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DOCTOR OF PHILOSOPHY

Control of cognitive processes in task-switching

Grange, James A.

Award date:
2010

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Control of Cognitive Processes in Task-Switching

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Thesis submitted for the degree of Doctor of Philosophy in Psychology

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1st March 2010



“...if you are doing an experiment, you should report everything that you think might make it invalid — not only what you think is right about it: other causes that could possibly explain your results; and things you thought of that you’ve eliminated by some other experiment, and how they worked — to make sure the other fellow can tell they have been eliminated. Details that throw doubt on your interpretation must be given, if you know them. You must do the best you can — if you know anything at all wrong, or possibly wrong — to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it... ...although you may gain some temporary fame and excitement, you will not gain a good reputation as a scientist if you haven’t tried to be very careful in this kind of work... ... Science is a way of trying not to fool yourself. The first principle is that you must not fool yourself, and you are the easiest person to fool.”

— **Richard P. Feynman** (Feynman, 1974)

ACKNOWLEDGEMENTS

The past two years have been an incredibly busy time, but have also been most enjoyable and rewarding. For this, there are many people I would like to thank.

I would like to thank my PhD supervisor, Dr. George Houghton, who made it possible for me to pursue my doctorate in an area of research I was so interested in. He has given me the freedom to explore my own ideas with the knowledge he was always there to keep me on track if I got too carried away. If I am fortunate enough to obtain a career in experimental psychology, I hope I have in him a life-long collaborator.

Thanks are extended also to the other members of my PhD research committee, Professor Steve Tipper, and Dr. Paloma Marí-Beffa, who provided much advice during my yearly review meetings.

Much of the work in this thesis has also benefited from the expert reviews I received for published articles. These extensive reviews helped strengthen my research, and have improved this thesis as a consequence. Thanks go to Erik Altmann, Katherine Arbuthnott, Kate Arrington, Bernard Hommel, Ronald Hübner, Iring Koch, Ulrich Mayr, Nachshon Meiran, W. Trammell Neill, Darryl Schneider, and Paul Verhaeghen.

I would also like to thank my good friend Rhys Pritchard, who provided so much encouragement and support (not to mention tea!) during my early research days. It was this period of work that sparked my passion for research, and started me off on the road I am still traveling. For that, I will be forever thankful.

Thanks to Julian Breeze, for introducing me to \LaTeX . As a consequence, I have a nice-looking thesis (although I also have less hair as a consequence of pulling it all out when \LaTeX wouldn't compile!). Thanks also to Alex Kirkham, fellow lab member, who has helped me out of a \LaTeX -induced pickle more than once.

My parents, Stephen and Carole Grange, have provided so much emotional (and financial!) support over my postgraduate years, without which this thesis

would not be possible. Thanks for always being there, and for believing in me when at 16 I told you I didn't ever want to go back to education! You gave me the freedom to make my own decisions, and I believe it was this that got me to where I am today. Thanks also to my bro' Ed for always being a good friend (and for the tickets to go see the 49ers!).

Last, but far from least, I would like to thank Shell. You have been the support I needed, always there to listen when I needed to moan, always there to make me laugh (are you reading these?). I can think of no other person I would rather share this adventure with. I can't wait to share many more with you.

Dwi'n caru chdi Shell, always. Now go get the guitar...

Components of this thesis have been communicated as follows:

Journal Articles

Grange, J.A., & Houghton, G. (2009). Temporal cue-target overlap is not essential for backward inhibition in task switching. *Quarterly Journal of Experimental Psychology*, 62, 2069-2080.

Grange, J.A., & Houghton, G. (in press). Cue-switch costs in task-switching: Cue-priming or control processes? *Psychological Research*.

Grange, J.A., & Houghton, G. (in press). Heightened conflict in cue-target translation increases backward inhibition in set switching. *Journal of Experimental Psychology: Learning, Memory, & Cognition*.

Presentations

Grange, J.A. (2009, September 1). *Cue-target translation & the cost of switching cues*. Paper presented at the British Psychological Society 26th Cognitive Section Conference, University of Hertfordshire, UK.

Grange, J.A. (2009, July 9). *The role of cue-target relationships in task switching*. Poster presented at the 2nd Welsh Institute of Cognitive Neuroscience Conference, Deganwy, North Wales, UK.

Grange, J.A. (2008, July 31). *Does task switching require an endogenous act of control?* Paper presented at the PsyPAG Annual Conference, University of Manchester, UK.

Contents

DECLARATION AND STATEMENTS	ii
SUMMARY OF PHD	xiii
CHAPTER 1: INTRODUCTION	2
The task-switching paradigm	3
Alternating-runs procedure	4
Task-set inertia	7
Explicit-cuing paradigm	9
Cue-based preparation	12
Altmann & Gray's model	19
Problems with the cuing paradigm	20
Logan and colleagues' models	22
Challenges to Logan's models	28
Inhibitory mechanisms in task switching	31
Goals of the PhD	44
Overview of chapters	45
CHAPTER 2: BACKWARD INHIBITION & NEGATIVE TRANS-	
FER	49
Abstract	49
Literature Review & Introduction	50
Experiment 1	53
Method	53
Results	55
Discussion	58
Experiment 2	58
Method	59
Results	61

Discussion	62
Experiment 3	63
Method	63
Results	65
Discussion	67
General Discussion	67
Chapter Summary	69
 CHAPTER 3: BACKWARD INHIBITION WITH 2 CUES PER	
TASK	71
Abstract	71
Literature Review & Introduction	72
Experiment 4	77
Method	77
Results	79
Discussion	81
Experiment 5	82
Method	84
Results	86
Discussion	87
General Discussion	89
Chapter Summary	92
 CHAPTER 4: CUE-TARGET OVERLAP & BACKWARD INHI-	
BITION	94
Abstract	94
Literature Review & Introduction	95
Experiment 6	98
Method	99

Results	101
Discussion	104
Experiment 7	106
Method	106
Results	107
Discussion	109
Experiment 8	110
Method	110
Results	112
Discussion	113
General Discussion	114
Chapter Summary	116
CHAPTER 5: CUE-TRANSPARENCY & SWITCH COSTS	119
Abstract	119
Literature Review & Introduction	120
Experiment 9	125
Method	126
Results	127
Discussion	129
Experiment 10	129
Method	130
Results	132
Discussion	135
Experiment 11	136
Method	136
Results	137
Discussion	139
Experiment 12	140

Method	141
Results	142
Discussion	145
General Discussion	145
Chapter Summary	149
CHAPTER 6: CUE-TRANSPARENCY & EXTENDED RUNS DE-	
SIGN	158
Abstract	158
Literature Review & Introduction	159
Experiment 13	167
Method	167
Results	168
Discussion	171
Experiment 14	173
Method	174
Results	175
Discussion	177
General Discussion	177
CHAPTER 7: GENERAL DISCUSSION	182
Overview of findings	182
Cognitive control processes in our paradigm	184
Theoretical implications of research	187
Limitations of research	189
Under-specification of task processes	189
<i>N</i> -2 repetition costs & preparation	190
Do <i>n</i> -2 repetition costs really reflect inhibition?	194
Correlated costs and benefit problem (CCB)	195
Individual differences	197

Conclusion	200
References	201
Appendices	215
CSI manipulation between-subjects	216
Method	216
Results	217
Working Memory Capacity & Backward Inhibition	220
Method	220
Results	222

List of Figures

1	Example of list-based task switching paradigm	4
2	Cue-target interval (Meiran, 1996)	10
3	Word cue sequences from Houghton et al., (2009)	41
4	Word cue sequences from Houghton et al., (2009)	42
5	Cues & targets from Experiment 1	54
6	Cues & targets from Experiment 2	60
7	Cues & targets from Experiment 3	64
8	Cues & targets from Experiment 4	78
9	Reaction times from Experiment 4	80
10	Reaction times after median split from Experiment 4	81
11	Cues & targets from Experiment 5	85
12	Reaction times from Experiment 5	87
13	Processing model for $n-2$ repetition costs	89
14	Cues & targets from Experiment 6	100
15	Cues & targets from Experiment 10	131
16	Reaction times from Experiment 10	133
17	Cues & targets from Experiment 11	137
18	Reaction times from Experiment 11	139
19	Cues & targets from Experiment 12	142
20	Reaction times from Experiment 12	144
21	Reaction times and errors from Experiment 13	169
22	Restart costs from Experiment 13	172
23	Restart costs from Experiment 14	176
24	CDF plots from Experiment 1 (standard-cues)	192
25	CDF plots from Experiment 1 (switched-cues)	193
B1	Timeline of the AOSPAN task	221

List of Tables

1	Mean RT and errors for ABA & CBA sequences in Experiment 1. . .	56
2	Mean RT and errors for ABA & CBA sequences in Experiment 2. . .	62
3	Mean RT and errors for ABA & CBA sequences in Experiment 3. . .	66
4	Percent errors for ABA & CBA sequences in Experiment 5.	88
5	Mean RT & errors for ABA & CBA sequences in Experiment 6. . .	102
6	Mean RT & errors for ABA & CBA sequences in Experiment 7. . .	108
7	Mean RT & errors for ABA & CBA sequences in Experiment 8. . .	112
8	Mean RT & errors for Switch & Repeat sequences in Experiment 9.	128
9	Mean RT & errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 10.	134
10	Mean RT & errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 11.	138
11	Mean RT & errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 12.	143
12	Model parameters & fits for Experiment 10	152
13	Model parameters & fits for Experiment 11	153
14	Model parameters & fits for Experiment 12	154
A1	Mean RT & errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment A1.	217
B1	Correlation matrix from AOSPAN experiment	223

SUMMARY OF PHD

The explicitly-cued task-switching paradigm is a popular tool for measuring the processes that enable flexible cognitive control. In this paradigm, participants are typically presented with a stimulus that affords multiple tasks (e.g. numbers). Participants know which task to perform on a given trial by way of a task cue. In the present thesis, I investigate what cognitive control processes allow the correct task to be performed in the face of conflicting irrelevant tasks. Participants were presented with a cued attention-switching paradigm, wherein most aspects of the task-set were kept constant (i.e. stimulus display, response processes etc.), and participants merely switched attention between cue-target pairings.

