UNCOVERING EVIDENCE FOR THE INHIBITION OF RETURN EFFECT IN THE NON-SPATIAL DOMAIN

UNCOVERING EVIDENCE FOR THE INHIBITION OF RETURN EFFECT

IN THE NON-SPATIAL DOMAIN

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ABSTRACT

Our attentional system has the remarkable ability to allow familiar contexts to guide attentional orienting, while still retaining the ability to orient rapidly to novelty in our environment. Many cognitive paradigms have been used to investigate the particular process that is responsible for orienting attention to novel events, but each paradigm has produced a unique set of boundary conditions. One such paradigm has studied an effect labelled Inhibition of Return (IOR), which has been argued to tap into an attentional mechanism that rapidly orients attention to novelty, but only in the spatial domain. The IOR effect was initially taken as evidence of a fundamental difference between spatial attentional orienting and non-spatial attentional orienting. However, there were a small number of early studies that questioned the view that the IOR effect can only be observed in the spatial domain.

In this dissertation, I built upon the evidence for non-spatial IOR by uncovering the effect using a Target-Target (TT) procedure. Although a number of prior studies had failed to observe non-spatial IOR using a TT procedure, I was able to uncover non-spatial IOR effects using a TT procedure by introducing an intervening event. The IOR-like effect that was uncovered using this procedure was labelled the intervening event effect. I introduced a dual process framework to explain the intervening event effect. According to the dual process framework, intervening events between consecutive targets can disrupt an episodic integration process, allowing the influence of a separate opposing process to be measured more directly. Using the dual process framework, I studied the level of processing of the intervening event that was necessary to disrupt episodic integration, as well as the context-sensitivity of the episodic integration process. Lastly, I investigated the role of subjective expectancy in the studies used to measure non-spatial IOR in this thesis.

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To my committee, Scott Watter and Karin Humphreys, thank you for your patience throughout this entire process and always being ready to give me constructive feedback. My research line has been strengthened enormously from your valuable input. I also want to thank you for continually being a strong presence at Cognition Reading Group and at Coggie Talks. Both of those venues gave me the invaluable opportunity to share my research with the department.

I also want to thank my parents, John and Mary Spadaro, for always supporting my decisions even when they didn’t make any sense. I am extremely fortunate to have a strong, supportive family, and I may not express it often enough, but here it is in writing. Thank you for your advice, your encouragement, and always trying to put me in the best position possible. There is nothing more anyone could ask of their parents. I cannot forget my little sister, Jenelle (aka Nell), who has continually made me laugh since she was born.

During my time in graduate school, I was extremely fortunate to collaborate and learn from my lab members, Dave Thomson, Maria D’Angelo, Ellen MacLellan, Chris Fiacconi, Mitch LaPointe, Sandra Monteiro, Tamara Rosner, and the “Spanish Girls”. All of you have helped me in numerous ways and I’ve learnt so many things from working with all of you. Dave, you are one of the strongest critical thinkers I have ever seen. Maria, I’m not entirely sure if there is something you can’t do. Ellen, you are always the voice of support and absolutely essential to the lab family. Chris, you are an exceptionally creative researcher. You walk a fine line between savant and genius; I have no doubt that you will make a world-class professor. Mitch, I always thought you have the most interesting research line and your curiosity knows no bounds. I benefited greatly from working with all of you, and I have no doubt that each of you will find success.

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TABLE OF CONTENTS

ABSTRACT.................................................................................................................................. iv

ACKNOWLEDGMENTS ............................................................................................................. v

LIST OF FIGURES ....................................................................................................................... x

LIST OF TABLES........................................................................................................................ xv

DECLARATION OF ACADEMIC ACHIEVEMENT............................................................... xvi

CHAPTER 1: GENERAL INTRODUCTION .............................................................................. 1

Familiarity, Novelty and Attention Orienting................................................................................. 2

Negative Priming............................................................................................................................ 5

Repetition Blindness ...................................................................................................................... 7

Visual Marking .............................................................................................................................. 8

Paradigm Specific Mechanisms versus Broad Theoretical Principles............................................ 9

Inhibition of Return....................................................................................................................... 10

Inhibition of Return as a Consequence of Reorienting Attention................................................. 12

Challenges to the Reorienting Hypothesis.................................................................................... 14

The Repetition Effect: An Obstacle to Measuring Inhibition of Return....................................... 17

Non-Spatial Inhibition of Return.................................................................................................. 19

Biphasic Time Course?................................................................................................................. 22

Cue-Target versus Target-Target Procedures............................................................................... 23

Overview of the Empirical Work in the Thesis............................................................................ 24

References..................................................................................................................................... 25

CHAPTER 2: Response to an intervening event reverses nonspatial repetition effects in 2AFC tasks: Nonspatial IOR?........................................... 32

Preface........................................................................................................................................... 32

Abstract ........................................................................................................................................ 34

Introduction................................................................................................................................... 35

Study 1.......................................................................................................................................... 43

Methods......................................................................................................................................... 45

Results........................................................................................................................................... 49

Discussion .................................................................................................................................... 56

Study 2.......................................................................................................................................... 57

Methods......................................................................................................................................... 57

Results........................................................................................................................................... 58

Discussion .................................................................................................................................... 61

Study 3.......................................................................................................................................... 62

Methods......................................................................................................................................... 64

Results........................................................................................................................................... 65

Discussion .................................................................................................................................... 68

Study 4.......................................................................................................................................... 68

Methods......................................................................................................................................... 69

Results........................................................................................................................................... 70

Discussion .................................................................................................................................... 71

Study 5.......................................................................................................................................... 72

Methods......................................................................................................................................... 73

Results........................................................................................................................................... 74

Discussion .................................................................................................................................... 76

General Discussion....................................................................................................................... 77

Conclusions................................................................................................................................... 87

References..................................................................................................................................... 88

CHAPTER 3: ON THE ROLE OF ATTENDING AND RESPONDING TO AN INTERVENING EVENT FOR REVEALING NON-SPATIAL IOR......................................... 95

Preface........................................................................................................................................... 95

Abstract......................................................................................................................................... 96

Introduction................................................................................................................................... 97

Study 1........................................................................................................................................ 103

Methods....................................................................................................................................... 104

Results......................................................................................................................................... 107

Discussion .................................................................................................................................. 109

Study 2…………………............................................................................................................ 110

Methods....................................................................................................................................... 111

Results......................................................................................................................................... 111

Discussion .................................................................................................................................. 113

Study 3…………………............................................................................................................ 114

Methods....................................................................................................................................... 115

Results......................................................................................................................................... 116

Discussion .................................................................................................................................. 118

Study 4…………………............................................................................................................ 119

Methods....................................................................................................................................... 120

Results…………………............................................................................................................. 122

Discussion .................................................................................................................................. 124

General Discussion…………..................................................................................................... 125

References................................................................................................................................... 133

CHAPTER 4: THE EFFECT OF AN INTERVENING EVENT ON EPISODIC INTEGRATION: NON-SPATIAL IOR?................................................................................... 138

Preface......................................................................................................................................... 138

Abstract ...................................................................................................................................... 139

Introduction................................................................................................................................. 140

Study 1 ....................................................................................................................................... 149

Methods……………………....................................................................................................... 149

Results......................................................................................................................................... 152

Discussion .................................................................................................................................. 157

Study 2 ....................................................................................................................................... 157

Methods....................................................................................................................................... 158

Results......................................................................................................................................... 159

Discussion .................................................................................................................................. 162

Study 3 ....................................................................................................................................... 163

Methods....................................................................................................................................... 165

Results & Discussion.................................................................................................................. 166

General Discussion..................................................................................................................... 172

References................................................................................................................................... 179

CHAPTER 5: SUBJECTIVE EXPECTANCY AND INHIBITION OF RETURN: A DISSOCIATION IN A NON-SPATIAL TWO-ALTERNATIVE FORCED CHOICE TASK.......................................................................................................................................... 183

Preface......................................................................................................................................... 183

Abstract ...................................................................................................................................... 184

Introduction................................................................................................................................. 185

Methods……………………....................................................................................................... 192

Results......................................................................................................................................... 196

General Discussion .................................................................................................................... 200

Conclusion.................................................................................................................................. 206

References................................................................................................................................... 208

CHAPTER 6: GENERAL DISCUSSION.................................................................................. 213

The Intervening Event Effect…………………………………….............................................. 215

The Dual Process Framework..................................................................................................... 218

Perceptual Inhibition................................................................................................................... 220

Habituation.................................................................................................................................. 221

Detection..................................................................................................................................... 222

Conclusion.................................................................................................................................. 223

References.................................................................................................................................. 225

LIST OF FIGURES

CHAPTER 2

*Figure 1.* The sequence of events for a not-repeated trial in the intervening event condition of Experiment 1A is shown. In the experiment, the darker rectangle would have been blue and the lighter rectangle would have been yellow. In the no-intervening event condition (not shown), the intervening event was replaced by a blank screen that remained for approximately the same length of time as the intervening event. Experiments 1B and 1C were identical in design with the rectangles replaced by short and long lines, or the words “Right” and “Left”, respectively….... 48

*Figure 2.* Top Panel: Mean response times for T2 in Experiments 1A, 1B, and 1C (no-intervening event condition), collapsed across RSI and participants. Bottom Panel: Mean response times for T2 in Experiments 1A, 1B, and 1C (intervening event condition), collapsed across RSI and participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions............................................................................................ 52

*Figure 3.* Top Panel: Mean response times for T2, collapsed across RSI and participants, are shown for the 80/20 group. Bottom Panel: Mean response times for T2, collapsed across RSI and participants, are shown for the 20/80 group. Error bars represent the standard error of the difference between repeated and not-repeated conditions............................................................ 60

*Figure 4.* Top Panel: Mean response times for T2 across four different RSI conditions in Experiment 3A, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions. Bottom Panel: Mean response times for T2 across four different RSI conditions in the intervening event condition, and the single RSI condition in the no-intervening event condition, for Experiment 3B, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions...................................................................................................................... 66

*Figure 5.* Mean response times for T2 in Experiment 4, collapsed across RSI and participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions..................................................................................................................................... 70

*Figure 6.* Mean response times for T2 in Experiment 5, collapsed across RSI and participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions..................................................................................................................................... 74

CHAPTER 3

*Figure 1.* Left Panel: The sequence of events for a not-repeated trial in Experiment 1A is shown. Note that although the intervening event is presented here as a filled grey dot, on 75% of trials it was a filled grey dot and on 25% of the trials it was an unfilled grey dot. Participants were instructed to respond to both types of intervening event stimuli. Right Panel: The sequence of events for a not-repeated trial in Experiment 1B is shown. Again, although the intervening event is presented here as a filled grey dot, on 75% of trials it was a filled grey dot and on 25% of the trials it was an unfilled grey dot. Participants were instructed that neither type of intervening event required a response. ………………………….......................................... 106

*Figure 2.* Mean response times for T2 in Experiments 1A and 1B, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions. .................................................................................................................................. 109

*Figure 3.* Mean response times for T2 in Experiment 2, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions... 113

*Figure 4.* Mean response times for T2 in Experiment 3, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions... 118

*Figure 5.* Mean response times for T2 in Experiment 4, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions... 124

CHAPTER 4

*Figure 1.* Subset of the data presented in Experiment 1 of Taylor & Donnelly (2002). Identity-based IOR (i.e. non-spatial IOR) effects are broken down by same location trials, on the right side of the graph, and different location trials, on the left side of the graph………….………. 146

*Figure 2.* Left Panel: The sequence of events for a not-repeated trial in the no-intervening event condition of Experiment 1 is shown. Right Panel: The sequence of events for a not-repeated trial in the intervening event condition of Experiment 1 is shown. Participants were instructed to respond to the intervening event by pressing the response keys for “BLUE” and “YELLOW” simultaneously………………………………………………………………………………… 152

*Figure 3.* Mean response times for T2 in Experiment 1, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions…154

*Figure 4.* Mean response times for T2 in Experiment 2, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions…160

*Figure 5.* A comparison of the mean response times for T2 for only the intervening event condition in Experiment 1 and the Central-Only trials in Experiment 3, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions……………………………………………………………………………. 168

*Figure 6.* Mean response times for T2 in Experiment 3 for the Peripheral-Only trials, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions…………………………………………………………………… 170

*Figure 7.* Mean response times for T2 in Experiment 3 for the Central-Peripheral/Peripheral-Central trials, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions…………………………………….. 171

CHAPTER 5

*Figure 1.* The sequence of events for a Color-Response trial in the intervening event condition is shown. In the experiment, the darker rectangle would have been blue and the lighter rectangle would have been yellow. In the no-intervening event condition (not shown), the intervening event was replaced by a blank screen that remained for approximately the same length of time as the intervening event…………………………………………………………………………... 195

*Figure 2.* The sequence of events for an Expectancy-Response trial in the intervening event condition is shown. In the experiment, the darker rectangle would have been blue and the lighter rectangle would have been yellow. In the no-intervening event condition (not shown), the intervening event was replaced by a blank screen that remained for approximately the same length of time as the intervening event………………………………………………………... 196

*Figure 3*. Mean response times for T2 across the two intervening event conditions, collapsed across participants and RSI. Error rates for each condition are presented in parentheses. Error bars represent the standard error of the difference between repeated and not-repeated conditions…………………………………………………………………...…………………. 198

*Figure 4*. Mean proportion of expectancy responses as a function of whether a repetition or an alternation was expected across the two intervening event conditions, collapsed across participants and RSI. Error bars represent the standard error of the mean proportion of expectancy responses for both intervening event and no-intervening event conditions……..... 199

LIST OF TABLES

CHAPTER 2

*Table 1.* Mean response times and error rates for T2 (ms) for each condition in Experiments 1A, 1B, 1C, 2, 3A, 3B, 4, and 5…………………………………………………………………...… 51

*Table 2.* Mean responses times and error rates for T1 and the intervening event (where applicable) in Experiment 1A, 1B, 1C, 2, 3A, 3B, 4, and 5………………………………….… 55

CHAPTER 3

*Table 1.* Mean response times and error rates for T2 (ms) for each condition in Experiments 1A, 1B, 2, 3, &4……………………………………………………………………………………. 107

CHAPTER 4

*Table 1.* Mean response times and error rates for T2 (ms) for each condition in Experiments 1, 2, & 3…………………………………………………………………………………………….. 155

DECLARATION OF ACADEMIC ACHIEVEMENT

This dissertation is organized in the sandwich format, as approved by the McMaster University School of Graduate Studies, and consists of six chapters. Each of the four empirical chapters (Chapters 2-5) constitutes a complete manuscript. Chapters 2 and 5 are published manuscripts, and Chapters 3 and 4 are in preparation for re-submission after having been submitted for publication once each. In the published chapters, the manuscript pages have been renumbered for continuity within the dissertation, but the statistical notation and reference styles of each particular journal have been retained. Chapters 2 and 5 are reprinted here with the permission from the copyright holders.

I am the primary author of each of the four manuscripts. In consultation with my supervisor, Bruce Milliken, I developed the research line that runs through all of the experiments included in this dissertation. I created the stimuli, programmed the experiments, collected and analyzed the data, and wrote each chapter. The manuscripts that form my empirical chapters are an accurate representation of my doctoral research. The role of each coauthor in each manuscript is noted in the following section.

CHAPTER 1: General Introduction

I am the sole author of this chapter, which was written in consultation with my supervisor, Bruce Milliken, and two committee members, Scott Watters and Karin Humphreys.

CHAPTER 2: Response to an intervening event reverses nonspatial repetition effects in 2AFC tasks: Nonspatial IOR?

Reference: Spadaro, A., He, C., & Milliken, B. (2012). Response to an intervening event reverses nonspatial repetition effects in 2AFC tasks: Nonspatial IOR? *Attention, Perception, & Psychophysics*, *74*(2), 331-349.

Coauthor Contributions:

He, C. – collection of pilot experiment data, manuscript editing suggestions

Milliken, B. – experiment development, manuscript editing suggestions

Research Conducted: 2007-2010

CHAPTER 3: On the role of attending and responding to an intervening event for revealing non-spatial IOR.

Coauthor Contributions:

Lupiáñez, J – experiment development, manuscript editing suggestions

Milliken, B – experiment development, manuscript editing suggestions

Research Conducted: 2012-2013

CHAPTER 4: The effect of an intervening event on episodic integration: Non-spatial IOR?

Coauthor Contributions:

Milliken, B. – experiment development, data analysis, manuscript editing suggestions

Research Conducted: 2011-2013

CHAPTER 5: Subjective expectancy and inhibition of return: A dissociation in a non-spatial two-alternative forced choice task.

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Coauthor Contributions:

Milliken, B – data interpretation, manuscript editing suggestions

Research Conducted: 2010-2011

CHAPTER 6: General Discussion

I am the sole author of this chapter, which was written in consultation with my supervisor, Bruce Milliken.

CHAPTER 1: GENERAL INTRODUCTION

This thesis addresses a fundamental issue in cognition. In particular, our attention systems must somehow be tuned so that we behave efficiently both toward familiar events and toward novel events in our environment. Although there is a range of methods used to study this issue in the laboratory, the empirical work in this thesis focuses on a simple method used to measure trial to trial repetition effects.

Prior research that has studied trial to trial repetition effects has identified a robust spatial orienting phenomenon, the inhibition of return (IOR) effect, which appears to reflect more efficient orienting to novel spatial events than to familiar spatial events (Posner & Cohen, 1984). Yet, if the IOR effect reflects a broad principle that favours encoding of novel over familiar events, one might expect that it would be found not just for events defined spatially, but also for a range of non-spatial stimulus dimensions. Although a small number of studies have identified IOR-like effects for non-spatial stimuli, there remains debate over whether these effects reflect the same mechanism that causes spatial IOR effects.

The research strategy adopted in this thesis was to study the joint effects of familiarity and novelty on trial to trial repetition effects using non-spatial stimuli. The general idea is that IOR-like effects may not have been observed in many prior studies with non-spatial stimuli precisely because separate mechanisms that favour familiarity and novelty both contribute to performance in many task contexts. To measure the influence of a process that taps the process that favours novelty, one has to find methods to separate the influences of these two processes. Indeed, the results of the empirical work in this thesis demonstrate that trial to trial repetitions effects with non-spatial stimuli are affected both by a process that favours familiarity (i.e., trial to trial stimulus repetition) and by a process that favours novelty (trial to trial stimulus alternation). The remainder of the introduction to the thesis will first unpack the importance of sensitivity to both familiarity and novelty processes in experimental psychology in general, and then will address this issue specifically in the spatial and non-spatial orienting tasks used to measure the IOR effect.

**Familiarity, Novelty and Attention Orienting**

We routinely encounter both familiar and novel objects in our environment, whether it is a friend in a crowd, or a sudden alarm. Remarkably, our attention system adapts to either of these opposing experiences, and apparently does so without much difficulty. The ability to respond adaptively to either familiar or novel objects is likely quite critical in a world where having attention captured by novelty can be a matter of survival. Consider the consequences if our attentional system was tuned only to familiarity. We would be able to respond very effectively in familiar contexts, such as driving along the same route to work every day. However, the advantages afforded by familiarity would come at a cost in responding to novel events, such as the siren of an oncoming emergency vehicle. Although most people have experienced the feeling of being in “autopilot” mode while driving along a familiar route, our attentional system can be shifted out of “autopilot” by a novel event. This experience suggests that although our attentional system has been tuned to take advantage of our previous experience, our attentional system can detect events that do not fit with that prior experience and adjust accordingly.

Although our attentional system is tuned to both familiarity and novelty, how our mind accomplishes this feat is an unresolved issue, and a central question for cognitive psychologists, computational modelers, and neuroscientists (Grossberg, 1987; Johnston & Hawley, 1994; Sokolov, 1963; Treisman, 1992). The almost paradoxical ability of our mind to resolve this familiarity/novelty issue was elegantly underscored by Treisman (1992):

By creating accumulated traces of past perceptual objects or events, the world molds our mind to recreate earlier experiences. At the same time, we retain an impressive capacity also to represent any new object that fails to find its match in our prior assembly of stored tokens. (p. 874).

This distinction between recreating prior experiences on one hand, and representing novel experiences on the other hand, must be central to the sensitivity of the attentional system to both familiarity and novelty. Beyond this intuition, however, the answer to *how* our attention system accomplishes this feat is largely unknown from a mechanism point of view.

Our memory of prior experiences plays an important role in determining how attention is allocated to events in the present. Consider the analogy of attentional processes behaving like a quarterback in a football game. A quarterback’s performance largely depends on being able to find an open player and then pass the ball to that player, all in a limited window of time. Yet, if the quarterback has accrued a lot of prior practice plays, then the quarterback can rely on those prior experiences to instinctively guide where the ball will be thrown on a current play. Our attentional processes can operate similarly when encountered with a familiar experience. A familiar experience can be used to recruit a host of prior experiences that can automatically guide our attentional processes (Logan, 1988). The way in which our attentional processes functioned in the past can then rapidly determine how our attentional processes will function in the current context.

Familiarity in our environment may serve as the context that cues the recruitment of relevant prior experiences. By this view, the attentional processes that were useful at the time prior experiences occurred can be recruited and subsequently guide how attentional processes function in the present. The recruitment of attentional processes has been demonstrated empirically in the paradigms of visual search (Chun & Jiang, 1998; Wang, Cavanagh, & Green, 1994), negative priming (Deschepper & Treisman, 1996; Neill, Valdes, Terry, & Gorfein, 1992; Neill & Valdes, 1992), task-switching (Mayr & Kliegl, 2000; Wazak, Hommel, & Allport, 2003), and inhibition of return (Tipper, Grison, & Kessler, 2003; Grison, Paul, Kessler, & Tipper, 2005; Wilson, Castel, & Pratt, 2006). The type of mechanism proposed to recruit attentional processes differs from paradigm to paradigm, but how attentional processing functions for familiar events is largely dependent on how attentional processing functioned in the past.

In contrast, a novel experience cannot recruit attentional processes from related prior experiences simply because *there are no related prior experiences from which to recruit*. Therefore, the allocation of attention to novel experiences must follow a different principle. To this end, an attentional mechanism that can rapidly detect novel events and represent them in memory may be critical for processing novelty in our environment. Again, the quarterback analogy provides a suitable description of how attentional processes can differ between familiar and novel experiences. If a quarterback was prepared to run a practiced play, but an unexpected event occurred (e.g., a defender was coming in for a tackle), then the quarterback’s performance would depend on being able to shift out of the practice play mode and rapidly detect where to throw the ball. The current play would no longer fit within the planned scheme (i.e., prior experience), but instead would be represented as a unique play that could subsequently serve as a prior experience if that particular play were encountered in the future.

The attentional processes that underlie the encoding of novel events are proposed to have an inhibitory, or suppressing, effect on processes that underlie the recruitment of familiar events. Importantly, the putative function of inhibiting familiar events is to enhance the encoding and representation of novel events (Johnston, Hawley, & Farnham, 1993; Hawley, Johnston, Farnham, 1994). Curiously, the notion of an inhibitory attentional mechanism has reared its influence across the same paradigms that were also argued to be dependent on the recruitment of prior experiences: visual search (Klein, 1988; Klein & MacInnes, 1999; Wang & Klein, 2009), negative priming (Tipper, 1985; Tipper & Cranston, 1985), task switching (Allport, Styles, & Hseih, 1994; Mayr & Keele, 2000; Rogers & Monsell, 1995), and inhibition of return (Posner & Cohen, 1984; Taylor & Klein, 1998). The specifics of the inhibitory mechanism differ across each paradigm, but the general function is to slow the processing of familiar events in favour of processing of novel events.

The familiarity/novelty issue that was central in the quarterback analogy may be equally central to performance in a range of experimental paradigms in cognitive psychology, including those mentioned above (visual search, negative priming, task switching, inhibition of return), and perhaps also repetition blindness (Kanwisher, 1987, 1991) and visual marking (Watson & Humphreys, 1997). At first glance, it may appear improbable that any single principle involving attention to familiar/novel objects would capture variability in performance across all of these paradigms. However, to evaluate this proposal carefully, it is worth taking a close look at these paradigms in some detail. Three such paradigms are examined in detail below.

**Negative Priming**

The term “negative priming” was introduced by Tipper (1985) to refer to an effect that measures the inhibition of representations associated with irrelevant distractor objects. In particular, the distractor inhibition account of negative priming assumes that it measures a selective attention mechanism that allows attention to be directed to relevant information presented amidst irrelevant information. Typically, the negative priming effect is measured using a task that includes prime and probe events. A prime containing a target and distractor is presented first, followed by a probe that also contains a target and distractor. The task instruction is to attend selectively and respond to the target in each of the prime and probe displays. Importantly, in the ignored repetition condition, the target in the probe display matches the prime distractor that was just previously ignored. Performance in this condition is compared to that in the control condition, in which the probe target is unrelated to both the prime target and distractor. Responses are typically slower in the ignored repetition condition than in the control condition. According to the distractor inhibition hypothesis, this effect is attributed to a selective attention mechanism that inhibits processing of ignored, or unattended, information.

From another perspective, however, the negative priming effect may reflect a processing advantage for relatively novel events (targets in the control condition) over familiar events (targets in the ignored repetition condition) when looking at the effect through the familiarity/novelty lens. In support of this alternative perspective, Milliken, Joordens, Merikle and Seiffert (1998) noted that negative priming effects can occur without the requirement of selective attention in the prime display. Milliken et al. used a procedure in which only one stimulus (e.g., the word ‘TABLE’) was presented briefly and then masked in the prime display. A probe display subsequently followed that required participants to respond selectively to a target word presented in red that was interleaved with a distractor word presented in green. Responses were slower when the target word matched the prime word compared to when the target word mismatched the prime word. As resolving competition between prime target and distractor could not possibly have caused this effect, Milliken et al. attributed this effect to a mechanism that oriented more effectively to targets in the control condition (i.e., relatively novel targets) than to targets in the ignored repetition condition (i.e., relatively familiar targets).

It is worth noting that performance is not always less efficient for familiar than for novel targets in negative priming experiments. In particular, in the attended repetition condition of many negative priming experiments, participants respond to a probe target that matches the prime target. In this condition, at least when the task requires identification of target objects, performance is more efficient for the attended repetition condition than the control condition. In this case, as noted above, the probe target may cue the retrieval of encoding procedures used to encode the prime target. This facilitation effect offers a view of the other side of the familiarity/novelty issue of central interest in this thesis.

**Repetition Blindness**

Kanwisher (1987) noted that when the same object is presented twice in a rapid serial visual presentation (RSVP) stream, the second occurrence of that object is often not reported. This effect is commonly known as the repetition blindness (RB) effect (Kanwisher, 1987; 1991; Park & Kanwisher, 1994). One account of the RB effect focuses on the distinction between encoding experiences as episodic representations, called *tokens*, or semantic representations, called *types*. According to this hypothesis, recognizing an event depends on two mechanisms: type activation and token individuation. Type activation involves gaining access to the semantic information about an event. For example, if a blue jay lands on a tree branch, then an observer would recognize the blue jay as being an example of the general category of birds; that is, activation of semantic information associated with the type representation of birds would allow the observer to assign meaning to their current experience. At the same time, particular episodic details of the experience, such as where and when the blue jay was observed, are stored as part of the token representation of that event. Kanwisher argued that when the same event occurs twice in relatively rapid succession, the token representation of the second event may not be individuated from that of the first event. Consequently, participants may fail to report two tokens of the same event.

The token individuation account of RB effects fits well with the view that attention is guided by both familiarity and novelty. In particular, the RB effect can be thought of as occurring because a mechanism dedicated to encoding representations of relatively novel events is not activated for repeated items within the RSVP stream. In effect, the encoding of token representations of familiar events is disrupted. Along these lines, Johnston and Hawley (1994) proposed that the RB effect results from the suppression of bottom-up processing of familiar perceptual input.

At the same time, Kanwisher (1987) pointed out that repetition priming for repeated events within an RSVP stream can also be measured. This repetition priming effect was observed specifically when the repeated event was the final event in the RSVP stream. Again, the more efficient encoding and response to a repeated target under these conditions illustrates that attention can benefit from both novelty and familiarity.

**Visual Marking**

Visual marking (VM) refers to a visual search mechanism that appears to inhibit the selection of “old” (i.e. familiar) objects by prioritizing the selection of novel objects (Watson & Humphreys, 1997; Watson, Humphreys, & Olivers, 2003). The typical task used to study the VM effect has participants perform a conjunction search for a predefined target (e.g., a red “E”) object that appears amongst a number of distractors (i.e., an array of red “F” and blue “E” distractors). In a typical conjunction search task, the time to locate the target increases with the number of distractors. However, if one set of distractor stimuli (e.g. only the blue “E” distractors) is previewed briefly before the onset of the other set of distractors and the target, then search times for the target decrease substantially. The benefit in search times for targets in the preview condition compared to the conjunction search condition is attributed to a selective attention mechanism that inhibits the previewed distractors from capturing attention. In other words, the mechanism underlying the VM effect actively biases attention against selecting old objects.

The VM effect is commonly interpreted to reflect a paradigm-specific selection mechanism, rather than one of a number of effects that reveal a common bias favouring attention orienting to novelty. In particular, research in the domains of inhibition of return (Posner & Cohen, 1984) and attention capture to new objects (Yantis & Jonides, 1984; 1990) could be argued to point to visual search mechanisms that favour orienting to novel over familiar search targets. With this in mind, it may be more parsimonious to view VM effects as caused by one and the same mechanism as inhibition of return and attention capture by new objects. Yet, VM has been argued to differ from effects measured with other attentional paradigms, as it appears to involve the inhibition of multiple old objects in parallel, its time course appears to differ from other effects, and it shows a different sensitivity to luminance changes than other effects (Donk & Theeuwes, 2001). Whether or not the VM effect reflects an entirely distinct mechanism from that underlying other attentional orienting effects, there is little doubt that it reflects an example of how attentional selection for the purpose of perceptual encoding can favour novel over familiar objects.

**Paradigm Specific Mechanisms versus Broad Theoretical Principles**

Whether a broad theoretical framework can encompass the results of all of the above paradigms is an open question, and one that will not be resolved in this thesis. However, using a “broad strokes” principle to understand how an attentional mechanism that underlies performance in one paradigm might also underlie performance in a separate paradigm holds the potential benefit of parsimony. The alternative approach would assume that each of the attentional paradigms selectively tap into separate paradigm-specific mechanisms, each of which happen to favour the perceptual encoding of novel events over familiar events.

Application of a familiarity/novelty principle that can tie together a wide variety of effects under the same theoretical umbrella constitutes a broad long-term goal. However, a first step towards this goal would be to examine how familiarity-based and novelty-based mechanisms interact within a particular attentional paradigm. As such, the focus of this thesis was to study how the familiarity/novelty principle could be applied within a paradigm used to study an effect known as inhibition of return (IOR).

**Inhibition of Return**

Orienting attention to visual events in our environment may seem like a routine process, but it is more complex than it appears. To orient attention efficiently around an environment, orienting must be flexible. In particular, attention must orient rapidly to both familiar events that are well predicted by prior experience and to unexpected novel events. Without the ability to orient attention efficiently toward novelty in the environment, we would be stuck efficiently processing only those experiences with which we have already had experience. Such a system would not seem well suited to an activity that humans engage in routinely; that is, engaging in learning of new properties of an environment.

The IOR effect is a behavioural effect argued to reflect a particular attentional mechanism that allows for rapid orienting to novel locations. In the seminal paper on the IOR effect, Posner and Cohen (1984) discovered what at that time was a counterintuitive result – detecting visual events that occurred in locations at which attention had recently been captured was surprisingly poor. Posner and Cohen used an abrupt peripheral onset spatial cueing task to measure this effect. In this type of spatial cueing task, a non-predictive spatial cue is often presented briefly in one of two peripheral locations. The spatial cue is usually a brief flash, and it functions to orient attention to the location at which it occurs. Following the offset of the cue, a variable duration of time then passes before the onset of a visual target. The task of the participant is then to detect the onset of the visual target, which appears with equal likelihood in either the cued location or the uncued location. Posner and Cohen observed that when the time between the onset of the cue and the onset of the target, the cue-target stimulus-onset asynchrony (SOA), was relatively short, responses to detect targets that appeared in cued locations were faster than responses to detect targets that appeared in uncued locations. Importantly, the opposite pattern of performance was observed when the SOA was longer (greater than 300 ms); that is, responses to detect targets in cued locations were slower than for targets in uncued locations. The slower reaction time for detecting cued targets relative to uncued targets was the critical result given the name ‘inhibition of return’ (Posner et al., 1985).

The IOR label is now often used both as a name for the spatial cueing effect *and* as a description of the mechanism assumed to produce the effect. As the name implies, IOR is thought to reflect a mechanism that inhibits the return of attention to locations at which it has been previously oriented (Posner et al., 1985; Rafal et al., 1989). Interestingly, the effect of a separate facilitative mechanism is also expressed at locations recently captured by attention (Jonides, 1981). As such, there is an apparent struggle between a facilitative mechanism that favours attentional processing at cued locations and an inhibitory mechanism that favours attentional processing at uncued locations.

**Inhibition of Return as a Consequence of Reorienting Attention**

The reorienting hypothesis (Posner & Cohen, 1984; Posner et al., 1985; Klein, 2000) proposes that the disengagement of attention from a cued location is the mechanistic trigger that inhibits subsequent processing at that location (Prime, Visser & Ward, 2006). Support for the reorienting hypothesis stems from the dependence of spatial cueing effects on cue-target SOA. At short cue-target SOAs, the facilitative effect for targets that appear at cued locations is generally attributed to the automatic capture of attention at the location of the cue. However, as time passes from cue onset, and because cues are non-predictive, there is no benefit to maintaining attention at the cued location. As such, participants are presumed to re-orient attention to the central location, so as to be equally prepared to orient to either the cued or uncued location (Danziger & Kingstone, 1999; Ivanoff & Klein, 2001). It is at the point of disengaging attention that an inhibitory mechanism is presumed to occur, which then slows the reorienting of attention to the cued location. In tasks that require target detection, the cue-target SOA at which spatial cueing effects change from a facilitative effect to an IOR effect is often about 300 ms. According to the re-orienting hypothesis, then, the disengagement of attention that produces the IOR effect occurs about 300 ms after onset of the cue in simple detection tasks.

The re-orienting hypothesis can also explain why the SOA at which facilitation gives way to IOR varies as a function of task. In particular, the SOA at which the IOR effect emerges is later in discrimination tasks than in detection tasks (Lupiáñez, Milán, Tornay, Madrid, & Tudela, 1997). One way to explain this finding within the re-orienting hypothesis framework is to suggest that more difficult tasks increase the attentional dwell time at the cued location (Klein, 2000; but see Lupiáñez, Milliken, Solano, Weaver, & Tipper, 2001). If the cue is processed more deeply when the task is more demanding, then attention may disengage from the cue at a later point in time than is observed in detection tasks.

Finally, the re-orienting hypothesis appears to fit well with the fact that, in at least some circumstances, the IOR effect can be affected by presentation of a central cue between presentation of peripheral cue and target. Posner and Cohen (1984) introduced presentation of a central cue between peripheral cue and target with the idea that it would ensure disengagement of attention from the peripheral cue in a stimulus-driven manner. Although presentation of a central cue appears not to be critical to IOR measured with detection tasks, several studies have shown that a central cue can impact IOR in discrimination tasks (Cheal & Chastain, 1999; Kingstone & Pratt, 1999; Pratt & Abrams, 1999; Pratt, Kingstone, & Khoe, 1997; Prime & Ward, 2004). In particular, whereas IOR typically emerges at relatively long SOAs (e.g., 700 ms; Lupiáñez et al., 1997) in discrimination tasks when no central cue is used, IOR effects emerge at shorter SOAs in discrimination tasks when a central cue is used (Prime et al., 2006). This result is consistent with the reorienting hypothesis in the sense that orienting attention to a central cue might force disengagement to occur at an SOA at which it would not have occurred without the central cue. In turn, disengagement of attention may then trigger the inhibitory mechanism that underlies the IOR effect.

Taken together, the time course and the effect of a central cue on spatial cueing effects are consistent with the reorienting hypothesis for IOR. In particular, these findings suggest that the IOR effect may be generated by the disengagement of attention from a peripheral spatial cue. In the following section, several results are summarized that challenge the reorienting hypothesis.

**Challenges to the Reorienting Hypothesis**

Whereas the reorienting hypothesis assumes that IOR is triggered by the disengagement of attention from a peripheral cue, several studies have reported IOR effects under conditions where attention is presumably still maintained at the cued location (Berlucchi, Tassinari, Marzi, & Di Stefano, 1989; Chica, Lupiáñez, & Bartolomeo, 2006; Lambert, Spencer, & Hockey, 1991; Tassinari, Agliotti, Chelazzi, Peru, & Berlucchi, 1994). For example, Tassinari et al. conducted a study in which the typical facilitative effect at cued locations did not precede the inhibitory effect at cued locations, even when the cue-target SOA was reduced to zero. If there is no initial facilitative effect for cued locations, there seems no need to attribute IOR to the disengagement of attention from the cued location. In another study, the likelihood that the target would appear in the cued location was manipulated, which allowed measurement of IOR for both expected and unexpected locations (Lupiáñez, Decaix, Sieroff, Chokron, Milliken & Bartolomeo, 2004). Importantly, IOR was observed for both expected and unexpected locations, implying again that IOR does not depend on the disengagement of attention from the cue; in this case, IOR occurred at a location from which attention was not disengaged (see also Berlucchi, Chelazzi & Tassinari, 2000; Berger, Henik & Rafal, 2005).

