The subsequent reoccurrence of a prior context triggers the retrieval and reinstatement of those attentional priorities. Our results show however, that all the features of the environment may not be treated equally and that only task-relevant features are used to probe memory and guide attention.

Another key result of this study concerned the different stimuli used as context cues. Across the three conditions, we varied the type of context image and dimensions. We manipulated the type of image presented, including both objects (identity and color dimensions) and faces (gender and racial dimensions). We also manipulated whether a single set of three repeating images were presented (object and social) or a set of non-repeating images were presented (social/non-repeating). Across all three conditions, we found no evidence that using different stimuli had an influence on the size or direction of the CSPC demonstrating generalizability of this phenomenon. Furthermore, CSPC effects were present even when using non-repeating images which suggests that context-dependency did not rely on image-specific associations but higher-order, learned categorical information.

Traditional models of person perception posit that social categories are automatically activated in the presence of social stimuli (e.g., Brewer, 1988; Devine, 1989; Fiske and Neuberg, 1990). Cañadas et al. (2013) however, found that directing participants to think about faces in terms of individual features eliminated context-specific attention effect and suggested that momentary motivations may influence the automaticity of social categorization. Our results add to this literature by observing the influence of momentary motivations (i.e., task-relevance) when there is competition between two salient social categories. Specifically, we found that participants could categorize on the basis of one social cue at the expense of the other, and subsequently switch between them. These findings may speak to issues of automaticity in social categorization (Macrae and Bodenhausen, 2001) as well as understanding how the situational context can prime one social identity over another (Crisp and Hewstone, 2007). Furthermore, we found no evidence for differences between the social and object conditions. This suggests that categorization within the CSPC task is quickly and easily learned when the task supports such learning and is non-unique to social stimuli.

In sum, our results provide new evidence that changes in task-relevance can update the contextual recruitment of selective attention in a CSPC flanker task. We demonstrated that making one context cue task-relevant produced a CSPC effect, even in the presence of a competing contextual cue. More important however, we found that changing the task-relevance of the contextual cues across blocks of trials was accompanied by predictable changes in the congruency effects. These effects were found to be generalizable across two different kinds of stimuli and occurred even when using non-repeating images, and implicate an important role for task-relevance in producing context-dependency and are consistent with memory-based accounts of CSPC phenomena.

# CHAPTER 5

# GENERAL DISCUSSION

Modern views on selective attention and cognitive control often bin processing into dichotomies like controlled/automatic, top-down/bottom-up, and endogenous/exogenous. Theoretical treatments of attention, however, have often included a third kind of attentional control whereby attention is guided by internal memory representations created through experience. Though they rarely elaborate on how such memory systems would operate and instead vaguely appeal to associative learning principles. More recent empirical work has also demonstrated that prior experiences can influence selective attention in a seemingly automatic, stimulus-driven manner. This evidence has been difficult to reconcile with dichotomous frameworks of cognitive control (Botvinick et al., 2001; Braver, 2012; Chiu and Egner, 2019; De Pisapia and Braver, 2006; Jiang et al., 2014).

Based on these theoretical and empirical gaps, I argued that there is a need to examine attention using a memory-based framework and proposed an instance-based memory theory. The instance theory of automatic attentional control relies on four assumption (e.g., Logan, 1988; Hintzman, 1984): (1) *obligatory memory encoding*; (2) *”instance” or ”exemplar” memory representation*; (3) *obligatory memory retrieval*; and (4) *the preservation of cognitive processing details*. The current thesis investigated general principles of the instance theory using a converging operations approach.

To further situate the contribution of the thesis, the following sections will briefly discuss the findings of the empirical work and their implications for theories of attention and cognitive control. In the first section, I will briefly summarize the critical aspects of the empirical findings in the thesis. Following that, I will discuss some limitations and open questions regarding the instance theory and automatic attentional control. Finally, I will discuss some broader implications of the current thesis.