Across five experimental chapters, I provide evidence that task performance involves activation of relevant working memory (WM) representations that guide behaviour, together with inhibition of irrelevant representations. In the first half of the thesis, I investigate inhibitory mechanisms during task-switching. Although typically believed to suppress response-related processes of trial performance, I provide converging evidence that inhibition can also target earlier, cue-based preparatory stages of task performance. Specifically, I suggest that inhibition targets WM representations that are formed after cue-presentation, and that inhibition does not target the cue itself. In the second half of the thesis I investigate in more detail the activation processes that serve to form these WM representations by separating cue-related processes from task-switching processes using two cues per task. By varying the degree to which a cue provided the necessary WM representation (cue-transparency), I was able to show that cue-switch costs arise from cognitive control processes. Additionally, I provide evidence that cue-transparency has no carry-over effect on target processing by separating cue- and target-related processes using an extended runs design.

The findings suggest control of cognitive processes requires a balance between inhibitory and activation processes that serve to manage access to a WM representation of what to do.

Chapter 1

General Introduction

CHAPTER 1: INTRODUCTION

The question as to how humans organise and control their ongoing cognitive processes is fundamental in cognitive psychology. The question is fundamental as humans live in an extremely rich, multi-task environment, which often requires selecting and switching between relevant operations in order to achieve goal-directed behaviour. For example, the simple act of making a cup of coffee requires many cognitive processes that need to be implemented: walking to the kitchen (which requires attentional resources), retrieval from memory where the coffee is stored, mental rotation to read the coffee label to assure you don't select the de-caf, coordination of both hands to open the coffee jar, and so on. Despite our impressive knowledge of how individual processes such as these are implemented (Diedrichsen, Shadmehr, & Ivry, 2010; Siu, Chou, Mayr, vanDonkelaar, & Woollacott, 2009; Vilberg & Rugg, 2008; Zacks, 2008), much less is known about how they are controlled and selected appropriately (Monsell & Driver, 2000b).

The problem of how humans select appropriate cognitive processes is compounded when stimuli afford several actions, many of which are irrelevant to the current task. For example, there are many operations that can be performed on a printed word: it can be read aloud, read silently, translated into another language, categorised semantically etc. (Monsell & Driver, 2000a). However, all of these operations would be totally inappropriate if the task were to name the colour the word was printed in (MacLeod, 1991; Stroop, 1935). In order not to allow behaviour to be stimulus-driven in this manner, top-down control mechanisms are required to select the goal-relevant action (Waszak, in press). Selection failure is often seen in every day action slips (such as putting a tea bag in your mug instead of coffee; Reason, 1984). Pathologically, damage to the prefrontal cortex is sometimes associated with "utilisation behaviour" (Lhermitte, 1983), where patients are unable to inhibit goal-irrelevant actions afforded by

stimuli presented to them.

Selection is not the only problem the cognitive system has to overcome. Once selection has occurred, the system needs to ensure that the selected task dominates behaviour, preventing intrusion from competing tasks. Thus, the system needs to ensure *stability* of tasks once selection has occurred. Somewhat paradoxically, this stability needs to be *flexible*, in that tasks must be removed and replaced when goals change. The tension between these competing demands has been called the *stability-flexibility dilemma* (Goschke, 2000), and understanding the mechanisms that allow this balance to occur is a major challenge to researchers of cognitive control.

The task-switching paradigm

One tool to investigate the control of cognitive processes that has garnered much attention in the literature over the past decade is the task-switching paradigm (see Meiran, in press; Monsell, 2003, for reviews; c.f. Altmann, 2003). The first empirical study of task-switching was introduced by Jersild (1927). Jersild presented participants with two lists of stimuli (e.g. numbers), and compared the time it required for participants to work through each list. One list required participants to perform the same task on each stimulus (e.g. addition), and the second list required participants to switch between two tasks (addition on first stimulus, subtraction on second stimulus etc.). Jersild found that list completion times were longer for lists requiring task-switching compared to repetitions (an effect the reader can replicate in figure 1; see also Spector & Biederman, 1976).

Based on this finding, Jersild (1927) suggested that in order to perform the correct task, participants must collate in mind a set of task-relevant rules that allow correct performance of the task, which takes time to implement. This "mental-set" guides behaviour in situations where stimuli afford more than one task (i.e. when stimuli are *bivalent*). During the alternating list, participants must

19, 33, 26, 58, 11, 73, 78

Figure 1. Example of list-based task switching paradigm, a variant of which was used by Jersild (1927). Initially, work through the list by adding 3 to each number (example of task repetition). After this, work through the list again, adding 3 to the first number, subtracting 3 from the next, and repeating this pattern until the list is complete (example of task-switching). List completion should take longer when task-switching compared to repeating the same task throughout.

update their mental-set at every stimulus, unlike in the repetition list where only one mental-set is relevant throughout. The concept of mental set has been somewhat updated (now called a *task-set*), and is typically now defined as "*the configuration of perceptual, attentional, mnemonic, and motor processes critical for a particular task goal*" (Mayr & Keele, 2000, p.5.). Establishment of a relevant task-set has been suggested as being one key way that the cognitive system shields itself from interference in multi-task situations (Dreisbach & Haider, 2008, 2009), ensuring stability during task performance.

Alternating-runs procedure

Although the list paradigm is sometimes used today (Baddeley, Chincotta, & Adlam, 2001; Bryck & Mayr, 2005; Emerson & Miyake, 2003), there are certain flaws within its design that suggest it is not a clean measure of cognitive control. The main concern with the list paradigm is that in the alternating condition, two task-sets must be held accessible in memory, whereas only one is required for the repetition list (see Los, 1996; Philipp, Kalinich, Koch, & Schubotz, 2008; Poljac, Koch, & Bekkering, 2009; Rubin & Meiran, 2005, for a related finding). Related to this, the alternating lists require memory for where in the sequence one is, a problem not relevant in the repetition lists. Thus the difference in list completion times are likely due to memory-load differences rather than task-switching operations.

Rogers and Monsell (1995) addressed this problem by introducing the alternating runs procedure. In this paradigm, participants switch between two

tasks every second trial in a predictable manner (e.g. AABBAABB...). This paradigm allows a measure of the time taken for task-repetitions and task-switches within the same block (e.g. AA and AB respectively), thus overcoming the memory-load problems inherent in the design of Jersild (1927). To reduce the impact of memory-load for where participants were in the sequence of tasks, stimuli were presented within a 2 x 2 grid, with the stimulus location rotating between each quadrant clockwise after every trial. Stimuli were mostly bivalent, consisting of a number and a letter. The two relevant tasks were a parity judgement on the number stimulus (odd/even) or a consonant/vowel judgement on the letters. The relevant task was signalled by the location of the stimulus within the 2 x 2 grid: one task was relevant when the stimuli were in the upper-two quadrants, and the task switched to the alternative task when the stimuli location rotated into the lower two quadrants (thus producing the AABBA... structure).

Rogers and Monsell (1995) replicated and extended the findings of Jersild (1927) by finding that RTs to a task-switch were slower and more error-prone than task-repetitions, an effect they called the "switch cost". Rogers and Monsell suggested that this switch cost was the behavioural manifestation of a time-consuming reconfiguration process that enabled a switch from one task-set to another. This endogenous reconfiguration occurs on task-switch trials as the previously relevant task-set is no longer relevant, and needs to be altered; task-repetitions do not require reconfiguration as the system is supposedly configured to the correct task already. Thus, by this logic¹, the switch cost provides a useful window into the temporal dynamics of cognitive control processes in operation.