These findings have led some researchers to consider an alternative to the re-orienting hypothesis. Specifically, rather than assuming that exogenous spatial cueing effects are guided by a facilitatory mechanism at short cue-target SOAs and then by an inhibitory mechanism at longer cue-target SOAs, it may be that both mechanisms simultaneously contribute to attentional orienting at all cue-target SOAs. From this perspective, whether the cueing effect is facilitative or inhibitory (IOR) is then dependent on the net effect of the two opposing attentional mechanisms (Berlucchi, 2006; Danziger & Kingstone, 1999; Francis & Milliken, 2003, Klein, 2000; Tassinari et al., 1994; Tipper et al., 1997). From this perspective, the conventional facilitative spatial cueing that occurs at short cue-target SOAs implies that the facilitative component of spatial cueing outweighs the inhibitory component, rather than that the inhibitory component does not exist at all. Similarly, the emergence of IOR that typically occurs at longer cue-target SOAs implies either that the facilitative component decreases, or that the inhibitory component increases, with increases in cue-target SOA, and not that the inhibitory component is triggered only at longer cue-target SOAs.

Another implication of this alternative to the re-orienting hypothesis is that the type of task used to measure spatial cueing effects ought to affect whether IOR is observed. If a particular set of task characteristics happens to increase the contribution of the facilitative component of spatial cueing effects, then the IOR effect might well occur only at longer cue-target SOAs, or perhaps not at all. In contrast, if a particular set of task characteristics happens to eliminate the facilitative component of spatial cueing effects, then the IOR effect might well occur at all cue-target SOAs.

The task dependence of IOR in a variety of visual search contexts was tested in a study by Dodd, Van der Stigehel and Hollingsworth (2009). Four groups of participants viewed the same scenes, but the task was manipulated such that one group of participants performed a conventional visual search task, another group performed a memorization task, another group performed a pleasantness-rating task, and the last group freely viewed the scene. Regardless of the type of task, participants were instructed that a probe would appear in the scene, and when it appeared they should immediately fixate their eyes on the probe. The probe was presented to allow for a measure of saccadic reaction time to the location of the probe. Importantly, the probe could occupy a location that was fixated on previously or it could occupy a novel location. An IOR effect based on saccadic reaction time was determined by comparing how fast participants made a saccade to a probe that appeared in a novel location compared to a probe that appeared in a previously fixated location. Dodd et al. found that an IOR effect was observed in the visual search task, but not in any of the other tasks. In fact, a facilitation of return (FOR) effect was observed in the three remaining tasks; that is, participants were faster at making a saccade to a previously fixated location than to a novel location. As a consequence of these results, the authors suggested that biased attention in favour of novelty (i.e., an IOR effect) is not a standard result in all spatial orienting contexts, but instead will occur when a set of task characteristics emphasizes the efficient detection of novelty in a visual scene. In contrast, when a task emphasizes the construction of a memory representation across time, then attentional orienting may be guided by a mechanism that is sensitive to familiarity rather than novelty.

A related study that examined attention orienting in normal scene viewing was conducted by Smith and Henderson (2009). These researchers looked at the probability of participants re-fixating at a previously fixated location or at a location directly opposite from a previously fixated location. In addition, they looked at the time it took for participants to initiate a fixation to the previously fixated location compared to a location directly opposite to the previously fixated location. Smith and Henderson found that there was a greater probability of re-fixating previously fixated locations than locations directly opposite previously fixated locations. Yet, this result occurred together with a temporal delay in initiating fixations to previously viewed locations. These opposing effects were taken as evidence that spatial attention was being guided simultaneously by two opposing mechanisms. While one mechanism inhibited the orienting of attention to familiar locations, a separate mechanism was facilitating the orienting of attention to familiar locations. This finding is consistent with the view that both familiarity and novelty can bias spatial orienting, and that in any given experimental context spatial orienting effects will depend on the extent to which particular task demands tap into the process(es) that bias attentional orienting toward familiarity or novelty, respectively.

The finding that IOR effects differ in detection and discrimination tasks (Lupiáñez et al., 1997) fits nicely with this view. In a detection task, performance requires a rapid response to the onset of a new event. Due to the relatively simple nature of the task, there may be no utility in relying on a memory representation of the cue to generate a response to the target; indeed, a prior cue may interfere with perception of onset of a target at the same location. As a result, any mechanism that biases orienting to familiar events would serve as a source of interference in a detection task. In contrast, performance in a discrimination task hinges more on analytical perceptual processing, which could divert attention from the task of detecting onsets, which in turn would allow the spatial overlap between cue and target to play a larger role in determining orienting. From this point of view, it is not surprising that IOR is more evident in detection than in discrimination tasks.

**The Repetition Effect: An Obstacle to Measuring Inhibition of Return**

The repetition effect in two-alternative forced-choice tasks was first studied over 40 years ago (Bertelson, 1961). The task used by Bertelson required participants to respond to the onset of a light presented just left or right of fixation by pressing a response key with either the left or right hand. Bertelson recorded participants’ RTs for repeated and alternated targets with either a very short (50 ms) or long (500 ms) temporal interval between the release of response to one target and the onset of the following target. The results indicated that when the temporal interval between trials was short, RTs were significantly faster for repeated targets than for alternated targets even when the two target types were equally likely. The faster RTs to repeated signals compared to alternated signals was labeled the repetition effect.

Bertelson (1963) was interested in testing two possible broad interpretations of the repetition effect:

The existence of the repetition effect suggests either (a) that different mechanisms are involved in reactions to repeated signals and in reactions to new signals, or (b) that the same mechanisms are involved but work faster in the case of repetitions, due to some sort of facilitative aftereffect. (Bertelson, 1963)

In the end, he concluded that neither of those two possible accounts was likely to be entirely correct, and that although there did appear to be a process that allowed particularly fast responses to repetitions, that process was not relied upon for responses to repetitions in all cases. In effect, he pointed to the possibility that performance on repeated trials could depend on either of two processes, and that one of those processes must certainly offer a performance advantage for repeated trials relative to alternated trials.

The nature of the process that affords this advantage for repeated trials has been the subject of considerable study since the seminal work of Bertelson (1961). Many of these studies (e.g., Pashler, 1991; Rabbitt, 1968; Smith, 1968) converged on the contemporary view that response to a first event leads response codes to be bound together with perceptual codes in an episodic representation that Hommel (1998) calls an event file (see also Kahneman, Treisman & Gibbs, 1992). On repeated trials in a 2-afc task, retrieval of the event file from the immediately preceding trial allows participants to respond without engaging in a more time consuming analytic response selection process that follows the stimulus-response (S-R) mapping rule (see also Logan, 1988). Within the context of Bertelson’s (1961) first interpretation of the repetition effect, the mechanism that he described as being involved in responses to “repeated signals” can be characterized as an episodic integration process. According to that view, an episodic integration process contributes to performance when the current perceptible event shares the same S-R mapping as a previously experienced event. Repeating the S-R mapping allows for the processing of two matching events to be integrated into a single event file, or episode, which provides a selective performance benefit for repeated events.

However, a second implication of Bertelson’s (1961) seminal work has received less study. In particular, if responses to repeated trials can be facilitated by episodic integration processes, but yet responses to repeated trials are sometimes driven by processes other than episodic integration, then how does repetition affect these other processes? The challenge in answering this question is that these effects are likely to be obscured in many empirical contexts by episodic integration processes that produce robust repetition benefits. Thus, if repetition were to produce a subtle effect on some other process(es) that opposes the robust repetition benefit produced by episodic integration processes, this effect would be difficult to detect in performance. In fact, detection of this effect in performance would require a form of task analysis that teases apart the influence of repetition on these two processes. This conceptual issue is central to the empirical work in this thesis, which aims specifically at the notion that IOR-like effects with non-spatial stimuli are obscured by episodic integration effects that produce the opposite effect.

**Non-Spatial Inhibition of Return**

Although IOR effects are conventionally measured with exogenous spatial cueing procedures (Posner & Cohen, 1984), a small number of studies have focused on the possibility that IOR-like effects can occur with non-spatial stimuli. Kwak and Egeth (1992) were the first researchers to focus on the consequences of exogenous orienting to both spatial and non-spatial attributes. They argued that if the IOR effect reflects an inhibitory mechanism related uniquely to spatial orienting, then an IOR effect should occur for cued spatial attributes but not for cued non-spatial attributes. Kwak and Egeth used a continuous-responding procedure, which required participants to detect the onset of a target event on every trial. In other words, with this procedure, the “cue” for the current target on trial *N* was the previous target on trial *N*-1. A noteworthy property of the continuous-responding procedure is that IOR effects observed with this procedure cannot be attributed to response inhibition caused by the withholding of a response to a preceding cue. Importantly, the target on each trial could match or mismatch the target on the preceding trial in either or both of location and colour. Kwak and Egeth observed an IOR effect for spatial location but not for color. That is, responses were slower for location repetitions than for location alternations from one trial to the next, but not different for colour repetitions and colour alternations. These results led researchers at the time to conclude that IOR was an effect uniquely related to spatial orienting.

A similar set of results was reported by Tanaka and Shimojo (1996). They employed a continuous-responding procedure in which participants performed four different tasks across separate experimental sessions: a detection task, a location discrimination task, a color discrimination task, and an orientation discrimination task. Across the four tasks, the same stimuli were used and the cueing effect was measured as the difference between response times to targets appearing in the cued location (i.e., the previous target location) and targets appearing in the uncued location. Tanaka and Shimojo observed an IOR effect for the detection and location discrimination tasks, which replicated numerous prior studies (Kwak & Egeth, 1992; Maylor, 1985; Posner & Cohen, 1984). However, a “facilitation of return” (FOR) effect was observed for the colour discrimination and orientation discrimination tasks.

Tanaka and Shimijo proposed this set of results to be consistent with separate contributions of the “what” versus “where” pathways to the various tasks. In particular, detection and location discrimination task effects were proposed to be products of the dorsal visual pathway, while color and orientation discrimination effects were proposed to be products of the ventral visual pathway (Livingstone & Hubel, 1988). The putative function of the “where” pathway is to orient attention rapidly to novel, or unexpected, events that are outside the current focus of attention. Attentional processing at novel locations should be facilitated along this pathway, at the cost of inhibiting attentional processing at the currently attended location. Conversely, the “what” pathway is assumed responsible for identifying objects through in-depth feature analysis. The type of feature analysis associated with the “what” pathway may be related to the creation and integration of episodes. The separate functions of the mechanisms along the “what” and “where” pathways are not questioned here, but subsequent research suggested that it may be problematic to assume that there is a one-to-one relation between these two visual pathways on the one hand, and the two opposing effects of repetition (IOR and FOR) on the other hand.

Law, Pratt, and Abrams (1995) were the first researchers to report a non-spatial IOR effect. Law et al. required participants to detect the onset of a target colour patch that was preceded by a cue colour patch of the same or different colour. Importantly, a neutral colour patch – that is, a color patch different in color from the cue and target – was presented temporally between the cue and target in one experiment, but not in a second experiment. Across both experiments, the cue, target, and neutral color patches were all presented centrally, and the task was simply to detect the onset of the target color patch. Responses were slower for cue and target color patches that matched than for cue and target color patches that mismatched, but only when a neutral color patch was presented temporally between the cue and target.

Law et al. (1995) argued that the failure to observe non-spatial IOR effects in the continuous-response procedure used by Kwak and Egeth (1992) was that attention was continuously maintained on a non-spatial attribute. The neutral colour patch in their procedure was meant to disrupt the maintenance of attention from the color of the cue, which in turn would allow IOR to be measured. According to this view, just as removal of attention from a cued location is necessary to observe spatial IOR (but see Berlucchi, Chelazzi & Tassinari, 2000; Berger, Henik & Rafal, 2005; Lupiáñez et al., 2004), removal of attention from a non-spatial cue may be necessary to observe non-spatial IOR.

**Biphasic Time Course?**

The non-spatial effect reported by Law et al. (1995) was met with skepticism, as it was noted that the inhibitory effect reported by Law et al. did not follow the same biphasic time course found in spatial cueing studies (Taylor & Klein, 1998b). Taylor and Klein used the same non-spatial cueing task as Law et al., with the exception that they included relatively short SOAs that fell within the typical facilitatory period of the biphasic time course. Response times were slower for cued than for uncued trials across the entire time course (i.e., for both short and long SOAs). Given that spatial cueing effects are often facilitative at short SOAs, with IOR emerging only at longer SOAs, this result might be taken to imply that the effect reported by Law et al. is caused by a different mechanism than that which causes spatial IOR effects.

At the same time, it has since been shown that the typical biphasic time course found in spatial cueing studies can also be found in non-spatial cueing studies, albeit when a discrimination task rather than a detection task is used (Francis & Milliken, 2003; Hu & Samuel, 2011). In both of these studies, participants performed a non-spatial discrimination response to a target that was preceded by a matching or mismatching cue. Again, the type of task, detection versus discrimination, was shown to have a profound influence on the type of cueing effect that was observed. Both sets of researchers attributed the initial facilitative effect found in discrimination tasks to an event-file updating mechanism that masked an opposing inhibitory mechanism at relatively short SOA intervals. In contrast, at longer SOA intervals, the influence of the event-file integration process dissipates, and gives way to an opposing mechanism that speeds the processing of novel events.

**Cue-Target versus Target-Target Procedures**

In addition to the time course issues discussed above, a challenge to the view that non-spatial IOR-like effects have the same cause as spatial IOR effects concerns the nature of the task used to measure these effects. In particular, until recently, non-spatial IOR-like effects had been reported only with cue-target procedures, and not with target-target procedures. Yet, spatial IOR effects had long been known to occur in both cue-target (Posner & Cohen, 1984) and target-target procedures (e.g. Maylor, 1985).

To appreciate why this task issue might be meaningful, consider that in a cue-target procedure, a cue is presented but not responded to, and then followed by a target that is responded to. In contrast, in a target-target procedure, a target is presented and responded to, and then followed by a second target that is also responded to. An important implication of this task difference is that cued trials in a cue-target procedure may match in location (or colour), but yet mismatch in terms of the response coding for cue (withhold a response) and target (respond). In contrast, cued trials in a target-target procedure would match both in terms of location (or colour) and in terms of response coding for cue and target. Consequently, IOR effects observed with cue-target procedures could be related to the response mismatch between cue and target, whereas for target-target procedures this cannot be the case. In turn, it could be argued that non-spatial IOR-like effects, having been observed only with cue-target procedures, may be due to response mismatches rather than a “true” IOR process (Welsh & Pratt, 2006). Indeed, the notion that response mismatches of this sort slow performance is consistent with a wide array of studies that show similar “partial match” costs (see Hommel, 1998; 2004; Hommel, Musseler, Aschersleben, & Prinz, 2001).

In contrast, partial match costs cannot account for spatial IOR effects that have been observed with target-target procedures. Furthermore, if there is a common mechanism underlying spatial and non-spatial IOR effects, and if this mechanism is related broadly to orienting attention to novelty, then it should be possible to observe IOR effects using target-target procedures in both spatial and non-spatial domains. However, our preceding discussion suggests that non-spatial IOR-like effects in a target-target procedure may well be difficult to observe, as they may be obscured by robust facilitative effects of repetition that co-determine the effect of repetition on performance.

**Overview of the Empirical Work in the Thesis**

Following the above logic, the premise for the first empirical study in this thesis was that the contribution of episodic integration processes to performance in the 2-afc task would have to be disrupted for a non-spatial IOR-like effect to be observed. The results from this study show very clearly that non-spatial IOR-like effects can be observed in a target-target procedure if participants are required to respond to an intervening event between targets. We presume that the response to the intervening event does the job of disrupting processes that produce a facilitative effect that opposes the IOR-like effect.

The second study examined in more detail the processing of the intervening event necessary to produce the non-spatial IOR-like effect. Whereas responding to an intervening event is certainly sufficient to produce the non-spatial IOR-like effect, the results from this study show that it is not necessary. Rather, it appears that engagement in response selection may be the critical process.

The third study introduced spatial variation to our non-spatial 2-afc procedure, with the aim of resolving some contradictions between previously published results, and the theoretical framework introduced to explain the results of the prior two studies. The results of this study revealed that the introduction of spatial variation may change fundamentally how event representations are encoded, which in turn can have a strong influence on repetition effects. Indeed, the results demonstrate precisely the same stimuli can produce qualitatively opposite repetition effects in contexts with and without spatial variation.

The final study describes some preliminary results that examine the relation between repetition effects in our procedure, and subjective expectancy. Whereas one might be tempted to attribute repetition effects in such a simple procedure to mechanisms of expectancy, the results from this study show that there is no simple mapping between repetition effects in our 2-afc task and the subjective expectancies of repetition reported by our participants.

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CHAPTER 2: Response to an intervening event reverses nonspatial repetition effects in 2AFC tasks: Nonspatial IOR?

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Preface

The IOR effect has long been viewed as a spatial attention phenomenon. By this view, the attentional mechanism underlying the effect is a dedicated spatial orienting mechanism that preferentially orients attention to novel locations. Nonetheless, a small number of studies have tested whether similar IOR-like effects can be measured in non-spatial orienting tasks. Despite several studies that have demonstrated IOR-like effects with non-spatial stimulus dimensions, critics have pointed out that non-spatial IOR-like effects likely tap into an attentional mechanism that is different from the mechanism responsible for spatial IOR.

One of the key limitations of previous studies that have attempted to measure non-spatial IOR effects is that they have used a Cue-Target (CT) rather than a Target-Target (TT) procedure. In fact, several studies that have used TT procedures have observed the directionally opposite effect of IOR – a benefit rather than a cost of repetition. These results are important precisely because spatial IOR effects have been observed with both CT and TT procedures. If the mechanism responsible for spatial IOR can also produce a non-spatial IOR effect, then it ought to be possible to produce non-spatial IOR effects with a TT procedure. The aim of the present study was to examine why TT procedures often produce a repetition benefit rather an IOR-like pattern of results, and to create a TT procedure capable of measuring non-spatial IOR.

Abstract

The repetition effect in 2-afc tasks is a cornerstone effect in human cognition. Yet, the experiments described here show that the customary benefit of repetition reverses to a cost of repetition when participants respond to an irrelevant event between targets. In Experiments 1A-C, participants made manual 2-afc decisions to both of two consecutive targets in a trial, and on some trials also made a manual response to an intervening event that appeared between the two targets. A repetition benefit was observed when no intervening event appeared, whereas a repetition cost was observed when a response was required to an intervening event. Experiment 2 ruled out a solely strategic interpretation of the repetition cost effect observed on intervening event trials. In Experiments 3A-B, an intervening event that required a simple vocal response “Go” also produced a repetition cost. In Experiment 4, a repetition cost was observed when the intervening event was changed to a tone presented aurally. In Experiment 5, the repetition benefit was observed when a response was withheld to intervening event. A dual process interpretation of these results is discussed, with one process related to episodic integration, and the other related to processes that produce inhibition of return.

Keywords: attention; inhibition of return; priming

Introduction

Cognitive psychologists measure performance in carefully designed tasks, with the idea that these measures tell us something useful about basic cognitive processes. If experimental tasks mapped neatly, in a one-to-one relation, onto specific cognitive processes, then our job would be relatively simple. Yet, tasks often measure more than one cognitive process, and worse still, varying a task parameter can result in an effect on more than one of those processes. Although problems associated with the lack of process purity of tasks have taken on a high profile in the memory literature (Jacoby, 1991), they may be equally problematic in other research domains. In this manuscript, we focus on this potential problem in the attention and performance domain, and in particular in studies that measure trial-to-trial repetition effects.

The general point made in this article is that an assumption of process purity with respect to a very simple task, that used to measure repetition effects in two-alternative forced choice (2-afc) tasks, may have obscured the fact that inhibition of return (IOR; Posner & Cohen, 1984) can be measured in both spatial and non-spatial tasks. We report a series of experiments that require participants to make 2-afc decisions to non-spatial properties (e.g., colour, size, identity) of targets on all trials. The key result reported here is that under conventional testing conditions we observe faster responses for repetitions than for non-repetitions, as might be expected. Yet when participants are forced to respond to an event intervening between prime and target, we observe the opposite effect; that is, slower responses for repetitions than for non-repetitions. This result suggests that more than a single process underlies repetition effects in 2-afc tasks, and that one of these processes may be similar to that which underlies inhibition of return (Posner & Cohen, 1984) in tasks that measure spatial repetition effects.

To set the context for the empirical work reported here, the remainder of the introduction addresses the following three issues. First, we describe the spatial inhibition of return effect and discuss briefly how it is interpreted. Second, we review briefly the literature that addresses whether inhibition of return like effects can occur outside the domain of spatial orienting studies. Finally, we introduce our research strategy, in which we measure repetition effects in simple 2-afc tasks that require a response to targets on all trials. The key variable that we manipulate in these experiments is the presence or absence of a requirement to respond to an intervening event between consecutive targets. As mentioned above, the result of note is that repetition priming is observed without an intervening event, while the opposite result is observed when participants are required to respond to an intervening event.

*Spatial Inhibition of Return*

A spatial cueing method using non-predictive peripheral cues is typically used to measure the inhibition of return effect. This method involves presenting an abrupt onset cue at one of two or more spatial locations, and then after some time has elapsed a target appears at either the cued peripheral location or some other location (Posner & Cohen, 1984; see Klein, 2000; Lupiáñez, Klein & Bartolomeo, 2006; Taylor & Klein, 1998a for useful reviews). The relative proportions of cued and uncued trials are set so that the cue does not provide predictive information about the location of the following target, thus allowing the inference that cueing effects tap into reflexive orienting processes, rather than strategic orienting processes. When the time between onset of cue and target is relatively short, say less than about 300 ms, responses to detect, localize, or identify the target are typically faster for cued than for uncued trials. In contrast, when the time between onset of cue and target is longer than about 300 ms, responses are typically slower for cued than for uncued trials (see also Lupiáñez, Milán, Tornay, Madrid, & Tudela, 1997; Samuel & Kat, 2003 for a more detailed discussion of time course issues). This effect was labeled ‘inhibition of return’ to reflect the idea that attention may initially be captured by the abrupt onset cue, but then inhibited from returning to that location after it has been disengaged and re-oriented to the central fixation location. Following many other researchers, we use the acronym IOR to refer to this effect throughout this article.

In large part, the term IOR has come to take on meaning that is tied quite specifically to shifts of attention in space. The underlying assumption is that attention acts as a “spotlight” that shifts from location to location in the visual field, and objects subject to the beam of this spotlight are processed with greater efficiency than objects in regions lying outside of the beam (Posner, 1980). By this view, IOR effects occur because attention is inhibited from shifting back to locations or objects that have already been attended. This theoretical account has appeal in terms of adaptive utility, in that the search of a visual environment would be inefficient if attention re-orients continuously to locations that have already been searched (Klein, 1988). Instead, an attentional bias that favors novel locations would support more efficient coverage of the search space. This proposed link between IOR and spatial orienting is broadly consistent with an oculomotor hypothesis offered by Rafal, Calabresi, Brennan & Sciolto (1989), in which they suggest that IOR may be directly related to activation of the oculomotor system that accompanies the programming of eye movements (but see Chica, Klein, Rafal, & Hopfinger, 2010).

In addition to empirical links to oculomotor control, the idea that IOR reflects a dedicated spatial orienting mechanism stems, at least in part, from the assumption that repetition of non-spatial stimulus dimensions (e.g., colour, form) results in repetition priming rather than IOR. Indeed, there are plenty of studies in the literature that demonstrate benefits rather than costs of stimulus repetition (Bertelson, 1961; Kirby, 1976; Kornblum, 1973; Maljcovic & Nakayama, 1994; Rabbitt, 1968), to the point that repetition priming is a cornerstone construct in most introductory courses in human cognition. Nonetheless, a small number of studies has examined whether, when tested under conditions like those used in studies of spatial orienting, repetition of non-spatial dimensions might produce an IOR-like result rather than repetition priming. A brief review of these studies follows.

*Non-Spatial Inhibition of Return?*

Although most research conducted on IOR rests on the assumption that it reflects a process dedicated to the control of spatial orienting, a small number of studies have examined whether a similar effect occurs with non-spatial stimuli. For example, Kwak and Egeth (1992) presented stimuli to the left or right of central fixation, and the participants’ task was simply to detect the onset of target stimuli on all trials, much as in prior studies of IOR. Importantly, the stimuli on consecutive trials matched or mismatched not just in location, but also in colour. It had previously been shown that IOR for repeated locations occurs even when participants make a response to all targets in a series of trials (i.e., a target-target procedure), and not only when participants withhold a response to a cue and then respond to a following target (i.e., a cue-target procedure; Maylor & Hockey, 1985). In accord with this prior research, Kwak and Egeth (1992) observed slower responses to targets that appeared in the same location as an immediately preceding target than to targets that appeared in the location opposite the immediately preceding target. At the same time, responses were faster for targets that matched in color than for targets that mismatched in color with the immediately preceding target. In others words, these researchers observed an IOR effect with respect to spatial location together with a facilitation effect for color, suggesting that IOR indeed may be limited to conditions that measure the control of spatial orienting (see also Tanaka & Shimojo, 1996; Fox & De Fockert, 2001; Taylor & Donnelly, 2002).

However, Law, Pratt, and Abrams (1995) noted that attention might not have been disengaged effectively from the target color from one trial to the next in the Kwak and Egeth (1992) study. They addressed this issue using a task that required detection of a target following presentation of a cue that either matched or mismatched the target in color. All cues and targets were squares presented centrally, and importantly a neutral color square that matched neither the cue nor target was presented at a temporal position between the cue and target. The rationale for the use of this neutral stimulus was that it ought to disengage attention from the color of the preceding cue, and if IOR requires disengagement of attention from the cue, then an IOR-like effect for color repetition might well occur here where it failed to occur in the study by Kwak and Egeth (1992). The results were in accord with this prediction; responses were slower for targets that matched the color of the preceding cue, and this IOR-like effect was observed only when a neutral distractor was presented between cue and target.

Taylor and Klein (1998b) examined whether the color-based effect reported by Law et al. follows the same time course as spatial IOR. In particular, in studies of spatial orienting, one often observes facilitation for short cue-target SOAs that gives way to IOR at longer cue-target SOAs. In contrast, Taylor and Klein (1998b) found that Law et al.’s color-based repetition cost was insensitive to cue-target SOA, and therefore concluded that it was not caused by the same mechanism as spatial IOR (see also Fox and de Fockert, 2001, for a similar interpretation). Although this specific conclusion can be debated (Francis & Milliken, 2003; see also General Discussion), perhaps the more important point raised by Taylor and Klein (1998b) is a general one, that non-spatial repetition costs in performance may or may not be caused by the same process that underlies spatial IOR effects.

Following on this theme, our concern here is how one might go about evaluating whether the same or different processes underlie spatial IOR effects and non-spatial repetition effects. One straightforward approach would be to measure spatial and non-spatial repetition effects under comparable conditions (e.g., Kwak & Egeth, 1992), and then to compare whether the effects are qualitatively similar, or qualitatively different. However, even if spatial and non-spatial repetition *effects* appear to be qualitatively different, there is no guarantee that each of the effects measure one and only one process, and therefore also no guarantee that the processes underlying the two effects are qualitatively different. A concrete description of this problem in the current research context may make this issue more transparent.

Consider one of the simplest methods for measuring repetition effects, the 2-afc task. Here, participants are asked to hit one of two response keys for either of two possible targets on each trial. In a task that required participants to *identify* target letters on each trial, Bertelson (1961) noted long ago that responses to repeated targets were faster than those to alternating targets, and related results have since been reported by many researchers in a variety of tasks (Maljcovic & Nakayama, 1994; Hillstrom, 2000; Huang, Holcombe, & Pashler, 2004; Campana & Casco, 2009; Kristjánsson & Campana, 2010). Yet, it has also long been known that when participants are required to *locate* target stimuli on each trial of a 2-afc task, responses are typically slower for repeated targets than for alternating targets; in other words, an IOR effect is observed (Maylor & Hockey, 1985). Given these qualitatively different effects of repetition for non-spatial and spatial stimulus dimensions, it is tempting to conclude that the process that underlies IOR in the spatial 2-afc task plays no role in determining the repetition effect in the non-spatial 2-afc task.

However, this conclusion follows logically only if the repetition effects measured in the two tasks are determined by one and only one process. If this assumption were incorrect, then some form of process analysis would be necessary to determine whether the process underlying spatial IOR effects might also contribute to non-spatial repetition effects. In particular, the non-spatial repetition effect could owe to the joint contribution of two processes; one that speeds responses for repetitions relative to alternations, and another that slows responses for repetitions relative to alternations, with the sign of the repetition effect ultimately determined by the relative strength of these two processes. Indeed, a similar dual process framework has been offered within the spatial orienting literature to explain the time course of IOR (Klein, 2000), a framework that also fits some compelling spatial orienting data gathered from split-brain patients (Tipper et al., 1997).

*The Present Study*

Following the dual process logic described above, the present set of experiments asked whether a non-spatial 2-afc task might reveal an IOR-like cost of repetition under testing conditions designed to disrupt the process responsible for repetition benefits. To address this issue, we followed a precedent set in prior studies showing that an event intervening between cue and target is sometimes necessary to observe IOR-like effects in studies of non-spatial orienting (Law et al., 1995; Fox & de Fockert, 2001). However, we surmised that the mere presentation of a “neutral” intervening event might not be sufficient to entirely disrupt the process responsible for repetition benefits in non-spatial 2-afc tasks. In particular, we assume that processes that allow participants to respond to trial n by retrieving a stimulus-response episode of trial n-1 play an important role in producing repetition benefits in non-spatial 2-afc tasks (Hommel, 1998; Logan, 1988; Pashler & Baylis, 1992; Rabbitt, 1968). Given that disruption of this type of event integration process was the goal, the starting point for our study was to evaluate the influence of requiring participants to attend and respond to a “neutral” event that intervened between two targets in a non-spatial 2-afc task.

As such, the procedure in the experiments reported here was straightforward. Participants were required to respond by identifying a non-spatial property of a target stimulus presented centrally for two consecutive displays on each trial. On half of the trials, the target stimuli were identical in consecutive displays, while on the other half of the trials the targets were different. In addition, we manipulated whether an intervening event did or did not occur between presentations of the two consecutive targets. Following Bertelson (1961) and many other researchers, when no intervening event occurred between consecutive targets, we expected faster responses when the targets were identical than when they were different; that is, a repetition priming effect ought to be observed. The more critical issue was whether the requirement to attend and respond to an intervening event between consecutive targets would alter this pattern of results. In particular, we were interested in whether response to a task-irrelevant intervening event would disrupt the process that speeds responses for repeated relative to not-repeated trials, and thereby reveal the influence of a process that slows responses for repeated relative to not-repeated trials.

In Experiments 1A, 1B, and 1C, we examined the influence of an intervening event on non-spatial repetition effects using colours (Experiment 1A), line lengths (Experiment 1B), and words (Experiment 1C) as stimuli in a 2-afc discrimination task. In all of these experiments the intervening event was a small colored circle that appeared centrally and that participants responded to by pressing both of two response keys upon its onset. Indeed, in all three experiments repetition costs were observed, but only when an intervening event was responded to between consecutive targets. In Experiment 2, we confirmed that this effect was not due to a simple strategy difference for intervening event and no-intervening event trials presented in separate blocks, as the same effects were observed when these two trials types were mixed within the same block. In Experiments 3A and 3B, we examined whether the timing of the intervening event relative to the first and second targets was critical, as well as whether the repetition costs observed in Experiments 1A, 1B, and 1C depend on the particular response made to the intervening event. In these experiments, we observed a repetition cost in the intervening event condition that did not depend on the timing of the intervening event, and that occurred despite a change in the modality used to respond to the intervening event. In Experiment 4, the intervening event was changed from a visual stimulus (a red dot) to an auditory tone, and the results were the same; a repetition cost was observed only in the intervening event condition. Finally, in Experiment 5 we tested our initial assumption that the mere presentation of an intervening event would not be sufficient to observe an IOR-like repetition effect in non-spatial 2-afc tasks. Indeed, in this experiment we observed a repetition benefit both with and without an intervening event.

Experiments 1A, 1B and 1C

On each trial in Experiments 1A, 1B, and 1C, a single target appeared in a first display, which we call T1. Participants were to respond manually to this target, which ultimately led to the onset of a second target, which we call T2. A manual response that followed the same stimulus-response mapping as for T1 was required for T2. T1 and T2 could be either of two stimuli. They were identical on half of the trials and different on half of the trials, and thus the identity of T1 provided no predictive information about the identity of T2. In Experiment 1A, participants were required to discriminate whether T1 was a blue or yellow rectangle, and then do the same for T2. In Experiment 1B, participants discriminated whether T1 was a short or long line, and then did the same for T2. Finally, in Experiment 1C, participants discriminated whether T1 was the word “Left” or “Right”, and then did the same for T2.

In addition, for half of the trials in each of the two repetition conditions, a red dot appeared centrally after response to T1 and prior to onset of T2. On trials with this intervening event, participants were asked to respond to its onset by pressing both response keys simultaneously, which then initiated presentation of T2. Our objective was to compare repetition effects for trials that included an intervening event to those for trials that did not include an intervening event.

Again, our logic was as follows. We assumed that an episodic integration process facilitates responses to repeated trials relative to not-repeated trials under conventional 2-afc testing conditions. This assumption is consistent with a wide range of theoretical proposals both within the 2-afc repetition effect literature (Rabbitt, 1968; Pashler & Baylis, 1991), and in the broader attention and performance literature (Hommel, 1998; Kahneman, Treisman, & Gibbs, 1992; Logan, 1988; Logan, 1990). The central idea is that onset of a target event can cue the retrieval of a memory representation in which various attributes of a prior target event, including the response made to that target, are bound together. On repeated trials, the retrieval of this memory representation offers a more efficient basis of responding to the current target than application of the analytic stimulus-response rule. We assume further that response to an intervening event might selectively disrupt this episodic integration process. If these two assumptions hold, then the intervening event condition should allow us to evaluate whether repetition effects in non-spatial 2-afc tasks are co-determined by two processes; one that speeds responses to repeated relative to not-repeated trials, and another that slows responses to repeated relative to not-repeated trials. In particular, if response to an intervening event disrupts the episodic integration process, then responses might well be slower for repeated trials than for not-repeated trials.

Method

*Participants*

All participants were recruited from an introductory psychology course or a second year cognitive psychology course from McMaster University, and participated for course credit. All participants had normal or corrected-to-normal vision.Twenty-five, seventeen, and eighteen undergraduate students participated in Experiments 1A, 1B, and 1C, respectively.

*Apparatus and Stimuli*

All experiments were run on a PC using MEL experimental software. Subjects sat directly in front of a 15” SVGA computer monitor, at adistance of approximately 57 cm. A plus sign was presented as the fixation point in the center of the screen, and subtended a visual angle of 0.6 degrees horizontally and 0.7 degrees vertically. The target stimuli were presented centrally against a black background.

In Experiment 1A, both T1 and T2 were either a blue or yellow rectangle, and each subtended a visual angle of 6.3 degrees horizontally and 1.2 degrees vertically. In Experiment 1B, two white lines that differed in length were used as T1 and T2 rather than two rectangles that differed in color. The short line subtended a visual angle of 1.75 degrees horizontally and the long line subtended a visual angle of 6.75 degrees horizontally. Both short and long lines subtended a visual angle of 0.2 degrees vertically. In Experiment 1C, the words “Right” and “Left” were used as T1 and T2. The word “Right” subtended a visual angle of 4.0 degrees in width and the word “Left” subtended a visual angle of 3.0 degrees in width, while both words subtended a visual angle of 1.5 degrees in height. In all three experiments, the intervening event was a red dot presented centrally, with radius subtending .25 degrees of visual angle.

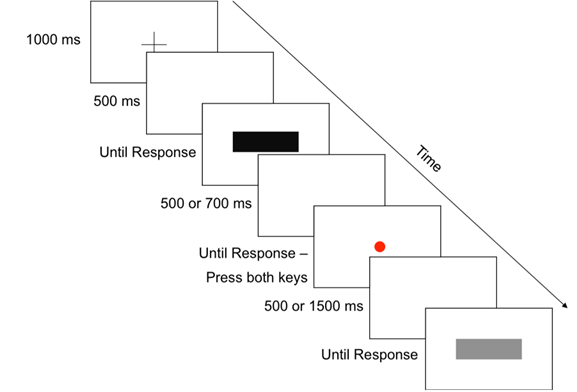
*Procedure and Design*

The experiment consisted of two blocked conditions; an intervening event condition and a no-intervening event condition. Each condition had an initial practice block consisting of 16 trials, followed by nine experimental blocks of 16 trialseach.