**Summary of empirical chapters**

In Chapter 2, I investigated the obligatory nature of memory encoding of attentional control. In the selective attention domain, the gold-standard measures of attention are conflict paradigms, like the Stroop (1935) and flanker (Eriksen and Eriksen, 1974) tasks. Learning associations between context cues and selective attention, using these tasks, are thought to occur implicitly, without awareness, and in an obligatory fashion. However, these tasks present a very specific situation where a distracting irrelevant dimension interferes with our ability to attend to the relevant dimension. Here, I looked at whether participants would learn context-specific attentional control strategies in a novel bi-dimensional sampling task. I found participants did not spontaneously learn context-specificity. Instead, participants had to be instructed to adopt context-specific attentional strategies before any learning occurred.

There were two important differences between my task and traditional congruency tasks. First, there was no conflict between the two dimensions, in the traditional sense, and both dimensions were task-relevant. Second, attentional selection occurred after the presentation of the target stimulus (e.g., Sperling, 1960). The finding that learning was not spontaneous suggests that learning context-attention associations is not error-driven, but requires participants to be engaged in the attentional strategy before, or while the target stimulus is presented.

In Chapter 3, I investigated the long-term preservation of single experiences with conflict. One of the more distinguishing predictions made by the instance theory is that single experiences are preserved in the long-term and, if retrieved from memory, can produce automatic attentional control. Most theories of automatic attentional control, in contrast, propose that automaticity requires extensive practice (e.g., Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977; A. M. Treisman, 1969). Here, I used a large set of unique context items to determine whether a single experience applying attentional control on an item can influence a second presentation after many intervening trials. If so, I predicted a pattern of results similar to the congruency sequence effect. Across three experiments I found evidence that single experiences can influence selective attention after many intervening trials (4 to 319). However, I also note some boundary conditions, failing to find such effects when the repeated items differed in superficial details (color).

In Chapter 4, I investigated how automatic attention could be controlled by manipulating memory-retrieval via task-relevance. Automaticity, according to the instance theory, is a memory-retrieval process. This suggests that there should be some flexibility in whether learning is expressed depending on which features of the environment are used to cue memory. In this chapter, I examined whether manipulating the relevance of context features would cause changes in context-dependent attentional control.

I adopted a paradigm similar to the context-specific proportion congruent (CSPC) transfer design. However, unlike previous CSPC designs where contexts were defined by a single discriminating feature (e.g., upper vs. lower screen locations), contexts were defined by two feature dimensions and critically, the frequency unbiased context (50% congruent) always shared a feature with each of the frequency-biased contexts (0% and 100% congruent). To manipulate which overlapping feature was task-relevant I used a secondary counting task. Halfway through the experiment participants received new instructions, changing the task-relevant context feature. Across three different stimulus sets, I found that changing the task-relevance caused predictable changes in the congruency effects. This result implicates an important role for task-relevance in producing and manipulating context-dependency in complex environments.

## Re-evaluating the instance theory of automatic attentional control

The central aim of the current thesis was to evaluate general principles of the instance theory of automatic attention as it applies to selective attention. One fundamental question then, is whether the presented body of empirical work provides evidence for the instance theory of automatic attentional control. The answer, in my view, is both yes and no. The evidence provided here certainly is *consistent* with an instance theory. Whether all possible alternative explanations can be ruled out is more difficult to say. Part of the problem is that most theories of automatic attention are vague, so it is difficult to determine what kinds of predictions they would make. I do think the evidence here however, is more consistent with the predictions made by an instance theory than any of ther alternatives.

For one, theories of learning and attention posit practice as the single most important prerequisite for automaticity (e.g., Logan, 1988b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977; A. M. Treisman, 1969). Under the instance theory, the role of practice is more nuanced. Automaticity from this perspective is not about laying down a certain number of memory traces, but instead about how easily those memory traces are retrieved. On the one hand, if memory retrieval is inefficient or difficult, many memory traces might be required for automaticity to develop. On the other hand, if a single memory is distinctive enough to support efficient retrieval, it should produce automatic attentional control. This is exactly what I found in Chapter 3: single, distinctive experiences were sufficient to influence performance in a selective attention task after many intervening trials.

Second, theories of learning and attention posit that once automaticity develops it is difficult to suppress or modify. They seem to be suggesting, on the one hand, that automatic processes become hardwired and the mere presence of a stimulus should trigger its associated attentional control in a ballistic, rigid manner. On the other hand, they also suggest that these automatic processes can be controlled under some circumstances but do not make any predictions about when automatic processes can be controlled or explain how (e.g., Logan, 1988b; Posner and Snyder, 1975a, 1975b; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977).