Besides the advantage of reducing memory load for which task is relevant, the alternating runs paradigm allows some degree of control over how much time a participant has to engage in readying themselves for the switch in task-set. Rogers and Monsell (1995) argued that if reconfiguration processes are the source of the

¹A logic, we will come to discover, that has not met universal acceptance in the literature (Altmann, 2003a; Altmann & Gray, 2008; Logan, 2003; Schneider & Logan, 2005)

switch cost, and if reconfiguration is an endogenous control mechanism, then some degree of reconfiguration might be able to occur in advance of the task stimulus. By manipulating the time between a response on the previous trial ($n-1$) and the onset of the stimuli for the current trial (n), Rogers and Monsell (1995) were able to manipulate preparation time (this interval is called the response-stimulus interval, or RSI). Rogers and Monsell proposed that extended preparation intervals, especially intervals longer than an assumed reconfiguration process might take, should reduce the switch cost, as much of the reconfiguration can occur in advance. They tested this hypothesis in their Experiments 2-5 by manipulating the RSI between 150 milliseconds (ms) and 1,200ms. Despite a significant reduction of the switch cost at RSI , of up to half a second, no further improvement was observed, and a significant cost still remained at the longest RSI. Rogers and Monsell suggested this "residual switch cost" may reflect an exogenous influence of stimuli, impervious to endogenous control.

The suggestion of an exogenous influence of stimuli was sensible given Rogers and Monsell (1995) design, as stimulus display consisted of stimuli from both possible tasks. Therefore on any given trial, the stimulus from the irrelevant task might activate the irrelevant task-set (Allport, Styles, & Hsieh, 1994; Allport & Wylie, 2000; Koch & Allport, 2006; Waszak, Hommel, & Allport, 2003, 2004, 2005; Waszak, in press) much like interference from Stroop stimuli (MacLeod, 1991; Stroop, 1935) activates the task-set of word reading despite its inappropriateness. Stimulus-induced interference between task-sets may be increased as the tasks in Rogers and Monsell (1995) share response-keys, and thus a decision on the correct stimulus-response (SR) mapping might be part of any putative reconfiguration process (e.g. if number is odd, press left key). If stimuli activate their relevant SR mapping, then irrelevant stimuli in the bivalent display will increase interference during selection of the appropriate SR mapping. Indeed, Rogers and Monsell (1995) suggested that only part of reconfiguration can occur in advance; the remainder is only completed upon presentation of the stimuli. Although this

hypothesis serves to explain the experimental findings, one might ask what functionality this delayed process might provide the cognitive system (Altmann, 2003b).

Task-set inertia

A similar account of the residual switch cost was provided by Allport et al. (1994). Rather than appealing to a homunculus reconfiguration metaphor, Allport and colleagues (see also Allport & Wylie, 2000) explained switch costs as arising from familiar memory based processes such as priming and interference (a path continued by Altmann, 2002; Altmann & Gray, 2002; Altmann, 2003a, 2004a, 2004b; Altmann & Gray, 2008). Specifically, Allport and colleagues suggest that when a task switches, the activation of the now-irrelevant task persists and hinders activation of the relevant task. Implementation of the relevant task thus involves its activation, and the suppression of the activation levels of the irrelevant task (Arbuthnott & Frank, 2000; Mayr & Keele, 2000). Thus, the switch cost arises as a by-product of positive priming of the irrelevant task, and negative priming (or inhibition) of the relevant task, rather than a specific switching mechanism.

To examine this proposal, Allport et al. (1994) presented participants with incongruent Stroop stimuli (e.g. the word "Yellow" written in blue ink), and participants had to name either the word or the colour of the ink. In terms of switching between these tasks, the reconfiguration hypothesis suggests that switching to word reading (e.g. Colour—*Word*) should be fast, likely as word reading is a well practiced task and configuration of this task-set should be straightforward. Conversely, switching to colour naming should be slower, as it is a less-well practiced task. Despite overall RTs being slower for colour naming (the typical Stroop effect), switch costs were much larger for word naming than for colour naming, a "reverse-stroop effect" (Allport et al., 1994; Allport & Wylie, 2000). Allport and colleagues explained this effect by suggesting that in order to perform the more difficult colour naming task, the easier task of word naming would interfere, and thus must be negatively primed (inhibited). At the same

time, the more difficult task of colour naming must be activated. When a switch occurs from colour naming to word naming, the positive priming (activation) of the colour task persists, as does the negative priming (inhibition) of the word task. These combined conditions of greater activation of the irrelevant task and inhibition of the relevant task make switching to the easier task more difficult. Conversely, switching from word naming to colour naming (an easy task switching to a more difficult task) would produce less interference, as there would be less negative priming of the difficult task and less positive priming of the easy task. This effect has been replicated in a number of studies (Arbuthnott, 2008a; Meuter & Allport, 1999; Monsell, Yeung, & Azuma, 2000; Yeung & Monsell, 2003, but see Schneider & Anderson, in press, for an alternative explanation), and is a challenge to the reconfiguration metaphor (Rogers & Monsell, 1995), as this theory posits no carry-over of previous task-activation once a switching operation has been triggered.

Allport et al. (1994) called this persistence of task activation "task-set inertia" (TSI). To explain reduction of switch costs at longer RSI intervals, TSI posits that at extended intervals, the irrelevant (to-be switched away from) tasks activation levels have time to dissipate somewhat. At shorter intervals, the previous task is still highly active, and the relevant task is still negatively primed, thus making switching more difficult. TSI explains residual switch costs as the positive priming of the irrelevant task and the negative priming of the relevant task persisting over a long period (Allport & Wylie, 2000; Wylie & Allport, 2000).

The TSI hypotheses does not automatically assume that cognitive control is not required in the task-switching procedure (Logan, 2003), as it is likely that proactive interference from irrelevant tasks is reduced by inhibitory control (Mayr & Keele, 2000; Mayr, 2007). It does however argue that the switch cost is not a valid measure of cognitive control operations being executed (Meiran, 1996; Rogers & Monsell, 1995). The cognitive system faces the same problem on switch trials *and* repetition trials of ensuring that the relevant task is the most active

among competing representations (Altmann & Gray, 2008).

Explicit-cuing paradigm

There exists a certain degree of conflict between the two theories in deciding whether the switch cost reflects control processes. The best evidence for cognitive control during task switching is the reduction of switch cost when there exists an opportunity for advanced preparation. However, the TSI explains the reduction of switch cost at prolonged preparation intervals in a more elementary fashion. The alternating runs procedure is unable to distinguish between these two hypotheses. A solution to this impasse was provided by Meiran (1996), who introduced the explicitly-cued task switching paradigm (see also Sudevan & Taylor, 1987). Within this paradigm trials are presented randomly; participants know which task to perform on a given trial as a valid pre-cue is provided. For example, Meiran (1996) presented participants with a 2 x 2 grid in which a smiley face symbol would appear within one of the four quadrants. Participants had to decide whether the symbol was in the upper- or lower-half of the grid, or whether it was on the left or the right side. Cues used were a pair of arrows, either pointing up and down (cuing the upper/lower judgement) or left and right (see figure 2).

Trials were organised post-hoc into repetition and switch trials by comparing the cue used on trial $n-1$ to that on n . The elegance of this paradigm lies in its ability to separate preparation and proactive interference interpretations of the switch cost. Specifically, preparation time can now be manipulated independently of the effects of proactive interference by varying the temporal distance between the onset of the cue and the onset of the stimulus (the cue-stimulus interval, or CSI), whilst keeping the RSI constant (which is still defined in the same manner as in the alternating runs procedure, i.e. the time between the response on one trial and the stimulus for the next trial). The constant RSI ensured that any modulation of switch cost due to CSI was due to preparation processes only, as any proactive interference from trial $n-1$ to n would be equivalent in all cases. The

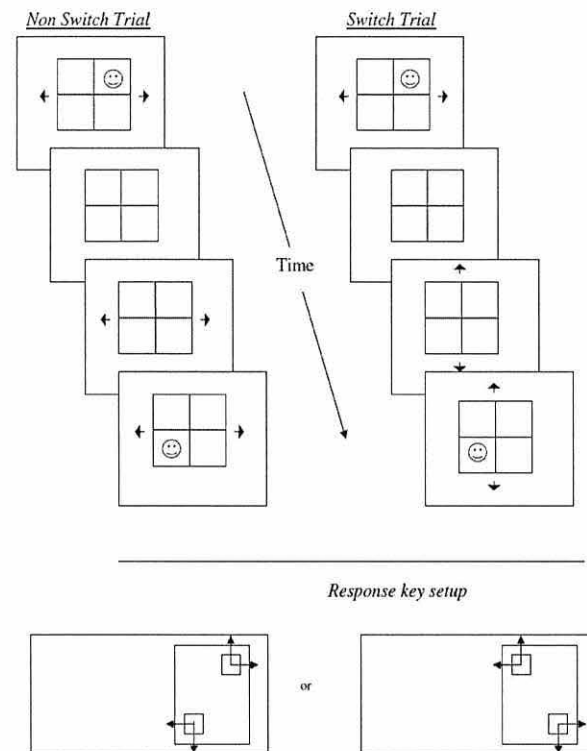


Figure 2. Example of the manipulation of cue-target interval from "Reconfiguration of processing mode prior to task performance" by N. Meiran, 1996, *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 22, 1423-1442.