For both conditions, each trial began with the appearance of a fixation cross in the middle of the computer screen for 1000 ms, and then a blank screen for 500 ms. In the no-intervening event condition, T1 then appeared and remained on the screen until the participant made a key press response. A blank interval of variable length, either 1500 ms or 2500 ms**,** then followed after the key press to T1. T2 was then presented and remained on the screen until the participant made a second key press response. Participants were instructed to press the “/” key to indicate the presence of a blue rectangle and to press the “z” key to indicate the presence of a yellow rectangle in Experiment 1A. Experiments 1B and 1C used similar response mappings, with the “z” key corresponding to the short line in Experiment 1B, and to the word “Left” in Experiment 1C. Participants used the index finger of their left hand to press the ”z” key and the index finger of their right hand to press the “/” key. Response time was measured as the latency between onset of the target stimulus and key press response.

The intervening event condition differed from the no-intervening event condition from the point after the participant responded to T1. A blank interval of either 500 ms or 700 ms followed response to T1. The length of this interval was chosen at random between these two values with the intention of producing some temporal uncertainty as to the onset of the intervening event. Following the blank interval, the intervening event (a red dot) appeared and remained on the screen until the participant pressed both the “z” and the “/” keys in unison. After this response to the intervening event, a blank interval of either 500 or 1500 ms occurred prior to onset of T2. These intervals were chosen so as to roughly equate the response-stimulus interval (RSI) for T1 and T2 across the intervening event and no-intervening event conditions, assuming a mean response time for the intervening event of about 400 ms. Two RSI conditions (500 and 1500 ms) were included in the design merely as an exploratory measure of the time course of the repetition effect. The different RSIs were not meant to be contrasted against the short and long time intervals typically used in IOR studies, since even the shortest RSI condition in the intervening event condition exceeded the shortest time interval typically used to measure facilitation effects. As was the case for T1, T2 remained on the screen until participants responded to its identity by pressing the “/” key or the “z” key.

For both the intervening event condition and the no-intervening event condition, task instructions were displayed on the screen prior to the start of the practice block. Prior to each block of trials within each condition, the message “Press B to begin block” appeared, allowing participants to rest between blocks when needed. For all trials in both conditions, there was a 2000 ms inter-trial interval that started once a response was made to T2. The procedure for trials in both conditions is displayed in Figure 1.



*Figure 1.* The sequence of events for a not-repeated trial in the intervening event condition of Experiment 1A is shown. In the experiment, the darker rectangle would have been blue and the lighter rectangle would have been yellow. In the no-intervening event condition (not shown), the intervening event was replaced by a blank screen that remained for approximately the same length of time as the intervening event. Experiments 1B and 1C were identical in design with the rectangles replaced by short and long lines, or the words “Right” and “Left”, respectively.

Three within-subject variables were manipulated in the experiment; intervening event (no intervening event/intervening event); repetition (repeated/non-repeated) and response-stimulus interval (RSI: 1500ms/2500ms). The order in which subjects performed the two intervening event conditions (intervening event condition first or second) was the only between-subject variable, and was counterbalanced across participants. The intervening event condition was manipulated between blocks. In the no-intervening event condition, participants responded to T1 and T2 without the appearance of a red dot between T1 and T2, whereas in the intervening event condition participants responded to T1, then to the intervening red dot by pressing both response keys, and then to T2. Repetition was manipulated within blocks. In the repeated condition, T1 and T2 were identical colored rectangles (Experiment 1A), lines (Experiment 1B), or words (Experiment 1C) whereas in the not-repeated condition, T1 and T2 were different. RSI was also manipulated within blocks. In the no-intervening event condition, the 1500 ms and 2500 ms RSI conditions were measured precisely as the latency between response to T1 and onset of T2, whereas in the intervening event condition these RSI values were approximate, as described above.

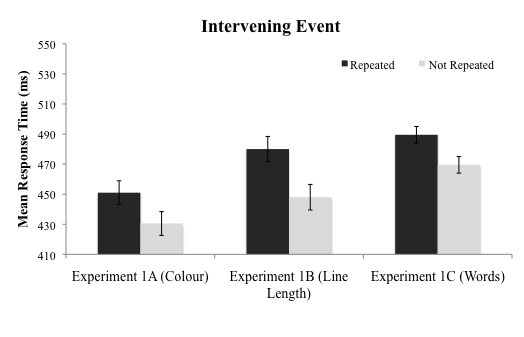
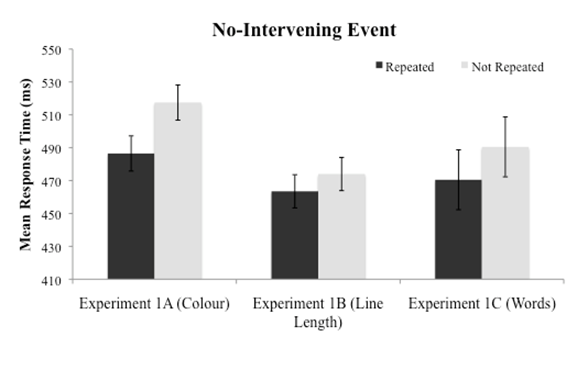
Results

As our primary interest was in performance for T2 as a function of its relation to T1, a trial was coded as correct if responses to both T1 and T2 were correct, and as an error if response to T1 was correct and that to T2 was incorrect, or vice-versa. Response times (RTs) for correct responses to T2 on all trials in Experiments 1A, 1B, and 1C were submitted to an outlier analysis that eliminated suspiciously long RTs (Van Selst & Jolicoeur, 1994). These outlier analyses eliminated 3.2%, 2.9%, and 3.0% of correct RTs from further analysis in Experiments 1A, 1B, and 1C, respectively. Mean RTs and error rates for each condition in each experiment were then computed based on the remaining observations, and these data were submitted to mixed factor analyses of variance that included Repetition (repeated or not-repeated), RSI (short or long), and Intervening event (intervening event or no-intervening event) as within-subject factors and Order (intervening event condition first or second) as a between-subjects factor. The alpha criterion was set to .05 for all analyses. Means RTs and error rates for each condition, collapsed across participants, are listed in Table 1, and displayed in Figure 2 collapsed across participants and RSI (Table 2).

Our initial RT analysis included Order as a variable to examine whether our counterbalancing manipulation interacted with any of the primary effects of interest. Although Order did enter into a significant interaction in three of the experiments reported here, it was a different interaction in each case, and in no case did Order modulate the two-way interaction between Repetition and Intervening Event that is of most interest here. As such, Order was omitted as a variable in the final analyses of both RTs and error rates, leaving us with repeated measures designs with Repetition, Intervening Event, and RSI as factors.

*Table 1.* Mean response times and error rates for T2 (ms) for each condition in Experiments 1A, 1B, 1C, 2, 3A, 3B, 4, and 5.





*Figure 2.* Top Panel: Mean response times for T2 in Experiments 1A, 1B, and 1C (no-intervening event condition), collapsed across RSI and participants. Bottom Panel: Mean response times for T2 in Experiments 1A, 1B, and 1C (intervening event condition), collapsed across RSI and participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Experiment 1A*

The data from one participant were not included in the final analyses reported here because the RTs from this participant were more than two standard deviations slower than the mean RT of all participants. All analyses were conducted with and without the data from this participant, and exclusion of this participant’s data did not change the pattern of results significantly.

In the analysis of RTs, there was a significant interaction between Intervening Event and Repetition, F(1,23) = 29.11, p < .001, hp2 = .56. To examine this interaction in more detail, simple main effects of repetition were analyzed separately for the intervening event and no-intervening event conditions. In the intervening event condition, RTs were slower for repeated (451 ms) than for not-repeated trials (430 ms), F(1,23) = 13.48, p < .001, hp2 = .39. In contrast, in the no-intervening event condition, RTs were faster for repeated (486 ms) than for not-repeated trials (518 ms), F(1,23) = 10.21, p = .004, hp2 = .31.

There was one additional significant statistical effect in the overall analysis of RTs that is of less theoretical significance, but that we report here for the benefit of the reader. In particular, there was a significant main effect of Intervening Event, F(1,23) = 7.20, p = .01, hp2 = .24. Responses to T2 were faster with an intervening event (441 ms) than without an intervening event (502 ms). This effect may have occurred because the intervening event acted as a warning signal to allow participants to better predict the onset of T2 (Bertelson, 1967). In the analysis of error rates there were no statistically significant effects.

*Experiment 1B*

In the analysis of RTs, there was a significant three-way interaction between Repetition, Intervening Event, and RSI, F(1,16) = 15.42, p = .004, hp2 = .41. To examine this interaction further, separate ANOVAs were conducted for the short and long RSIs.

For the short RSI condition, there was a significant interaction between Repetition and Intervening Event, F(1,16) = 15.63, p = 0.001, hp2 = .49. Responses were slower for repeated trials (491 ms) than for not-repeated trials (447 ms) in the intervening event condition, F(1,16) = 24.10, p < .001, hp2 = .60, whereas responses were numerically faster for repeated trials (463 ms) than for not-repeated trials (482 ms) in the no-intervening event condition, although this latter effect only approached significance, p < .10. In other words, the pattern of results for the short RSI condition was similar to that observed in Experiment 1A.

For the long RSI condition, the interaction between Repetition and Intervening Event failed to reach significance, p = 0.06. However, the direction of the interaction is consistent with that observed in the short RSI condition, with the only difference being that the repetition priming effect for the short RSI appeared not to persist to a long RSI for the no-intervening event condition.

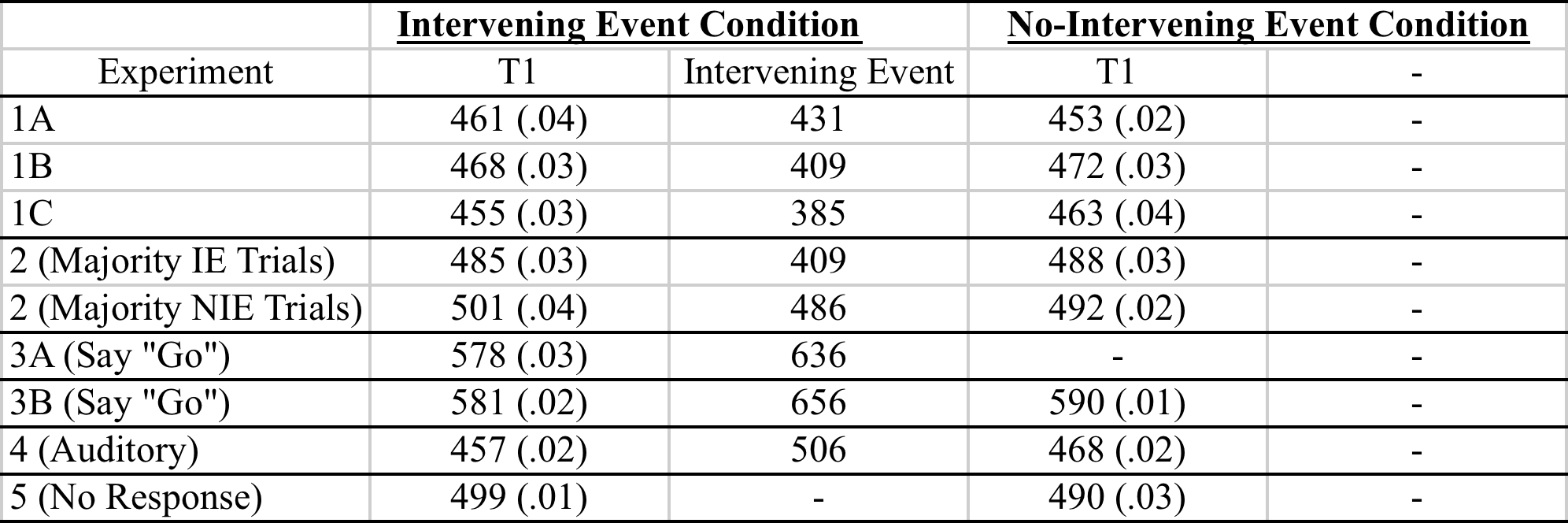
In the analysis of error rates, there was a significant interaction between Repetition and Intervening Event, F(1,16) = 5.25, p = .03, hp2 = .23. To examine this effect further, separate analyses were conducted for the intervening event and no-intervening event conditions. In the intervening event condition, more errors were made on repeated trials (.04) than on not-repeated trials (.02), F(1,16) = 5.28, p = .03, hp2 = .24. In the no-intervening event condition, the difference in error rates between repeated (.01) and not-repeated conditions (.03) was in the opposite direction but failed to reach significance, p > .10. Importantly, the direction of this interaction is consistent with that reported for the mean RTs, ruling out a speed-accuracy tradeoff interpretation of the RT results.

*Experiment 1C*

In the analysis of RTs, there was a significant interaction between Intervening Event and Repetition, F(1,17) = 16.51, p < .001, hp2 = .49. This interaction was examined further by conducting separate analyses for the intervening event and no-intervening event conditions. In the intervening event condition, responses were slower for repeated trials (489 ms) than for not-repeated trials (470 ms), F(1,17) = 18.87,p < .001, hp2 = .53. In contrast, in the no-intervening event condition, responses were faster for repeated trials (471 ms) than for not-repeated trials (490 ms), F(1,17) = 5.84, p = .03, hp2 = .26.

There were no other significant effects in the analysis of RTs, and no significant effects in the analysis of error rates.

*Table 2.* Mean responses times and error rates for T1 and the intervening event (where applicable) in Experiment 1A, 1B, 1C, 2, 3A, 3B, 4, and 5.



*Discussion*

The results of the three experiments were all very similar and straightforward. One critical result is that responses were faster for repeated targets than for not-repeated targets in the no-intervening event condition. This result should not come as a surprise, as similar results were first reported in 2-afc tasks half a century ago (e.g., Bertelson, 1961). Clearly, there is nothing inherent in our procedure that makes observing repetition priming effects for consecutive targets difficult to measure. With this result as context, the results in the intervening event condition are striking. With an intervening event, responses were slower for repeated than for not-repeated targets. As noted above, we assume that the intervening event eliminated, or greatly reduced, the contribution of an episodic integration process that facilitates performance for repeated relative to not-repeated targets. In the absence of such a process, an effect is revealed that implicates more efficient processing of not-repeated relative to repeated targets.

Although the overall pattern of results was quite consistent across the experiments, the repetition benefit in the no-intervening event condition was not significant for the long RSI condition in Experiments 1B and 1C. This result may be related to one reported long ago by Kirby (1976), in which the repetition benefit declines with increasing RSI and ultimately reverses to a repetition cost in some cases. Kirby (1976) attributed this effect to an increasing expectation for alternation with increasing RSI. This issue is discussed in more detail in the General Discussion, but the key point here is that the reversal of the repetition effect as a function of the intervening event manipulation, which is observed in the short RSI condition of all of the experiments, merits further study.

Experiment 2

In Experiments 1A-C, intervening event trials were presented in a separate block from no-intervening event trials. As a result, the qualitatively opposite repetition effects for these two conditions could be attributed to different block-wide strategies adopted by participants. To address whether this was the case, intervening event trials and no-intervening event trials were mixed at random throughout the testing session in Experiment 2. To address further the contribution of strategies to the different repetition effects for intervening event and no-intervening event trials, the relative proportions of these two trial types was manipulated between two groups of participants. For one group (80/20 group), intervening event trials occurred 80% of the time and no-intervening event trials occurred 20% of the time. For the other group (20/80 group), these proportions were reversed. To the extent that the blocked manipulation of the intervening event conditions was responsible for the results of Experiment 1A-C, then different results ought to be observed in the 80/20 group than in the 20/80 group in this experiment. In particular, repetition effects ought to align with the trial type that is most frequent for that group; repetition costs for the 80/20 group and repetition benefits for the 20/80 group. However, if strategies play a minimal role in the results observed in Experiments 1A-C, then repetition effects ought to align with the intervening event type, regardless of group, with repetition costs observed for intervening event trials and repetition benefits observed for no-intervening event trials.

Method

*Participants*

Twenty-four McMaster Universityundergraduate students recruited from either an introductory psychology course or a second year cognitive psychology course participated for course credit or $10 remuneration. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

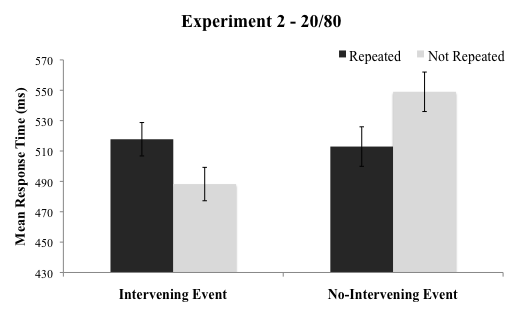
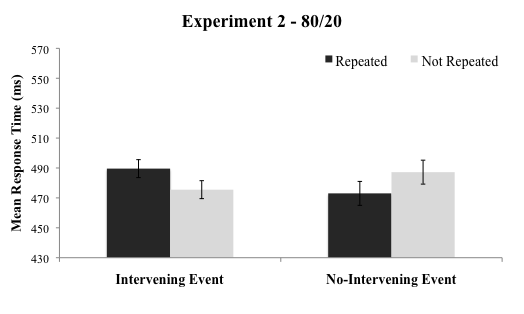
The apparatus and stimuli used in this experiment were the same as in Experiment 1A.

*Procedure and Design*

The procedure used in this experiment was the same as in Experiment 1A with the following exceptions. Instead of presenting the two intervening event conditions (intervening event/ no-intervening event) in separate blocks, intervening event trials and no-intervening event trials were mixed at random across the experimental session. Additionally, participants were randomly assigned to one of two experimental groups: the 80/20 group had 80% intervening event trials and 20% no-intervening event trials, while the 20/80 group had the reverse proportions of these two trial types.

Results

Correct trials were defined as in Experiments 1A, 1B, and 1C. RTs for correct responses to T2 were submitted to the same outlier elimination procedure used in prior experiments (Van Selst & Joliceur, 1994), which resulted in the exclusion of 2.2% of the response times from further analysis. Mean RTs in each condition were then computed based on the remaining observations, and these mean RTs and corresponding error rates were submitted to mixed analyses of variance that treated Repetition (repeated or not-repeated), RSI (short or long), and Intervening event (intervening event or no-intervening event) as within-subject factors and Proportion (80/20 or 20/80) as a between-subject factor. The alpha criterion was set to .05 for all analyses. Means of mean RTs and error rates for each condition, collapsed across participants, are listed in Table 1, and the mean RTs are displayed in Figure 3.



*Figure 3.* Top Panel: Mean response times for T2, collapsed across RSI and participants, are shown for the 80/20 group. Bottom Panel: Mean response times for T2, collapsed across RSI and participants, are shown for the 20/80 group. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

In the analysis of RTs, there was a significant main effect of Proportion, F(1,24) = 8.9, p = .042, hp2 = .27. Responses by participants in the 80/20 group were faster (481 msec) than responses by participants in the 20/80 group (517 msec). Tellingly, Proportion did not interact with any of the other within-subject factors, which suggests that strategies adopted for the majority trial type played little role in determining the repetition effects.

However, as in Experiments 1A-C, there was a significant interaction between repetition and intervening event, F(1,24) = 21.9, p < .0001, hp2 = .65. Simple main effect analyses were performed to interpret this interaction. In the intervening event condition, responses were slower for repeated trials (504 ms) than for not-repeated trials (482 ms), F(1,12) = 14.1, p = .0028, hp2 = .54. In the no-intervening event condition, responses were faster for repeated trials (492 ms) than for not-repeated trials (518 ms), F(1,12) = 13.9, p = .0029, hp2 = .53.

There were no significant effects in the analyses of error rates.

*Discussion*

The results of Experiment 2 show the same pattern of repetition effects as in Experiments 1A-C despite the intervening event manipulation being intermixed rather than blocked. A repetition cost was observed in the intervening event condition, and a repetition benefit was observed in the no-intervening event condition. Moreover, there was no evidence that manipulating whether intervening event trials constituted the majority or minority of the trials impacted this pattern of results. Clearly, the pattern of repetition effects observed in Experiments 1A-C and Experiment 2 cannot be explained by reference to strategy differences allowed by presenting the two intervening event conditions in separate blocks.

Experiments 3A and 3B

Experiments 3A and 3B were similar to Experiment 1A in that manual two-alternative forced choice responses were required to T1 and T2. However, these experiments were conducted to address two important issues related to our dual process interpretation of the results of the prior experiments. One issue concerned the relation between the response made to the intervening event and the responses made to T1 and T2. To address whether making task-relevant responses to the intervening event (i.e., responding to T1, T2, and the intervening event with the same buttons) is critical to the result observed in Experiments 1A-C, in this experiment participants were required simply to say “Go” aloud upon onset of the intervening event. If making task-relevant responses to the intervening event is critical to the repetition costs observed in Experiments 1A-C, then a similar repetition cost should not be observed here. In contrast, if some more general form of engagement of attention in the intervening event is critical, we might well observe the same repetition cost in this experiment as in prior experiments.

A second issue concerned the timing of the intervening event relative to T1 and T2. The rationale for manipulating this factor was rooted in the potential for episodic memory to explain both repetition benefits in the no-intervening event condition and repetition costs in the intervening event condition. Hommel (1998; see also Kahneman, Treisman & Gibbs, 1992; Milliken, Joordens, Merikle & Seiffert, 1998; Neill & Mathis, 1998 for related discussions) outlined a framework for interpreting repetition effects in which onset of a target cues the retrieval of a representation of the immediately preceding stimulus-response episode, which he called an event file. A principle that predicts performance efficiency well across a broad range of experimental contexts is that partial matches between event files for consecutive targets slow performance relative to both perfect matches and complete mismatches. According to this principle, performance might well be slow for repeated targets in the intervening event condition because the intervening event disrupts the perfect match between T1 and T2. Indeed, processing of the intervening event might well be bound temporally to the processing of T1, implying that a repeated T2 would cue the retrieval not only of T1 processing, but of T1 processing bound together with that associated with the intervening event. In turn, retrieval of processing associated with the intervening event might interfere with processing of a repeated T2, and ultimately slow responding.

In Experiments 3A and 3B, we tested the possibility that the intervening event introduces retrieval interference by virtue of being temporally bound to the T1 stimulus-response episode. Our initial hypothesis was that intervening events presented closer in time to T1 would be more likely to be encompassed in the same episodic representation than intervening events presented further away in time from T1, resulting in greater partial match costs when T2 is identical to T1. Experiments 3A and 3B differed only in that no-intervening event trials were not included in Experiment 3A, while equal numbers of intervening event and no-intervening event trials were intermixed at random in the test session of Experiment 3B. Our aim here was to examine whether uncertainty about the eventual occurrence of an intervening event (in Experiment 3B) would modulate the episodic integration processes described above. To foreshadow our results, the timing of the intervening event had no influence on the repetition effect at all, and this was true both when intervening events occurred on every trial (Experiment 3A) and when intervening event and no-intervening event trials were intermixed at random (Experiment 3B. As such, we found no evidence favoring the partial match interpretation offered above.

Method

*Participants*

Twenty-four McMaster Universityundergraduate students (twelve participants in Experiment 3A and twelve participants in Experiment 3B) recruited from either an introductory psychology course or a second year cognitive psychology course participated for course credit. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

The apparatus and stimuli used in this experiment were the same as in Experiment 1A, with the exception that a voice key was used to record the onset of the participant’s response to the intervening event.

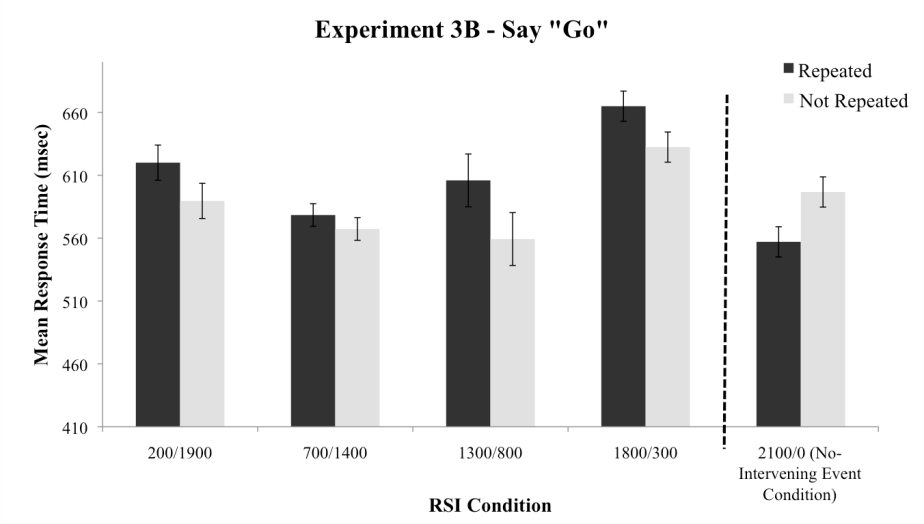
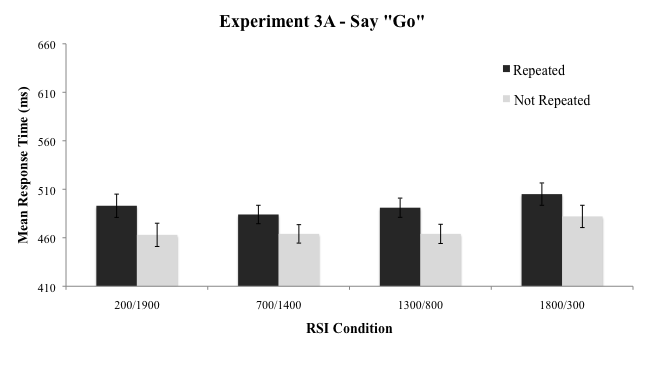
*Procedure and Design*

The procedure used in this experiment was the same as in Experiment 1A with the following exceptions. First, participants were instructed to say “Go” when the intervening dot was presented between T1 and T2. A voice key detected the onset of this vocal response and the red dot was immediately removed from the screen. The experimenter coded each trial as usable or a spoil. A spoil was defined as any trial in which a noise other than that of the participant’s “Go” response triggered the voice key. Data from spoiled trials were not included in any further statistical analysis. Second, the time intervals between response to T1 and onset of the intervening event (RSI-1) and between response to the intervening event and onset of T2 (RSI-2) varied within-subject and randomly from trial to trial across four levels. In the 200/1900 condition, RSI-1 was 200 ms while RSI-2 was 1900 ms. The remaining conditions were labeled and defined similarly as 700/1400, 1300/800, and 1800/300. The sum of RSI-1 and RSI-2 in all conditions was 2100 ms, and assuming a 400 ms response time for the intervening event the RSI between response to T1 and onset of T2 was the same as the long RSI in Experiments 1A-1C.

The only distinction between Experiments 3A and 3B was that no-intervening event trials were not included in Experiment 3A, while equal numbers of intervening event and no-intervening event trials were mixed at random within the tests session in Experiment 3B.

Results

Correct trials were defined as in Experiments 1A, 1B, and 1C. RTs for correct responses to T2 were submitted to the same outlier elimination procedure used in prior experiments (Van Selst & Joliceur, 1994), which resulted in the exclusion of 2.8% (Experiment 3A) and 3.0% (Experiment 3B) of the RTs from further analysis[[1]](#footnote-1). Additionally, 1.8% of trials in Experiment 3A and 2.4% of trials in Experiment 3B were excluded because of microphone failures to intervening event responses. Mean RTs in each condition were then computed based on the remaining observations, and these mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that treated Repetition (repeated/not-repeated) and RSI (200/1900, 700/1400, 1300/800, 1800/300) as within-subject factors for Experiment 3A. The same within-subject factors were included in analysis of intervening event trials in Experiment 3B, and an additional analysis was conducted for the no-intervening event trials only. The alpha criterion was set to .05 for all analyses. Means of mean RTs and error rates for each condition in Experiment 3A, collapsed across participants, are listed in Table 1 along with the mean RTs and error rates for each condition in Experiment 3B. The mean RTs for both Experiment 3A and 3B are displayed in Figure 4.



*Figure 4.* Top Panel: Mean response times for T2 across four different RSI conditions in Experiment 3A, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions. Bottom Panel: Mean response times for T2 across four different RSI conditions in the intervening event condition, and the single RSI condition in the no-intervening event condition, for Experiment 3B, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Experiment 3A*

In the analysis of RTs, there was a significant main effect of repetition, F(1,11) = 18.68, p < .001, hp2 = .63. Responses for repeated trials were slower (493 ms) than responses for not-repeated trials (468 msec). Interestingly, this main effect did not vary as a function of RSI, p > .10. Finally, although not of any obvious theoretical importance, there was a significant main effect of RSI, F(3,33) = 3.42, p = .02, hp2 = .24, which appeared to be due to particularly slow responses for the 1800/300 RSI condition. There were no significant effects in the analysis of error rates.

*Experiment 3B*

The analysis of the intervening event trials revealed a significant main effect of repetition, F(1,11) = 6.5, p = .03, hp2 = .28. Responses were slower for repeated trials (617 ms) than for not-repeated trials (587 ms). Importantly, the effect of repetition did not vary as a function of the RSI, p > .4. However, there was a significant main effect of RSI, F(3, 33) = 9.7, hp2 = .39. A post hoc Tukey test revealed that responses to the 1800/300 RSI condition were significantly slower (648 msec) than the other three RSI condition, (604 msec, 572 msec, and 582 msec, for the 200/1900, 700/1400, and 1300/800 RSI conditions, respectively).

A separate analysis of the no-intervening event trials revealed a significant main effect of repetition, F(1,11) = 7.8, p = .02, hp2 = .30. Responses were faster for repeated trials (557 ms) than for not-repeated trials (596 ms).

No significant effects were found in the analyses of variance for error rates.

*Discussion*

The key result in this experiment was that a repetition cost was observed despite the fact that the intervening event was responded to vocally by saying “Go”, rather than manually using the same response keys as for T1 and T2. This result rules out the idea that the repetition costs observed in Experiments 1A-C are related to the specific motor response made to the intervening event. Instead, it appears that a more general form of disruption, perhaps brought about by any form of responding to an intervening event, is sufficient to produce a repetition cost. Additionally, the results from this experiment offer no support for the idea that repetition costs in the intervening event condition depend on the proximity of the timing of the intervening event to T1.

Experiment 4

Whereas in Experiment 3 we changed the response to the intervening event from a manual key press to a vocal response, in Experiment 4 we changed the presentation of the intervening from the visual modality to the auditory modality. In particular, the intervening event in this experiment was an auditory tone, and participants responded to it as in Experiment 1 by pressing both response keys upon its onset. This experiment allowed us to test whether the reversal of the repetition effect in the intervening event condition depends on presentation of the intervening event in the same modality as T1 and T2.

Method

*Participants*

Eighteen McMaster Universityundergraduate students recruited from either an introductory psychology course or a second year cognitive psychology course participated for course credit. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

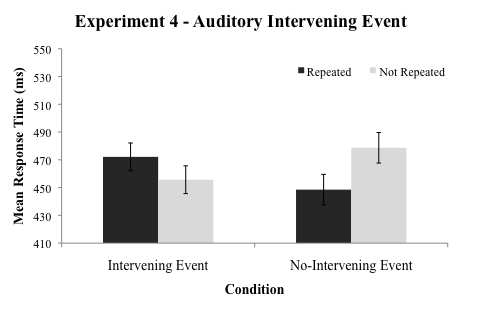
The apparatus and stimuli used in this experiment were the same as in Experiment 1A, with the exception that a high frequency tone (1000 Hz) was presented as the intervening event.

*Procedure and Design*

The procedure used in this experiment was the same as in Experiment 1A with one exception. In the intervening event condition, a tone was presented from the CPU speakers as the intervening event. Participants were instructed to respond to the tone by pressing the “blue” and “yellow” buttons down at the same time, as in Experiment 1A, which would cease the presentation of the tone.

Results

Correct trials were defined as in Experiments 1A, 1B, and 1C. RTs for correct responses to T2 were submitted to the same outlier elimination procedure used in prior experiments (Van Selst & Joliceur, 1994), which resulted in the exclusion of 2.9% of the RTs from further analysis. Mean RTs in each condition were then computed based on the remaining observations, and these mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that treated Repetition (repeated/not-repeated), RSI (short/long), and Intervening Event (intervening event or no-intervening event) as within-subject factors. The alpha criterion was set to .05 for all analyses. Means of mean RTs and error rates for each condition, collapsed across participants, are listed in Table 1. Repetition effects for the two intervening event conditions are contrasted in Figure 5.



*Figure 5.* Mean response times for T2 in Experiment 4, collapsed across RSI and participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

The analysis of RTs revealed a significant Repetition by Intervening Event interaction, F(1,17) = 15.87, p < .001, hp2 = .48. In the intervening event condition, RTs were faster for the not-repeated condition (455 ms) than the repeated condition (473 ms), F(1,17) = 5.39, p = .03, hp2 = .24. In the no-intervening event condition, RTs were faster for the repeated condition (443 ms) than the not-repeated condition (474 ms), F(1,17) = 12.14, p = .003, hp2 = .42.

Also from the omnibus ANOVA, the effect of RSI was significant, F(1,17) = 5.32, p =.03, hp2 = .24, which was due to RTs at short RSIs being slower (467 ms) than RTs at long RSIs (455 ms). No additional effects or interactions came out significant in the omnibus ANOVA. There were no significant effects in the overall analysis of error rates.

*Discussion*

The results were similar to those observed Experiments 1A-C. In the no-intervening event condition, responses were faster for repeated than for not-repeated trials. In contrast, in the intervening event condition, responses were slower for repeated trials than for not-repeated trials. As in the prior experiments, we assume that the requirement to respond to the intervening event disrupted an episodic integration process that would otherwise have produced faster responses for repeated relative to not-repeated trials. The results of Experiments 3A, 3B and 4 together suggest that the disruption of this episodic integration process does not require the response modality for the intervening event to match that for T1 and T2, and it does not require the stimulus modality of the intervening event to match that for T1 and T2.

Experiment 5

In Experiments 1A-C, 2, 3A-B, and 4 we learned that either a manual response that overlapped with that used to respond to T1 and T2, or a vocal response that was quite dissimilar from that used to respond to T1 and T2, were both sufficient to reverse the repetition benefit to a repetition cost. The issue addressed in this experiment is whether visual presentation of the intervening event on its own, without any response at all, is sufficient to produce such an effect. To address this issue, we replicated Experiment 1A with the exception that no response was required to the intervening event in the intervening event condition.

The results of prior studies might lead one to believe that the mere presentation of an intervening event would be sufficient to reverse the repetition benefit to a repetition cost (Law et al. 1995; Fox & de Fockert, 2001). However, these studies used a cue-target procedure in which a response was required only to the second of two events on a trial. When a response is made to both of two events on a trial, it seems reasonable to assume that a stimulus-response episode for the first event becomes available for use when responding to a repeated second event, a process that we call episodic integration. Whether something other than mere presentation of an intervening event is needed to disrupt this episodic integration process is the focus of this experiment.

Method

*Participants*

Twenty-four McMaster Universityundergraduate students recruited from either an introductory psychology course or a second year cognitive psychology course participated for course credit. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

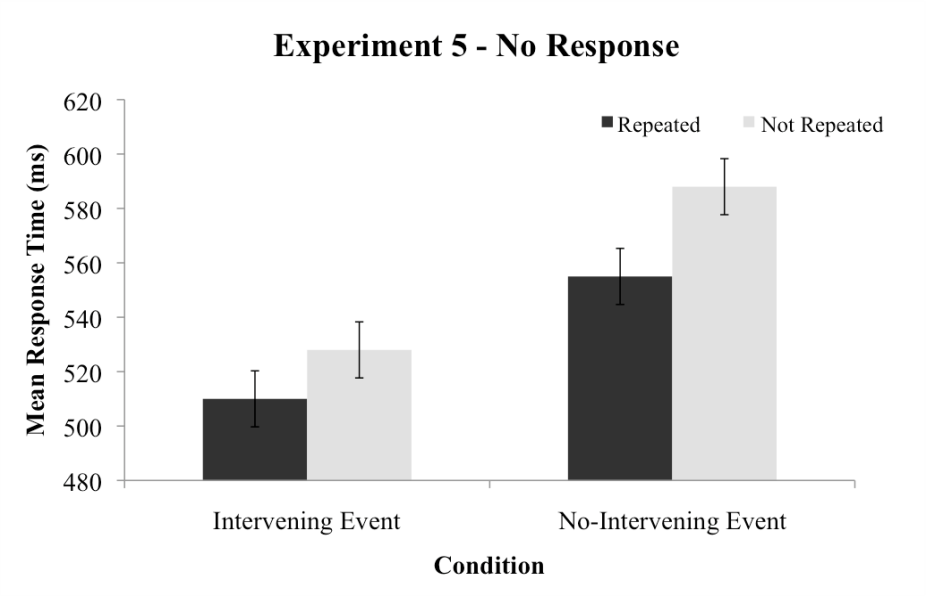
The apparatus and stimuli used in this experiment were the same as in Experiment 1A.

*Procedure and Design*

The procedure used in this experiment was the same as in Experiment 1A with two exceptions. First, participants were not instructed to respond to the presence of the intervening event, and instead were told that the intervening event would disappear from the screen after a short duration. The second change concerned the length of time the intervening event appeared on the screen. Rather than remaining on the screen until response, the intervening event appeared for 500 ms and then disappeared.