The instance theory in contrast, allows associations to form between stimuli and attentional control – by way of encoding multiple memory traces – but also makes predictions about whether that learning will be expressed in subsequent experiences which is dictated by memory retrieval. The extent to which automaticity can be controlled then, is determined by the extent to which memory retrieval can be controlled (e.g., Logan, 1988; 1992). In Chapter 4, I demonstrated that participants learned an association between contextual cues and attentional control in the first block of trials. Then, by shifting which features were task-relevant, demonstrated that participants could ignore the previous block and learn a new association. This result would be difficult to reconcile with the view that once learned, automaticity is difficult to modify, but easily accommodated by an instance theory. Under this view, by making some features task-relevant, I was modifying which features of the stimuli were being used to cue memory and dictating which prior experiences were being retrieved.

However, there is still no conclusive evidence for the instance theory's key assumption: instance representation. Although Chapter 3 presented evidence that single experiences can influence selective attention, this does not provide evidence that every experience is stored in a separate memory trace. It seems likely that associative learning could account for all the data provided here without this assumption. Associative learning differs from the instance theory in that learning is modeled as the gradual accumulation of excitatory or inhibitory connections between stimulus units. The associative strength is summative and represents the entire history of learning (e.g., Rescorla and Wagner, 1972). If taken at face value, this would separate learning from memory and implies that learners do not remember the events of separate trials or experiences, which seems quite extreme (e.g., Fagot and Cook, 2006; Miller, Barnet, and Grahame, 1995; Vaughan and Greene, 1984; Voss, 2009). However, others have argued that the underlying architectures of associative and exemplar models are homogulous and the "structures of current associative learning being just graphical depictions of relationships … common to both kinds of theory" (Estes, 1991, p. 11). In which case the summative representation is more of a computational convenience than a theoretical statement about memory representation. In either case, associative learning models are similar enough to exemplar models that they could produce the same kinds of predictions (e.g., Blough, 1998; Jamieson et al., 2012, 2010), but without assuming instance representation.

## Limitations and open questions

### The limitations of verbal theories

Although the evidence is consistent with an instance theory, it is difficult to rule out all possible alternative explanations for the data presented. In part, this difficulty is due to the limitations of verbal or descriptive theories. This issue is most apparent in Chapter 3, where I discussed a variety of alternative theoretical explanations for the long-term congruency sequence effect. The long-term congruency sequence effect was an interesting finding that was predicted by the instance theory. However, many theories could also accommodate the finding with only small adjustments. This is one limitation of verbal theories: Verbal theories are inherently flexible, open to interpretation and difficult to falsify.

Take for example the feature integration theory of congruency sequence effects (Hommel, 1998; Hommel et al., 2001, 2004). Here is a theory developed to explain trial-to-trial adjustments of the congruency effect using an event-binding model (e.g., Kahneman et al., 1992). This theory explains congruency sequence effects as the result of a process attempting to bind sequential events together and makes no assumptions about long-term storage. However, because the theory does not state any explicit assumptions about long-term preservation, it is open to interpretation, and if one wanted to allow for long-term storage, then it could accommodate long-term effects. In effect, both the existence and non-existence of long-term phenomena would be consistent with this theory.

A second, related, limitation is that verbal theories are under-specified and do not make very precise predictions. This makes it difficult to determine whether data are consistent with the theory. In Chapter 3 for example, the long-term congruency sequence effect was found when the prime and probe shared colors, but disappeared when the colors of the images changed from prime to probe. Unfortunately, it is not clear whether that result is consistent or inconsistent with the instance theory. The instance theory makes the broad prediction that the currently presented stimulus will cue the retrieval of similar items in memory. It does not however, make precise predictions about how similar items need to be for retrieval. So, if we accept that changing colors reduces similarity to the point of impaired retrieval, the data are still consistent with the instance theory.