CSI was manipulated independently of the RSI in Meiran (1996, ; Experiments 2-3) by placing the cue for the current trial close to the response on trial $n-1$ (and hence further away from the stimulus on the current trial, allowing for greater preparation) or further from the response on $n-1$ (closer to the stimulus on current trial, not allowing much preparation time). Meiran predicted that if the reduction of switch cost found by Rogers and Monsell (1995) was due to active preparation processes, switch costs should be reduced at prolonged CSIs. However, if TSI was the primary explanation of switch costs (Allport et al., 1994), switch costs should be equivalent between CSI conditions (as the remoteness of n from $n-1$ is equivalent in all conditions).

Meiran (1996) found switch costs in this paradigm, demonstrating such costs were not unique to the alternating runs procedure. Additionally, Meiran reported that the switch costs were significantly reduced at extended CSIs, consistent with Rogers and Monsell's (1995) account. However, despite very long CSIs (up to

1,908ms in Experiment 5), a significant residual switch cost remained. This suggests that some part of the switch cost may be due to proactive interference from the preceding trial (Allport et al., 1994). This possibility was investigated by Meiran, Chorev, and Sapir (2000), who controlled for preparation intervals whilst varying the degree of interference from trial $n-1$ (Experiments 1 & 2). This was achieved by varying the temporal distance between the response on trial $n-1$, and the cue for trial n . This response-cue interval (RCI) allows passive decay of the previously executed task, as no specific preparation can be performed during this interval due to the randomness of task presentation. Meiran et al. (2000) manipulated the RCI between 132-3,032ms with a constant CSI of 117ms, and found that switch cost reduced at longer RCIs, consistent with the idea that some proportion of the switch cost is caused by non-preparation processes such as TSI. With this empirical separation of CSI and RCI, we can see that in the alternating runs procedure, the RSI is an inseparable mixture of both CSI and RCI (Meiran et al., 2000).

To unequivocally examine the role of preparation processes in reducing the switch cost, Meiran et al. (2000, Experiment 3) varied the CSI whilst using a constant, but long, RCI. As the switch cost was drastically reduced when RCI was extended up to 500ms, and only a smaller drop in cost was found for RCIs from 500ms up to 3,000ms, Meiran et al. (2000) suggested that proactive interference from the preceding trial has dissipated to an acceptable level with an RCI of 1,000ms. Therefore, to investigate preparatory processes independent of proactive interference, Meiran et al. used an RCI of 1,017ms. Results showed the predicted reduction of switch cost at longer CSIs. Despite this, a residual cost of 40ms was still evident with a CSI of 3,000ms.

Based on these findings Meiran et al. (2000) suggested that the switch cost consisted of three independent components: the passive dissipation of previously executed tasks (and possibly the dissipation of suppression of the relevant task), a preparatory component that readies the system for changing task demands, and a

residual component.

For the remainder of the introduction, and this thesis, we will focus on the preparatory component of task performance, as this element seems the most related to the study of cognitive control. In the explicitly cued task switching paradigm, preparation is initiated by the cue, and so relevant theories of cue encoding will be discussed. Preparatory processes are also closely related to the other two components. Specifically, proactive interference from the preceding task may be overcome by the preparation of the current task, possibly by employing inhibitory mechanisms to irrelevant (but active) representations (Grange & Houghton, in pressb, 2009; Houghton, Pritchard, & Grange, 2009; Mayr & Keele, 2000; Mayr & Kliegl, 2000; Mayr, 2007). Additionally, the residual component has been explained in terms of failure to employ preparatory processes (DeJong, 2000; Nieuwenhuis & Monsell, 2002; Verbruggen, Liefoghe, Vandierendonck, & Demanet, 2007).

Cue-based preparation

Cues that guide behaviour are not unique to the task switching paradigm; indeed, we encounter cues frequently in our daily lives, which must be successfully translated into relevant actions. For example, when driving, we are presented with many cues in the guise of road signs. Sometimes these cues are explicit (such as a number, which represents the maximum speed limit), and sometimes the cues are more abstract, and rely on a pre-learned association with an action (e.g. a red circle with a horizontal white rectangle placed within it means "No-Entry"). Cues are incredibly important when there are multiple actions available. For example, when approaching a traffic light, you can decide to carry on driving or stop. However, the most appropriate action is cued by the colour of the light. Likewise, in the task switching paradigm with random task sequences, it is impossible to decipher the relevant task with bivalent stimuli unless provided with a cue. This is the critical distinction between cues and primes, which are often used in attentional research (see for example inhibition of return, Klein, 2000). Primes are

useful for performance, but not necessary, whereas cues are necessary for performance (Schneider & Logan, 2009; Sudevan & Taylor, 1987). How one translates cues into relevant actions, especially if the cue triggers a change of what you are currently engaged in (e.g. a phone ringing whilst you are writing your PhD), is paramount to our understanding of cognitive control.

That task cues aid performance even when they are not necessary for performance was demonstrated by Koch (2003). Koch combined the alternating runs procedure with the explicit cuing paradigm by having participants alternate between two tasks in a predictable sequence (e.g. AABBA...). However, unlike Rogers and Monsell (1995) paradigm where position of stimuli served as a cue for which task to perform, stimuli in Koch's study were presented centrally. One group had to recall the sequence from memory, whereas another group received a task cue in addition to the predictable sequence. This allowed investigation of the difference between cue-based performance and performance from memory recall alone. Results showed that at extended preparation intervals (here manipulated by RSI), the switch cost was reduced to a greater extent in the group with the task cue compared to the no-cue group. These results show that purely memory based reconfiguration is much weaker than cue-based preparation (see also Koch, 2001).

However there still remained a residual switch cost. Indeed, the presence of such costs in the explicitly-cued task switching paradigm suggest that the reduction of switch cost given extended preparation intervals is not due to cognitive control processes (Allport et al., 1994), or at least that advanced reconfiguration is in some way limited (Rogers & Monsell, 1995; Meiran, 1996). DeJong (2000) proposed that perhaps advanced reconfiguration was not limited (see also DeJong, Berendsen, & Cools, 1999), but rather participants did not fully prepare themselves on all trials. This *Failure to Engage* (FTE) theory posits that task preparation is an all-or-none process, and that participants have the capacity to be fully prepared for a task switch. By this notion, participants should perform equivalently on task-switch trials and task-repetition trials, especially given

sufficient preparation time.

The transient failure to fully prepare (a situation DeJong and colleagues also call *Goal Neglect* DeJong et al., 1999) is suggested to be driven by one (or a combination of) three factors: i) a lack of goal-driven intention (i.e. lack of motivation), ii) reduced environmental support (i.e. no task cue or insufficient feedback on task performance), and iii) fatigue. In the cued task-switching paradigm, environmental support is relatively strong, so DeJong (2000) suggested that in this scenario, FTE emerged from a combination of a lack of motivation and fatigue.

To demonstrate his hypothesis, DeJong (2000) suggested that analysing the whole RT distribution would highlight the dynamics of prepared and unprepared trials. Specifically, at long preparation intervals, DeJong suggested that RTs consisted of a mixture of fully prepared trials and fully un-prepared trials. By this logic, fully prepared task-switch RTs at long preparation intervals should be as fast as task-repetition trials (in which full preparation is assumed to be present), and that fully un-prepared task-switch RTs at long intervals should be just as slow as task-switch RTs from very short preparation intervals (where preparation is assumed to be zero). To analyse this mixture model, DeJong constructed cumulative distribution functions (CDFs) for task-switch and task-repetition trials at long and short preparation intervals. CDFs are constructed by rank ordering individual participants raw RTs for all conditions. Then for each condition and each participant separately, quantile cut-off points are calculated at various degrees of separation (e.g. 10th percentile, 20th percentile, 30th...etc.). Once these are calculated, quantiles for each condition are averaged across participants (Ratcliff, 1979). This procedure provides a clear picture of the dynamics of all RTs across the whole distribution (i.e. from fastest RTs to slowest), and comparisons between conditions across these distributions can be made.

The hypothesis of DeJong (2000) was confirmed with the CDFs. RTs for task-repetition trials were faster than task-switch trials at short preparation

intervals across the entire RT distribution. However, RTs for switch-trials given longer preparation time were closer to repetition RTs at the faster end of the RT distribution, and closer to switch-trials given no preparation time at the slower end of the RTs. This suggests that when participants were fully prepared, performance was as good as task-repetition trials (where full preparation is likely), and that residual switch costs likely are a product of the tail-end of the RT distribution, which reflects a proportion of trials where participants are fully un-prepared.