Results

Correct trials were defined as in prior experiments. RTs for correct responses to T2 were submitted to the same outlier elimination procedure used in prior experiments (Van Selst & Joliceur, 1994), which resulted in the exclusion of 2.9% of the RTs from further analysis. Mean RTs in each condition were then computed based on the remaining observations, and these mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that treated Repetition (repeated/not-repeated), RSI (short/long), and Intervening Event (intervening event or no-intervening event) as within-subject factors. The alpha criterion was set to .05 for all analyses. Means of mean RTs and error rates for each condition, collapsed across participants, are listed in Table 1. Repetition effects for the two intervening event conditions are contrasted in Figure 6.



*Figure 6.* Mean response times for T2 in Experiment 5, collapsed across RSI and participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

The data from one participant were not included in the final analyses reported here because the mean RT for that participant differed from the mean RT of all participants by more than two standard deviations. All analyses were conducted with and without the data from this participant, and exclusion of this participant’s data did not change the pattern of results significantly.

In the analysis of RTs, there was a significant interaction between Intervening Event and Repetition, F(1,23) = 3.19, p = .03, hp2 = .32. Simple main effects were analyzed separately for the intervening event and no-intervening event conditions. In the intervening event condition, RTs were faster for repeated trials (510 ms) than for not-repeated trials (528 ms), F(1,23) = 4.59, p = .02, hp2 = .24. Similarly, in the no-intervening event condition, RTs were faster for repeated trials (554 ms) than for not-repeated trials (588 ms), F(1,23) = 10.91, p = .006, hp2 = .31. Thus, rather than reversing the repetition effect, in this case the intervening event produced a modest attenuation of the repetition benefit, a benefit that remained significant for both intervening event conditions.

Several other effects with less obvious theoretical significance were significant in the overall analysis of RTs. The Intervening Event by RSI interaction was significant, F(1,23) = 9.21, p = .01, hp2 = .35. Analysis of simple main effects revealed no significant difference between trials with a short RSI (519 ms) and trials with a long RSI (519 ms), F(1,23) < 1, in the intervening event condition. In contrast, trials with a short RSI were responded to slower (587 ms) than trials with a long RSI (555 ms), F(1,23) = 28.97, p <.001, hp2 = .39, in the no intervening event condition. There was also a significant main effect of Intervening Event, F(1,23) = 8.39,p = .003, hp2 = .33. Responses for trials with an intervening event were faster (519 ms) than for trials without an intervening event (571 ms). There was a significant main effect of RSI, F(1,23) = 6.52, p = .03, hp2 = .29. Responses for trials with a short RSI were slower (553 ms) than for trials with a long RSI (537 ms). Finally, there was a significant main effect of Repetition, F(1,23) = 10.02, p = .003, hp2 = .35. Responses for repeated trials were faster (532 ms) than for not-repeated trials (558 ms).

In the analysis of error rates, there was a significant interaction between Intervening Event and RSI, F(1,23) = 8.75, p = .01, hp2 = .24. Simple main effects analyses revealed that in the intervening event condition, more errors were made on trials with a short RSI (.04) than on trials with a long RSI (.03), F(1,23) = 8.90, p = .02, hp2 = .28. In the no-intervening event condition, there was no significant difference between trials with short and long RSIs.

*Discussion*

The critical finding from Experiment 5 was that responses to repeated targets were faster than responses to not-repeated targets in the intervening event condition. This result contrasts with those in Experiments 1A-C, 2, 3A-B and 4, in which a response to an intervening event resulted in repetition costs rather than repetition benefits. At least in this experimental context, then, the mere presentation of an intervening event on its own was not sufficient to reverse the repetition benefit to a repetition cost. Whether this result occurred because participants learned not to attend to the intervening event when a response to this event was not required, or because responding to the intervening event itself is critical to the effect, is an issue that merits further study.

General Discussion

The results of the present study were straightforward. The repetition benefit customarily seen in 2afc tasks reversed to a repetition cost when participants responded to an intervening event presented between T1 and T2. This reversal of the repetition effect occurred when participants responded to the intervening event by simultaneously pressing the two manual response keys used to respond to T1 and T2 (Experiments 1A, 1B, and 1C), when intervening event trials were mixed with no-intervening even trials (Experiments 2 and 3B), when participants responded to the intervening event merely by saying “Go” aloud (Experiments 3A and 3B), and when the intervening event was presented aurally (Experiment 4). In contrast, when the intervening event appeared but was not responded to, the customary repetition benefit was observed (Experiment 5). Together, these results demonstrate clearly that attending and responding to an intervening event has the effect of reversing the repetition effect in two-alternative forced choice tasks.

We proposed a dual process account of these results, in which one process that *speeds* performance for repetitions operates concurrently with another process that *slows* performance for repetitions. This opposition between two concurrent processes is the key property of the dual process account offered here, and holds even if there remains some debate about the precise nature of the processes themselves. From this perspective, the key finding here is that response to an intervening event affects the relative contributions of these two processes. Under usual testing conditions in 2afc procedures, although both processes may contribute to performance, the contribution of a process that speeds performance for repetitions outweighs the influence of a process that slows performance for repetitions, together producing faster responses for repeated trials than for not-repeated trials. In contrast, in the intervening event conditions of the experiments reported here, we propose that the influence of a process that speeds performance for repeated trials was disrupted, thus revealing the influence of a process that slows performance for repeated trials.

This dual process proposal is not entirely novel. As noted in the Introduction, Klein (2000; see also Tipper et al., 1997) proposed a similar dual process account to explain the time course of exogenous spatial cueing effects. According to this account, the exogenous cue leads to a shift in attention toward its location, which results in fast responses to targets that appear at the same location as the cue. However, at the same time, a second process may impede performance on cued trials either by inhibiting attention from returning to the cued location (Reuter-Lorenz, Jha, & Rosenquist, 1996) or by inhibiting responses to targets at cued locations (Klein & Taylor, 1994). The overall cueing effect is presumed to reflect the relative contributions to performance of these two opposing processes. By this view, the facilitation effect at short cue-target SOAs occurs because the benefit to performance caused by the target appearing at the attended location is larger than the cost to performance caused by some other process that slows processing of targets at cued locations. With longer SOAs, it is assumed that attention is removed from the cued location, reducing the benefit for cued trials, and thus revealing an overall cost for cued trials relative to uncued trials.

Taken literally, the dual process account of exogenous spatial cuing effects forwarded by Klein (2000) could not possibly explain the repetition effects reported in the present study, as the two processes in Klein’s (2000) account are related specifically to shifts of visual attention in space. Nonetheless, the spirit of Klein’s (2000) dual process account is similar to that offered here. In the following sections, we describe more specifically the types of processes that might fit our dual process framework. Although at this point we have no way to evaluate such a possibility, we have opted for process descriptions that are sufficiently broad that they might encompass those offered to explain exogenous spatial cueing effects.

*Episodic Integration*

Some of the earliest research on repetition effects attributed the faster performance to repeated targets to an automatic priming process triggered by presentation and response to the first of two targets (Kornblum, 1973; Kirby, 1976). Subsequent research that focused specifically on whether the repetition benefit owed to stimulus repetition or response repetition (e.g., Bertelson, 1965, Smith, 1968) eventually gave way to the view that stimulus-response (S-R) bindings play an important role (Rabbitt, 1968; Pashler & Baylis, 1991; Hommel, 1998). According to this view, response to repeated targets is particularly fast because these targets automatically cue the retrieval of the S-R binding from the immediately preceding trial, and use of this S-R binding offers a savings to performance relative to the more analytic process of assigning a response based on the task-defined S-R rule (see also Logan, 1988). Following these ideas, we have proposed that one of the processes contributing to performance in our tasks involves integration of T1 and T2 stimulus-response episodes. By this view, responses to repeated T2 targets are typically fast because of the benefit associated with retrieving a stimulus-response episode for a similarly encoded and responded-to T1 (Logan, 1988; Kahneman et al., 1992; Hommel, 1998).

We have assumed further that the requirement to respond to an intervening event disrupts this episodic integration process, and that in the absence of the benefit afforded by episodic integration, we can measure the influence of a separate process that slows performance for repeated relative to not-repeated trials. However, it is worth considering whether episodic integration itself might lead to slower performance for repeated relative to not-repeated trials under conditions in which participants respond to an intervening event. Note that if episodic integration itself could produce repetition costs, then there would be no need for a dual process account; episodic integration would explain both repetition benefits and repetition costs.

One way in which episodic integration processes could produce repetition costs stems from the idea that onset of T2 could cue the retrieval of a T1 processing episode that is inappropriate for transfer to T2 (see Hommel, 1998 for application of this idea to performance in 2-afc tasks, and Neill & Mathis, 1998; Wood & Milliken, 1998 for application of this idea to studies of negative priming). According to this view, this inappropriate transfer effect occurs when there is a “partial match” between the T1 and T2 processing episodes. In the case of a partial match, some additional processing may be required prior to response, processing dedicated to resolving discrepancies between the current target and associated task requirements on the one hand, and the retrieved S-R episode on the other hand.

To apply this idea to the results of our experiments, one might assume that processing of the intervening event would be bound to the processing episode of T1, which would then be retrieved when T2 is identical to T1. The T1-intervening event bound episode would then contain relevant processing needed to efficiently process a repeated T2, but also the irrelevant processing associated with the intervening event. If a repeated target retrieves both relevant and irrelevant processing, then some additional time may be required to integrate selectively just the relevant aspects of the retrieved episode into the current processing episode.

In Experiment 3, we examined this issue by manipulating the temporal interval between T1 and the intervening event from relatively short in duration (200 msec) to long (1800 msec). The rationale was that events occurring relatively close in time would be more likely to be bound together in a single memory representation than events appearing further apart in time. Yet, we observed repetition costs that did not differ in magnitude across these conditions, a finding that fails to support our particular test of an episodic integration account of the repetition costs. In the absence of evidence favoring an episodic integration account of repetition costs, we are left to consider an account in which episodic integration processes are disrupted by the requirement to respond to an intervening event, and the repetition costs measured under these conditions reflect the contribution of some other process. We turn now to the nature of this other process.

*Non-spatial Inhibition of Return*

As mentioned in the Introduction, there have been a handful of studies that have examined whether the process that causes spatial IOR effects might cause analogous effects in studies with non-spatial stimuli (Fox & de Fockert, 2001; Francis & Milliken, 2003; Kwak & Egeth, 1992; Law et al, 1995; Taylor & Klein, 1998b). Differences in empirical properties can indeed be observed across studies of spatial and non-spatial orienting (e.g., Kwak & Egeth, 1992; Taylor & Klein, 1998b), which has led some researchers to conclude that the process responsible for spatial IOR effects does not contribute to performance in studies of non-spatial orienting. Yet, we have pointed out here that such inferences often depend on an assumption that empirical effects are pure measures of a single process. If this assumption is incorrect, then qualitatively different spatial and non-spatial repetition effects may occur when at least one common underlying process is involved.

This possibility was examined here with specific reference to performance in tasks that require a response to targets on consecutive trials (i.e., a target-target procedure). Prior research has shown that target-target spatial repetition procedures commonly lead to IOR effects (e.g., Kwak & Egeth, 1992; Maylor & Hockey, 1985), whereas target-target non-spatial repetition procedures commonly lead to repetition priming effects (e.g., Bertelson, 1961; Kwak & Egeth, 1992; Tanaka & Shimojo, 1996, Taylor & Donnelly, 2002). Indeed, non-spatial IOR-like effects have been reported to date solely with procedures in which a response is withheld to a first event, and then made to a second event (i.e., a cue-target procedure; see Fox & deFockert, 2001; Francis & Milliken, 2003; Law et al., 1995), and not when a response is required to both of two consecutive events (i.e., a target-target procedure). The IOR-like effects observed with a target-target procedure in the present study are therefore novel, and important because they rule out the idea that all non-spatial IOR-like effects are a byproduct of response inhibition processes that are independent of a “true” IOR process (Welsh & Pratt, 2006).

These different effects across spatial and non-spatial procedures might lead one to conclude that different processes underlie these effects. Yet, in the present study, the mere requirement to respond to an intervening event between non-spatial targets revealed effects that are similar in direction to spatial IOR effects. Of course, the presence of effects that are similar in direction across spatial and non-spatial procedures does not imply that similar mechanisms cause these effects, but these results do highlight that the similar mechanisms hypothesis cannot be ruled out.

One might reasonably ask why our intervening event procedure was required to observe repetition costs here when no such procedure is required to observe spatial IOR effects in 2-afc target localization tasks (Maylor & Hockey, 1985). One interpretation of this discrepancy is that fundamentally different processes underlie the repetition costs observed in spatial and non-spatial 2-afc tasks. However, an alternative interpretation is that spatial and non-spatial 2-afc tasks differ not in terms of the process that causes repetition costs, but in the process that causes repetition benefits. If repetition benefits in 2-afc tasks are driven primarily by the retrieval of prior S-R episodes, then it may be that this episodic integration process plays a much larger role in non-spatial tasks than in spatial tasks. If retrieval of a prior S-R episode affords a much larger benefit in non-spatial 2-afc tasks in the spatial 2-afc tasks, then it stands to reason that disrupting this process may be much more important in non-spatial tasks than in spatial tasks to observe the presence of an opposing process on performance.

To be clear, we acknowledge that the present data do not require the conclusion that spatial IOR effects and non-spatial repetition costs are caused by the same mechanism. At the same time, the present data do invite consideration of whether spatial IOR effects need be attributed to processes dedicated specifically to controlling the orienting of attention in space. In particular, it is worth asking whether a broader orienting principle that favours processing of novel relative to familiar perceptual events would constitute a parsimonious alternative. In line with this possibility, Dukewich (2009) has recently argued against a dedicated spatial orienting process account of the IOR effect, instead proposing that IOR is caused by habituation of the orienting response.

Support for this broader view may also be found in studies on masked response priming. In these studies, a prime stimulus (e.g., an arrow pointing right) is presented briefly and masked, and then is followed by a compatible (i.e., an arrow pointing right) or incompatible (i.e., an arrow pointing left) target (Eimer & Schlaghecken, 2003). When the temporal interval between prime and target is very brief (e.g., less than 100 ms), responses are typically faster for compatible than for incompatible targets. In contrast, when the temporal interval between prime and target is longer, responses are slower for compatible than for incompatible targets (Eimer & Schlaghecken, 2003; see also Sumner, 2007). An interpretation of this result that fits broadly with our dual process framework is that responses for the short prime-target SOAs are driven predominantly by integration of activation from the prime and target within the same event representation, leading to facilitation effects. In contrast, for longer prime-target SOAs performance may not benefit from this episodic integration process, leaving performance to be affected predominantly by an opposing inhibition process (see also Bodner & Masson, 2001 for a discussion of episodic influences in masked priming).

In summary, a dual process framework in which episodic integration processes are responsible for repetition benefits, and in which a broad orienting principle that favours perceptual processing of novel over familiar events is responsible for repetition costs, fits well with the results reported here (see also Dukewich, 2009; Hu, Samuel, & Chan, 2011; Eimer & Schlaghecken, 2003). Yet, there are other candidate processes to explain repetition costs that could as easily fit within a dual process framework, and that merit some consideration here. We turn now to a discussion of two such candidate processes.

*Expectation for Alternation*

Kirby (1976) noted that participants respond faster to alternations than to repetitions when the RSI between trials in a 2-afc task is relatively long. To explain this result, Kirby proposed that an expectation favoring alternation builds across time between trials, such that for long RSI trials this expectation has a stronger influence on performance than an automatic process that speeds performance for repeated relative to not-repeated trials. The issue that merits consideration here is whether expectation for alternation might explain the repetition costs observed in the present study. In other words, could the requirement to respond to an intervening event somehow induce an expectation for alternation rather than repetition? Although this account cannot be ruled out, without some additional evidence it seems somewhat circular. In particular, if any condition that produces repetition costs is interpreted as increasing expectation for alternation, it becomes hard to distinguish between the effect and the mechanism that causes the effect. Some deliberate manipulation of expectation, or measure of expectation separate from the repetition effect itself, might be used to evaluate this idea further in subsequent research.

*Backward Inhibition*

If not expectation for alternation, then the repetition costs observed might be argued to reflect a form of backward inhibition effect (Mayr & Keele, 2000). The backward inhibition effect is reflected in particularly slow performance when participants are required to shift back to a task that has been performed recently, relative to when they shift to a task that was not performed recently. In effect, performance in task A is more efficient in the task series CBA than in the task series ABA. To demonstrate the backward inhibition effect, Mayr and Keele (2000) assigned multiplication questions as task A, addition questions as task B, and subtraction questions as task C. Participants first completed either a block of task A trials or a block of task C trials. Then they completed a block of task B trials, followed by a block of Task A trials. Performance was slower in the third block when participants had performed the same task in the first block than when they performed a different task in the first block. Mayr and Keele (2000) coined the term backward inhibition to describe this effect, with the idea that switching from task A to task B requires inhibition of the representation of task A, which then makes that representation difficult to access when participants shift back to that task.

To explain our results by reference to a similar backward inhibition process, we would have to assume that responding to the intervening event results in inhibition of task-related representations associated with T1. However, slower responses to repeated items in our study cannot simply be explained by reference to inhibition of the colour identification task set, as both repeated and not-repeated T2 trials require colour identification. Without some additional assumption, inhibition of the T1 task representation would result in slow response times for both repeated and not-repeated T2 trials. To salvage a backward inhibition account, one might propose that task representations are bound to stimuli to which they are applied, in which case inhibition is directed at a representation of the colour identification task that is bound to a particular colored T1. In this case, inhibition might only be expected to slow performance if T2 cues the retrieval of this particular representation; that is, when T2 matches the color of T1. This episodic variant of the backward inhibition hypothesis merits consideration.

At the same time, there are aspects of our results that are difficult to reconcile with a backward inhibition account. For instance, if response to the intervening event in our task requires a shift in task set that triggers inhibition of the prior task representation, then responses to T2 should have been slower in the intervening event condition than in the no-intervening event condition. Yet, there was no evidence in any of our experiments for such an effect. In fact, in Experiments 1A and 4, the opposite pattern was observed; that is, participants responded faster to T2 in the intervening event condition than in the no-intervening event condition. Nonetheless, given the face similarity between the procedure used here and those used to measure the backward inhibition effect in task switching, additional research on this issue seems warranted.

Conclusion

We propose that repetition effects in 2-afc tasks may be caused by two separate and opposing processes. This proposal is supported by the finding that the customary repetition benefit reverses to a repetition cost when participants are required to respond to an intervening event between consecutive targets. This effect was observed for a range of different target stimuli, intervening event stimuli, and modes of responding to the intervening event. Although it is unclear how best to describe the processes themselves, a compelling possibility is that the repetition costs observed here constitute a non-spatial variant of the IOR effect. According to this view, response to the intervening event disrupts an episodic integration process that is the basis of the repetition benefit commonly observed in such tasks. In the absence of this episodic integration process a repetition cost in performance is revealed, perhaps caused by a habituation process that can be observed with both spatial and non-spatial orienting methods (Dukewich, 2009).

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CHAPTER 3: On the role of attending and responding to an intervening event for revealing non-spatial IOR

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Preface

One of the key findings from Chapter 2 was that presentation of an intervening event on its own did not turn the repetition benefit into a repetition cost. Rather, the reversal of the repetition effect occurred only when participants were required to respond in some way to the intervening event. However, the results in Chapter 2 did not pin down precisely why a response to an intervening event was necessary to reverse the repetition effect. Was it the response to the intervening event itself that was critical, or was it the attention one pays to events that require a response that was critical? To examine this issue, a method is needed that teases apart the requirement to attend to the intervening event from the requirement to initiate a response to the intervening event. This issue was the focus of the experiments in Chapter 3.

Abstract

In a recent study, it was shown that repetition priming in a simple 2-afc task with non-spatial targets (e.g., color, line length, identity) can reverse to a repetition cost if participants are forced to respond to an event intervening between consecutive targets (Spadaro, He, & Milliken, 2012). Spadaro et al. described this effect as a form of non-spatial inhibition of return (IOR). The link between responding to an intervening event and this non-spatial IOR effect was examined in more detail in the present study using a Go/No-Go method. Across four experiments the following four results were observed: (1) responding to intervening events always resulted in repetition costs (Experiments 1A,2, 3, 4); (2) consistent withholding of response to all intervening events resulted in a repetition benefit (Experiment 1b); (3) consistent withholding of response to particular No-Go events resulted in a null effect (Experiments 2 and 3); (4) withholding of response to a No-Go event that previously served as a Go event resulted in repetition costs (Experiment 4). Together, the results suggest that responding to an intervening event is not necessary to produce non-spatial IOR in this procedure, but engagement of response selection processes may be critical. The results are described in the context of a dual-process framework in which episodic integration processes lead to repetition benefits, and new event detection/encoding processes lead to repetition costs.

Introduction

This article focuses on the processes underlying a widely studied attentional orienting effect known as inhibition of return (IOR). The IOR effect is commonly measured using a spatial orienting task in which a non-predictive abrupt onset cue appears either to the left or right of fixation. Following the cue, a target appears in either of those locations, and the participant’s task is often simply to detect the target’s onset. The IOR effect is defined by slower responses to targets appearing in the cued location than in the uncued location, a result that occurs reliably at cue-target stimulus onset asynchronies (SOAs) greater than about 300 ms (Posner & Cohen, 1984; see Samuel & Kat, 2003 for a meta-analysis of the time course of IOR). A similar result is observed in studies in which participants detect or localize two targets on consecutive trials (i.e., a target-target procedure), rather than respond to a single target following presentation of a passively perceived cue (i.e., a cue-target procedure). That is, in a simple 2-afc task that requires participants to localize targets, responses are often slower for repetitions than for alternations (Maylor & Hockey, 1985; for reviews see Klein, 2000, & Taylor & Klein, 1998a).

The fact that IOR is measured so reliably in spatial orienting studies has fostered the idea that it reflects a mechanism that facilitates the scanning of one’s environment (Klein, 1988; Klein & MacInnes, 1999; Posner & Cohen, 1984). The basic idea is that when attention shifts away from a location (or object) a mechanism marks that location (or object) as having been attended previously, and subsequent shifts of attention are then biased away from that location, thus preventing continuous reorienting to previously attended locations. To the extent that this “foraging facilitator” hypothesis focuses exclusively on mechanisms that control shifts of attention in space, one might reasonably expect to observe IOR effects in spatial orienting tasks, but not in task domains that involve repetition/alternation of other stimulus features. On the other hand, if IOR effects are driven by a mechanism with a broader scope, one that perhaps favours orienting to novelty more generally, then one might expect to observe IOR effects in both spatial and non-spatial task domains.

*Non-spatial IOR?*

Although preliminary studies suggested that IOR effects were indeed specific to spatial orienting (Kwak & Egeth, 1992; Tanaka & Shimojo, 1996), there is a growing literature on IOR effects that are not specific to spatial orienting. The first published study to report a non-spatial IOR effect was reported by Law, Pratt, and Abrams (1995), who found that the time required to detect a centrally presented color patch that matched in color with a preceding cue was slower than that to detect a color patch that mismatched in color with a preceding cue. Although the link between this result and spatial IOR was questioned by some researchers (Taylor & Klein, 1998; Fox & deFockert, 2001), Francis and Milliken (2003) later reported a similar result in both detection and discrimination tasks with line lengths serving as stimuli. In particular, the time to detect a simple line segment was longer when that line matched rather mismatched in length with a preceding cue line. When participants judged the length of the target line rather than simply detecting its onset, a similar result was observed, and indeed this effect followed the usual time course of spatial orienting effects, with slower responses for line length repetitions relative to alternations emerging at SOAs beyond about 300 ms. Most recently, Hu, Samuel, and Chan (2011) found additional evidence for a non-spatial IOR effect that emerged after a temporal interval similar to the spatial IOR effect (i.e. 350 ms). Using a richer stimulus display than previous non-spatial IOR tasks, Hu et al. found an inhibitory effect of repeating the same non-spatial attribute, as well as repeating the same spatial attribute.

However, a potential criticism of the studies described above is that all used a cue-target procedure; that is, participants were instructed to withhold a response to the first stimulus on each trial (the cue), and to respond only to the second stimulus on each trial (the target). This criticism draws from other attention and performance domains (e.g., negative priming) in which it has been argued that retrieval of response information that is bound to a prime (or cue) episode, and that mismatches the response requirements of a target episode, can disrupt performance (Neill, Valdes, Terry & Gorfein, 1992; Hommel, 1998). By this view, an IOR effect measured with a cue-target procedure could conceivably contain a component that reflects a “true” IOR effect and a component that reflects a response mismatch induced negative priming effect (Coward, Poliakoff, O’Boyle, & Lowe, 2004; Welsh & Pratt, 2006). Consequently, prior studies that claimed to have measured non-spatial IOR effects with a cue-target procedure might have inadvertently measured a response mismatch effect (i.e., withhold a response to the cue, then respond to the target) rather than a true IOR effect. Clearly, a convincing demonstration of non-spatial IOR requires use of a target-target rather than a cue-target procedure.

Yet, it has long been known that stimulus repetition results in benefits rather than costs in 2-afc continuous responding procedures with non-spatial targets (Bertelson, 1961). Indeed, it is likely the ubiquity of repetition benefits in such procedures that led researchers to assume, at least initially, that IOR was a strictly spatial attention phenomenon. However, a recent study introduced a procedure that shows how an IOR effect can be measured with a target-target procedure in spite of processes that push performance in the opposite direction (Spadaro, He, & Milliken, 2012). The key to observing IOR with a target-target procedure appears to be the introduction of an intervening event between consecutive targets, and processing requirements for this intervening event that disrupt processes that oppose the IOR effect.

*The Intervening Event Effect*

In the study by Spadaro et al. (2012), participants were required to perform a two-alternative forced choice to two sequential non-spatial targets (T1 and T2) that appeared in a central location. On some trials an intervening event appeared temporally between the two targets, while on other trials no intervening event appeared. In most of the experiments, participants were required to make a simple detection response to the onset of the intervening event. The key result was that repetition effects were qualitatively different across the two intervening event conditions. For the no-intervening event condition, responses to T2 were faster on repeated trials (i.e., when T1 and T2 were identical) than on not-repeated trials, an effect that might well be considered a replication of the repetition benefit reported by Bertelson (1961). In contrast, for the intervening event condition, responses to T2 were faster on not-repeated trials than on repeated trials. The favoured interpretation of this effect was a dual process account; response to an intervening event disrupted the processes responsible for repetition benefits, and revealed a co-present non-spatial IOR effect.

This dual process account ultimately hinges on an understanding of the separate contributions to performance of two processes. The results of Spadaro et al. (2012) are generally supportive of this account in that response to an intervening event appeared to affect one of the processes but not the other. However, our current understanding of how responding to an intervening event led to IOR is far from complete. Although prior studies of both spatial and non-spatial IOR have also employed an intervening event to measure IOR in detection tasks (Hu et al., 2011; Law et al., 1995), there are some important differences between these prior studies and that of Spadaro et al. For example, in studies of spatial orienting, it is often assumed that disengagement of attention from the cue is critical to measure IOR, and an abrupt onset presented centrally (but not responded to) is sufficient to disengage attention from the cue. Similarly, in studies of non-spatial orienting, some researchers have found that IOR effects occur only when a neutral cue is flashed (but not responded to) between presentation of cue and target (Law et al., 1995; Fox & deFockert, 2001). In other words, although prior studies have noted the important role that can be played by an intervening event in measuring IOR, none of these studies have found that responding to the intervening event is critical. The link between IOR and the requirement to respond to an intervening event in the Spadaro et al. procedure therefore requires further study.

*The Present Study*

The purpose of the present study was to examine whether responding to an intervening event is necessary to observe a non-spatial IOR effect when using a target-target procedure. The results of the Spadaro et al. (2012) study suggest that this may be the case. In particular, they found that non-spatial IOR effects occurred when participants responded to an intervening event, whereas repetition benefits were observed when an intervening event was merely displayed but not responded to. However, an alternative interpretation of these results is that it is attention to an intervening event that is necessary to observe non-spatial IOR, and attention to the intervening event may have occurred only in the conditions in which the intervening event was responded to. The present study addressed this issue by using a Go/No-Go method, which ensured that participants attended to the intervening event when it was not responded to.

In Experiments 1A and 1B, we replicate a finding reported by Spadaro et al. (2012), with a non-spatial IOR effect observed when a response was always made to intervening events, and a repetition benefit observed when a response was never made to an intervening event. In Experiment 2, we introduced a Go/No-Go procedure with respect to the intervening event, to ensure that participants attended to the intervening event both when they responded and when they did not respond to the intervening event. The repetition effect for No-Go trials in this experiment was not significant, suggesting that attention to the intervening event was sufficient to disrupt the repetition benefit, but insufficient to invert the effect to a repetition cost. In Experiment 3, the difficulty of the Go/No-Go discrimination was varied to determine whether a greater level of attentional scrutiny to the intervening event would reveal a non-spatial IOR effect on No-Go trials Interestingly, the difficulty of the Go/No-Go perceptual discrimination did not impact repetition effects at all; the results for both easy and difficult discriminations were very similar to those from Experiment 2. Finally, in Experiment 4, the Go/No-Go response mappings for the two possible intervening events switched from block to block. With this procedure, we did observe a non-spatial IOR effect for No-Go trials when participants withheld a response to No-Go targets to which they had previously responded. Together, the results point to the importance of response selection to the intervening event in producing non-spatial IOR using a target-target procedure.

Experiments 1A and 1B

The first goal of the present study was to replicate one of the primary findings from the Spadaro et al. (2012) study. In that study, when participants responded to an intervening event on all trials, a non-spatial IOR effect was observed. In contrast, when participants withheld a response to an intervening event on all trials, a repetition benefit was observed (Spadaro et al, Experiment 5).

To that end, in Experiment 1A of the present study, participants responded to the color (blue or yellow) of two sequential targets (T1 and T2) on each trial, both of which appeared in the center of the stimulus display. Following response to the first of the two targets, they were also required to respond to an intervening event. The intervening event was either a small filled or unfilled dot (i.e., a small circle). On every trial, participants were instructed to respond to the intervening event by pressing the response keys for both “blue” and “yellow” at the same time, regardless of whether the intervening event was a filled or unfilled dot. Experiment 1B followed the same procedure as Experiment 1A, with the exception that participants were instructed to attend but not to respond to the intervening event

Method

*Participants*

For both Experiments 1A and 1B, 13 participants were recruited from either an introductory psychology course or a second year cognition course at McMaster University, and participated for course credit. All participants reported to have normal or corrected-to-normal vision.

*Apparatus and Stimuli*

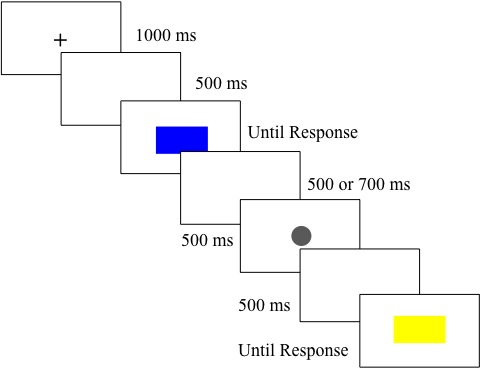
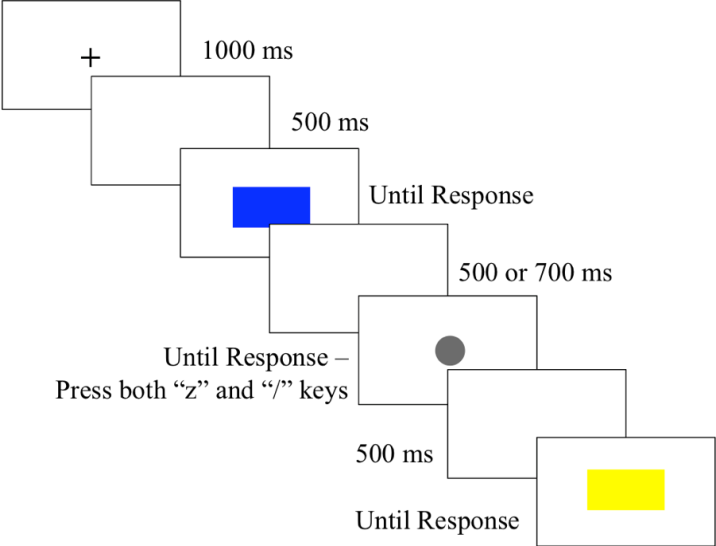
The experiment was run on a PC using E-Prime software (Schneider, Eschman, & Zuccolotto, 2002). Subjects sat directly in front of a 15” SVGA computer monitor, at adistance of approximately 57 cm. A plus sign was presented as the fixation point in the center of the screen, and subtended a visual angle of 0.6 degrees horizontally and 0.7 degrees vertically. The target stimuli (T1 and T2) were presented centrally against a black background.

Both T1 and T2 were either a blue or yellow rectangle, and subtended a visual angle of 6.3 degrees horizontally and 1.2 degrees vertically. The intervening event was either a filled or unfilled grey dot presented centrally, with radius subtending .45 degrees of visual angle.

*Procedure and Design*

Experiments 1A and 1B both consisted of an initial practice block of 16 trials, followed by 18 experimental blocks of 16 trials. For both experiments, a trial started with the appearance of a fixation cross presented centrally for 1000 ms, and then a blank screen for 500 ms. The first target (T1) then appeared centrally, and remained on the screen until participants made a key press response (“z” for blue rectangles, “/” for yellow rectangles) to its color. A blank interval of either 500 or 700 ms followed the key press response to T1. One of the two time intervals was randomly selected on each trial to create temporal uncertainty as to the onset of the intervening event. Following the blank interval, a filled grey dot appeared on 75% of the trials, while an unfilled grey dot appeared on the remaining 25% of the trials. Regardless of the type of intervening event (filled or unfilled), participants were required to respond to the onset of the intervening event in Experiment 1A, and to withhold a response to the intervening event in Experiment 1B. In Experiment 1A, the intervening event remained on the screen until onset of the participant’s response, whereas in Experiment 1B, the intervening event remained on the screen for 500ms. Following offset of the intervening event, a blank interval of 500 ms occurred prior to onset of T2. T2 remained on the screen until participants responded to its identity by pressing the “/” key or the “z” key. Response time was measured as the latency between onset of the target stimulus and key press response.

Task instructions were displayed on the screen prior to the practice block. Prior to each block of trials, the message “Press B to begin next block” appeared, allowing participants to rest between blocks when needed. For all trials in both experiments, a 2000 ms inter-trial interval followed response to T2. The sequence of events on each trial in Experiments 1A and 1B is displayed in Figure 1.



*Figure 1.* Left Panel: The sequence of events for a not-repeated trial in Experiment 1A is shown. Note that although the intervening event is presented here as a filled grey dot, on 75% of trials it was a filled grey dot and on 25% of the trials it was an unfilled grey dot. Participants were instructed to respond to both types of intervening event stimuli. Right Panel: The sequence of events for a not-repeated trial in Experiment 1B is shown. Again, although the intervening event is presented here as a filled grey dot, on 75% of trials it was a filled grey dot and on 25% of the trials it was an unfilled grey dot. Participants were instructed that neither type of intervening event required a response.

The designs for Experiment 1A and Experiment 1B were identical. There were two within-subject variables: Intervening event (filled/unfilled), and Repetition (repeated/not-repeated). Both Intervening event and Repetition were manipulated mixed within blocks. In the repeated condition, T1 and T2 were identical in color, whereas in the not-repeated condition, T1 and T2 were different colors. Although not a key variable in this experiment, the nature of the intervening event (filled/unfilled) was included in the design and analysis of this experiment for purpose of comparison with Experiment 2.

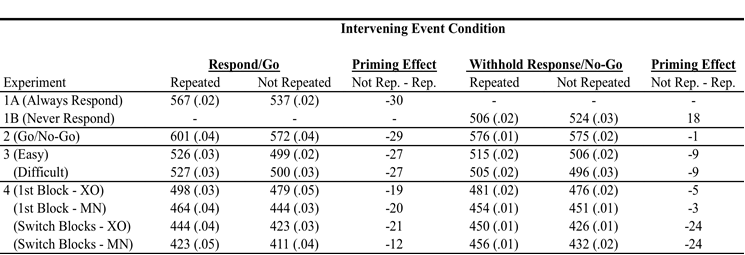
Results

For both Experiments 1A and 1B, a trial was coded as correct if responses to both T1 and T2 were correct, and as an error if a correct response was made to T1 and an incorrect response was made to T2. Response times (RTs) measured on correct trials were submitted to an outlier analysis (Van Selst & Jolicoeur, 1994) that eliminated 3.0 % of the RTs from further analysis. Mean RTs for each condition were then computed based on the remaining observations. These mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that included Intervening Event (filled or unfilled), and Repetition (repeated or not-repeated) as within-subject factors. The alpha criterion was set to .05 for all analyses. Mean RTs for each condition, collapsed across participants and Intervening Event, are displayed in Figure 2 and Table 1.

*Experiment 1A*

In the analysis of RTs, there was a significant main effect of Repetition, F(1,12) = 6.53, p = .027, p2 = .35. RTs were slower for repeated trials (567 ms) than for not-repeated trials (535 ms). There were no significant effects involving the Intervening Event factor. There were also no significant effects in the analysis of error rates.