Overall, I think the instance theory provides a good theoretical framework to assess the role of memory in guiding selective attention. To generate further predictions however, I think it will be necessary to refine the theory to formulate more concrete hypotheses and finer predictions. One possible route is to develop computational models that better specify the hypothesized mechanisms that underlie memory encoding, storage, and retrieval. For instance, there are open questions about how memories are aggregated on retrieval. It is unclear from the current theory, what to predict after multiple exposures to the same stimulus. Some exemplar models conceptualize retrieval as a race which retrieves a single memory trace (Logan, 1988b), others as an accumulation of evidence, weighted by similarity (Palmeri, 1997), and others still as similarity-based aggregation (Hintzman, 1984). Therefore, one path forward is to use these computational models to better refine the instance theory and generate new hypotheses.

**Do we need to assume attentional control settings are preserved in memory?**

One of the general assumptions of the instance theory of automatic attentional control is that processing details are preserved in memory (Estes, 1972; Kolers and Roediger, 1984). It is worth considering whether this assumption is necessary to explain the influence of prior experience on selective attention. One alternative is that associations form between stimuli and diagnostic features of attentional priority. For instance, a stimulus might be frequently paired with a reward. The re-presentation of that stimulus could trigger the retrieval of prior experiences, indicating that it was rewarded in the past, and attention prioritizes its processing based on that retrieved reward. Here, we do not need to assume that the attentional priority per se was stored and retrieved, but instead the level of reward was stored and retrieved and used as a diagnostic feature for determining attentional priority. Other features like predictiveness and conflict could be used in a similar manner. This approach suggests that the attentional control system uses some set of pre-defined diagnostic features to determine priority and when memory encodes experiences they are tagged with associated features.

We need not stop there however. If we assume that all features of the experience are stored in a memory trace, then prior experiences can be evaluated on any dimension *after* retrieval. That is to say we do not need to assume that the level of reward is stored if we assume that the stimulus, response, and outcome are stored together. Whether the outcome was rewarding can be assessed after retrieval. Similarly, if the word RED in a blue colored font is stored in memory, then it does not need to store an associated level of 'conflict' because it can be evaluated for conflict after retrieval. This is actually consistent with the conflict-monitoring model, that assumes there is a dedicated cognitive process for evaluating the current level of conflict and triggering adjustments in control (Botvinick et al., 2001; Braver, 2012; De Pisapia and Braver, 2006). It need only be extended to evaluate the conflict of retrieved prior experiences as well as the currently experienced conflict.

In some ways, these approaches are more easily reconciled with traditional views of controlled and automatic processes than the instance theory. Attention, from these perspectives, is a controlled process and does not need to become automatic. Instead, the attentional control process leverages evidence from prior experiences to help determine attentional prioritization. For example, if the word RED in blue colored font retrieves many experiences that contain conflict, then the current experience feels more conflicting than if it did not retrieve experiences containing conflict. Attention is adjusted in response to the experienced conflict and experienced conflict is influenced by both the currently presented stimulus and memory. This differs from the instance theory which proposes that an attentional response is automatically reinstated, circumventing executive control entirely (e.g., Logan, 2005; Moors and De Houwer, 2006; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977).

One inherent problem for these kinds of approaches however, is that they do not provide a mechanism for indicating how attention should be adjusted, only when attention should be adjusted. For example, if the word RED in blue colored font triggers a need for control, because of experienced/retrieved conflict, how does the attention system know that the color dimension should be prioritized and the word dimension ignored? The instance theory obviously does not have this issue because attentional control priorities are stored in memory.

Additionally, the instance theory is the most general account because it assumes all cognitive processing details are preserved. Therefore, any cognitive control procedure can be stored and retrieved without the need to identify specialized diagnostic features (e.g., predictiveness, conflict) or specialized monitoring processes (e.g., the conflict-monitor). The instance theory then, can accommodate other cognitive control phenomena that have shown long-term contextual control effects (e.g., visual search, task-switching, negative priming, etc.) by appealing to the same underlying mechanisms. So although there does seem to be plausible alternatives to the assumption that cognitive procedures are stored and reinstated, the instance theory still seems like the most parsimonious and general account.