However, Nieuwenhuis and Monsell (2002) directly attempted to reduce the proportion of FTE trials by adding motivational incentives to participants. Nieuwenhuis and Monsell provided financial incentives for improvement of RTs and error rates throughout the experiment. Additionally, block length was kept very short in order to avoid fatigue. Despite this additional incentive to engage in preparation (and despite improved RTs), residual switch costs were still evident (although statistically smaller than in conditions with no incentive). Based on these findings, Nieuwenhuis and Monsell concluded that although FTE may explain some portion of the residual switch cost, the residual does reflect a limitation of advanced reconfiguration. In a similar vein, Lien, Ruthruff, Remington, and Johnston (2005) proposed that residual costs, instead of reflecting full preparation some of the time (FTE), they reflect the preparation of some of the task all of the time. By this explanation, residual costs emerge as a steady-state (but partial) preparation process.

Verbruggen et al. (2007) however provided an answer to the impasse. They noted that in the cuing paradigm, the cue is often retained on the screen, co-present with the imperative stimulus. In this instance, participants need not engage in advanced preparation as it is not essential to perform the task; indeed they can just wait for stimulus onset to engage in any putative preparatory process. To investigate this issue, Verbruggen and colleagues compared conditions in which the cue at longer preparation intervals either remained on the screen or disappeared after a very short presentation. The results of this manipulation were

clear: when the cue was presented for a short period during long preparation intervals, the residual switch cost disappeared. This finding is consistent with the FTE theory (DeJong, 2000), and inconsistent with the idea that task preparation is limited (Nieuwenhuis & Monsell, 2002), and that participants prepare some of the task all of the time (Lien et al., 2005). However, these latter cases may still be viable when the cue remains on the screen as it is likely that the strategy participants adopt between experiments differs.

It remains an open question what work the cognitive system is doing when presented with a task cue (Altmann, 2004a). One natural assumption, given the above reviewed literature, is that task cues allow implementation of a relevant task-set. Logan and Gordon (2001) described, in their theory of ECTVA², a task-set as a collection of parameters required to perform one task over all possibilities; such parameters include attention-setting and attentional bias to relevant aspects of stimulus display. These parameters are explained to be part of the cognitive control system, and that these parameters feed-forward and set lower-level parameters that allow execution of subordinate (i.e. single) tasks. When a task set changes, new parameters are fed into the system which allows selection of a new task. Switch costs from this perspective arise as more task-set parameters change on switch trials than on repetition trials. Advanced preparation aids performance as many of these parameters can change ahead of stimulus presentation.

The idea that switch costs reflect to a certain degree the time required to update task-set parameters was supported by Arrington, Altmann, and Carr (2003), who suggested that switching between similar tasks would require less parameter alterations than switching between tasks that are very different from one another. According to Logan and Gordon (2001), the fewer parameters that require updating, the less time required for the system to switch tasks. To test this, Arrington and colleagues had participants switch between four tasks, each one being a judgement on a presented rectangle: height, width, hue, and brightness.

²Executive Control of the Theory of Visual Attention

Arrington and colleagues argued that height and width tasks are very similar, as they both require a spatial judgement attentional setting (Logan & Gordon, 2001); similarly, hue and brightness may be considered to share the attentional setting of "colour". By this logic, Arrington and colleagues found greater switch costs for switching between tasks of limited similarity (e.g. Width - Brightness) than for switching between tasks that share task-set components (e.g. Hue - Brightness).

Mayr and Kliegl (2000) suggested that task preparation processes involve retrieval of relevant task rules from long-term memory (LTM), and their installation into working memory (WM). Mayr and Kliegl suggested that it was un-parsimonious to assume that switching between two tasks requires holding both tasks ready for selection in WM. This would require the selected task to be activated, and at the same time be co-present with an activated competitor task in WM (albeit at an activation level below selection threshold). Such a scenario could leave the system prone to selection errors, requiring an extra mechanism to overcome the interference between tasks. Mayr and Kliegl (2000) proposed that selection of a task and its activation in WM are concurrent processes, and that a non-selected task is not in WM. Evidence for this suggestion is also provided by the backward inhibition paradigm (Mayr & Keele, 2000), which shows that selection of a task requires the inhibition of WM contents. By holding only one task active in WM would also resolve any potential interference that the system may experience due to multiple tasks in WM activated below selection threshold.

Based on this proposal, to select a task requires its retrieval from LTM. To test this hypothesis, Mayr and Kliegl (2000) manipulated the difficulty of a tasks retrieval from LTM and seeing whether it interacted with switch costs. Mayr and Kliegl proposed that if some (or all) of the switch cost reflected a time consuming process of LTM retrieval of task rules, switch costs should be greater for tasks with more involved LTM processing. Difficulty of LTM retrieval was manipulated by comparing two conditions: the semantic condition required judging a presented word on its size (i.e. bigger or smaller than a football) or whether it was living or

non-living; the episodic condition required recalling a recently learned association during a learning phase between the word and a) its position on the computer screen, and b) the colour of the font. During the experimental phase all words were presented centrally and in white font. It was proposed that episodic retrieval from LTM should be harder than semantic retrieval, as the former is recently learned.

Mayr and Kliegl (2000) found the predicted pattern of larger switch costs when switching to an episodic task (e.g. *size-position*) than switching to a semantic task. This pattern was true even when $n-1$ was also an episodic task (e.g. *colour-position* compared to *size-living*). This effect was determined to be due to LTM retrieval difficulty rather than overall task difficulty (Experiment 2), and that this retrieval could be accomplished with extended preparation intervals (i.e. the CSI reduced the switch cost for episodic retrieval; Experiment 3). The precise nature of the task representation that is retrieved via the cue is unclear, but must clearly involve some specification of the desired stimulus-response mapping (or "rule"). For instance, Mayr and Kliegl (2003) simply state that "task rules" are retrieved. Jost, Mayr, and Rösler (2008) state that retrieval results in a "relatively abstract description of what has to be done with the next stimulus" (p.75). Mayr and Kliegl (2000, Experiment 3) suggested that the rules are specific stimulus-response mappings (e.g., if stimulus is small animal then press left, if large animal then press right) rather than more abstract task specifications (e.g., respond according to stimulus size).

This model of task-rule retrieval for performance in the explicitly-cued paradigm is similar to that offered by Rubinstein, Meyer, and Evans (2001). However, Rubinstein and colleagues suggested that rule activation occurs after stimulus identification (c.f. Mayr & Kliegl, 2000), and that cue-based preparation involves goal-setting (or goal-switching if the cue indicates a switch). Mayr and Kliegl (2000, see also Mayr & Kliegl, 2003) alternatively suggest that stimulus onset triggers the application of the retrieved rules to the stimulus. This second stage of the model offers an alternative explanation for why residual switch costs

are still evident at prolonged preparation intervals in the explicit cuing paradigm, as complete reconfiguration must await stimulus onset (however, there is some evidence that this process can begin earlier Jost et al., 2008; Monsell & Mizon, 2006). No clear explanation has been provided for why the cognitive system benefits from awaiting stimulus onset to complete reconfiguration. One suggestion provided by Altmann (2003b) is that waiting for stimulus onset may allow the system to "hedge its bets" (p.603) and await evidence from the stimulus that a new task is required. However, this seems rather uneconomic in task-switching situations where the cue is 100% valid, and therefore no uncertainty should be present. More likely, residual switch costs within Mayr and Kliegl's model might reflect sporadic retrieval failures from LTM due to insufficient preparation (DeJong, 2000). As this is a more parsimonious assumption, the second stage of Mayr and Kliegl's model might be called into question.

Altmann & Gray's model

Altmann's approach to explaining task-switching effects is rooted in well defined and established memory processes that are integral to performance in other situations outside of task-switching, (e.g. activation & interference; Altmann, 2002, 2003a, 2004a, 2004b; Altmann & Gray, 2002, 2008), without the need for reconfiguration mechanisms during switch trials. For example, (Altmann, 2004a) suggested that the same processes run on repetition as on switch trials, processes that ensure the desired task is the most active. The switch cost from this perspective is seen as repetition priming, rather than a cost emerging from a dedicated switching mechanism (c.f. Rogers & Monsell, 1995). There are several pieces of evidence that support this view, the two most prominent being that firstly CSI affects repetition trials as well as switch trials (Altmann, 2004a; 2004b; Koch, 2001), and secondly when a cue is followed by a run of a number of stimuli upon which the cued task is to be performed, there is a reaction time cost on the cued trial (trial 1) compared to other cue-less trials in the run (e.g. trials 2-6) even if the cue indicated a repeat of task (Allport & Wylie, 2000; Altmann, 2002;

2006; 2007; Altmann & Gray, 2002; Gopher, Armony, & Greenspan, 2000; Poljac, Koch, & Bekkering, 2008). This "restart cost" has been attributed by Altmann to the time taken to re-activate task representations, which will have decayed since the last cue exposure. The restart cost is important theoretically, as it suggests that encoding and activation processes run on repetition trials as well as switch trials, a view not compatible with the reconfiguration view of a dedicated set of processes that run on switch trials only.