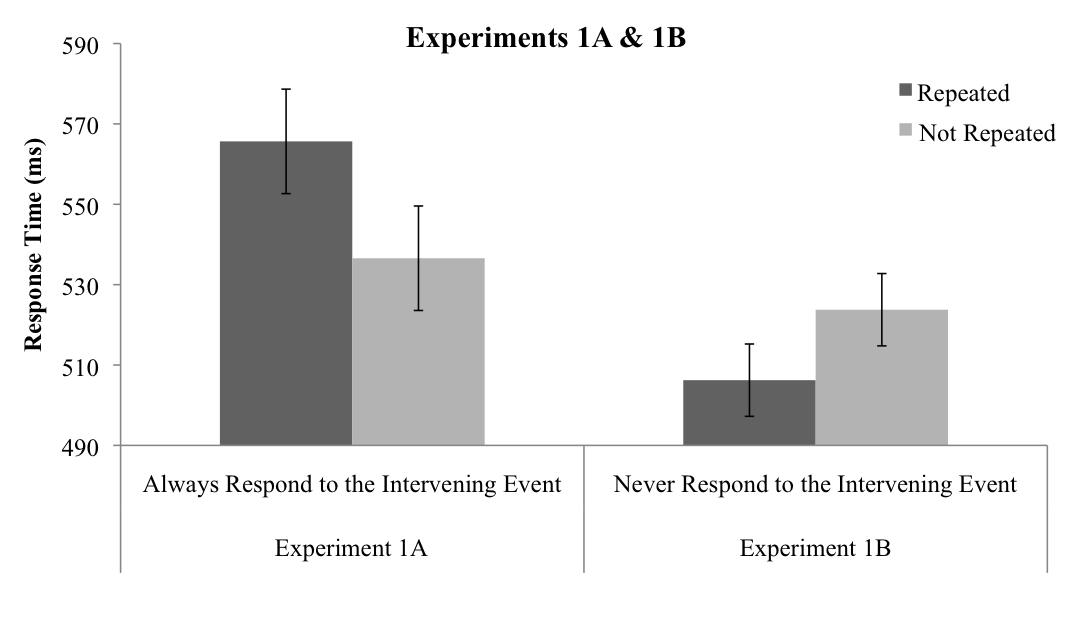
*Table 1.* Mean response times and error rates for T2 (ms) for each condition in Experiments 1A, 1B, 2, 3, & 4.



*Experiment 1B*

In the analysis of RTs, there was a significant main effect of Repetition F(1,12) = 6.15, p = .029, p2 = .34. RTs were faster for repeated trials (507 ms) than for not-repeated trials (527 ms). As in Experiment 1A, there were no significant effects involving the Intervening Event factor, F<1.

In the analysis of error rates, there was a significant main effect of Repetition, F(1,12) = 5.21, p = .040, p2 = .21. Participants had a higher error rate for not-repeated trials (.03) than for repeated trials (.02).



*Figure 2.* Mean response times for T2 in Experiments 1A and 1B, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Discussion*

The results of Experiments 1A and 1B nicely replicated the pattern of repetition effects observed by Spadaro et al. (2012). A non-spatial IOR effect was observed on trials in which a response was made to an intervening event, whereas a repetition benefit was observed on trials in which a response was always withheld to an intervening event. Yet, it remains unclear whether it is the response itself, or attention processes that typically accompany the requirement to respond, that is critical to this intervening event effect. In particular, the contrasting results of Experiments 1A and 1B could in principle be explained by the fact that participants only pay attention to the intervening event in Experiment 1A; that is, they may completely ignore the intervening event in Experiment 1B. This issue was addressed in the following experiments using a Go/No-Go procedure.

Experiment 2

To address whether it was responding itself, or attentional processing that typically accompanies responding, that produced the different results in Experiments 1A and 1B, we introduced a Go/No-Go procedure in Experiment 2. The same two types of intervening events (filled and unfilled dots) used in Experiments 1A and 1B were used in the current experiment. However, in this experiment, participants were instructed to respond to the filled dots and to withhold a response to the unfilled dots. This Go/No-Go procedure for the intervening event was introduced to ensure that participants attended to all intervening events but responded to some intervening events while withholding a response to others.

If response to the intervening event was the key factor that produced the different results across Experiments 1A and 1B (see also Spadaro et al., 2012), then the results of the present experiment ought to parallel those of the prior experiments. That is, a repetition benefit should be observed for No-Go trials, and a non-spatial IOR effect should be observed for Go trials. In contrast, if attention rather than responding was critical to the different results in Experiments 1A and 1B, then the results of the present experiment ought to differ from those of the prior experiments. In particular, for the No-Go trials in which participants attend but do not respond to the intervening event, a non-spatial IOR effect rather than a repetition benefit may be observed.

Method

*Participants*

Twenty-two participants were recruited from an introductory psychology course or a second year cognition course from McMaster University, and participated for course credit. All participants reported to have normal or corrected-to-normal vision

*Apparatus and Stimuli*

The apparatus and stimuli used in Experiment 2 were the same as those used in Experiments 1A and 1B.

*Procedure and Design*

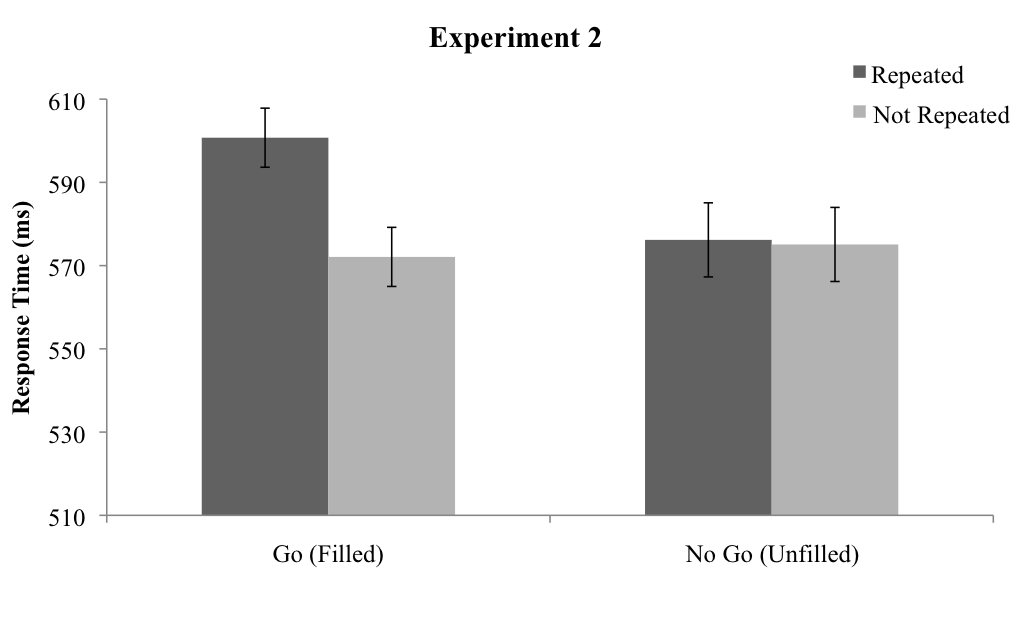
The procedure and design were identical to those used in Experiments 1A and 1B, with the following exception. Rather than consistently responding to the intervening event, as in Experiment 1A, or consistently withholding a response to the intervening event, as in Experiment 1B, the type of intervening event (filled or unfilled dot) determined whether or not a response to the intervening event was required. Participants were instructed to respond to filled intervening events and to withhold a response to unfilled intervening events. As such, we refer to the conditions of this variable using the labels Go and No-Go.

Results

Correct and error trials were defined in the same way as in Experiments 1A and 1B, except that now a trial was also omitted if a response was made to an intervening event on a No-Go trial. RTs for correct trials were submitted to the outlier analysis used in the previous experiments (Van Selst & Jolicoeur, 1994), which eliminated 2.5 % of the RTs from further analysis. Mean RTs for each condition were then computed based on the remaining observations. These mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that included Intervening Event (Go or No-Go), and Repetition (repeated or not-repeated) as within-subject factors. Mean RTs for each condition, collapsed across participants, are displayed in Figure 3 and Table 1.

In the analysis of RTs, there was a significant interaction between Intervening Event and Repetition, F(1,21) = 7.87, p = .011, p2 = .27. Simple main effects were analyzed separately for the Go and No-Go conditions. When the intervening event was a Go event, RTs were slower for repeated trials (601 ms) than not-repeated trials (572 ms), F(1,21) = 16.02, p = .001, p2 = .43. This effect replicates again the non-spatial IOR effect reported in our prior work (Spadaro et al., 2012) and in Experiment 1A. When the intervening event was a No-Go event, RTs on repeated trials (576 ms) were not significantly different than RTs on not-repeated trials (575 ms), F < 1. This result implies that attending but not responding to an intervening event in response to its identity produces a different pattern of results than consistently withholding a response to intervening events on all trials (Experiment 1B). Although a non-spatial IOR effect was not observed in this condition, the repetition benefit observed in Experiment 1B was also clearly not observed.

In the analysis of error rates, there was a significant main effect of Intervening Event, F(1,21) = 5.41, p = .03, p2 = .22. Participants made more errors on Go trials (.04) than on No-Go trials (.02).



*Figure 3.* Mean response times for T2 in Experiment 2, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Discussion*

The repetition benefit observed in Experiment 1B, in which participants consistently withheld a response to all intervening events, was not observed for the No-Go trials in the present experiment. This result suggests that the engagement of attention on the intervening event can play a role in modulating repetition effects in this task. Although a non-spatial IOR effect was not observed for No-Go trials in this experiment, the results do implicate attention to the intervening event as a process that modulates repetition effects in this task. Furthermore, the results raise the possibility that a non-spatial IOR effect could possibly occur without an overt response to the intervening event, in particular if a method were used that led to a stronger engagement of attention to the intervening event. To examine this issue, we manipulated the difficulty of the Go/No-Go discrimination task in the following experiment.

Experiment 3

The aim of Experiment 3 was to determine whether a more difficult Go/No-Go discrimination task for the intervening event might lead to non-spatial IOR effects for both Go and No-Go trials. Experiment 3 used a similar procedure to Experiment 2, in that participants performed a Go/No-Go discrimination for an intervening event on each trial. However, in the present experiment there were two Go/No-Go conditions, one with a relatively easy discrimination task and one with a more difficult discrimination task. In the easy Go/No-Go condition, the intervening event was either an O or an X, whereas in the difficult Go/No-Go condition the intervening event was an M or an N. Participants in both of these conditions were required to respond to one of the two intervening events and to withhold a response to the other (see Lupiáñez et al., 2001 for a similar discrimination difficulty manipulation in a spatial orienting task). The rationale for this manipulation is that the difficulty of the discrimination task might modulate the extent to which attention engages in processing of the intervening event on No-Go trials, with a stronger engagement of attention to intervening events predicted to occur for the more difficult discrimination. Assuming this to be the case, we predicted that a non-spatial IOR effect might occur for No-Go trials in the difficult discrimination condition but perhaps not in the easy discrimination condition.

Method

*Participants*

Twenty-four participants were recruited from an introductory psychology course and a second year cognition course at McMaster University. All participants reported to have normal or corrected-to-normal vision and were given course credit in exchange for their participation.

*Apparatus and Stimuli*

The apparatus and stimuli used in Experiment 3 were nearly identical to those used in Experiment 2, except for the type of stimuli used as the intervening event. All the intervening event stimuli (“X”, “O”, “M”, and “N”) were scaled to fit within the dimensions of the intervening event when it was presented as a grey dot in prior experiments (i.e., the intervening event fit within an invisible circle with a radius subtending .45 degrees of visual angle).

*Procedure and Design*

The procedure was similar to the one used in Experiment 2 with the following exceptions. The difficulty of the Go/No-Go discrimination was manipulated between blocks, such that one difficulty condition was presented in the first half of the experiment, and the other difficulty condition was presented in the second half of the experiment. The order in which the easy and difficult conditions were presented was counterbalanced so that half the participants performed the easy condition first and half performed the difficult condition first. As the experiment was now divided into two halves, a second instruction screen was presented at the halfway point to notify participants of the new Go/No-Go discrimination they would be performing in the second half of the experiment.

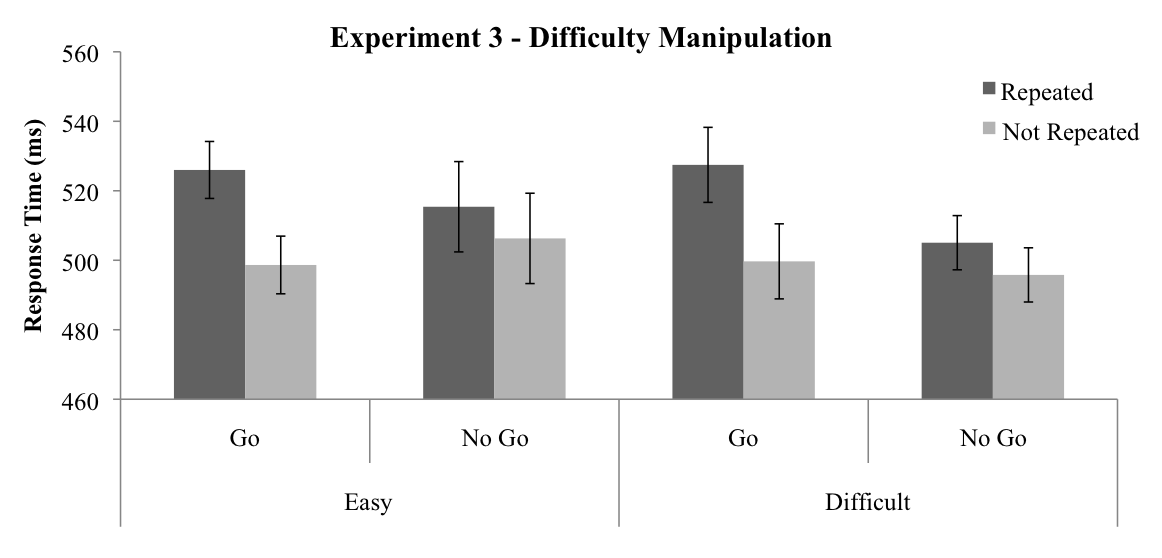
The design for Experiment 3 now included Difficulty (easy/difficult) as a within-subject factor, in addition to the within-subject factors Intervening Event (Go/No-Go), and Repetition (repeated/not-repeated). The Intervening Event and Repetition factors were both manipulated mixed within blocks, while the Difficulty factor was manipulated between blocks.

Results

The data from one participant were excluded from analyses because their RTs in the difficult condition were more than two standard deviations longer than the mean RTs for all participants. Correct trials were defined in the same way as in Experiments 1A and 1B. RTs measured on correct trials were submitted to the outlier analysis used in the previous experiments (Van Selst & Jolicoeur, 1994), which eliminated 2.9 % of the RTs from further analysis. Mean RTs for each condition were then computed based on the remaining observations. These mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that included Difficulty (easy/difficult), Intervening Event (Go/No-Go), and Repetition (repeated/not-repeated) as within-subject factors. Mean RTs for each condition, collapsed across participants, are displayed in Figure 4 and Table 1.

To ensure that the difficulty manipulation was effective, RTs measured on participant’s correct responses to the intervening event were also submitted to the outlier analysis, which eliminated 2.2% of the RTs from the computation of mean RTs for the two intervening event conditions. A one-tailed paired-sample T-test confirmed that responses on easy Go/No-Go trials were faster (447 ms) than RTs on difficult Go/No-Go trials (491 ms), *t*(22) = 2.57, p = .017, *d* = .48.

In the analysis of colour identification RTs for T2, the only significant effect in the ANOVA was the main effect of Repetition, F(1,22) = 10.91, p = .003, p2 = .33. Responses were slower for repeated trials (518 ms) than for not-repeated trials (500 ms). However, the primary focus was the repetition effect for No-Go trials. Separate planned comparisons of the repetition effects for the Go and No-Go conditions, collapsed across Difficulty, revealed a significant non-spatial IOR effect for Go trials, t(22) = 3.07, p = .006, *d* = .64, but not for No-Go trials, p > .10. Moreover, this result was very consistent across the two difficulty conditions (see Figure 4). If the difficulty manipulation had led participants to engage attention in processing of No-Go intervening events selectively for the difficult condition, then we might have expected this pattern to differ across the difficulty conditions, perhaps leading to a three-way interaction between Difficulty, Intervening Event, and Repetition. As can be seen in Figure 4, there was no hint of this three-way interaction, F<1. No significant effects were observed in the analysis of error rates.



*Figure 4.* Mean response times for T2 in Experiment 3, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Discussion*

The Go/No-Go procedure used here and in Experiment 2 requires participants to attend to the intervening event on all trials. As in Experiment 2, this requirement to attend to the intervening event appears to have eliminated the repetition benefit that was observed in Experiment 1B, when participants could consistently ignore the intervening event. However, the specific objective of this experiment was to examine whether manipulating the difficulty of the Go/No-Go discrimination might impact the repetition effect on No-Go trials, with difficult No-Go discriminations perhaps leading to a repetition cost (i.e., a non-spatial IOR effect). The results show clearly that this was not the case. As in Experiment 2, a non-spatial IOR effect was observed for Go trials, but not for No-Go trials, and this result did not depend on the difficulty of the Go/No-Go discrimination. From these results we must conclude that an increase in the perceptual processing demands for the intervening event is unlikely to result in a non-spatial IOR effect for No-Go trials.

Experiment 4

In both the easy and difficult discrimination conditions of Experiment 3, a particular intervening event target was consistently mapped to either the Go or No-Go response categories. As such, although perceptual discrimination difficulty differed across the two conditions, response selection difficulty may not have differed. The present experiment was designed to address whether response selection difficulty modulates the repetition effect for No-Go trials.

In the present experiment, participants were initially instructed to respond to one type of intervening event (e.g. “O”) and withhold a response to another type of intervening event (e.g. “X”). The results from this initial phase of the experiment were of course predicted to be similar to those observed in Experiments 2 and 3, with a null repetition effect observed for No-Go trials. However, in a subsequent phase of the experiment, the Go/No-Go instructions changed from block to block. In other words, participants learned initially to respond to an “O” and withhold a response to an “X”, but then had the instructions switch so that they were required to respond to an “X” and withhold a response to an “O”. This switch of instructions occurred at three points within the experiment so that we were able to gather a sufficient amount of data from trials in which the current Go/No-Go response mapping differed from that in a recent prior block. By continually changing the response mappings for the intervening events, we assumed that No-Go targets would not only have to be attended from a perceptual standpoint, but they would also cue response selection processes. If the activation of response selection processes for the intervening event, without necessarily evoking an overt response, is critical to the non-spatial IOR effect, then we might well observe a non-spatial IOR effect for No-Go trials in the later “switch” phase of this experiment.

Method

*Participants*

Forty-eight participants were recruited from an introductory psychology course and a second year cognition course at McMaster University, and participated for course credit or $10 remuneration. Twenty-four participants were assigned to the XO intervening event condition, and another twenty-four participants were assigned to the MN intervening event condition[[2]](#footnote-2). All participants reported to have normal or corrected-to-normal vision.

*Apparatus and Stimuli*

The apparatus and stimuli used in Experiment 4 were identical to those used in Experiment 3.

*Procedure and Design*

The procedure closely followed the overall procedure of Experiment 3, but a number of changes were made to include blocks of trials where the response-mapping instructions to the intervening events switched. In short, this objective was achieved by assigning one of two intervening events to be the Go intervening event and the other to be the No-Go intervening event at the outset of the experimental session. This assignment of stimuli to Go/No-Go roles was maintained for the first half of the experiment. Beginning at the outset of the second half of the experiment, the Go and No-Go roles switched every 30 trials, so that for the remainder of the experimental session Go and No-Go intervening events did not have a consistent mapping to roles across the experimental session.

For one group of participants, “X” and “O” were used as the intervening events, while for the other group of participants, “M” and “N” were used as the intervening events. Within each of these groups, half of the participants started the experimental session with each of the two possible intervening events as Go targets. An instruction screen appeared at the start of the first block of trials that informed participants which intervening event required a response, and which intervening event did not require a response. The instruction screen was presented for ten seconds before the first block of trials started. This first block consisted of 100 trials, with the first 10 trials being used as practice trials. Following the first block of trials, participants performed three subsequent blocks of 30 trials each, which were labeled the Switch blocks. Prior to each of the Switch blocks, an instruction screen was presented that informed participants of the new response mappings for the intervening event stimuli, which alternated at the start of each of the Switch blocks. As an example, if a participant was instructed to respond to an O and withhold a response to an X in the first block of trials, they would then be instructed to respond to an X and withhold a response to an O in the first Switch block, to respond to an O and withhold a response to an X in the second Switch block, and to respond to an X and withhold a response to an O in the final Switch block.

For the purpose of analysis, the data from all three of the switch blocks were collapsed together and compared with performance in the first block of trials. As such, the design for Experiment 4 included Group (XO or MN) as a between-subjects variable and Block Type (first block or switch block), Intervening Event (Go or No-Go), and Repetition (repeated or not-repeated) as within-subject variables.

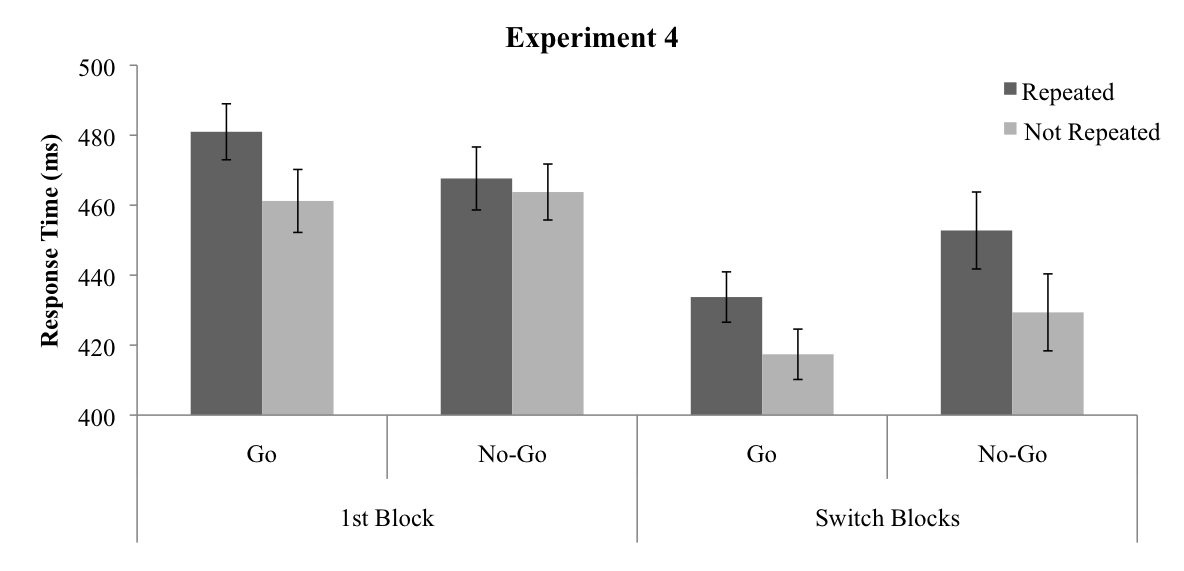
Results

RTs on correct trials were submitted to the same outlier analysis as in previous experiments (Van Selst & Jolicoeur, 1994), which eliminated 3.5 % of the RTs from further analysis. Mean RTs for each condition were then computed based on the remaining observations. These mean RTs and corresponding error rates were submitted to mixed-design analyses of variance that included Block Type (first block or switch block), Intervening Event (Go or No-Go), and Repetition (repeated or not-repeated) as within-subject factors, and Group (XO or MN) as a between-subjects factor. Mean RTs for each condition, collapsed across participants, are displayed in Figure 5 and Table 1.

In the overall analysis of RTs, there was a significant main effect of Block Type, F(1,46) = 23.75, p < .001, p2 = .34. RTs were slower on trials in the first block (468 ms) than on trials in the switch blocks (433 ms), which could be attributed simply to a practice effect. More important, there was a significant interaction between Block Type, Intervening Event and Repetition, F(1,46) = 5.52, p = .023, p2 = .11. To examine this interaction further, separate analyses of variance were performed for the two Block Types, both of which treated Group (XO/MN) as a between-subjects factor, and Intervening Event (Go/No-Go) and Repetition (repeated/not-repeated) as within-subject factors.

In the analysis of the first block, there was a significant interaction between Intervening Event and Repetition, F(1,46) = 4.81, p = .033, p2 = .095. On Go trials, RTs were slower for repeated trials (481 ms) than for not-repeated trials (461 ms), F(1,46) = 12.52, p < .001, p2 = .21. In contrast, on No-Go trials, there was no difference between RTs for repeated trials (467 ms) and not-repeated trials (463 ms), F < 1. This pattern of RTs for the first block of trials provides a nice replication of the results from Experiments 2 and 3 – a non-spatial IOR effect for Go trials, and the absence of a repetition effect for No-Go trials.

In the analysis of the switch block, there was a main effect of Repetition, F(1,46) = 13.85, p = .003, p2 = .23, with slower RTs for repeated trials (443 ms) than for not-repeated trials (423 ms). Importantly, Repetition did not interact with Intervening Event, F<1. Planned comparisons of the repetition effect for Go and No-Go trials, collapsed across Group, revealed slower RTs for repeated trials than for not-repeated trials both for Go trials, t(47) = 3.49, p = .001, *d* = .50, and for No-Go trials, t(47) = 3.03, p = .004, *d* = .44. As can be seen in Figure 5, the non-spatial IOR effects for the Go trials and the No-Go trials are very similar in magnitude.



*Figure 5.* Mean response times for T2 in Experiment 4, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

Also in the analysis of the switch block, there was a significant interaction between Group and Intervening Event, F(1,46) = 5.10, p = .029, p2 = .10. This interaction was not of immediate theoretical interest; nonetheless, separate analyses of the XO and MN groups for the switch block revealed a significant main effect of the Intervening Event in the MN group, F(1,23) = 31.19, p < .001, p2 = .58, with faster responses for Go trials than for No-Go trials, but not in the XO group, F<1.

In the analysis of error rates, there was a significant main effect of Intervening Event, F(1,46) = 4.90, p = .03, p2 = .08. Participants made more errors on Go trials (.03) than on No-Go trials (.02).

*Discussion*

The aim of the present experiment was to examine whether engagement in response selection for the intervening event is critical for observing a non-spatial IOR effect. The results were indeed consistent with this hypothesis. In particular, a non-spatial IOR effect was observed without requiring a response to the intervening event, but only when participants withheld a response to an intervening event that had required a response in an earlier block in the experiment (i.e., in the switch block). The difference between the patterns of repetition effects that were observed in the first block compared to the switch blocks highlights the importance of response selection, rather than response execution, to the intervening event effect.

General Discussion

The present study examined non-spatial IOR effects in a 2-afc task, and in particular whether such effects require participants to respond to an intervening event (Spadaro et al., 2012). As in our prior work, a non-spatial IOR effect was observed in all experiments when participants did respond to an intervening event. Interestingly, across the experiments a wide range of results occurred when participants withheld a response to the intervening event. When a response was consistently withheld to intervening events on all trials, a repetition benefit rather than a repetition cost was observed (Experiment 1B). When a Go/No-Go method was introduced, requiring participants to respond to some intervening events and withhold a response to others, neither a repetition benefit nor a repetition cost was observed on No-Go trials (Experiments 2 & 3). Finally, when participants were required to withhold a response to an intervening event that had previously required a response in an earlier phase of the experiment, a repetition cost was observed for No-Go trials (Experiment 4). Indeed, the repetition effects for Go and No-Go trials in Experiment 4 did not differ.

As such, the experiments in the current study reveal an important property of the intervening event effect that was not evident in the original study by Spadaro et al. (2012). It appears that execution of a response to the intervening event is not necessary to observe non-spatial IOR effects in a target-target variant of the 2-afc task. Rather, it appears that response selection in the absence of response execution may be sufficient to observe non-spatial IOR in this context. This conclusion follows from an assumption that No-Go intervening events that had served as Go intervening events previously in the experimental session automatically and reliably elicited response selection processes, whereas No-Go intervening events that were never responded to may have elicited response selection processes much less reliably. To the extent that response selection constitutes a “bottleneck” process critical in encoding one event as separate from another (Wyble, Potter, Bowman, & Nieuwenstein, 2011), eliciting response selection processes for the intervening event may have led to the generation of separate event representations for T1, the intervening event, and T2. The remainder of the discussion takes these empirically oriented observations in two separate directions, outlining two potential frameworks for explaining why non-spatial IOR effects in a target-target variant of the 2-afc task might depend on engagement in response selection for an intervening event.

*The intervening event effect: A reorienting framework*

The spatial IOR effect is commonly attributed to a process that inhibits the reorienting of attention to previously attended locations (Posner & Cohen, 1984). We refer to this interpretation as the reorienting hypothesis for IOR. According to this hypothesis, the mechanism that produces the IOR effect is related directly to the withdrawal, or disengagement, of attention from the location at which attention is initially captured by an abrupt onset cue (Klein, 2000). A common method for ensuring that attention has been withdrawn from a cued location is to present a central cue during the time between offset of the peripheral cue and onset of the target (Pratt & Fischer, 2002; Prime, Visser, & Ward, 2006). The putative function of the central cue is to capture attention back at fixation, thus ensuring that attention is withdrawn from and therefore not oriented to the cued location at the time of onset of the target (although presentation of a central cue is not always necessary to observe spatial IOR effects, see Prime et al., 2006). This assumption has led researchers to use the term “cue-back” to refer to methods in which a central cue is used in spatial cueing tasks.

The surface similarity between the intervening event in our procedure and the central cue (or cue-back) used in spatial cueing studies makes it tempting to explain the intervening event effect observed here with reference to the reorienting hypothesis. Indeed, the function of the intervening event in our studies of non-spatial IOR effects could be analogous to the function of a central cue in spatial cueing studies. By this view, the non-spatial IOR effects observed here and in Spadaro et al. (2012) could be explained by reference to an inhibitory process elicited by the disengagement of attention from T1. If engagement in response selection in response to the intervening event is crucial to the disengagement of attention from T1, then the reorienting hypothesis might well provide a useful framework for explaining the intervening event effect.

However, the reorienting hypothesis has difficulty explaining a growing set of findings indicating that spatial IOR effects can be measured at cued locations from which attention could not have been disengaged (Berger, Henik, & Rafal, 2005; Berger, Chelazzi, & Tassinari, 2000; Berlucchi, 2006; Lupiáñez, Decaix, Siéroff, Chokron, Milliken, & Bartolomeo, 2004; Tassinari, Agliotti, Chelazzi, Peru, & Berlucchi, 1994). For instance, Tassinari et al. observed an IOR effect when the SOA between cue and target was zero, a result that undermines a key assumption of the reorienting hypothesis (Posner & Cohen, 1984; Maylor & Hockey, 1985). Furthermore, researchers from a variety of labs and with a variety of methods have observed that IOR is observed even when participants either actively (Berger et al, 2005; Berlucchi et al., 2000; Lupiáñez et al, 2004) or passively (Martín-Arévalo, Kingstone & Lupiáñez, 2013) maintain attention at the cued location. If IOR effects can be observed when attention is maintained at a cued location, then it seems unlikely that disengagement of attention from a cued location triggers the mechanism responsible for IOR.

Another relevant set of findings in the spatial cueing literature concerns the different effect of intervening (cue-back) events in tasks requiring detection and discrimination. Whereas intervening (cue-back) events are sometimes necessary to observe IOR in discrimination tasks (such as the 2-afc task used in the present study), they appear to be unnecessary to observe IOR in detection tasks (Hu et al., 2011). Taking into account the above findings, Lupiáñez and colleagues (Lupiáñez, 2010; Lupiáñez, Ruz, Funes, & Milliken, 2007; Lupiáñez, Martín-Arévalo, & Chica, 2013) have proposed that the reorienting of attention is not required to observe an IOR effect. Instead, they propose that the role of a central cue in spatial cueing studies is to disrupt an episodic integration process that produces an effect on target discrimination that opposes IOR. By this alternative view, the presentation of intervening (cue-back) events in spatial orienting studies can help to reveal IOR effects, but do not play a critical role in causing those effects. Instead, the mechanism proposed to cause IOR effects is one that favors the detection and encoding of new events, a process that incurs a cost when detecting and encoding targets that match spatially with preceding cues. According to this view, intervening (cue-back) events are not necessary to observe IOR in detection tasks because there are no perceptual discrimination benefits afforded by encoding a similar old event (the cue episode) that have to be disrupted to reveal the cost associated with detecting/encoding a similar new event.

Applied to the current set of results, this new event detection/encoding hypothesis does not attribute the non-spatial IOR effects to an inhibitory process triggered by the orienting of attention to the intervening event. Instead, the encoding of the intervening event as a separate event from both T1 and T2 is assumed to disrupt a process that would normally speed responses to repeated relative to not-repeated trials. In the absence of this repetition benefit, the contribution of a separate opposing process to performance is then revealed. The following section outlines in more detail how this type of dual process framework accounts for the pattern of non-spatial IOR effects observed here and in prior studies (Spadaro et al., 2012; Hu et al., 2011).

*The intervening event effect: A dual process framework*

Similar to dual process accounts of the spatial IOR effect (see: Klein, 2000; Tipper et al., 1998), Spadaro et al. (2012) incorporated the idea of two opposing processes to explain non-spatial IOR effects observed with a 2-afc target-target procedure. Essentially, this framework assumes that two processes could simultaneously underlie performance even in a simple 2-afc task. By this view, one process is responsible for speeding performance to repeated (i.e. familiar) events relative to not-repeated (i.e. novel) events, while a separate process is responsible for slowing performance to repeated events relative to not-repeated events. The joint contribution of both processes could determine the nature of the repetition effect in any given context. Importantly, Spadaro et al. argued that the relative contributions of these two processes could be modulated by the intervening event manipulation.

In particular, we assume that the facilitative process that normally speeds performance on repeated trials relative to not-repeated trials is related to episodic integration (Logan, 1988; Kahneman, Treisman, & Gibbs, 1992; Hommel, 1998). This episodic integration process contributes to performance when a current target event cues the retrieval of other similar target events that have already been encoded. On a repeated trial, onset of T2 could cue the retrieval of the previous episode (i.e., the T1 episode) from the same trial. As a consequence, responses to T2 on repeated trials would be affected by integration of a previously encoded T1 event with ongoing processing of T2. It follows that processing of T2 on repeated trials, but not on not-repeated trials, would benefit from the fluid integration of the T1 processing episode with processing of T2, producing faster performance on repeated trials than on not-repeated trials.

We assume that episodic integration on repeated trials will be robust to the extent that the context between T1 and T2 remains undisturbed, as would be the case when there is no intervening event between T1 and T2. However, the requirement to encode an intervening event holds the potential to disrupt the context between T1 and T2, which in turn can disrupt episodic integration. If episodic integration is the mechanism that speeds responses for repeated trials relative to not-repeated trials, then the encoding of a new event between T1 and T2 might reasonably eliminate this effect on performance. To varying degrees across our Go/No-Go experiments, the encoding of a new event is assumed to have disrupted episodic integration, with the most compelling disruption occurring when participants engaged in response selection for the intervening event (Experiment 4). A key issue here may be that engagement in response selection for the intervening event made inaccessible at the onset of T2 the colour-response binding for T1. This dual process framework then assumes that the absence of episodic integration, either because integration was disrupted (Spadaro et al., 2012, the present experiments), or because episodic integration is irrelevant for the task at hand (Dodd, Van der Stigchel, & Hollingworth, 2009), offers an opportunity to observe the influence of an opposing process that slows processing on repeated relative to not-repeated trials.

The precise nature of this opposing process, one that slows responding to repeated relative to not-repeated events, is still an open question. However, the fact that IOR-like effects appear to occur for both spatial and non-spatial stimulus properties suggests it is a process that is broadly important in preferential orienting to novel events. Indeed, Dukewich (2009) has recently re-considered the idea that habituation processes (Sokolov, 1963) may contribute to IOR. In a related vein, and as noted above, Lupiáñez (2010) has proposed that IOR may reflect a cost associated with detecting the onset of events that match memory representations of prior events (Lupiáñez et al., submitted; Hu et al., 2011). For our purpose, the key point to note is that non-spatial IOR effects in a target-target variant of the 2-afc procedure forces consideration of some broadly important process in the efficient encoding of novel relative to repeated events.

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CHAPTER 4: DISRUPTION OF CONTEXT-SPECIFIC SPATIAL BINDING PROCESSES REVEALS NON-SPATIAL ior

Adam Spadaro & Bruce Milliken

Preface

In Chapter 4, the dual process framework was tested to determine whether it could account for the pattern of repetition effects observed when spatial variability is introduced to the procedure. In the experiments reported in previous empirical chapters the non-spatial target events (T1 and T2) appeared in the same central location. In contrast, in the present study the location of the target events was unpredictable, although the task remained to respond to the colour of T1 and T2. The introduction of spatial uncertainty for T1 and T2 added to the complexity of the results. Nonetheless, the results replicated a pattern that has been reported in prior published studies (Taylor & Donnelly, 2002; Milliken et al., 2000), and fit well with the dual process framework described in previous chapters of this thesis. In addition, the results suggest that a context-specific spatial binding process may contribute to performance when spatial location changes unpredictably from trial to trial.