**Do we actually need three kinds of attentional control?**

Traditionally, theories of attentional control assert a dichotomy between top-down and bottom-up control. The former reflecting current goals and the latter reflecting physical salience of the stimuli. I, as well as many others, have argued that there is a need for third kind of attentional control reflecting the influence of prior experiences; an automatic or memory-guided attentional control (e.g., Awh et al., 2012; Egner, 2014; Failing and Theeuwes, 2018; Hutchinson and Turk-Browne, 2012). Again, here I consider whether this assumption is necessary.

One alternative, not previously entertained, is that bottom-up attentional control and memory-guided attentional control both reflect learned attentional priorities. Primitive physical features of our environment like sudden onsets, loud noises, or high contrast objects, are thought to automatically capture attention. It might be the case that these low level features have become associated with high attentional priority through experience. Evidence for this comes from work showing that bottom-up attentional capture is subject to top-down control similar to other automatic, learned behaviors (e.g., Anderson and Folk, 2010; Eimer and Kiss, 2008; Folk, Remington, and Johnston, 1992).

Additionally, some models of attention (Kahneman and Daniel, 1973; Schneider and Shiffrin, 1977; Shiffrin and Schneider, 1977) allow automatic attention to interject at any point during perceptual processing. Accordingly, early perceptual processing could cue memory with primitive features directing attention automatically before the stimuli are even fully processed. The strength by which some features of our environment seem to direct our attention automatically (e.g., the sudden appearance of a new object) might simply reflect extensive experience or how early in perceptual processing memory is cued. Therefore, it seems plausible, at least on the surface, that all bottom-up attentional phenomena could reflect the same memory-retrieval process as the one proposed here.

## Broader implications

A global aim of this empirical work is to determine how memory for specific prior experiences guides performance in the present moment. In the current thesis, I have outlined a theoretical framework describing how memory could influence cognitive control in an automatic, stimulus-driven manner. The empirical work presented here, together with the prior research, lends support this theory. The instance theory is a general theory of how control processing can be preserved and automatically reinstated. As such, it has implications for theories of cognitive control more generally. Furthermore, a better understanding of how memory for prior experiences can influence cognitive control may have more practical implications.

In everyday life, we would expect that memory for prior cognitive control operations routinely optimizes performance. When in familiar environments, people would gain the benefit of applying memory-based procedures for regulating the flow of information without the effortful cost of deliberation. These insights could be important for how we think about training in tasks where learning and attention are critically important. For instance, an important task for airport security is to use x-rays to screen baggage for dangerous items (e.g., Biggs and Mitroff, 2015; McCarley, Kramer, Wickens, Vidoni, and Boot, 2004; Wolfe, 2010). This is an extremely attention-demanding task and resembles a variety of laboratory phenomena (e.g., visual search, congruency effects, vigilance/mind-wandering). Developing expertise could be streamlined if we know which kinds of experiences facilitate the development of automaticity. For instance, a few easily retrievable, context-rich experiences might improve training compared to many, poorly-defined experiences. Furthermore, developing generalization might require different kinds of training procedures.

Additionally, problems in regulating the flow of information can result from the inappropriate use of, or failure to rely on memory. For example, deliberate control may need to override memory based control when memory-guided procedures are inappropriate yet the environment acts as a strong cue. Or, when memory fails to encode or retrieve cognitive control procedures, people may be forced to rely on taxing voluntary control processes to supply the control. Evaluating deficits in executive functioning and cognitive control using a memory-based framework could provide important insights from a clinical perspective.

For instance, many clinical disorders like obsessive compulsive disorder (OCD) and attention-deficit/hyperactivity disorder (ADHD) are associated with difficulties selectively attending, shifting attention, inhibiting inappropriate responses, and slowed motor response. These difficulties are typically explained as deficits in executive functioning (Olley, Malhi, and Sachdev, 2007; Snyder, Kaiser, Warren, and Heller, 2015). However, they could reflect deficits in memory based control. For example, what appears to be the inability to suppress unwanted behaviors, or to inhibit irrelevant information, might reflect an inability to properly contextualize task-relevant information, triggering automatic attentional responses to task-irrelevant cues. Similarly, overall slow motor responses could reflect a failure to retrieve cognitive control procedures, forcing people to rely on slow, taxing voluntary control processes.

# CHAPTER 6

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