Altmann and Gray (2008) utilised the cognitive architecture of ACT-R (J. R. Anderson, 2007; J. R. Anderson et al., 2004) to model task-switching performance utilising these activation-based memory processes. Altmann and Gray suggest that cue encoding results in a retrieval of a task code from episodic memory, from which the meaning of the cue is retrieved. After this, the stimulus is encoded, and its meaning is retrieved. Once this encoding is complete, the meaning of the cue and the meaning of the stimulus are used to retrieve the correct response. Thus, cue encoding is the first stage of a general encoding episode, and if the meaning of the cue is different from that most recently attended to (i.e. if it is a switch trial), the now relevant task cue meaning must be activated over and above the old cue meaning to achieve selection. The extra time taken to achieve this required activation is reflected in the task-switch cost. From the examples given by Altmann and Gray, it appears that the cue meaning retrieved during encoding is used to probe semantic memory to retrieve specific S-R mappings (i.e. if Even then left, if Odd then right, Altmann & Gray, p.608).

Problems with the cuing paradigm

A critical problem with the explicitly cued task-switching paradigm was identified by two labs concurrently. Both Logan and Bundesen (2003) and Mayr and Kliegl (2003) reported that using one cue per task confounds cue-switching with task-switching. Specifically, they noted that every time a task repeats, so too does the cue; conversely, a task-switch always requires a switch of cue. Therefore the possibility exists that switch costs within this paradigm reflect cue-related

processing rather than cognitive control processes. To overcome this problem, both labs introduced the two-cue per task paradigm (hereafter a 2:1 mapping), where each task is cued by one of two cues. This new manipulation allows three types of sequence, two of which are familiar: *Cue-Repetition* (both cue and task repeat; e.g. Magnitude - *Magnitude*), *Task-Switch* (both cue and task switches; e.g. (Parity - *Magnitude*), and the new sequence possibility of *Cue-Switch* (task repeats but cue switches; e.g. High/Low - *Magnitude*). This paradigm allows separation of cue-related processes to the switch cost. Specifically, task-switch costs are measured within this paradigm by subtracting cue-switch RT from task-switch RT, as in both cases the cue has switched from $n-1$ to n , and therefore the effects of cue-switching are controlled. Additionally, the time taken to encode a new cue without the additional burden of switching tasks can be measured by subtracting cue-repetition RT from cue-switch RT.

Both Logan and Bundesen (2003) and Mayr and Kliegl (2003) found substantial costs of switching cues in the absence of a task switch. These *cue-switch costs* suggest that a significant component of the switch cost with one cue per task is due to the extra processes required to encode a new cue. Mayr and Kliegl (2003) additionally found a task-switch cost over and above that explained by cue-switching. They took this as evidence for supporting their two stage model of task-switching (Mayr & Kliegl, 2000). Specifically, they suggested that the cue-switch cost reflected utilisation of a new retrieval route to obtain task rules from LTM. When the cue repeats, the retrieval path is primed and thus speeds responses. Mayr and Kliegl (2003) found that the cue-switch cost was reduced given extended preparation intervals, consistent with their earlier work (Mayr & Kliegl, 2000) suggesting that cue-based preparation involves retrieval of task rules from LTM. The task-switch cost in their paradigm was insensitive to preparation, consistent with the second stage of their model (application of retrieved rules to the stimulus display) which must await stimulus onset (but see Verbruggen et al., 2007).

However, Logan and Bundesen (2003) found no difference between cue-switching and task-switching. By this formulation, they questioned the presence of cognitive control processes in the explicitly-cued task-switching paradigm, and suggested that the switch cost with one cue per task reflected priming of cue-encoding processes on task repetition trials. They developed their theory (and competing "reconfiguration" theories) into explicit mathematical models, which I detail below. To anticipate their findings, they found that the model with no cognitive control built into its assumptions fit the data better than reconfiguration models.

Logan and colleagues' models

The models initially presented in Logan and Bundesen (2003) have been a serious challenge to the notion that cognitive control processes can be measured by the cued task-switching paradigm. Instead, they suggested that participants adopt a "compound cue" strategy whereby the cue and the stimulus are combined to retrieve the correct response from LTM. For example, the cue "Odd/Even" and the stimulus "7" uniquely retrieve the response "Odd" from LTM. By this notion, no switch of task is required, as on every trial participants encode the cue, encode the stimulus, and use them together to retrieve the response. Switch costs from this perspective are seen as repeated cue-encoding benefits when the cue switches (regardless of "task"). To explain the preparation effects found in the literature, which had been taken to reflect cognitive control operations, Logan and Bundesen suggested that longer cuing intervals mean cue encoding on cue-switch trials can be completed before stimulus onset.

Model 1

Logan and Bundesen (2003) initially modeled the reconfiguration metaphor of Rogers and Monsell (1995) and Meiran (1996), which assumes an endogenous act of control. In this model, the cue is encoded, which takes μ_c ms to complete. If the cue that is encoded is identical to the cue on the previous trial, no further

executive control is required, as the correct task-set is assumed to be already implemented. If however the cue differs, as in a task-switch scenario, the cognitive system must retrieve the correct task-set and install it, which Logan and Bundesen state takes μ_s ms to complete. Based on these assumptions, and given no preparation time (i.e. $CSI = 0$ ms), reaction time for a repetition trial is formalised as:

$$RT = RT_{Base} + \mu_c \quad (1)$$

and reaction time for a task-switch is:

$$RT = RT_{Base} + \mu_c + \mu_s \quad (2)$$

In both cases, RT_{Base} is the estimated time to encode the stimulus and respond. The authors assume, in this model and the following, that cue encoding time is exponentially distributed, so the probability that cue encoding is complete before stimulus onset increases as a function of the CSI. To model the effects of extended preparation intervals on performance, model 1 now estimates RT for task repetition trials as:

$$RT = RT_{Base} + \mu_c \cdot \exp\left(\frac{-CSI}{\mu_c}\right) \quad (3)$$

and RTs for task-switches as:

$$RT = RT_{Base} + \exp\left(\frac{-CSI}{\mu_c}\right) \cdot (\mu_c + \mu_s) + \frac{\frac{1}{\mu_c} - \frac{1}{\mu_s}}{\frac{1}{\mu_c} - \frac{1}{\mu_s}} \cdot \left[\exp\left(\frac{-CSI}{\mu_s}\right) - \exp\left(\frac{-CSI}{\mu_c}\right) \right] \cdot \mu_s \quad (4)$$

Note that model 1, and indeed the reconfiguration metaphor as it originally stood (Rogers & Monsell, 1995; Meiran, 1996, 2000; Meiran et al., 2000), makes no allowance for cue-switch RT to be any different from task-repetition RT, as in

both cases no reconfiguration (μ_s) is required. Qualitatively, we can already see that model 1 will provide a poor fit to new data from the 2:1 mapping paradigm due to this.

Model 2

Model 2 is the "compound-cue model", and it assumes no cognitive control process (μ_s). Rather it suggests that task-switching performance can be explained by differential cue-encoding times. The authors explain this approach in terms of short-term- (STM) and long-term-memory comparisons between the presented cue and the desired compound stimulus (a stored amalgamation of cue-stimulus-response compounds). They describe this comparison as a race, and whichever process finishes first determines performance. In the case of a cue-repetition, the comparison between the presented cue and STM traces finishes before the comparison between the presented cue and LTM (as the cue from the previous trial is still active in STM), thus producing faster encoding time (see Schneider & Logan, 2005, 2009, for a more detailed overview of this process). In the case of a task (cue) switch, the current cue does not match representations in STM, and so has to rely on retrieving from LTM, a longer process. Cue-encoding time for cue-repetitions (μ_r) can be expressed as:

$$\mu_r = \frac{1}{V_{STM} + V_{LTM}} \quad (5)$$

and cue-encoding time for cue-switches (μ_a) as:

$$\mu_a = \frac{1}{V_{LTM}} \quad (6)$$

where V_{STM} and V_{LTM} are parameters estimating comparison rates to short-term and long-term memory respectively. As a result of these expressions, $\mu_r < \mu_a$, formalising the unique prediction that task repetitions are faster due to a *benefit* from cue encoding and comparisons to STM traces rather than a *cost* of switching tasks. This benefit should appear at smaller CSI's, as cue encoding will not have

had time to complete before the stimulus is presented.