Abstract

Spadaro, He, & Milliken (2012) discovered that attending and responding to an intervening event presented between two consecutive centrally presented non-spatial targets led to a reversal of repetition priming; that is, this procedure produced an inhibition of return (IOR) like effect. Spadaro et al. proposed a dual process framework in which the same attentional process produces spatial and non-spatial IOR effects, and in which disruption of an episodic integration process is particularly critical to observing non-spatial IOR effects. The aim of the current study was to determine whether the dual process framework can accommodate findings from similar experiments in which the targets appear unpredictably in one of two (Experiment 2) or three (Experiment 3) locations, rather than predictably in the same central location. The results were consistent with the dual process framework with one additional assumption; that when targets are presented unpredictably in different locations, but not when targets are presented predictably in the same central location, a context-specific spatial binding process supports faster responses for repeated than not-repeated trials.

Introduction

Inhibition of return (IOR) is an attentional orienting effect typically defined by slowed responses to targets that appear at the same location as a preceding abrupt onset peripheral cue (Posner & Cohen, 1984). A common interpretation of this effect is that it reflects a mechanism that slows re-orienting to locations that attention has recently visited. Such a mechanism might well have adaptive value in search contexts in that it would favour shifts of attention to locations not already visited, thereby offering efficient coverage of the search space (for a review, see Klein, 2000).

Although the IOR effect is most often measured in studies of spatial orienting, a number of studies have reported IOR-like effects for non-spatial stimuli (Law, Pratt & Abrams, 1995; Fox & de Fockert, 2001; Francis & Milliken, 2003; Hu, Samuel & Chan, 2011; Spadaro, He & Milliken, 2012). The relation between spatial IOR effects and non-spatial IOR-like effects is an important conceptual issue in the domain of attention because it points to the possibility that the mechanism responsible for IOR effects has a broader scope than originally proposed. In particular, IOR-like effects with non-spatial stimuli invite consideration of a general mechanism that favours the encoding of novel over familiar events, rather than a specific mechanism that favours spatial orienting to novel events (Dukewich, 2009; Lupiáñez, Martín-Arévalo, & Chica, 2013).

The idea that similar mechanisms cause spatial IOR and non-spatial IOR-like effects is appealing from the standpoint of parsimony. An account in which a single mechanism explains a wide range of effects is preferable to an account in which many different task-specific mechanisms explain the same effects. At the same time, there appear to be some salient differences between the effect of stimulus repetition in the spatial and non-spatial domains. A particular challenge for those who have argued that non-spatial IOR-like effects have the same underlying cause as spatial IOR effects is the fact that spatial IOR effects occur with both a cue-target procedure (where participants withhold a response to a cue and respond to a following target) and a target-target procedure (where participants respond to targets on all trials; Maylor & Hockey, 1985). In contrast, a target-target procedure that measures non-spatial effects of stimulus repetition has long been known to produce repetition priming (Bertelson, 1965) rather than an IOR-like effect.

However, in a recent study we discovered that IOR-like effects can indeed occur in a target-target procedure with non-spatial stimuli, as long as participants are required to attend and respond to an event intervening between consecutive targets (Spadaro et al., 2012; see Law et al., 1995; Fox & de Fockert, 2001 for a similar result using the cue-target method). We proposed that the requirement to attend and respond to an intervening event disrupts an episodic integration process that normally speeds responses for repeated relative to not-repeated targets, thus revealing an underlying non-spatial IOR-like effect. If this hypothesis is correct, then it suggests that an empirical cornerstone in the attention and performance domain, the repetition effect in two-alternative forced-choice (2-afc) tasks (Bertelson, 1965) must be interpreted with caution. In particular, it may be inappropriate to use the presence of “repetition priming” in such a task as a basis for distinguishing between effects of spatial and non-spatial stimulus repetition. A mechanism that produces IOR effects in tasks that involve spatial repetition could conceivably contribute to performance in tasks that involve non-spatial repetition.

In the present study, we aimed to extend this dual-process model to a task that involves repetition of both spatial and non-spatial stimulus attributes. To set the current empirical work in context, the following two sections review prior dual-process proposals in the IOR literature, and prior empirical work that has examined both spatial and non-spatial repetition effects in the same task.

*Dual process accounts of spatial and non-spatial orienting effects*

The procedure used to measure spatial IOR effects often involves presentation of an abrupt onset visual cue in one of two or more peripheral locations, followed by a target at the same location as the cue or at an uncued location. When the cue-target stimulus onset asynchrony (SOA) is relatively short (less than 300 ms) responses to targets are often faster for targets that appear at the same location as the cue than for targets that appear at a different location than the cue. However, when the cue-target SOA is longer than 300 ms, this effect inverts and responses are slower for cued targets than for uncued targets – the IOR effect (Posner & Cohen, 1984). This biphasic property of performance led researchers to propose a dual process view of spatial orienting effects, with facilitation for short cue-target SOAs caused by one process and the IOR effect at longer SOAs caused by a different process.

A common variant of this dual process view assumes that the facilitation effect at short SOAs and the IOR effect at longer SOAs offer relatively pure measures of two separate mechanisms. On the one hand, the facilitation effect occurs because the abrupt onset cue captures attention, and attention then speeds responses to targets that appear at the cued location. On the other hand, the IOR effect is linked directly to the disengagement of attention from the cued location, which is presumed to occur about 300 ms after onset of the cue (see the reorienting hypothesis - Posner, Rafal, Choate, & Vaughan, 1985). Importantly, the contributions of these two processes to performance are assumed not to overlap; disengagement of attention from the cued location both terminates the process responsible for the facilitation effect and initiates the mechanism responsible for the IOR effect.

An alternative dual process view assumes that the process responsible for IOR is initiated upon onset of the cue, but that the effect of this mechanism on performance is initially masked by the facilitation effect at the cued location. According to this view, the emergence of IOR only at longer SOAs occurs because the opposing facilitation effect overrides the IOR effect at short SOAs but not at longer SOAs (Danziger & Kingstone, 1999; Klein, 2000; Tipper et al., 1997). According to this alternative dual process account, orienting effects are the net result of two independent effects – a facilitation effect produced by one mechanism and an inhibitory effect produced by a separate mechanism.

Empirical support for this hypothesis has come from studies in which the two orienting effects have been dissociated in split-brain patients. Tipper and colleagues (1997) discovered that responses to a target presented within a cued object produced two different orienting effects depending on whether the cued object remained within the same visual hemifield, or moved between the two visual hemifields. Patients without an intact corpus callosum (CC) showed the typical IOR effect for cued trials relative to uncued trials when presentation of the stimuli was restricted to the same hemifield. However, the same patients showed a facilitation effect for cued targets when the placeholder for cued object crossed between the two hemifields between presentation of the cue and target. The dissociation between the same-hemifield IOR effect and the opposite-hemifield facilitation effect was taken as evidence that separate mechanisms mediate these two effects.

Additional evidence for the separate mechanism hypothesis comes from studies that have examined the relation between IOR and expectancy. In particular, several studies have revealed IOR effects for targets at locations at which a target is expected to appear (Berger, Henik, & Rafal, 2005; Berlucchi, Chelazzi, & Tassinari, 2000; Lupiáñez, Decaix, Sieroff, Chokron, Milliken, & Bartolomeo, 2004). If participants attend to locations at which targets are expected to occur, these results suggest strongly that attention need not be disengaged from a location for IOR to be observed, and instead favour a view in which IOR is not directly related to the engagement/disengagement of attention. Stated more generally, the mechanism responsible for spatial IOR may be independent of processes that produce facilitation effects.

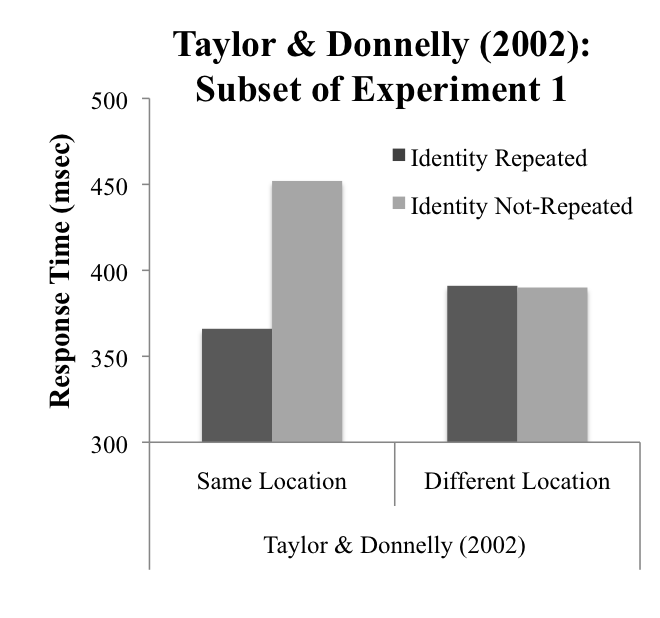
A recent study of non-spatial IOR-like effects (Spadaro et al., 2012) also offers support for a dual process view. The objective of the Spadaro et al. study was to examine whether an IOR-like effect might be observed in a simple 2-afc task, a task that has long been known to produce repetition benefits rather than repetition costs (Bertelson, 1965). Following the dual process logic described above, the key to doing so would be to introduce an event between consecutive targets that would disrupt the processing that usually produces repetition benefits, perhaps revealing a co-existing mechanism that produces repetition costs.

In 2-afc tasks, it is widely assumed that response to a first target leads to the encoding of an episodic representation in which the target stimulus is bound to the response made to that stimulus (Rabbitt, 1968; Pashler & Baylis, 1991; Hommel, 1998). In turn, when the following target is identical, its onset cues the retrieval of this stimulus-response episode, which results in faster performance than would be possible if response selection were driven by analytic use of a S-R mapping rule (Rabbitt, 1968; Logan, 1988; Pashler & Baylis, 1991). Spadaro et al. (2012) reasoned that this episodic integration process would have to be disrupted to observe an IOR-like effect. To that end, participants were required to attend and respond to an intervening event between consecutive targets on some trials but not on other trials, with the idea that attention and response to an intervening event would disrupt the episodic integration process. Indeed, responses to repeated targets were slower than responses to not-repeated targets when participants responded to an intervening event between targets, whereas the customary repetition benefit was observed when no intervening event was presented.

*Measuring the joint influence of spatial and non-spatial repetition*

The dual process framework in which separate processes produce facilitation and IOR is generally consistent with findings from studies that have examined the joint influence of spatial and non-spatial repetition on performance. In particular, two prior studies converged on the conclusion that episodic integration effects might co-occur with IOR effects (Milliken, Tipper, Houghton & Lupiáñez, 2000; Taylor & Donnelly, 2002). The study reported by Taylor and Donnelly (2002) is particularly relevant for the present purpose. They used a procedure in which participants responded to a non-spatial attribute (e.g., shape, orientation) of a target stimulus on each display, with the target appearing in either of two marked spatial locations to the left or right of fixation. Repetition effects were measured as a function of the relation between the target on one trial and the target on the following trial; that is, a target-target procedure was used. The targets on consecutive trials could repeat or alternate on either or both of two dimensions: a non-spatial dimension such as shape (e.g., circle vs. square), and a spatial dimension such as location (e.g., left vs. right).

The results from one of the experiments in this study are depicted in Figure 1, and are generally representative of the data pattern observed across several published experiments that have used similar procedures (Milliken et al., 2000; Taylor & Donnelly, 2002). In the experiment depicted in Figure 1, participants responded to the shape of targets presented on each trial, and consecutive targets could match or mismatch in either shape or location.



*Figure 1.* Subset of the data presented in Experiment 1 of Taylor & Donnelly (2002). Identity-based IOR (i.e. non-spatial IOR) effects are broken down by same location trials, on the right side of the graph, and different location trials, on the left side of the graph.

Figure 1.

First, consider the pattern of location repetition effects. Responses were slower for same location trials than for different location trials only when the identity of the target changed from one trial to the next. In other words, an IOR-like pattern was not observed when the target identity repeated from one trial to the next. In line with the dual process ideas discussed above, this set of results can be explained by assuming that an episodic integration process produces an effect on identity repeated trials that works in opposition to the spatial IOR effect. For example, retrieval of the location-identity binding formed when responding to a first target could conceivably speed performance on trials in which both location and identity repeat from one target to the next (Kahneman, Treisman & Gibbs, 1992). If this were the case, then a spatial IOR effect might well occur only if this integration process were disrupted. In line with this idea, a spatial IOR effect was observed when identity shifted from one trial to the next. As such, these results seem consistent with the view that, although the mechanism responsible for spatial IOR may be operative in all cases, it may be revealed in performance only when an episodic integration process that produces the opposite result does not contribute to performance.

Now consider the pattern of shape repetition effects. One might reasonably assume that a shift in spatial location from one target to the next ought to disrupt episodic integration in the same manner that a shift in identity from one target to the next appeared to disrupt episodic integration. Following the logic of Spadaro et al. (2012), a non-spatial shape-based IOR-like effect ought then to have been observed on different location trials. Yet, the right half of Figure 1 reveals no such effect. Responses were equally fast for the shape repeat and shape alternate conditions when location alternated. These results therefore constitute an important challenge for the dual process framework of Spadaro et al. (2012). Any claim that the processes that produce IOR have a scope that includes both spatial and non-spatial orienting must offer a satisfactory account of behaviour in tasks that involve both spatial and non-spatial orienting.

*The present study*

To address this issue, we conducted a series of three experiments. Experiment 1 was a replication of Spadaro et al. (2012), in which a single target stimulus was presented centrally on all trials, and participants were to respond to the color of that target stimulus. In one of two conditions, there was also an intervening event between consecutive targets. The key finding reported by Spadaro et al., and replicated here, is that the usual repetition benefit is observed when no intervening event is responded to between targets, whereas this effect inverts to a repetition cost when an intervening event is responded to between consecutive targets. In Experiment 2, a similar procedure was used, but targets on each trial were now presented either to the left or right of fixation, meaning that targets could either repeat or alternate both in color and in spatial location. The results for same location trials differed from those in Experiment 1, suggesting that location repetition may play a particularly prominent role in episodic integration when stimuli vary on the location dimension. The results of Experiment 3 confirmed this conclusion by demonstrating that the non-spatial IOR-like effect for centrally presented targets reported in Experiment 1 changed qualitatively when central targets were presented in the context of other trials in which the targets could occur peripherally. All told, the results are consistent with a dual process view (Spadaro et al., 2012), but with the caveat that location repetition can serve as a robust cue for processes that oppose the non-spatial IOR-like effect.

Experiment 1

The procedure used for Experiment 1 was an exact replication of Experiment 1A in the Spadaro et al. (2012) study. In both intervening event and no-intervening event conditions, participants performed a 2-afc decision (blue or yellow) to a first target (T1) and then did the same for a second target (T2), with both targets presented centrally. In the no-intervening event condition, the screen remained blank for the time interval between response to T1 and presentation of T2. In the intervening event condition, a red dot was presented centrally after response to T1 and prior to presentation of T2. Participants in the intervening event condition were required to respond to the red dot by pressing the blue and yellow response keys simultaneously. T1 and T2 were equally likely to match (repeated trials) or mismatch (not-repeated trials) in color. In our prior study, performance was faster for repeated than for not-repeated trials in the no-intervening event condition, while the opposite result (i.e., an IOR-like effect) was observed in the intervening event condition.

Method

*Participants*

Twenty-four students from either an introductory psychology course or a second year cognitive psychology course at McMaster University participated in exchange for course credit. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

All experiments in this study were run on a PC using MEL experimental software (Schneider, 1988). Subjects sat directly in front of a 15” SVGA computer monitor, at adistance of approximately 57 cm. A plus sign was presented as the fixation point in the center of the screen, and subtended a visual angle of 0.6 degrees horizontally and 0.7 degrees vertically. The target stimuli were presented centrally against a black background. T1 and T2 were either a blue or yellow rectangle, and subtended a visual angle of 6.3 degrees horizontally and 1.2 degrees vertically. The intervening event was a red dot that also appeared centrally, and its radius subtended a visual angle of .25 degrees.

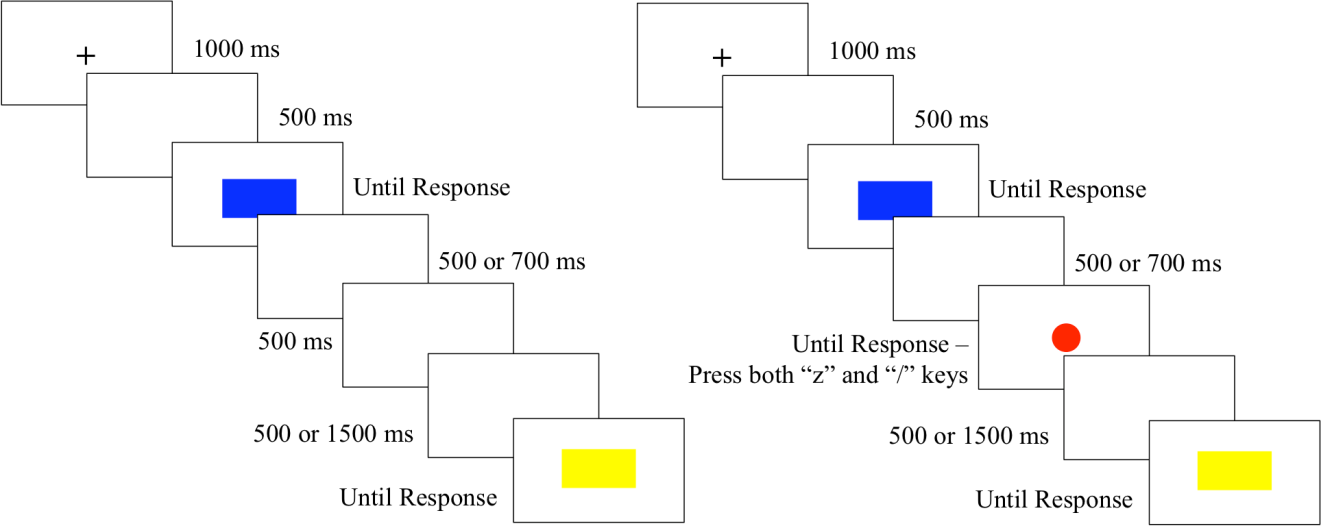
*Procedure & Design*

For both the intervening event and no-intervening event conditions, participants were required to identify whether T1 was blue or yellow, and then to identify whether T2 was blue or yellow. The responses to T1 and T2 were recorded by pressing one of two keys on a keyboard. In the intervening event condition, participants were also required to respond to the onset of the intervening event (a red dot), which appeared after response to T1 but before onset of T2, by pressing both the blue and yellow response keys simultaneously.

Each trial began with a 1000 ms presentation of a fixation cross in the middle of the screen, followed by a 500 ms blank interval. T1 then appeared centrally and remained on the screen until participants responded to its color. Participants were instructed to respond to the color of T1 by pressing “z” for a blue rectangle or “/” for a yellow rectangle. In the no-intervening event condition, a blank interval of either 1500 ms or 2500 ms followed the response to T1. T2 was then presented and remained on the screen until participants recorded their response to its color. In the intervening event condition, response to T1 was followed by a blank interval of either 500 ms or 700 ms. The intervening event (a red dot) was then presented centrally, and remained on the screen until participants responded by pressing the “z” and “/” keys simultaneously. Following response to the intervening event there was a blank interval of either 500 ms or 1500 ms, followed by presentation of T2. The time intervals between response to T1 and onset of the intervening event, and between response to the intervening event and onset of T2, were selected to equate as best as possible the total duration between response to T1 and presentation of T2 across the intervening event and no-intervening event conditions (see also Spadaro et al., 2012). Following response to T2, there was a 2000 ms inter-trial interval.

The intervening event and no-intervening event conditions were tested in separate blocks, with the order of the two blocks counterbalanced across participants. Each block contained 9 sub-blocks of 16 trials, with the first sub-block treated as practice. At the start of both blocks, task instructions were presented on the screen for 30 seconds, and then participants were invited to ask questions to clarify the task requirements. Between each sub-block of 16 trials, there was a break in which the message “Press B to begin next block” appeared, and participants could then initiate the sub-block of 16 trials when they were prepared to continue. The sequence of events on each trial in Experiment 1 is displayed in Figure 2.

The design included three within-subject variables: intervening event (intervening event/no-intervening event), color repetition (repeated/not-repeated), and response-stimulus interval (RSI: 1500 ms/2500 ms). The intervening event variable was manipulated between blocks, whereas the color repetition and RSI variables were mixed randomly within blocks. The order in which the intervening event conditions were tested was included as a between-subjects counterbalancing variable.



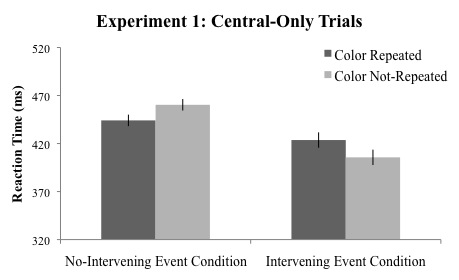
*Figure 2.* Left Panel: The sequence of events for a not-repeated trial in the no-intervening event condition of Experiment 1 is shown. Right Panel: The sequence of events for a not-repeated trial in the intervening event condition of Experiment 1 is shown. Participants were instructed to respond to the intervening event by pressing the response keys for “BLUE” and “YELLOW” simultaneously.

Results

The dependent variable of primary interest was response time (RT) to T2 on trials in which both T1 and T2 were responded to correctly. Error analyses were also conducted on trials in which T1 was responded to correctly but T2 was responded to incorrectly. RTs for T2 on correctly responded to trials in each condition were submitted to an outlier analysis that eliminated 2.1% of RTs from further analyses (Van Selst & Jolicoeur, 1994). The remaining RTs for each condition were used to calculate mean RTs for each condition, and these mean RTs and corresponding error rates were then submitted to mixed-factor analyses of variance that included color repetition (repeated/not-repeated), RSI (1500 ms/2500 ms), and intervening event (intervening event/no-intervening event) as within-subject factors. In preliminary analyses, we also included condition order (intervening/no-intervening vs. no-intervening/intervening) as a between-subject factor. However, this factor did not interact with any of the other factors in the design, and so it was not included in the final set of analyses. The alpha criterion was set to .05 for all statistical effects. Means RTs and error rates for each condition, collapsed across participants, are listed in Table 1.

In the analysis of RTs, there was a significant two-way interaction between color repetition and intervening event, F(1,23) = 35.7, p < .001, p2 = .61. This interaction was examined further by conducting separate analyses for the intervening event and no-intervening event conditions. In the intervening event condition, there was a main effect of color repetition, F(1,23) = 12.2, p = .002, p2 = .35, with slower responses for repeated trials (424 ms) than for not-repeated trials (406 ms). In contrast, in the no-intervening event condition, there was also a main effect of color repetition, F(1,23) = 11.9, p = .002, p2 = .34, but in this case responses were faster for repeated trials (444 ms) than for not-repeated trials (460 ms). The dependence of the color repetition effect on the intervening event condition is depicted in Figure 3.

Figure 3.



*Figure 3.* Mean response times for T2 in Experiment 1, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Table 1.* Mean response times and error rates for T2 (ms) for each condition in Experiments 1, 2, & 3.



Although of less obvious theoretical significance, there was a significant main effect of intervening event, F(1,23) = 22.22, p < .001, p2 = .49, with faster responses in the intervening event condition (415 ms) than in the no-intervening event condition (452 ms). There was also a significant interaction between intervening event and RSI, F(1,23) = 8.35, p = .008, p2 = .27. Subsequent analyses revealed that responses were faster for the 2500 ms RSI condition than for the 1500 ms RSI in the no-intervening event condition, F(1,23) = 14.44, p = .001, p2 = .39, but not so in the intervening event condition (see Table 1). Although this interpretation is somewhat speculative, it appears that response to an intervening event may have obviated some form of proactive interference from T1 to T2, thus speeding performance overall, and particularly so for the shorter of the two RSIs. It is worth noting that responses have been faster on trials with intervening events in numerous similar prior experiments (see Spadaro et al., 2012).

In the analysis of error rates, there was a significant interaction between color repetition and intervening event F(1,23) = 12.10, p = .002, p2 = .35. To examine this interaction further, separate analyses were performed on the error rates for the intervening event and no-intervening event conditions. In the intervening event condition, more errors were made for repeated trials (.04) than for not-repeated trials (.01), F(1,23) = 12.85, p = .002, p2 = .36. In the no-intervening event condition, there was also a significant effect of repetition, F(1,23) = 5.41, p = .03,p2 = .19, but in this case fewer errors were made for repeated trials (.03) than for not-repeated trials (.04). This pattern of error rates is similar to that described above for the RTs, thus mitigating a speed-accuracy trade-off interpretation of the RT results.

*Discussion*

The results from this experiment constitute a successful replication of Experiment 1A from the study by Spadaro et al. (2012). Without an intervening event, responses were faster for repeated than for not-repeated trials, whereas with an intervening event the opposite result was observed. This result fits with the view that responding to an intervening event disrupts an episodic integration process that speeds responding on repeated relative to not-repeated trials, and that in the absence of this episodic integration process an IOR-like effect can be observed.

Experiment 2

Kahneman et al. (1992) proposed that integration of visual episodes, or object files, is cued by a spatio-temporal correspondence process. By this view, onset of a target results in a comparison of the spatio-temporal co-ordinates of the current target and those of existing object files in memory. If a match is found, then the existing object file is updated with the contents of the current target, a process that supports the continuity of visual events across time. In contrast, if a match is not found, then a new object file must be created. In effect, this framework predicts that a switch in location from one trial to the next disrupts episodic integration, and therefore ought to produce the same influence on performance as our intervening event manipulation in Experiment 1.

To examine this issue, an experiment identical to Experiment 1 was conducted, but with the targets presented either to the left or right of fixation. With this procedure, the effects of color repetition can be measured for four separate conditions: (1) location repetitions – no response to an intervening event; (2) location switches – no response to an intervening event; (3) location repetitions – response to an intervening event; and (4) location switches – response to an intervening event. Conditions (1) and (2) offer a conceptual replication of Taylor and Donnelly (2002), whereas conditions (3) and (4) test those same conditions with the addition of a response to an intervening event.

One set of predictions for this experiment follows from the view that location shifts from T1 to T2 on the one hand, and response to an intervening event between T1 and T2 on the other hand, disrupt episodic integration processes in precisely the same way. By this view, we might expect color repetition benefits to occur only in the condition in which location repeats and response to an intervening event is not required, as this is the only condition in which episodic integration would not be disrupted (either by location switches, or by response to an intervening event). In all other conditions, episodic integration ought to be disrupted one way or another, and an IOR-like color repetition effect ought to be observed. It is noteworthy that the Taylor and Donnelly (2002) results contradict this prediction, which was one of the key motivating factors for conducting this experiment; to examine whether the Taylor and Donnelly (2002) result can be replicated with our materials.

Method

*Participants*

Twenty-four students from either an introductory psychology course or a second year cognitive psychology course at McMaster University participated for course credit. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

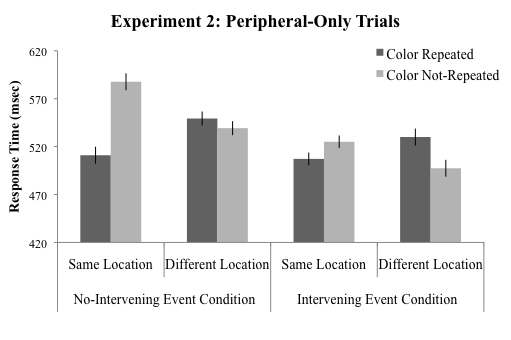
The only difference between Experiments 1 and 2 was that T1 and T2 now appeared either to the right or left of fixation rather than appearing at fixation. Specifically, the targets appeared centered on either of two locations that were 8.3 degrees left or right of fixation on the horizontal axis.

*Procedure & Design*

The procedure and design of Experiment 2 were identical to Experiment 1 with two exceptions. First, a constant RSI of 1500 ms was used. Second, the location of T1 and T2 was manipulated within-subject. As such, T1 and T2 appeared either left or right of fixation, and therefore also appeared either in the same location or in different locations.

Results

Correct trials were defined as in Experiment 1. RTs to T2 on correct trials were submitted to an outlier analysis that eliminated 2.5% of RTs from further analyses (Van Selst & Joliceour, 1994). Mean RTs in each condition were then computed based on the remaining observations, and these mean RTs and corresponding error rates were submitted to their respective repeated-measures ANOVAs that included color repetition (repeated/not-repeated), location repetition (repeated/not-repeated), and intervening event (intervening event/no-intervening event) as within-subject factors. The alpha criterion was set to .05 for all statistical comparisons. Means of mean RTs and error rates for each condition, collapsed across participants, are displayed in Figure 4 and listed in Table 1.



*Figure 4.* Mean response times for T2 in Experiment 2, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

In the analysis of RTs, there was a significant main effect of intervening event, F(1,23) = 4.78, p = .039, p2 = .17, with faster responses in the intervening event condition (515 ms) than in the no-intervening event condition (547 ms). More important, there was a significant three-way interaction between color repetition, location repetition, and intervening event, F(1,23) = 5.85, p = .024, p2 = .20. To examine this interaction further, separate analyses were performed for the intervening event and no-intervening event conditions.

In the no-intervening event condition, there was a significant interaction between color repetition and location repetition, F(1,23) = 54.56, p < .001, p2 = .70. Subsequent analyses revealed that when location was repeated, responses were faster for color repeated trials (511 ms) than for color not-repeated trials (588 ms), F(1,23) = 80.72, p < .001, p2 = .78. In contrast, when location was not repeated, responses for the color repeated (549 ms) and color not-repeated (539 ms) conditions did not differ significantly, F = 1.29. This pattern of results nicely replicates that reported by Taylor and Donnelly (2002; see Figure 1).

In the intervening event condition, there was a significant interaction between color repetition and location repetition, F(1,23) = 21.40, p < .001, p2 = .48. Subsequent analyses revealed that when location was repeated, responses were faster for color repeated trials (506 ms) than for color not-repeated trials (526 ms), F(1,23) = 4.27, p = .050, p2 = .16. In contrast, when location was not repeated responses were slower for color repeated trials (530 ms) than for color not-repeated trials (497 ms), F(1,23) = 22.07, p < .001, p2 = .49.

In the analysis of error rates, there was a significant interaction between color repetition and intervening event, F(1,23) = 12.09, p = .002, p2 = .35. To examine this interaction further, separate analyses were performed for the intervening event and no-intervening event conditions. In the intervening event condition, more errors were made for color repeated trials (.04) than for color not-repeated trials (.02), F(1,23) = 15.41, p = .001, p2 = .40. In the no-intervening event condition, the difference in error rates for color repeated (.03) and color not-repeated (.03) trials was not significant, p = 0.21.

*Discussion*

The pattern of color-based repetition effects observed in this experiment clearly contradicts the set of predictions outlined above. Recall that those predictions were based on the assumption that a switch in location from T1 to T2, and a response to an intervening event between T1 and T2, might produce equivalent disruptive effects on episodic integration. If this were the case, then non-spatial IOR-like effects of colour repetition ought to have been observed for all conditions except that in which location repeated and no response was made to an intervening event. Instead, the only condition in which an IOR-like colour repetition effect was observed was the condition in which location shifted from T1 to T2 *and* a response was required to the intervening event. Let us consider, in turn, the two conditions in which the dual process framework (Spadaro et al., 2012) predicted that an IOR-like colour repetition effect would occur but did not.

First, consider the results from the condition in which location switched from T1 to T2 and no response was required to an intervening event. Here, there was no difference in performance for colour repeated and colour not-repeated trials, a finding that constitutes a replication of the results reported by Taylor and Donnelly (2002). A primary aim of this experiment was to determine whether this result could be replicated with our procedure, and in this respect, the results are very clear. The Taylor and Donnelly (2002) result is a robust one that the dual process framework must accommodate. In particular, some explanation is needed for why a shift in location from T1 to T2 does not produce an IOR-like colour repetition effect.

Second, consider the results from the condition in which location repeated from T1 to T2 and a response was required to an intervening event. Here, despite T1 and T2 appearing in the same location, an IOR-like colour repetition effect did not occur; in fact, the opposite effect was observed. What requires an explanation here is why response to an intervening event in this condition failed to produce an IOR-like colour repetition effect when such an effect has been observed in many prior experiments in which T1 and T2 appear in the same location (see Experiment 1 of the present study; Spadaro et al., 2012).

In summary, without some modification, the dual process framework of Spadaro et al. (2012) fails to explain two components of the results of Experiment 2. To understand this shortcoming of the dual process framework, the role played by the different experimental contexts in Experiments 1 and 2 must be considered. In particular, in Experiment 1 targets were presented in the same location on every trial, whereas in Experiment 2 targets were presented in one of two locations on every trial. The idea that these two different contexts may have contributed to performance is supported by the fact that, in the intervening event condition, an IOR-like effect was observed for same location trials (which always occurred) in Experiment 1, whereas a repetition benefit was observed for same location trials (which sometimes occurred) in Experiment 2. The idea that context contributed to the performance difference in these conditions across the two experiments was examined more carefully in Experiment 3.

Experiment 3

As noted above, the discrepant results of Experiments 1 and 2 may owe to the fact that the spatial location of targets was manipulated in Experiment 2 but not in Experiment 1. The present experiment aimed to address whether presenting targets in the context of spatial variability indeed played a key role in the results of Experiment 2. How might such a context effect occur?

Consider that when targets appear in the same central location on all trials location may not be a relevant dimension on which events are defined, and therefore may not be encoded as part of perceptual episodes. In contrast, when targets appear in different locations from one trial to the next, location is a relevant dimension on which events are defined, and therefore location is likely to be encoded as part of perceptual episodes. The implication of this assumption is that there may have been episodic bindings involving spatial location encoded in Experiment 2 that were not encoded in Experiment 1. In turn, if IOR-like effects are observed only when access to relevant episodic bindings is disrupted, then a manipulation that disrupts all relevant episodic bindings in Experiment 1 might not do the same in Experiment 2. Specifically, response to an intervening event in Experiments 1 and 2 may have disrupted access to a binding between T1 colour and T1 response in both experiments, but in Experiment 2 an additional binding between location and colour may have continued to support faster performance for repeated than not-repeated trials. By this view, an IOR-like effect should only occur in Experiment 2 under conditions in which access to both a color-response binding and a location-colour binding were disrupted. Indeed, this may have occurred only on a location shift trial in the intervening event condition.

The idea that the different contexts of Experiments 1 and 2 played a critical role in the results reported thus far hinges on an assumption that the same location trials presented in the periphery in Experiment 2 were functionally equivalent to the (same location) trials presented centrally in Experiment 1. A stronger test of this idea would present targets centrally but in the context of other targets presented peripherally, and then compare the repetition effects for the centrally presented targets with the results from Experiment 1. This was the objective of Experiment 3, in which T1 and T2 could appear either peripherally or centrally. This method allowed us to compare the repetition effect for the subset of trials in which T1 and T2 were presented centrally in Experiment 3 with the corresponding conditions in Experiment 1.

Method

*Participants*

Forty-one students from either an introductory psychology course or a second-year cognitive psychology course at McMaster University participated for course credit. All participants had normal or corrected-to-normal vision.

*Apparatus and Stimuli*

In Experiment 3, T1 and T2 could appear either to the right or left of fixation, as in Experiment 2, but could also appear centrally, as in Experiment 1. The peripheral locations that were used in Experiment 3 were the same locations used in Experiment 2.

*Procedure & Design*

The procedure for Experiment 3 was identical to Experiment 2; the task of the participant was to respond to the colour of T1 and T2 irrespective of the target’s location. The design of Experiment 3 differed only in that targets could now be presented either peripherally or centrally.

*Data Analysis*

The design of Experiment 3 encompassed the designs of both Experiments 1 and 2. To facilitate comparison of the results of the present experiment to those experiments, separate analyses were performed depending on whether T1 and T2 both appeared centrally (the central-central analysis), T1 and T2 both appeared peripherally (the peripheral-peripheral analysis), or one of T1 or T2 appeared centrally and the other appeared peripherally (central-peripheral/peripheral-central analysis).

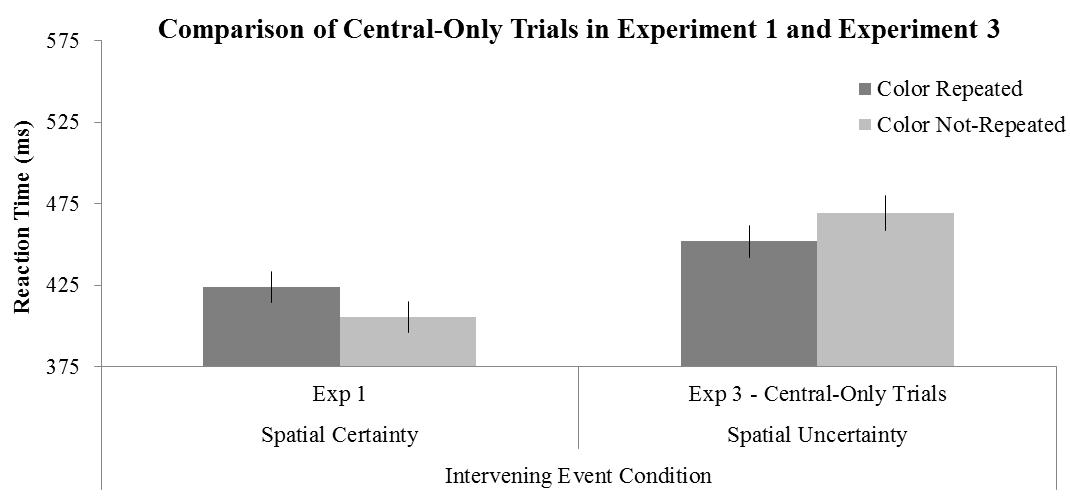
Results and Discussion

Correct trials were defined as in Experiments 1 and 2. Correct RTs were submitted to an outlier analysis that eliminated 2.4% (Central-Central Analysis), 2.1% (Peripheral-Peripheral Analysis), and 2.4% (Central-Peripheral/Peripheral-Central) of RTs from further analysis (Van Selst & Joliceour, 1994). Mean RTs were then computed based on the remaining observations, and these mean RTs and corresponding error rates were submitted to separate repeated-measures ANOVAs for each of the three analyses. For the Central-Central analysis, as well as for the Central-Peripheral/Peripheral-Central analyses, intervening event (intervening event/no-intervening event) and color repetition (repeated/not-repeated) served as within-subject variables. For the Peripheral-Peripheral analysis, location repetition (repeated/not-repeated) was also included as a within-subject variable. The alpha criterion was set to .05 for all statistical comparisons. Means of mean RTs and error rates for each condition, collapsed across participants, are listed in Table 1.

*Central-Central RT Analysis*

This analysis revealed a significant main effect of color repetition, F(1,40) = 13.46, p = .001, p2 = .25, with faster responses in the repeated condition (465 ms) than in the not-repeated condition (490 ms). Although the interaction between color repetition and intervening event did not reach significance (p = .14), we had an a prior interest in assessing the color repetition effect for the intervening event condition in this experiment, and comparing this effect to the results for the intervening event condition of Experiment 1. The rationale for focusing on this contrast was that these conditions involved identical trial types presented in different contexts. In Experiment 1, the central-central trials were presented on their own, whereas in Experiment 3, the central-central trials were presented in the context of other trials in which targets appeared peripherally. The color repetition effects for the intervening event trials in these two experiments are depicted in Figure 5. As noted previously, in Experiment 1, responses were significantly slower for repeated trials than for not-repeated trials. In contrast, in Experiment 3, responses were faster for repeated trials (452 ms) than for not-repeated trials (469 ms), F(1,40) = 4.29, p = .045, p2 = .10. An analysis that compared these two sets of data, treating experiment (Experiment 1/Experiment 3) as a between-subjects factor, revealed a significant colour repetition by experiment interaction, F(1,63) = 5.50, p = .022, p2 = .080.

The opposite repetition effects for these identical sets of trials must be attributed in some manner to the different contexts in which those trials were presented. One possibility is that when targets are presented in one of several locations (as in the present experiment), location becomes a salient feature of the episodic representation of the target event. In turn, repetition of location serves as a cue that re-instates processes that facilitate performance for color repeated trials. In contrast, when targets are presented in the same central location on every trial (as in Experiment 1), location is not a salient feature of the episodic representation of target events, and therefore location repetition does not cue the retrieval of processes the facilitate performance on color repeated trials. In the absence of such facilitation effects, IOR-like color repetition effects can be observed.



*Figure 5.* A comparison of the mean response times for T2 for only the intervening event condition in Experiment 1 and the Central-Only trials in Experiment 3, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

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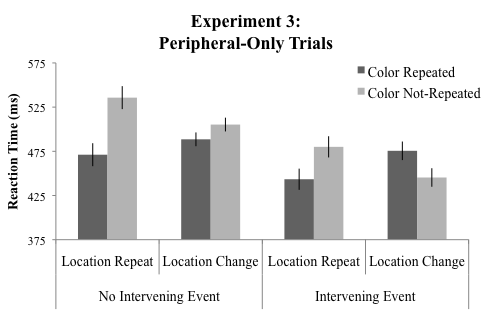
*Peripheral-Peripheral RT Analysis*

The conditions of interest in this analysis were identical to those in Experiment 2, and are presented in Figure 6. A quick comparison of Figures 4 and 6 reveals a very similar pattern of results. To facilitate comparison with the results from Experiment 2, the intervening event and no-intervening event conditions were analyzed separately (although the three-way interaction between intervening event, color repetition, and location repetition did not reach significance here, p = 0.228).

In the no-intervening event condition, there was a significant interaction between location repetition and colour repetition, F(1,40) = 17.64, p < .001, p2 = .31. Subsequent analyses revealed that for location repeated trials, responses were faster for colour repeated trials (471 ms) than for colour not-repeated trials (536 ms), F(1,40) = 39.96, p < .001, p2 = .50. In contrast, for location not-repeated trials, responses were not significantly different for colour repeated trials (489 ms) and colour not-repeated trials (505 ms), p = .069. Once again, the results from the no-intervening event condition offer a nice replication of the pattern of results reported by Taylor and Donnelly (2002).

In the intervening event condition, there was also a significant interaction between location repetition and colour repetition, F(1,40) = 41.36, p < .001, p2 = .51. Subsequent analyses revealed that for location repeated trials, responses were faster for colour repeated trials (443 ms) than for colour not-repeated trials (480 ms), F(1,40) = 12.33, p = .001, p2 = .24. In contrast, for location not-repeated trials, responses were slower for colour repeated trials (476 ms) than for colour not-repeated trials (445 ms), F(1,40) = 19.81, p < .001, p2 = .33. Again, this is precisely the pattern of results observed in Experiment 2. A non-spatial IOR-like effect was observed only when the location of targets shifted from T1 to T2 *and* a response was required to the intervening event.

This analysis also revealed a significant two-way interaction between intervening event and color repetition, F(1,40) = 18.29, p < .001, p2 = .31. Separate analyses of the colour repetition effect for the two intervening event conditions revealed faster responses for colour repeated trials (480 ms) than for colour not-repeated trials (520 ms) in the no-intervening event condition, F(1,40) = 27.65, p < .001, p2 = .41, but not in the intervening event condition (p = .65).



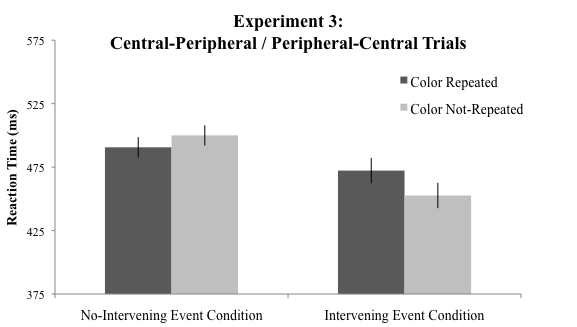
*Figure 6.* Mean response times for T2 in Experiment 3 for the Peripheral-Only trials, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Central-Peripheral / Peripheral-Central RT Analysis*

The central-peripheral and peripheral-central trials both constitute trial types in which T1 and T2 changed locations, and yet neither was a trial type tested in Experiments 1 or 2. These two trial types were collapsed together, and the resulting mean RTs are presented in Figure 7. The analysis of these mean RTs revealed a significant interaction between intervening event and color repetition, F(1,40) = 7.40, p = .010, p2 = .16. This interaction was examined further by testing the effect of colour repetition separately for the intervening event and no-intervening event conditions. In the no-intervening event condition, there was no significant difference between color repeated (491 ms) and color not-repeated (500 ms) conditions (p = .206). In contrast, in the intervening event condition, responses were slower for colour repeated trials (472 ms) than for color not-repeated trials (453 ms), F(1,40) = 11.36, p = .002, p2 = .22.

The pattern of color repetition effects observed here corresponded closely to that from corresponding conditions (in which location switched from T1 to T2) in both Experiments 2 and 3. In all cases, a non-spatial IOR-like effect was observed for location not-repeated trials only when a response was required to an intervening event.

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*Figure 7.* Mean response times for T2 in Experiment 3 for the Central-Peripheral/Peripheral-Central trials, collapsed across participants. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Additional Significant RT Results*

In each of the above three analyses, there was one additional significant result. For all three analyses, the main effect of intervening event was significant, F(1,40) = 10.01, p = .003, p2 = .20 (central-central), F(1,40) = 12.05, p = .001, p2 = .23 (peripheral-peripheral), F(1,40) = 19.28, p < .001, p2 = .33 (central-peripheral/peripheral-central). In all cases, responses were faster for the intervening event condition than for the no-intervening event condition.

*Error Rate Analysis*

There were no significant effects in the analysis of error rates for the central-central and peripheral-peripheral trials. However, in the central-peripheral/peripheral-central analysis, there was a significant main effect of color repetition, F(1,40) = 7.39, p = .010, p2 = .16, with more errors for color repeated trials (.05) than for color not-repeated trials (.02).

General Discussion

The goal of the present study was to examine whether a dual process account of non-spatial IOR-like effects (Spadaro et al., 2012) can accommodate performance in a task in which both spatial and non-spatial dimensions vary. This dual process account assumes that both spatial and non-spatial repetition effects are co-determined by episodic integration processes that speed performance for repeated relative to not-repeated events, and by orienting processes that slow performance for repeated relative to not-repeated events. By this view, to observe an IOR-like effect in either spatial or non-spatial repetition procedures, episodic integration processes that produce the opposite effect must be disrupted.

The results of several prior studies appear at odds with this dual process account. In particular, location shifts from one trial to the next might reasonably be assumed to disrupt episodic integration processes (Kahneman, Treisman & Gibbs, 1992), and yet prior studies have failed to reveal non-spatial IOR-like effects on location shift trials (Kwak & Egeth, 1992; Tanaka & Shimojo, 1996; Taylor & Donnelly, 2002; Milliken et al., 2000; Pratt & Castel, 2001). An example of this type of result in a task very similar to that used in the present study was reported by Taylor and Donnelly (2002). As such, a focal point in the present study was whether the result of Taylor and Donnelly (2002) could be replicated with our method, and if so, how this result might be reconciled with the dual process hypothesis.

The experiments reported here show clearly that the result reported by Taylor and Donnelly (2002) is replicable; in fact, it occurred in three separate conditions in the present study. In the no-intervening event condition of Experiment 2, a robust color repetition benefit was observed for location repeated trials, and a null color repetition effect was observed for location not-repeated trials. In the peripheral-peripheral condition of Experiment 3, the no-intervening event condition produced a very similar result. Finally, in the peripheral-central/central-peripheral condition of Experiment 3, the no-intervening event condition again produced a null color repetition effect. In all, as reported by Taylor and Donnelly (2002), a shift in target location from T1 to T2 was not sufficient to observe an IOR-like color repetition effect with our procedure.

At the same time, the results of the present experiments show just as clearly that a shift in target location from T1 to T2, together with a response to an intervening event between T1 and T2, reliably produces an IOR-like color repetition effect with our procedure. This result was observed in Experiment 2, and in both the peripheral-peripheral and peripheral-central/central-peripheral conditions of Experiment 3. In general, this result implies that although IOR-like color repetition effects can be observed with a procedure in which target location varies, response to an intervening event on its own is not sufficient to produce such an effect.

*The dual process hypothesis: Disruption of multiple episodic bindings*

So how might the dual process framework accommodate the present results? A key to understanding this issue is the context effect observed for central targets across Experiments 1 and 3. Recall that in Experiment 1, response to an intervening event produced an IOR-like color repetition effect, whereas in Experiment 3, the same central targets (but now presented in the context of peripheral targets) produced a color repetition benefit. This context effect is consistent with the idea that when targets vary in spatial location, location becomes a salient dimension of the target representation. As a consequence, episodic bindings involving the location dimension would have been generated in Experiment 3 but not in Experiment 1. The color repetition benefit in Experiment 3 but not in Experiment 1 suggests that response to an intervening event fails to disrupt retrieval of these location bindings in Experiment 3. At the same time, the bindings not disrupted by response to an intervening event appear to be disrupted by a location shift from T1 to T2, as indicated by the IOR-like effect observed when both of two conditions are met: (1) when a response is required to an intervening event, and (2) when target location changes from T1 to T2.

All told, the present results can be accommodated by the dual process framework (Spadaro et al., 2012) if the following assumptions are made. First, in Experiment 1, the relevant episodic bindings that are disrupted by response to an intervening event are those involving the task-relevant dimension of color and response. In the absence of retrieval of these color-response bindings, a non-spatial IOR effect is expressed in performance. Second, in Experiments 2 and 3, the relevant episodic bindings that are disrupted by a response to an intervening event were those involving the task-relevant dimension of color and response. However, in Experiments 2 and 3 there are also residual bindings involving the location dimension that support faster responses for color repeated than for color not-repeated targets. Of course, these residual location bindings support faster performance for repeated than not-repeated trials only when location repeats from T1 to T2. As such, response to an intervening event disrupts retrieval of color-response bindings, while a location shift from T1 to T2 disrupts other episodic bindings involving the location dimension. The dual process framework therefore accommodates the results of the present study by assuming that there may be multiple episodic bindings that support faster responses for repeated than for not-repeated trials, and the retrieval of all such bindings must be disrupted for a non-spatial IOR-like effect to be observed. In the present study, response to an intervening event and a shift in target location from T1 and T2 was necessary to disrupt retrieval of these bindings.

*The partial match hypothesis: Retrieval of inappropriate episodic bindings*

Although our preferred interpretation of the present results is the dual process framework described above, an alternative interpretation of the present results refers to partial match costs introduced by the retrieval of episodic bindings (Hommel, 1998; Hommel, 2004; Kahneman, Treisman & Gibbs, 1992). Although the retrieval of episodic bindings plays an important role in both of these theoretical accounts, there is an important distinction between how the two accounts explain the presence of IOR-like effects.

According to the dual process framework, IOR-like effects are attributed to a mechanism that favours orienting to novel events, and can be measured when the retrieval of task-relevant episodic bindings from T1 to T2 is disrupted. In contrast, a partial match account attributes IOR-like effects to the retrieval of episodic bindings for T1 that interfere with the task relevant bindings required for response to T2. To see how the partial match hypothesis might account for results from the present study, consider that the only condition in which an IOR-like effect occurred included a match between T1 and T2 in color/response, but a mismatch in location. If targets on color repeated trials in this condition cued the retrieval of a T1 representation in which the color/response ‘blue’ was bound to the location ‘left’, and response to T2 required the binding of the color/response ‘blue’ to a T2 presented on the right, then response to T2 might be slowed by the mismatch in location that is cued by the match in color/response (Hommel, 1998; 2004). Moreover, performance in the present study was most efficient when both color and location repeated from T1 to T2 (perfect matches), and also relatively efficient when both color and location alternated from T1 to T2 (perfect mismatches), which fits with the partial match hypothesis. In light of the good fit with the results of the present study, and the fact that retrieval of appropriate/inappropriate episodic bindings constitutes a single process rather than dual process account of performance, there is good reason to consider this alternative account.

In an earlier study of ours, we aimed to test the partial match account of our intervening event effect (Spadaro et al., 2012). In that study, as in Experiment 1, a repetition benefit was observed in a simple 2-afc procedure with no intervening event, whereas an IOR-like effect was observed with response to an intervening event. The repetition benefit without an intervening event fits the prediction that performance ought to be particularly efficient when T1 matches perfectly with T2 on all features. To explain the IOR-like effect with an intervening event, one must assume that response to the intervening event was associated with the T1 event file, such that when T2 matched T1 in color, retrieval of the T1 event file introduced a mismatch at the level of response; that is, the requirement to make one of two responses to the color of T2 may have mismatched with the retrieved response codes associated with responding to the intervening event. To test this idea, the timing of the intervening event relative to T1 was varied (Spadaro et al., 2012; Experiment 3), such that on some trials it occurred close in time to the T1 response, and other times it occurred far in time from the T1 response. If the IOR-like effect was caused by partial match interference, then this effect ought to have been larger when the intervening event occurred close in time to T1 than when it occurred far in time from T1, and yet no such effect occurred.

*IOR-like or IOR?*

Throughout this article we have been careful not to take a strong stance on whether the IOR-like color repetition effects reported here are caused by the same mechanism as IOR effects observed in spatial orienting tasks. We can be sure at this point only that responses can be relatively slow for repetitions on both spatial and non-spatial stimulus dimensions. Yet, over the past 20 years, there has been mounting evidence that IOR-like effects are readily observed in tasks that involve repetition of non-spatial stimulus dimensions, and that these effects have some of the same properties as IOR effects in spatial orienting studies. Non-spatial IOR-like effects have been observed in both detection tasks (Law et al., 1995; Fox & de Fockert, 2001; Francis & Milliken, 2003) and discrimination tasks (Francis & Milliken, 2003; Hu, Samuel & Chan, 2011; Spadaro et al., 2012), using both cue-target and target-target procedures (Spadaro et al., 2012), and in some cases have a time course similar to that observed in many spatial cueing studies (Francis & Milliken, 2003; but see Taylor & Klein, 1998). These results offer converging support for the view that a mechanism underlying spatial IOR effects has a broader role in behavior than originally proposed. Rather than a mechanism dedicated to the control of spatial orienting, the IOR effect could conceivably reflect a mechanism that biases orienting and learning toward novel events more generally, and therefore could impact performance across a broad range of tasks. Two recent proposals about the cause of IOR effects fit this broad profile. Dukewich (2009) recently suggested that IOR effects may reflect a habituation process that extends to both spatial and non-spatial stimulus dimensions, while Lupiáñez (2010; see also Hu, Samuel & Chan, 2011) has proposed that IOR reflects a fundamental cost in detecting the onset of a new event when it overlaps on either spatial or non-spatial dimensions with a recent prior event. The dual process framework outlined by Spadaro et al. (2012), and modified in response to the present results, would readily accommodate either of these mechanisms as the cause of both spatial and non-spatial IOR effects.

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CHAPTER 5: Subjective expectancy and inhibition of return: A dissociation in a non-spatial two-alternative forced choice task

Spadaro, A., & Milliken, B. (2013). Subjective expectancy and inhibition of return: dissociation in a non-spatial two-alternative forced choice task. *Psicológica*, *34*(2), 199-219.

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Preface

This empirical chapter focused on the possibility that the intervening event effect occurs because intervening events increase participants’ subjective expectancy for alternation rather than repetition. On critical trials in this study participants were presented with a T2 that contained no colour information, which prompted them to respond whether they expected T2 to be a repetition or alternation relative to the colour of T1. The results revealed that participants tended to expect T2 to be a colour alternation, but did so both in the intervening event condition and in the no-intervening event condition. As such, the results do not support the view that intervening events increase subjective expectancy for alternation.

Abstract

Inhibition of Return (IOR) is conventionally defined by slow responses to targets that appear at the same location as a prior attentional cue, relative to a condition in which targets appear at a different location from a prior attentional cue (Posner & Cohen, 1984). A number of recent studies have extended the study of IOR to non-spatial orienting tasks (Law, Pratt, & Abrams, 1995; Hu, Samuel, & Chan, 2011; Spadaro, He, & Milliken, 2012), which is consistent with the view that a fundamental process that favours the perceptual encoding of new events is responsible for IOR. However, an alternative account of IOR is that participants expect uncued targets to appear more often than cued targets even when these two target types are equiprobable. The aim of the current study was to examine directly the relation between performance and subjective expectancy in a task known to produce repetition benefits under one set of conditions, and IOR-like effects under another set of conditions. The performance measure (i.e. RTs) showed either repetition benefits or IOR-like effects depending on whether or not an intervening event was introduced. Interestingly, participants reported that they expected uncued targets more often than cued targets across both conditions, a result that is inconsistent with the view that repetition effects generally, and IOR-like effects specifically, are directly related to subjective expectancy.

Keywords: Inhibition of Return; Expectancy; Episodic Integration; Intervening Event

Introduction

Orienting to novelty is a fundamental property of an efficient attention system, as it ensures that attention shifts efficiently to events that violate predictions about the world based on prior experiences (Sokolov, 1963). Such a fundamental property of attention might be expected to contribute to performance in many behavioural tasks. Indeed, evidence of an attentional benefit for processing of novel events could be argued to contribute to novel pop-out (Johnston, Hawley & Farnham, 1993), new object benefits in visual search (Yantis & Jonides, 1984), visual marking effects in visual search (Watson & Humphreys, 1997), negative priming effects in identification and spatial localization, (Milliken, Joordens, Merikle & Seiffert, 1998), and inhibition of return (IOR) effects in spatial orienting tasks. The focus of the current study is the IOR effect (Posner & Cohen, 1984), and in particular the role of expectancy in producing IOR effects.

The IOR effect is typically measured using a spatial cueing procedure in which a non-predictive spatial cue is presented in one of two peripheral locations. A target can then appear in either the cued location or the uncued location. When the interval between onsets of the cue and target is greater than about 300 ms, response times to detect the target are slower for targets that appear in the cued location than for targets that appear in the uncued location. This result is often taken as evidence that attention is initially captured by the cue, then withdrawn from the cued location, and consequently inhibited from reorienting to the cued location. A similar result is observed in studies in which participants localize two targets on consecutive trials (i.e., a target-target procedure), rather than respond to a single target following presentation of a passively perceived cue (i.e., a cue-target procedure; Maylor & Hockey, 1985). In both cases, slower orienting to a previously attended location than to an unattended location constitutes an example of attentional preference for novelty.

To the extent that a broad mechanism favouring orienting to novelty underlies the IOR effect, one might expect a similar effect would occur in a task that involves non-spatial orienting. Early studies that addressed this issue failed to demonstrate a non-spatial variant of IOR, and instead found that repetition of non-spatial dimensions led to repetition priming (Kwak & Egeth, 1992; Tanaka & Shimojo, 1996). However, there are now quite a few studies that have demonstrated IOR-like effects with non-spatial stimulus dimensions, such as colour (Law, Pratt & Abrams, 1995; Fox & de Fockert, 2001; Hu, Samuel, & Chan, 2011; Spadaro, He & Milliken, 2012), auditory frequency (Mondor, Breau, & Milliken, 1998), line length (Francis & Milliken, 2003; Spadaro et al., 2012), and semantic relatedness (Fuentes, Vivas, & Humphreys, 1999; Spadaro et al., 2012). In many cases, the key to observing such effects where others had instead observed repetition priming (Kwak & Egeth, 1992; Tanaka & Shimojo, 1996) appears to be the insertion of an intervening event between cue and target in a cue-target procedure (Law et al., 1995), or the insertion of an intervening event that is responded to between consecutive targets in a target-target procedure (Spadaro et al., 2012). Although the precise reason why intervening events are often necessary to observe non-spatial IOR-like effects remains a matter of debate, a generic explanation is that intervening events interfere with processes that produce facilitation, which in turn allows an underlying IOR effect to be measured. In any event, these recent results suggest that spatial IOR effects and non-spatial IOR-like effects could conceivably reflect the same broad property of attention that favours orienting to novelty.

*The attentional momentum hypothesis*

An alternative account of spatial IOR effects proposes that it measures a tendency for attention to continue along the path it has followed most recently, rather than for attention to shift preferentially toward novelty (Pratt, Spalek, & Bradshaw, 1999; Spalek & Hammad, 2004). As noted above, in some variants of the IOR procedure a peripheral cue is followed by an intervening event, usually a cue presented centrally. By many accounts, attention is initially pulled to the location of the peripheral cue, but then shifts in the direction of the central cue upon its onset. According to the attentional momentum hypothesis, the IOR effect occurs because attention then moves more efficiently along the same trajectory than along other trajectories, as if the movement of attention is subject to momentum. By this view, orienting attention back to the cued location involves overcoming the momentum carrying attention in the opposing direction. As a result, responses to targets back at the cued location are slow.

The attentional momentum hypothesis is rooted in the idea that shifts of attention might obey learned environmental regularities – objects that move in one direction tend to continue moving in the same direction rather than abruptly shifting and moving in the opposite direction. In support of this general view, Spalek and Hammad (2005) discovered that the size of the IOR effect was sensitive to a left-to-right bias for English readers and a right-to-left bias for Arabic readers. To the extent that effects such as these hinge on targets matching predictions that derive from learned regularities, the conceptual distinction between the attentional momentum and orienting to novelty hypotheses for IOR is clear-cut. The orienting to novelty view assumes that IOR reflects a mechanism intended to overcome a bias that derives from prior experience, whereas the attentional momentum hypothesis assumes that IOR directly reflects the biases from prior experience itself.

The results of Spalek and Hammad (2004, 2005) offer compelling evidence that a form of expectancy can contribute to the size of the IOR effect. That is, if we think of learned environmental regularities as leading to predictions about future environmental states, these predictions, or expectancies, appear to modulate spatial cueing effects. At the same time, the proposal that expectancies of this sort are the cause of IOR effects is a more contentious issue. In particular, it may be that learned regularities contribute to spatial orienting performance independent of another process that produces IOR effects (see Snyder, Schmidt & Kingstone, 2001). In other words, there is room for expectancy derived from learned regularities to affect performance in spatial orienting tasks without expectancy being the direct cause of IOR effects.

An additional issue raised by the results of Spalek and Hammad (2004, 2005) concerns the distinction between two different uses of the term expectation. Expectation might derive from learned environmental regularities, in which case it would not be surprising for such expectancies to affect behaviour automatically and without accompanying awareness on the part of participants (Lambert, Naikar, McLachlan & Aitken, 1999; Lambert, Norris, Naikar & Aitken, 2000; Lambert, 1996). In contrast, a different use of the term expectation refers to controlled, strategic expectations that can be reported voluntarily by the participant, and that can produce behaviour that is either consistent or inconsistent with learned regularities (McCormick, 1997). Presumably, the results of Spalek and Hammad (2004; 2005) speak to the fact that expectancies that are expressed automatically in performance, but that are not open to conscious subjective report, can contribute to spatial orienting effects.

Yet, a subsequent study reported by Spalek (2007) offers a potentially more compelling link between subjectively reported expectancy and spatial orienting effects. The procedure used in this study was a modified variant of the spatial orienting procedure typically used to measure the IOR effect. Participants were first presented with a cue that could appear in one of eight locations. Following offset of the cue, participants were instructed to indicate in which of the eight locations they expected the following target to appear. In particular, participants were led to believe that a target location had been selected on every trial but not displayed to them, and they were to try to guess which location had been chosen as the target location. Expectation that the target would appear in a location opposite the cue was significantly greater than chance, while expectation that the target would appear in the cued location was significantly less than chance. These results were viewed as supporting the attentional momentum hypothesis for IOR, in the sense that the subjectively reported expectations of participants mirrored the usual pattern of response times observed with similar procedures (see Pratt, Spalek, & Bradshaw, 1999).

*Expectancy and the IOR effect*

Although the Spalek (2007) study describes an interesting set of results, it might be taken to imply that there is a direct link between IOR effects and consciously reported expectancies, rather than the original claim that IOR effects can be affected by automatically retrieved learned environmental regularities (Spalek & Hammad, 2004; 2005). Our concern with the idea that consciously reportable expectancies are at the root of IOR effects is two-fold. First, although the expectation results reported by Spalek (2007) mapped nicely onto prior behavioural results generated using a similar procedure (Spalek, Pratt & Bradshaw, 1999), there is no way to know whether the subjectively reported expectancies reported by Spalek (2007) constitute the mechanism that produced the response times for cued and uncued trials reported by Spalek et al. (1999). In effect, the two patterns of results co-vary in an interesting way, but the causal connection between them (if indeed there is one) is unclear. Second, the inference that IOR reflects greater expectancy for uncued than cued targets does not fit with results from studies that have manipulated expectancy directly and measured IOR effects. In particular, several studies have now shown that IOR effects are observed both when targets appear at unexpected locations and when targets appear at expected locations (Berger, Henik, & Rafal, 2005; Berlucchi, Chelazzi, & Tassinari, 2000; Lupiáñez, Decaix, Sieroff, Chokron, Milliken, & Bartolomeo, 2004). All told, the claim that IOR reflects greater explicit expectancy for uncued than for cued locations does not stand on particularly strong ground.

Nonetheless, the pattern of expectancy results reported by Spalek (2007) is an interesting one, and the relation between subjective expectancy and performance in tasks that measure repetition/cueing effects certainly merits further study. In particular, to examine the relation between expectancy and performance more closely we aimed to measure subjective reports of expectancy in two contexts; one in which participants respond faster to repeated (i.e., cued) events than to alternated (i.e., uncued) events, and another in which participants respond slower to repeated events than to alternated events. To the extent that expectancy determines performance, subjectively reported expectancies for repeated relative to alternated events ought to mirror behavioural performance; that is, opposite repetition effects in response time across two contexts ought to be accompanied by opposite patterns of subjective expectancies.

*The Present Study*

To measure the relation between expectation and performance in two different contexts, we adopted a non-spatial orienting procedure introduced by Spadaro et al. (2012). Importantly, this procedure offers the opportunity to measure both repetition benefits and repetition costs (i.e., IOR) in a simple two-alternative forced choice (2-afc) task. In the Spadaro et al. study, participants responded to the colour (blue or yellow) of two sequential targets appearing centrally within a trial. For half of the trials, the screen remained blank between the offset of the first target (T1) and the onset of the second target (T2); those trials belonged to the no-intervening event condition. For the other half of trials, participants had to respond to an intervening event that was presented between the offset of T1 and the onset of T2; those trials belonged to the intervening event condition. In a series of experiments that used this procedure, Spadaro et al. found that when no intervening event was presented, participants responded faster to T2 on repeated trials (trials in which T1 and T2 matched in color) than on alternated trials. However, when an intervening event was presented and responded to, participants responded faster to T2 on alternated trials than on repeated trials.

The dependence of performance on an intervening event between T1 and T2 makes it tempting to conclude that similar mechanisms underlie performance in this task and in spatial orienting tasks. Indeed, the method was designed to create a non-spatial analogue of the “cue-back” procedure in spatial orienting studies, and to examine whether response to an intervening event would produce an effect that is analogous to that produced by a central cue in spatial orienting tasks (Prime, Visser, & Ward, 2006). From this perspective, the repetition cost measured on the intervening event trials by Spadaro et al. might be considered a non-spatial variant of the IOR effect (see also Law et al., 1995; Francis & Milliken, 2003; Dukewich, 2009; Hu et al., 2010; Hu & Samuel, 2011). Nonetheless, we recognize that there are some salient differences between this method and those used in spatial orienting studies, and therefore any conclusions about the relation between performance and expectancy observed here should be applied cautiously to the domain of spatial IOR.

In any case, if response times in this task are determined by expectancy, then a qualitative shift in response times across the two intervening event conditions ought to be accompanied by a qualitative shift in subjective expectancy. In particular, in the intervening event condition, response times should be slower for repetitions than for alternations and expectancy for repetition ought to be lower than dictated by chance. In contrast, in the no-intervening event condition, response times should be faster for repetitions than for alternations, and expectancy for repetition ought to be greater than chance.

Method

*Participants*

17 participants were recruited from an introductory psychology course or a second year cognitive psychology course from McMaster University, and participated for course credit. All participants reported to have normal or corrected-to-normal vision.

*Apparatus and Stimuli*

The experiment was run on a PC using MEL experimental software. Subjects sat directly in front of a 15” SVGA computer monitor, at adistance of approximately 57 cm. A plus sign was presented as the fixation point in the center of the screen, and subtended a visual angle of 0.6 degrees horizontally and 0.7 degrees vertically. The target stimuli (T1 and T2) were presented centrally against a black background.

Both T1 and T2 were a colored rectangle, either blue or yellow, subtending a visual angle of 6.3 degrees horizontally and 1.2 degrees vertically. On trials in which participants were asked to indicate the color in which they expected T2 to appear, T2 was presented as a white outline of a rectangle with the same dimensions as the blue or yellow rectangles. The intervening event was a red dot presented centrally with radius subtending .25 degrees of visual angle.

*Procedure and Design*

The experiment consisted of two blocked conditions: an intervening event condition and a no-intervening event condition. Each condition had an initial practice block consisting of 16 trials, followed by nine experimental blocks of 16 trialseach.

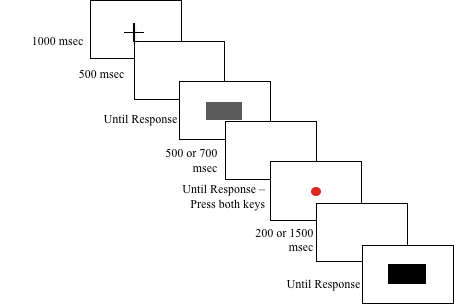
For both conditions, a trial began with the appearance of a fixation cross in the middle of the computer screen for 1000 ms, and then a blank screen for 500 ms. In the no-intervening event condition, T1 appeared and remained on the screen until the participant made a key press response (“z” or “/”) to the color of T1. A blank interval of either 1200 ms or 2500 ms followed the key press to T1. T2 was then presented and remained on the screen until the participant made another key press response (“z” or “/”) to the color of T2. Participants were instructed to press the “/” key to indicate the presence of a blue rectangle and to press the “z” key to indicate the presence of a yellow rectangle for both T1 and T2. Participants used the index finger of their right hand to respond to the ”/” key and the index finger of their left hand to respond to the “z” key. Response time was measured as the latency between onset of the target stimulus and key press response.

The intervening event condition differed from the no-intervening event condition from the point after the participant responded to T1. A blank interval of either 400 ms or 600 ms followed the response to T1 in the intervening event condition. The length of this interval was chosen at random between these two values with the intention of producing some temporal uncertainty as to the onset of the intervening event. Following this blank interval, the red dot appeared and remained on the screen until the participant pressed both the “z” and the “/” keys in unison. After this response to the intervening event, a blank interval of either 200 or 1500 ms occurred prior to onset of T2. These intervals were chosen so as to roughly equate the response-stimulus interval (RSI) for T1 and T2 across the intervening event and no-intervening event conditions. T2 remained on the screen until participants responded to its identity by pressing the “/” key or the “z” key.

Across both intervening event and no-intervening event conditions, participants were instructed that on some trials T2 would be presented as a white rectangle (the actual proportion of trials was .20). On these expectancy trials, participants were instructed to press the “z” or “/” key to indicate the color in which they expected T2 to appear, either blue or yellow.

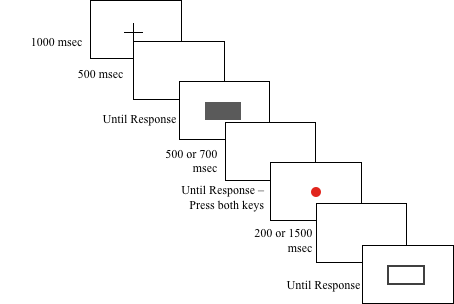
Task instructions were displayed on the screen prior to starting the practice block. Prior to each block of trials within each condition, the message “Press B to begin block” appeared, allowing participants to rest between blocks when needed. For all trials in both conditions, there was a 2000 ms inter-trial interval that started once a response was made to T2. The procedure for Color-Response trials is displayed in Figure 1, and the procedure for Expectancy-Response trials is displayed in Figure 2.

The design for the study differed slightly depending on whether T2 appeared as a rectangle filled by a particular color (Color-Response trials) or T2 appeared as a white outline of a rectangle (Expectancy-Response trials). For Color-Response trials, there were three within-subject variables: intervening event (no-intervening event/intervening event), repetition (repeated/alternated), and RSI (1,200 ms/2,500 ms). Intervening event was manipulated blocked within-participants, with the order of the two intervening event conditions counterbalanced across participants. Repetition was manipulated randomly within blocks. In the repeated condition, T1 and T2 appeared in identical colors, whereas in the alternated condition, T1 and T2 appeared in different colors. RSI was also manipulated within blocks. In the no-intervening event condition, the 1,200 ms and 2,500 ms RSI conditions were measured precisely as the latency between response to T1 and the onset of T2, whereas in the intervening event condition, these RSI values were approximated in accord with the estimated time to respond to the intervening event.



*Figure 1.* The sequence of events for a Color-Response trial in the intervening event condition is shown. In the experiment, the darker rectangle would have been blue and the lighter rectangle would have been yellow. In the no-intervening event condition (not shown), the intervening event was replaced by a blank screen that remained for approximately the same length of time as the intervening event.

For Expectancy-Response trials, the design was identical to the Color-Response trials with the exception that repetition was no longer a meaningful variable. As T2 was a white outline of a rectangle rather than a colored rectangle, repetition was undefined for the Expectancy-Response trials. The proportion of trials in which participants expected repetition was the dependent variable for the Expectancy-Response trials.



*Figure 2.* The sequence of events for an Expectancy-Response trial in the intervening event condition is shown. In the experiment, the darker rectangle would have been blue and the lighter rectangle would have been yellow. In the no-intervening event condition (not shown), the intervening event was replaced by a blank screen that remained for approximately the same length of time as the intervening event.

Results

For the Color-Response trials, a trial was coded as correct if responses to both T1 and T2 were correct, and as an error if the response to T2 was incorrect while the response to T1 was correct. Response times (RTs) on correct trials were submitted to an outlier analysis (Van Selst & Jolicoeur, 1994) that eliminated 2.9% of the RTs from further analysis. Mean RTs for each condition were then computed based on the remaining observations. These mean RTs and corresponding error rates were submitted to repeated measures analyses of variance that included Repetition (repeated or alternated), RSI (short or long), and Intervening Event (intervening event or no-intervening event) as within-subject factors.