Based on these assumptions, model 2 makes no distinction between cue-switch trials and task-switch trials, as in both cases the cue on trial n has switched from $n-1$. Therefore, estimated RT for cue-repetitions are formalised as:

$$RT = RT_{Base} + \mu_r \cdot \exp\left(\frac{-CSI}{\mu_r}\right) \quad (7)$$

and RT for cue-switch and task-switch trials as:

$$RT = RT_{Base} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (8)$$

Model 2+1

The final model expressed by Logan and Bundesen attempts to integrate the above two models. It assumes repeated cue-encoding benefits and an act of reconfiguration. RT for cue-repetitions is the same as before:

$$RT = RT_{Base} + \mu_r \cdot \exp\left(\frac{-CSI}{\mu_r}\right) \quad (9)$$

For cue-switches, cue-encoding does not benefit from repetition priming, and thus must be encoded anew (as per model 2):

$$RT = RT_{Base} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (10)$$

For task-switches, again cue-encoding does not benefit from repetition priming, but additionally it requires reconfiguration processes. This is expressed formally:

$$RT = RT_{Base} + \exp\left(\frac{-CSI}{\mu_a}\right) \cdot (\mu_a + \mu_c) + \frac{\frac{1}{\mu_a}}{\frac{1}{\mu_a} - \frac{1}{\mu_s}} \cdot \left[\exp\left(\frac{-CSI}{\mu_s}\right) - \exp\left(\frac{-CSI}{\mu_a}\right) \right] \cdot \mu_s \quad (11)$$

Across their experiments, Logan and Bundesen (2003) consistently found that

model 2 fit their 2:1 mapping data better than either of the competing models. This is hardly surprising, as they found no difference between cue-switching and task-switching. This pattern of results (and thus the model fits) was inconsistent with the data of Mayr and Kliegl (2003). To address these differences, Logan (2004) investigated procedural differences between the two labs. Logan and Bundesen (2004) suggested that the difference in cue-types used between the two sets of reports could explain the differences found. For instance, whilst Logan and Bundesen (2003) used cues that have a pre-experimental association with the tasks they were associated with (e.g. Odd/Even), Mayr and Kliegl (2003) used abstract cues with no pre-experimental association with the tasks (e.g. the letter "G" signalled a colour discrimination).

Pre-experimental associations between cues and their respective tasks has been called *cue-transparency* in the literature (Logan & Schneider, 2006a), and has been found to affect switch costs. For example, Arbuthnott and Woodward (2002) found that non-transparent cues (cues that have little pre-experimental association with the task) produce greater switch costs than transparent cues (which have strong pre-experimental association with the task). Logan and Schneider (2006a) suggested that when a cue is relatively non-transparent, a verbal mediator is retrieved which aids performance. For example, when presented with the cue "G", the participant will retrieve the mediator "Colour", which can then be used like a transparent cue to perform the task. To explain the results of Mayr and Kliegl (2003), Logan and Bundesen (2004) suggested that participants retrieve such a mediator. Once retrieved, this mediator is then employed much like a transparent cue, i.e. it is used jointly with the stimulus to act as a compound retrieval cue to select the correct response from LTM.

The notion of mediator retrieval can explain why Mayr and Kliegl (2003) found significant task-switch costs without appealing to cognitive control (or application of stimulus-response rule). On cue-repetition trials, the non-transparent cue requires retrieval of a mediator, which is used with the

stimulus to select a unique response. On cue-switch trials, the cue is different, so has to be encoded anew, but the mediator it retrieves is primed from the previous trial, as participants are assumed to use the same mediator for both cues. On a task-switch trial, neither the cue nor the mediator is primed from recency, and both must be encoded anew. Therefore, the priming of mediator retrieval explains why RTs are faster to cue-switch trials than task-switch trials for Mayr and Kliegl (2003). Conversely, Logan and Bundesen (2003) found no difference between cue-switch and task-switch RT as they used transparent cues; therefore, no mediator is retrieved, leading to no benefits of priming for cue-switch trials compared to task-switch trials.

Model 3

Logan and Bundesen (2004) formalised their thesis by comparing performance of their model (model 2) with that of a new model based on the two-stage process of Mayr and Kliegl (2003). This model assumes a benefit of repeated cue-encoding (explaining the cue-switch cost), but unlike model 2, assumes an extra set of processes that run when the stimulus is presented (their rule-application stage). In the models of Logan and Bundesen (2003), stimulus-based processes are estimated using the parameter RT_{Base} . Thus, a shift in this parameter was used to conceptualise the second stage of Mayr and Kliegl's model. RT for cue-repetitions is expressed formally as:

$$RT = RT_{BaseRep} + \mu_r \cdot \exp\left(\frac{-CSI}{\mu_r}\right) \quad (12)$$

and for cue-switches as:

$$RT = RT_{BaseRep} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (13)$$

both identical to model 2. However, for task-switches, as a new rule is being applied to the target display, a shift occurs for estimation of RT_{Base} . RT for task-switches is expressed as:

$$RT = RT_{BaseAlt} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (14)$$

All models were fit to a conceptual replication of Mayr and Kliegl (2003), and results showed that model 2 again was the better fit to the data. Based on these results, Logan and Bundesen (2004) concluded that no cognitive control processes were captured in the task-switch cost (see also Arrington & Logan, 2004b; Logan & Schneider, 2006b; Logan, Schneider, & Bundesen, 2007; Schneider & Logan, 2005, 2006, 2009, for further vindications of this model). Despite the bleak outlook for the task-switching paradigm in measuring cognitive control processes, there is evidence that suggests not all data can be explained by Logan and colleagues' models.

Challenges to Logan's models

A series of direct challenges to Logan's modeling was presented by Monsell and Mizon (2006). By manipulating the probability of a task switch, Monsell and Mizon found under certain circumstances a significant task-switch cost was present, which reduced with increased preparation. The other critical difference between the studies of Logan and Bundesen (2003) and Mayr and Kliegl (2003) was the probabilities of a task-switch. Given no constraints, selection between two tasks produces a $p(\text{task-switch})=.5$, which is what Logan and Bundesen used; however, Mayr and Kliegl used a $p(\text{task-switch})=.3$, so that cue-repetitions, cue-switches, and task-switches were equally likely on each trial. Monsell and Mizon argued that with a high probability of a task-switch, participants may engage in some form of switching reconfiguration before cue-onset (even if they have no foreknowledge of the upcoming trial). By preparing for a switch before cue-onset, participants may be fully ready for the switch task before the cue signals such a switch, and thus no switch cost in RT will be evident. Keeping the $p(\text{task-switch})$ low discourages this reconfiguration until the cue actually signals a switch is required, ensuring switch related processes are captured within the RT.

However, Logan et al. (2007) countered this proposal theoretically and empirically. They highlighted that manipulating the probability of a task-switch also manipulates the probability of one cue following another. Specifically, if $p(\text{task-switch})$ is low, then the probability of a cue for task B will follow a cue for task A is also lower. Logan and colleagues thus suggested that the "true" task-switch cost found by Monsell and Mizon (2006) might actually reflect infrequency effects, whereby participants respond slower to infrequent stimuli. Logan and colleagues also found that a slightly modified version of their model fit Monsell and Mizon's data without the assumption of cognitive control processes. Thus, based on this evidence, Logan's models still hold quite strong explanatory power.

However, empirical evidence has been forthcoming that suggests limitations in Logan's models. Altmann (2006) presented an experiment to examine whether the task-switching procedure produces behavioural effects not explained by Logan's models. We have already reviewed several effects found in the task-switching literature that can not be explained sufficiently by the model (e.g. residual switch costs, within-run slowing, full-run error switch cost), but Altmann was particularly interested in two main questions. Firstly, if cue encoding explains switch cost then no switch cost should be evident on cue-less trials within extended runs designs (e.g. Altmann, 2002). Altmann (2006) tested this by adding an extra trial following an instructive cue, within a 2:1 mapping paradigm. Results showed that task-switch costs were evident on trial 2 of the run, but cue-switch costs were zero. If task-switching was merely the cost of switching cues, then no task-switch cost should be evident on trial 2, as cue-encoding must have been completed on trial 1. Thus, this data suggests the switch cost is not entirely due to the effects of switching cues. Although the model of Altmann and Gray (2008) does not explicitly address paradigms with two cues per task, the cue-switch cost can be explained by the ACT-R architecture by repetition priming of perceptual identification of the task cue on cue-repetition trials (J. R. Anderson,

2007; J. R. Anderson et al., 2004).

Additionally, Altmann (2006) noted that reduction of switch cost at extended preparation intervals is not as pervasive as the literature might suggest (see e.g. Monsell, 2003; Logan, 2003). In earlier work, Altmann had shown that reduction in switch cost at extended preparation intervals was particular to a within-subjects manipulation of CSI (Altmann, 2004a, 2004b, see also Koch, 2001). When CSI is manipulated between-subjects, the CSI by Switch cost interaction was null.

Altmann (Altmann, 2004a, 2004b) has suggested that the system needs to be exposed to varying preparation intervals to appreciate the benefits of advanced preparation. The modeling of Logan and colleagues says that the reduction of switch cost given preparation is due to a pervasive cue-encoding process. However, this stance cannot explain the null interaction given a between-subjects design.

Arrington, Logan, and Schneider (2007) sought to separate cue-encoding processes from task-switching empirically in order to investigate whether task-switching in isolation produces switch costs. To achieve this, Arrington and colleagues had participants make separate responses to the cue and the stimulus. The logic of the design assumes that cue and stimulus processing are serial. If the type of response made to the cue is completed after successful cue-encoding, then all cue-switch costs should only appear in cue-RT, with no cue-switch costs in stimulus RT. If however the type of response to the cue is not a result of complete cue encoding, then cue processing will spill over into RT to targets, resulting in cue-switch costs to both cues and targets. The additional appeal of this design is that one can assess the final representation gained from cue encoding by comparing cue responses that resulted in successful separation to cue responses that were not successful.