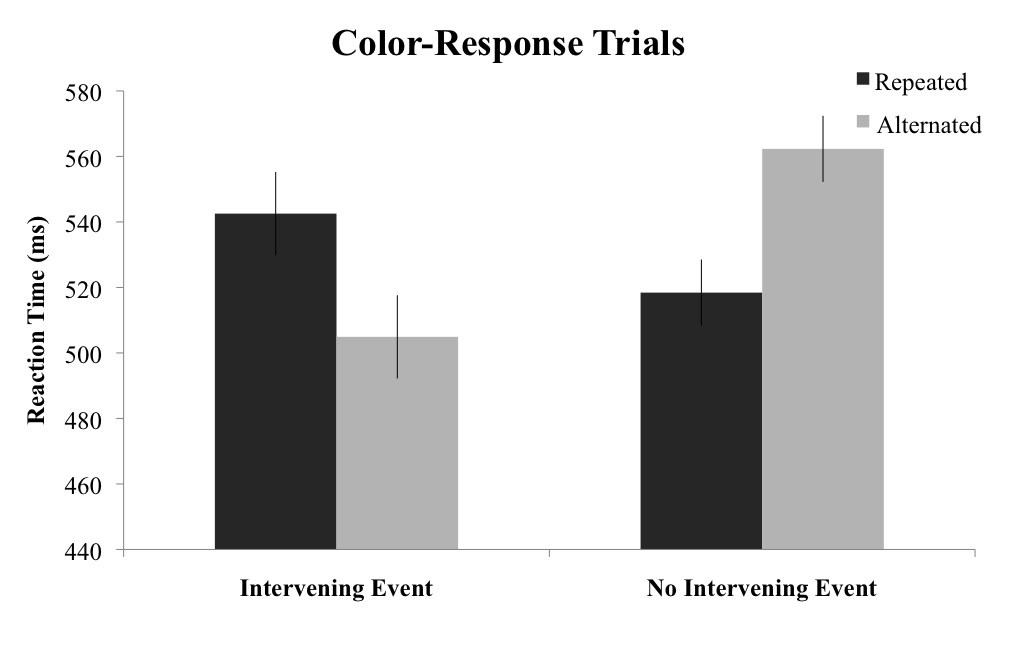
For the Expectancy-Response trials, the focus was on participant’s expectancy for T2 as a function of the relation between T1 and T2. Only trials in which a correct response was made to T1 were analyzed. The mean proportion of trials in which participants reported expecting a repetition for each condition were then submitted to a repeated measures analysis of variance that included RSI (short or long)[[3]](#footnote-3), and Intervening Event (intervening event or no-intervening event) as within-subject factors.

The alpha criterion was set to .05 for all analyses. Means RTs in each condition for the Color-Response trials, collapsed across participants and RSI, are displayed in Figure 3. Mean proportions of Expectancy-Response trials in which participants expected a repetition/alternation, collapsed across participants and RSI, are displayed in Figure 4[[4]](#footnote-4).

*Color-Response Trials*

In the analysis of RTs, there was a significant interaction between Intervening Event and Repetition, F(1,16) = 13.34, p = .002, p2 = .46. To examine this interaction in more detail, simple main effects for repetition were analyzed separately for the intervening event and no-intervening event conditions. In the intervening event condition, RTs were slower for repeated trials (543 ms) than for alternated trials (505 ms), F(1,16) = 8.81, p = .009, p2 = .36. In contrast, in the no-intervening event condition, RTs were faster for repeated trials (518 ms) than for alternated trials (562 ms), F(1,16) = 11.88, p = .003, p2 = .43. The opposite repetition effects for the two intervening event conditions nicely replicates the pattern of results reported by Spadaro et al. (2012).

In the analysis of error rates, there was a significant main effect of Intervening Event, F(1,16) = 3.50, p = .040, p2 = .28. Participants made more errors on intervening event trials (.03) than on no-intervening event trials (.02). The interaction between Intervening Event and Repetition was not significant, F < 1.



**(.02)**

**(.03)**

**(.03)**

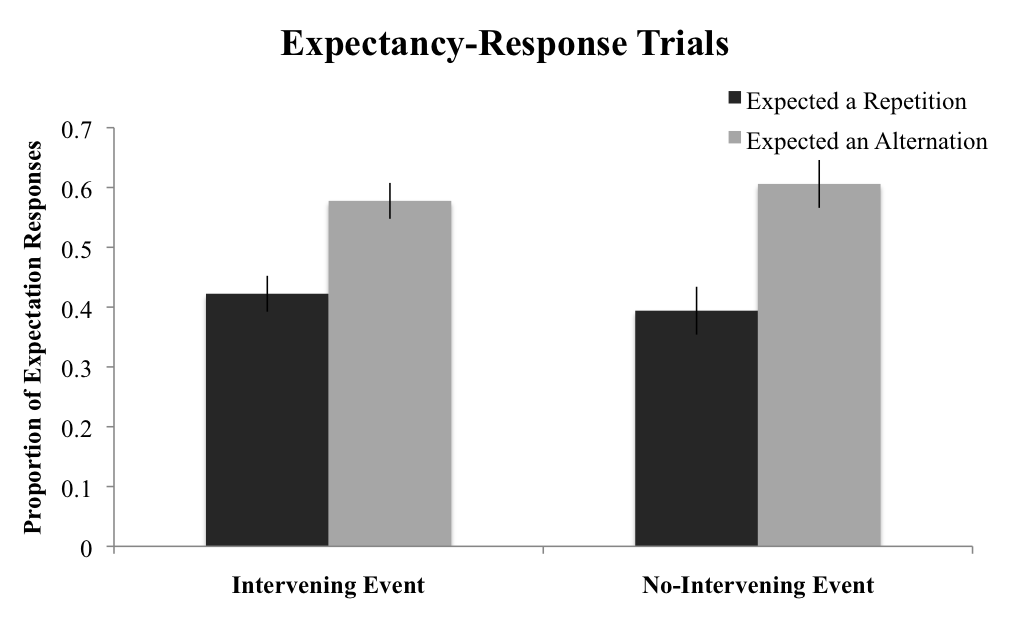
**(.02)**

*Figure 3*. Mean response times for T2 across the two intervening event conditions, collapsed across participants and RSI. Error rates for each condition are presented in parentheses. Error bars represent the standard error of the difference between repeated and not-repeated conditions.

*Expectancy-Response Trials*

If the RT pattern reported above were perfectly associated with participants’ subjective expectancies, then we ought to observe that the proportion of trials in which participants expected a repetition would vary as a function of the intervening event condition. In particular, expectancy for a repetition ought to be higher than .50 in the no-intervening event condition and lower than .50 in the intervening event condition. With this prediction as context, the key result here was a non-significant main effect of Intervening Event, F < 1. The proportion of trials in which participants expected a repetition was nearly identical for the intervening event (.41) and no-intervening event (.40) conditions. One-sample t-tests confirmed that the mean proportion of expected repetitions was significantly lower than chance (.50) for both the intervening event condition, *t*(16) = -2.40, p = .030, *d* = .58, and the no-intervening event condition, *t*(16) = -3.12, p = .007, *d* = .76.

The only other noteworthy results in this analysis were a non-significant main effect of RSI, F < 1, and a non-significant interaction between RSI and Intervening Event, F < 1. Interestingly, these results suggest that subjective expectancy for repetition/alternation was not modulated by the time interval between T1 and T2 (see Kirby, 1976).



*Figure 4*. Mean proportion of expectancy responses as a function of whether a repetition or an alternation was expected across the two intervening event conditions, collapsed across participants and RSI. Error bars represent the standard error of the mean proportion of expectancy responses for both intervening event and no-intervening event conditions.

General Discussion

The key result in this study was that subjective expectancies were aligned with performance in just one of the two intervening event conditions. On the Color-Response trials, participants responded faster to repeated events than to alternated events in the no-intervening event condition, and responded faster to alternated events than to repeated events in the intervening event condition, a result that replicates prior work by Spadaro et al. (2012). The new findings are those observed on the Expectancy-Response trials. In particular, participants reported that they expected alternated events to occur more often than repeated events in both no-intervening event and intervening event conditions. These results clearly illustrate the insufficiency of subjective expectancies in explaining the repetition effects across all of the conditions tested here.

*Separate influences of automaticity and expectancy?*

The repetition effects reported here cannot be explained entirely by reference to subjective expectancy, but it remains possible that expectancy and an additional process could handle the present findings. In particular, in the intervening event condition of our study, participants responded more quickly to alternations than to repetitions and also indicated that they expected alternations more often than repetitions. On their own, these data are consistent with the view that speed of responding to the color-response trials is related directly to subjective expectancies. However, in the no-intervening event condition, the same pattern of expectancy is accompanied by the opposite pattern of RT data. Clearly, these no-intervening event condition data must be attributed to a mechanism other than that which drives subjective reports of expectancy. One way to explain these data is by reference to separate influences on performance of expectancy and automaticity. In particular, an automatic process that produces repetition benefits may predominate in the no-intervening event condition, but this process may co-exist with an expectancy-based process that predominates and produces repetition costs in the intervening event condition (Kirby, 1976; Soetens, Boer, & Hueting, 1985). By this dual process view, our results are not necessarily inconsistent with the link between subjectively reported expectancy and IOR implied by the study of Spalek (2007). Rather, the apparent dissociation between repetition effects and expectancy reported here could be attributed to the fact that expectancy is really only expressed in performance in a pure form in the intervening event condition.

Yet, as noted in the Introduction, the results of several published studies contradict the idea that IOR is related to consciously controlled expectancies. In particular, several previous studies have manipulated endogenous expectancy and exogenous cueing orthogonally, and have found that IOR effects occur for targets appearing both at expected and at unexpected locations (Berger et al., 2005; Berlucchi et al., 2000; Lupiáñez et al., 2004). These results suggest that the processes producing IOR effects are separate from those responsible for implementing consciously controlled spatial expectancies. Given such results, we offer an alternative dual process account that does not hinge on any direct relation between subjective expectancies and performance.

*The intervening event effect: A dual process framework*

The pattern of RTs reported here closely replicates that reported recently by Spadaro et al. (2012), and supports the idea that IOR-like effects can be observed with non-spatial stimulus dimensions and a target-target method. To explain this pattern of RTs, Spadaro et al. (2012) proposed a dual process account somewhat like that described above. Again, the general idea is that there is the potential for two processes to contribute simultaneously to performance, with one process speeding responses to repeated events relative to alternated events, and another process doing the opposite. The relative contributions of these two processes can change across experimental contexts, and thus explain why opposite repetition effects are observed across the two intervening event conditions.

A candidate process that would speed performance for repeated relative to alternated trials is episodic integration (Logan, 1988; Kahneman, Treisman, & Gibbs, 1992; Hommel, 1998). By this view, T2 can cue the retrieval of episodic representations of similar events, which in the case of a repeated trial would result in retrieval of the T1 episode. As a result, response to T2 would depend on the rapid integration of the T1 episode into current processing of T2, rather than the encoding of a separate event representation for T2. The result of this episodic integration process would be particularly fast responses for repeated relative to alternated trials.

To explain the opposite pattern of results in the intervening event condition, Spadaro et al. (2012) argued that the requirement to respond to an intervening event disrupts the episodic integration process, and reveals a second process that slows responses to repeated relative to alternated events. This second process is assumed to have a broad scope, slowing the encoding of repeated relative to alternated events in both spatial and non-spatial contexts[[5]](#footnote-5). In line with this idea, Dukewich (2009) proposed recently that IOR may be caused by habituation of orienting that is not tied specifically to the spatial domain. A similarly broad argument has been forwarded by Lupiáñez and colleagues (Lupiáñez, 2010; Lupiáñez, Martin, & Chica, accepted pending minor revision; see also Hu et al., 2011), in which they argue that IOR effects reflect a cost specifically in the process of detecting old events relative to new events. In any event, the key distinction between this dual process account and the one described earlier is that expectancy is not the process driving the IOR effect. Rather, a process that generally favours the encoding of novel relative to familiar events is responsible for IOR.

As noted in the Introduction, the method used here to measure the intervening event effect was motivated by consideration of “cue-back” procedures in spatial orienting studies. As such, we favour an interpretation in which spatial and non-spatial IOR-like effects are attributed to the same cause. At the same time, there are certain to be different processes involved in our task and in spatial orienting tasks, and so conclusions drawn here about the relation between performance and expectancy should be applied cautiously to the domain of spatial orienting.

*What do subjective expectancies measure?*

An important implication of the dual process account favored here is that the subjective expectancies reported by participants should not be taken as faithful measures of the preparatory state of participants. Indeed, the processes that determine subjective expectancies may well depend in subtle ways on the task context in which they are measured (Danziger & Rafal, 2009). One response to this concern is to exercise great care so that the processing conditions associated with performance measures (e.g., the colour naming trials in our study) are as comparable as possible to the processing conditions associated with judgments of expectancy (the expectancy trials in our study). Perhaps if these processing conditions are very similar then the mapping between performance and expectancy measures would be a close one. In line with this view, one might argue that our method for measuring subjective expectancy, and in particular mixing together the color naming and expectancy trials, introduced a disrupting intervening task between presentation of T1 and report of subjective expectancy. In particular, presentation of the empty rectangle required a shift of task on the participants’ part from the usual color identification task to that of expectancy judgment. If this unexpected shift in task itself constitutes an “intervening event”, then it might well explain why an IOR-like pattern of expectancies was produced for the no-intervening event condition.[[6]](#footnote-6)

Although this interpretation of our results cannot be ruled out, the different patterns of performance and expectancy in our study may instead imply that, in many task contexts, the processes driving performance are fundamentally different than those that drive subjective reports of expectancy. This conclusion fits well with results from a recent study that examined subjective expectancy and the conflict adaptation effect (Jiménez & Méndez, 2012). The conflict adaptation effect refers to the finding that conflict effects (e.g., Stroop, flankers) tend to be smaller following an incompatible (or incongruent) trial than following a compatible (or congruent) trial (Gratton, Coles, & Donchin, 1992). One account of this effect is that participants adjust their expectancy on a trial-to-trial basis in accord with the type of trial that has just been completed; participants expect an incongruent trial following an incongruent trial, and they expect a congruent trial following a congruent trial. By this view, these putative shifts in expectancy have the consequence that participants are particularly well prepared to respond to an incongruent trial following an incongruent trial, resulting in relatively small interference effects on these trials. In contrast to this view, Jiménez and Méndez (2012) found that conflict adaptation effects and subjective expectancies can be dissociated. In particular, when participants performed a run of congruent Stroop trials they were more likely to report an expectancy favoring an incongruent Stroop trial, in line with the gambler’s fallacy (Jarvik, 1951). At the same time, interference effects tended to be large rather than small following a run of congruent trials, indicating that preparation for an incongruent trial was actually poor under conditions in which participants reported an expectancy for an incongruent trial. As in the present study, subjective expectancy appeared to be driven by a process very different from that which actually guided behaviour (see Perruchet, Cleermans, & Destrebecqz, 2006 for a similar dissociation).

Finally, whereas we have argued against the idea that expectancy is the mechanism that produces IOR effects, it is worth considering whether the opposite might be the case. Could a mechanism that favours encoding of novelty contribute to the pattern of subjective expectancies reported here? To understand how this might be the case, consider that the generation of subjective expectancies on the part of participants may be a constructive process. That is, participants may not have direct conscious access to their internal states of preparation, and instead may have to engage in a constructive retrieval process to infer their state of preparation (Nisbett & Wilson, 1977). To do so in the present study, participants may have responded to the empty probe rectangle (i.e., the cue to report their expectancy) by attempting to simulate repeated and alternated probes, and then evaluating the ease with which they were able to do so (Schacter, Addis & Buckner, 2007). They might then report expecting the type of target, repeated or alternated, that they were able to simulate with the most ease. To fit the present results, it would have to be the case that participants found it easier to simulate an alternated future event than a repeated future event. Although we have no direct evidence that this is the case, it seems a worthwhile hypothesis to pursue in future studies. In particular, if one of the constraints on participants’ attempts to simulate a future event is to create an episode that is distinct from anything recent that they have experienced, then they may well be able to simulate a distinct future event that involves an alternated target more easily than a repeated event. Moreover, the underlying principle that produces this difference in the ease of simulation could well be the same as that which makes it easier for participants to detect and encode a novel (uncued) target relative to a familiar (cued) target in more conventional studies of IOR. In other words, speed of responding might well be limited by the efficiency with which a target can be detected and encoded as a distinct event from the cue (Lupiáñez, 2010; Milliken, Tipper, Houghton & Lupiáñez, 2000; Milliken & Rock, 1997).

Conclusion

This article highlights problems associated with an assumption that performance is directly related to subjectively reported expectancies. In the present study, subjective expectancies aligned with performance in just one of the two intervening event conditions. Participants reported that they expected alternated events to occur more often than repeated events for both intervening event conditions. In contrast, responses were faster for alternated than for repeated events in the intervening event condition whereas the opposite effect was observed in the no-intervening event condition. Together, the results fit with the idea that response to the intervening event disrupts a process that speeds responses to repeated trials relative to alternated trials, and reveals a process that produces the opposite effect; that is, a process that favors the encoding of relative novel events over familiar events. Rather than assuming that participants’ subjectively reported expectancies cause performance effects in tasks like that used here, it seems instead that the cause of subjective expectancies themselves requires further study.

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CHAPTER 6: GENERAL DISCUSSION

Our attentional system is remarkably flexible. On one hand, it ensures that we are sensitive to stimuli that match with our prior experiences. As Treisman (1992) noted, this sensitivity implies that we can efficiently integrate familiar current experiences with memory representations of similar past experiences. On the other hand, our attentional system also ensures that we are sensitive to novel experiences. How our attentional system accomplishes both of these feats is still an open issue. At the heart of this issue is a need to understand how opponent processes that favour familiarity and novelty co-determine human performance.

Trial-to-trial repetition effects have long been used as a tool to study one of these processes. Specifically, faster responses for repeated relative to alternating targets in trial to trial performance situations stood as evidence of a mechanism that is relatively more efficient at processing familiar experiences over novel experiences (Bertelson, 1961). Since the 1960’s, a number of variants of the repetition effect have been studied (Kahneman, Treisman & Gibbs, 1992; Pashler, 1991; Rabbitt, 1968; Smith, 1968; Hommel, 1998), but for the most part the theoretical emphasis has been on mechanisms that allow a familiar event to be rapidly integrated into a memory representation of a similar prior event (e.g., an object or event file; Kahneman et al., 1992; Hommel, 1998). By this modal view, faster performance for trial-to-trial repetitions than trial-to-trial alternations can be attributed to reliance on a memory representation of a similar prior trial for repetitions but not for alternations.

However, evidence of a second and opposing mechanism that selectively favours attention to alternations over repetitions was reported in studies of spatial orienting. In studies of IOR, participants routinely detect targets more quickly when presented in the location opposite that of a prior attentional cue than in the same location as a prior attentional cue (Posner & Cohen, 1984; Posner, Rafal, Choate, & Vaughan, 1985). Theoretical accounts of IOR commonly assume that it is unique to the spatial domain, as repetition benefits are so commonly observed with non-spatial orienting tasks. Nonetheless, a small subset of studies has looked for examples of non-spatial IOR effects (Law, Pratt, & Abrams, 1995; Francis & Milliken, 2003; Hu, Samuel, & Chan, 2011). Although there has been disagreement about whether these effects are driven by the same mechanism as spatial IOR effects (Tanaka & Shimojo, 1996; Taylor & Klein, 1998), it is now relatively clear that IOR-like effects can be observed with non-spatial stimuli (Hu & Samuel, 2012).

A concern raised about prior findings of non-spatial IOR-like effects is that they were measured using a cue-target procedure (Law et al., 1995; Francis & Milliken, 2003; Hu et al., 2011). With this type of procedure participants are required to withhold their response to the first of two events (the cue), and then respond to the second of two events (the target). If onset of the target cues the retrieval of stimulus-response bindings for similar cue events, then repeated stimuli in a cue-target procedure might be associated with a form of “response-inhibition” cost (Hommel, 1998; Welsh & Pratt, 2006). Accordingly, a repetition cost effect observed with a cue-target procedure might be argued not to constitute an example of “true” IOR; that is, an effect that hinges on delayed orienting to a target rather than on a delay due to response inhibition.

Prior to the results reported in this thesis, to my knowledge, IOR had been observed using a target-target procedure only in tasks in which repetition/alternation was defined spatially. Indeed, this empirical property helped to support the interpretation that the IOR effect tapped an attentional mechanism that was dedicated to shifts of attention in space. The data reported in the empirical chapters of this thesis constitute the first demonstrations of a non-spatial IOR effect measured with a target-target procedure. These non-spatial IOR effects point to the broader interpretation that IOR reflects a mechanism that favours attentional orienting to novelty.

**The Intervening Event Effect**

The empirical chapters of this thesis describe the first empirical evidence of which we are aware that non-spatial IOR effects can be observed using a target-target procedure. This effect hinged on presentation of an intervening event between consecutive targets. Originally, the motivation for introducing an intervening event in this task was to create an analogue to the “cue back” (or central cue) manipulation used in spatial orienting tasks to observe IOR (see Law, Pratt, & Abrams, 1995 for the first demonstration). On one level, this surface similarity between the intervening event and a “cue back” could be taken as evidence of a similarity between the non-spatial IOR effects reported here and spatial IOR effects reported elsewhere. However, such conclusions should be drawn cautiously. The attentional processes triggered by the task of detecting and responding to the onset of an intervening event in the present studies could be quite different from the processes triggered by a sudden onset “cue back” stimulus that requires no response. Nonetheless, the question that was pursued through the empirical chapters of this thesis was how response to an intervening event between two non-spatial targets could conceivably reveal the same attentional mechanism that underlies the spatial IOR effect.

Chapter 2 introduced the intervening event effect using a simple two-alternative forced choice task that required participants to perform a discrimination response to two sequential non-spatial targets (T1 and T2). The critical manipulation across all the experiments in Chapter 2 was the presence or absence of an intervening event. The intervening event effect was observed across all the experiments in Chapter 2 in which participants performed a response to the onset of the intervening event (Experiments 1A, 1B, 1C, 2, 3A, 3B, & 4). In contrast, a repetition benefit was observed when an intervening event was absent, or did not require a response (Experiment 5). The robustness of the intervening event effect was demonstrated by the fact that the effect was observed across a range of non-spatial target stimuli (Experiments 1A, 1B, & 1C), when the intervening event was presented in a different sensory modality (e.g. aurally) than the targets (Experiment 4), and even when a vocal response was performed to the intervening event (Experiments 3A & 3B). Furthermore, the intervening event effect appeared not to be susceptible to strategic influences (Experiment 2) and was insensitive to the relative temporal proximity between the offset of T1 and the onset of the intervening event (Experiments 3A & 3B).

The intervening event effect was explained with reference to two opposing attentional processes that contribute to performance in both spatial and non-spatial orienting tasks. Briefly, this dual process account proposes that an attentional process that facilitates performance for repeated events can mask the contribution of a separate attentional process that slows performance for repeated events. This second attentional process could potentially be the cause of IOR effects in spatial orienting tasks. The intervening event manipulation was intended to disrupt the process that facilitates performance for repeated events, and thus reveal the contribution of an attentional process that slows response to repeated events, both spatial and non-spatial.

Chapter 2 also proposed an alternative view in which the intervening event effect might be driven by a change in expectation brought about by the intervening event manipulation. This issue was addressed in Chapter 5. This expectation for alternation account was motivated by the view that expectancy may play a role in IOR and other effects characterized by fast responses to alternations (Spalek, 2007; see also Kirby, 1976). In particular, Spalek (2007) found that participants reported that they “expected” a spatial target to appear significantly more often than chance in a location directly opposite to the cued location, and significantly less often than chance in the cued location. Because these verbally reported expectations were in-line with the usual behavioural measures (i.e., RTs) reported in studies of the IOR effect, Spalek suggested that expectation may underlie the IOR effect. The experiment reported in Chapter 5 examined whether non-spatial IOR effects occur in the intervening event condition because intervening events increase expectation for alternation. The results revealed an important dissociation between measures of RT and subjective expectancy. That is, although an IOR-like effect was observed in RTs only for the intervening event condition, participants reported that they expected alternated targets to occur more often than repetitions for both the intervening event *and* the no-intervening event conditions. These results cast doubt on the idea that non-spatial IOR effects reported in our prior study were due to an increase in expectation for alternation triggered by the intervening events.

Chapter 3 built upon an intriguing question that was raised at the end of Chapter 2, which was whether responding to the intervening event was critical for revealing the intervening event effect. In the series of experiments in Chapter 2, the only condition in which a repetition benefit was observed in the intervening event condition was when the requirement to respond to the intervening event was eliminated. The failure to observe the intervening event effect when a response was not made to the intervening event motivated the series of experiments in Chapter 3. A go/no-go manipulation was introduced in Experiments 2, 3, and 4 to determine whether conditions that require greater attentional scrutiny to the intervening event, without requiring a response, could also reveal an intervening event effect. In the end, Experiment 4 demonstrated that responding to the intervening event was not critical to observe the intervening event effect, but that selecting a response to the intervening event was likely critical.

An important limit to the intervening event effect was discovered in Chapter 4 when the targets no longer appeared in the same location. The intervening event effect was eliminated when the location of T1 and T2 was repeated. Repeating the location of the targets, even though the task was still to respond to the target’s colour, was argued to support the facilitative attentional process – episodic integration. In Experiment 1, an intervening event effect was observed when all the targets were presented centrally (as in Experiment 1A of Chapter 2). Presumably, the intervening event effectively disrupted the facilitative effect of repetition in this experiment. In Experiment 3, the targets were equally likely to appear in the central location, or in one of two peripheral locations. Here, for the same central trials that produced a repetition cost in Experiment 1, a repetition benefit was observed. As such, the results of Chapter 4 illustrate an interesting context effect; spatial unpredictability appears to produce an episodic integration effect that facilitates responses to repeated spatial events, and that is not entirely disrupted by response to an intervening event.

**The Dual Process Framework**

All of the results of the empirical chapters were explained in the context of a dual process framework. According to this framework, the repetition effects measured in our studies reflect the joint contribution of two processes, one that speeds performance for repetitions relative to alternations, and another that does the opposite. This specific assumption fits well with the broader notion that our attentional system is simultaneously tuned to capitalize on both familiarity and novelty in the environment.

The process that is proposed to speed performance for repeated events is episodic integration. Support for this idea comes from a wide array of attention and performance domains (Chun & Jiang, 1998; Neill, Valdes, Terry, & Gorfein, 1992; Mayr & Kliegl, 2000; Tipper, & Kessler, 2003). However, the most relevant support stems from other studies of trial-to-trial repetition effects. Many such studies have led researchers to conclude that onset of a current target can cue the retrieval of prior stimulus-response (S-R) bindings (Hommel, 1998; Pashler & Baylis, 1991; Rabbitt, 1968). Integration of current perceptual codes with these retrieved S-R bindings can facilitate responding when there is a good correspondence between bindings required for the current task and those retrieved from a similar prior event. In these cases, the attentional system can adopt the shortcut of relying on the S-R bindings that have already been created rather than the longer process of creating those S-R bindings from scratch (Hommel, 1998; Pashler & Baylis, 1991; Rabbitt, 1968).

A key idea underlying application of a dual process framework in the present thesis is that requiring participants to respond to an intervening event disrupts this potential shortcut to a response. In effect, in the language of object files introduced by Kahneman et al. (1992), response to the intervening event in the experiments reported here may have closed the object file for T1 that would otherwise have remained “open” for integration with T2. The disruption of event integration may hinge on the creation of a distinct representation, or object file, for the intervening event, which in turn serves to differentiate T1 from T2. This idea fits well with the view that integration of action-related codes with perceptual codes is fundamental to the creation of perceptual episodes (Hommel, 1998; Hommel, 2005; Hommel, Musseler, Aschersleben, & Prinz, 2001), and further with results from the Psychological Refractory Period (PRP) literature indicating a link between processes involved in response selection and short-term episodic consolidation (Hommel & Doeller, 2005; Jolicoeur & Dell’Acqua, 1999).

A second key idea underlying application of the dual process framework in this thesis is that disrupting the episodic integration process reveals the influence of a second process that is responsible for slowing responses to repeated events. A brief description follows of three candidate processes that have been discussed in the literature, and that have the breadth required to encompass both spatial and non-spatial forms of IOR.

**Perceptual Inhibition**

According to mismatch theory (Johnston & Hawley, 1994), the degree to which a current experience matches with an existing mental representation can dictate subsequent perceptual processing of that experience. The general idea is that when a current perceptual event is familiar, it is adaptive not to waste “data-driven” processing resources on that event, as it can be interpreted by reliance on an existing memory representation. Johnston and Hawley (1994) proposed that perceptual inhibition suppresses data-driven processing of familiar experiences. By inhibiting the “inefficient” perceptual processing of familiar events, our attentional system ensures that data-driven resources are selectively allocated to the processing of novel events that mismatch with pre-existing memory representations.

Indeed, studies within the spatial IOR literature fit well with the idea that a perceptual inhibition process slows the accumulation of perceptual information at cued locations (e.g., Abrams & Dobkin, 1994; Gibson & Egeth, 1994; Handy, Jha, & Mangun, 1999). Furthermore, there is nothing inherent to the mismatch theory that would restrict the effect of perceptual inhibition to the spatial domain. As such, non-spatial IOR effects might also be explained by a perceptual inhibition process that slows perceptual processing for repeated relative to alternating events defined by non-spatial dimensions such as colour and shape.

Importantly, Johnston and Hawley (1994) proposed that perceptual inhibition effects are often the product of two opposing mental biases, one that affords rapid orienting of attention to novel events, and an opposing bias that results in rapid processing of familiar events. The notion of opposing mental biases on attention dovetails with the dual process framework proposed here. Indeed, it follows that disruption of one mental bias that favours processing of familiar events might well reveal an opposing bias that favours processing of novel events.

**Habituation**

Similar to the mismatch theory, Dukewich (2009) built on comparator theory (Sokolov, 1963) to define IOR as the habituation of an attentional orienting response. Dukewich proposed that IOR effects are due to the degradation in the orienting response to familiar stimuli. According to comparator theory, orienting to a stimulus creates an increasingly accurate neuronal representation of that stimulus. The more accurate the neuronal representation becomes, the less need there is for additional orienting and refining of that representation. However, a novel stimulus will mismatch all existing neuronal representations, which will produce a relatively strong orienting response. In the context of the IOR paradigm, orienting attention to a previously cued location will produce a relatively weak orienting response as the same orienting response was recently generated at that location. Conversely, orienting attention to an uncued location will produce a novel neuronal representation for that location, which will generate a relatively strong orienting response.

Dukewich (2009) also argued that defining IOR as a habituation effect can account for a variety of contradictory findings within the IOR literature. Of most interest here, if IOR effects are due to the habituation of an orienting response, then IOR might be expected to occur for a wide variety tasks that tap a wide variety of orienting responses – motor orienting, perceptual orienting, and non-spatial orienting responses (Rose & Rankin, 2001).

The habituation framework can be extended to the intervening event effect if one assumes that responding to an intervening event terminates the orienting response to T1, just as a central cue might terminate the orienting response to a peripheral cue. This interpretation is similar to the notion that the function of the intervening event is to differentiate between the T1 and T2 events. In turn, the orienting response to T2 ought to be smaller when it matches T1 than when it mismatches T1, in part because the orienting response to a matching T2 is habituated, and in part because the prior orienting response to T1 was terminated by the intervening event.

**Detection Cost**

Similar to the habituation framework, Lupiáñez, Martín-Arévalo, and Chica (2013) argued that IOR effects reflect the rapid capture of attention by novelty and saliency in our environment (see also Martín-Arévalo, Kingstone, & Lupiáñez, 2013). By this view, novel attention-capturing events disrupt current attentional processing in order for the attentional system to assess the “relevancy” of the novel event and process the event accordingly. Lupiáñez and colleagues (2013) propose further that performance in spatial orienting tasks taps into multiple attention processes, but that only one of those processes is responsible for IOR – a detection process. In effect, IOR reflects an increased difficulty in detecting the onset of a target when it matches relative to when it mismatches a preceding cue. Thus, when perceptual similarity between cue and target events is high, the cost in detecting the onset of the target will also be high, and IOR ought to be observed.

Another important component of the detection cost framework is that IOR effects are often opposed by cue-target integration processes that produce the opposite effect – that is, a repetition benefit. For this reason, IOR tends to be observed under task conditions in which cue-target integration is likely to be disrupted, such as when the temporal lag between cue and target is relatively long (Lupiáñez et al., 1997), or when a salient distractor is added to the location opposite the target (Lupiáñez & Milliken, 1999). Essentially, any task conditions that bias the attentional system towards the detection of novel events and the subsequent creation of distinct episodes will be best suited for observing IOR effects.

The detection cost framework does not require a distinction between spatial and non-spatial IOR effects. Much like the dual process framework introduced in this thesis, an IOR effect might be expected under any conditions in which cue-target integration processes that produce facilitation effects are disrupted. Accordingly, the intervening event in the present experiments would serve to reveal an IOR effect, caused by slowed detection of targets whose perceptual and response features overlap with the preceding target.

**Conclusion**

The goal of the experiments reported throughout this thesis was to find evidence of a broad attentional orienting process that could produce IOR effects outside of the spatial domain. Each of the empirical chapters in this thesis built upon a dual process framework that explained IOR effects in the context of two opposing processes. The notion that two opposing processes might account for orienting effects is not new to the spatial IOR literature (Tipper et al., 1997; Klein, 2000). However, prior to the present set of studies, a dual process framework had not been applied outside of the spatial orienting domain, presumably because the existence of non-spatial IOR effects was doubted (Tanaka & Shimojo, 1996; Taylor & Klein, 1998a; Fox & de Fockert, 2001). One of the reasons why the existence of the non-spatial IOR effect was doubted was that non-spatial IOR effects had not been observed in Target-Target procedures. Instead, past studies with such procedures had produced the opposite effect, a repetition priming effect (Bertelson, 1961; 1963).

The results of the studies in this thesis show that a Target-Target procedure is not a limiting factor for observing non-spatial IOR. Rather, the introduction of an intervening event within a Target-Target procedure revealed a robust non-spatial IOR effect. I have argued that the existence of both spatial and non-spatial IOR effects invites a dual process framework with broad component processes. The dual process framework rests on the following features: 1) rapid responses to repeated target events is driven by an episodic integration process that retrieves prior instances from memory, and 2) disruption of that integration process reveals an opposing process that rapidly orients attention to novel events.

Although the notion of episodic integration as one of the key processes in this framework is unlikely to be controversial, the nature of the second process is less clear. I have pointed to three candidate processes that have the required breadth – perceptual inhibition (Johnston & Hawley, 1994), habituation (Dukewich, 2009), and detection failure (Lupiáñez, 2010). All three candidate processes fit the description of a broad process that leads to rapid orienting of attention to novel events, and that therefore can accommodate IOR effects for both spatial and non-spatial stimuli.

Of course, an understanding of how these two opposing processes interact with each other extends far beyond the scope of the IOR effect. Opponent processes dedicated to efficient processing of familiarity on the one hand, and novelty on the other hand, is fundamental to cognition. The dual process framework offered here to explain IOR effects offers one lens for viewing how our attentional system is simultaneously tuned to both novelty and familiarity.

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1. Two participants in Experiment 3B displayed slower overall reaction times (821 msec, 687 msec) relative to the overall mean for all participants (597 msec). We conducted separate analyses both with and without those two participants, which did not reveal any differences in the overall pattern of RTS. We decided to keep both subjects in the analyses to maintain an equal number of subjects for both Experiment 3A and 3B. [↑](#footnote-ref-1)
2. The data from the twenty-four participants assigned to the MN intervening event condition were collected subsequent to collecting the data from the twenty-four participants in the XO intervening event condition. The two sets of data are described together here as a single experiment for sake of brevity, and because there was again no indication that this variable (XO vs MN) impacted the effects of interest. [↑](#footnote-ref-2)
3. The RSI factor was manipulated to determine whether the proportion of subjective expectancies for alternation increases across time between events (Kirby, 1976). [↑](#footnote-ref-3)
4. The aim of Figure 4 was to represent the data in a similar manner to Figure 3, to allow comparison between participant’s performance on the Expectancy-Response and Color-Response trials. To that end, Figure 4 presents both proportions of expected repetitions and expected alternations, with the sum of these two measures equal to 1.0 for both intervening event conditions. [↑](#footnote-ref-4)
5. In follow-up work on this issue, we have discovered that a response to the intervening event is not required to observe the non-spatial IOR-like effect. Instead, it appears that engagement in response selection processes may be critical. In particular, using a procedure similar to the one reported here, we found that non-spatial IOR-like effects were observed when participants withheld a response to a NoGo intervening event that was identical to a previous Go intervening event (Spadaro, Lupiáñez & Milliken, submitted). [↑](#footnote-ref-5)
6. We thank Tom Spalek for suggesting this alternative interpretation of our results. [↑](#footnote-ref-6)