Across experiments, two cues were used for each task, and the type of response required for the cue varied between experiments, either indicating which cue was presented (i.e. a separate response for each cue, resulting in a 1:1 mapping of cues to responses) or which task was presented (one response for each

task, resulting in a 2:1 cue-response mapping). The results showed that with a 1:1 cue-response mapping, cue switch effects were still apparent in stimulus RT, suggesting cue encoding had not been separated from target processing. However, a successful separation did occur when a 2:1 response-cue mapping was utilised, with all cue-switch costs observable in cue RT only; stimulus RT only showed task-switch costs. The presence of these task-switch costs after cue encoding is complete suggests that task-switch costs cannot be explained in their entirety by cue-switching (c.f. Logan, 2003; Schneider & Logan, 2005). Additionally, these results suggest that cue encoding results in a semantic representation of the task to be performed, and not a representation of the cue itself.

Additional evidence for the dissociation of cue-switching from task-switching came from Jost et al. (2008). These authors investigate event-related potentials (ERPs) of cue-switching and task-switching performance, and found distinct neural responses to cue-switching and task-switching. Cue-switching affected negative ERP components about 300ms after cue-onset, with task-switching affecting negativity potentials around 400ms. Both of these responses had distinct topography, suggesting that they were emerging from distinct underlying neural responses.

Inhibitory mechanisms in task switching

One established task-switching phenomenon that cannot be adequately explained by Logan's models (nor by Altmann & Gray's, 2008, model) is the evidence for inhibitory mechanisms being employed during switching. Inhibition in task-switching implies that some task-specific component is being altered, and this cannot be explained by a model that assumes no cognitive control is in operation. Thus, inhibition might be the best evidence yet for supporting the notion task-switching requires cognitive control processes.

Backward inhibition

The concept of behavioural inhibition in cognitive psychology has been a controversial topic for many years (see Gorfein & MacLeod, 2007; MacLeod, Dodd, Sheard, Wilson, & Bibi, 2003; Nigg, 2000; Tipper, 2001, for recent discussions on the arguments for and against inhibition). The concept that task-switching requires inhibition has been popular since the studies of Allport and colleagues (Allport et al., 1994; Allport & Wylie, 2000; Wylie & Allport, 2000), who argued that switch costs to some degree reflect persisting inhibition of the switched-to task. But definitive evidence for inhibition was absent until the study of Mayr and Keele (2000). In this study, Mayr and Keele highlighted that it is impossible to investigate inhibition in task-switching using only two tasks. By introducing a third task, Mayr and Keele contrasted two switching sequences. In one sequence, participants performed three different tasks in succession (e.g. a CBA sequence, where A, B, & C are arbitrary labels for tasks); in the other sequence, participants were required to return to a recently performed task after one intermediate trial (e.g. ABA sequence). The tasks were simple perceptual discrimination tasks, where participants were presented with a screen with one rectangle in each quadrant of the screen. The participants task was to respond to the location of a odd-item out rectangle whose perceptual properties differed from the other rectangles based on a cued dimension. For example, the cue "Colour" required participants to respond to the rectangle whose colour was different to the others (e.g. a purple rectangle among blue rectangles). The other two relevant dimensions were orientation (one rectangle was tilted to the left or to the right) and movement (one rectangle was moving from left-to-right or up and down). Responses were to be made to which quadrant the odd rectangle occupied by making a spatially compatible key press.

They suggested that if inhibition is applied during task-switching, then inhibition of a recently performed task should persist and hinder its reactivation if the task is to be performed relatively soon again (for example task A in an ABA

sequence). During a CBA sequence, task A was inhibited longer ago, and thus has more time to overcome the inhibition applied to it. By this logic, Mayr and Keele (2000) predicted slower RTs to ABA sequences than to CBA sequences. Note that if task-switching merely requires activation of the relevant task (e.g. Altmann & Gray, 2008), ABA sequences should be faster than CBA sequences due to positive priming of task A. Indeed repetition priming is a fundamental psychological construct, and is observed in many different fields of research. Despite this, Mayr and Keele did find slower RTs to ABA than CBA sequences, thus supporting their notion of inhibition. They called this inhibitory mechanism "Backward Inhibition" (BI). For the remainder of this thesis, I shall use the more theoretically neutral term " $n-2$ repetition cost" to refer to the reaction time deficit in ABA sequences, retaining the term BI to refer to the mechanism postulated to be behind the cost (Koch, Gade, Schuch, & Philipp, in press; MacLeod et al., 2003).

Mayr and Keele (2000) proposed that BI is an inhibitory mechanism that is deployed proactively to remove the no-longer relevant task-set. In support of this, in their Experiment 3 they contrasted two situations: a "bottom-up" condition versus a "top-down" condition. In both, the stimulus display consisted of one deviant object among three identical distractors (plain rectangles), so that the relevant target could be gleaned from stimulus display alone. However, in the top-down condition, participants were presented with a valid cue that signalled the relevant dimension, whereas in the bottom-up condition, a line of asterisk (*****) were used instead of a cue. Mayr and Keele posited that if BI is a top-down mechanism, then $n-2$ repetition costs should only be observed in the top-down condition, as this is the only condition that allows for advanced task preparation. However, if $n-2$ repetition costs reflect a more reactive form of inhibition that is triggered during stimulus onset, costs should be equivalent between the two conditions. Despite this, Mayr and Keele only found $n-2$ repetition costs in the top-down condition, and suggested that preparation for a specific task triggers backward inhibition of the previously executed task.

$N-2$ repetition costs have been replicated in a number of studies using various different task demands (Altmann, 2007; Arbuthnott, 2005, 2008b, 2009; Arbuthnott & Frank, 2000; Arbuthnott & Woodward, 2002; Bao, Li, Chen, & Zhang, 2006; Dreher & Berman, 2002; Gade & Koch, 2005, 2007b, 2008; Grange & Houghton, 2009, in pressb; Houghton et al., 2009; Koch, Gade, & Philipp, 2004; Koch, Philipp, & Gade, 2006; Kuhns, Lien, & Ruthruff, 2007; Lien & Ruthruff, 2008; Masson, Bubb, Woodward, & Chan, 2003; Mayr, 2002, 2009, 2006; Philipp & Koch, 2005, 2006; Philipp, Gade, & Koch, 2007; Philipp, Jolicoeur, Falkenstein, & Koch, 2007; Schneider, 2007; Schneider & Verbruggen, 2008; Schuch & Koch, 2003; Sdoia & Ferlazzo, 2008; Sinai, Goffaux, & Phillips, 2007), and are a promising empirical marker for inhibitory mechanisms as they are (to-date) immune from non-inhibitory accounts (Koch et al., in press; Mayr, 2002, 2007). Additionally, $n-2$ repetition costs increase when inter-trial conflict is increased by reducing the interval between successive trials, as measured by the RCI (Gade & Koch, 2005; Grange & Houghton, 2009). Reduction of the RCI means that when a new task is switched-to, the previous task is still very active and therefore requires greater inhibitory control to reduce its activation allowing selection of the relevant task; a greater RCI allows passive decay of the previous tasks activation levels (Allport et al., 1994), reducing conflict when the new task is selected.

Response-related inhibition

However, it remains unclear what is exactly inhibited during task-switching. Mayr and Keele (2000) suggested that it is the task-set as a whole that is inhibited. As task-sets comprise of many components (Schneider & Logan, 2007), a more fine-grained approach might suggest that inhibition targets those aspects of the task-set that generates the greatest inter-trial conflict, as this is where conflict resolution mechanisms are most needed (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Grange & Houghton, in pressb, 2009; Houghton et al., 2009; Houghton & Tipper, 1996). Evidence for such a view comes from the work of Koch and colleagues, who have provided much evidence that inhibition might

target response processes of the task-set (Gade & Koch, 2007b; Koch et al., in press; Philipp & Koch, 2005, 2006; Schuch & Koch, 2003).

Philipp and Koch (2005) demonstrated that $n-2$ repetition costs are to some degree generated by response processes. They had participants perform a magnitude judgement on number stimuli, but were cued to use one of three response modalities: their hands (i.e. typical finger key press), their feet (using left/right foot pedals), or a vocal response. In this instance, ABA and CBA sequences were constructed not based on task sequencing, but response modality sequences (e.g. foot-vocal-foot is an ABA sequence). Philipp and Koch found $n-2$ modality-repetition costs, suggesting that when response modalities switch, the previously irrelevant modality is inhibited.

Schuch and Koch (2003) demonstrated the importance of response processes in generating $n-2$ repetition costs by combining the backward inhibition paradigm with a go/no-go manipulation. In an otherwise normal cued task-switching experiment, a go or a no-go signal was presented shortly after the presentation of the task cue. If presented with a go signal, participants had to respond to the stimulus in a regular fashion. However, on the small proportion of trials in which a no-go signal was presented, participants had to withhold their response to the stimulus (see Verbruggen, Liefoghe, Szmalec, & Vandierendonck, 2005; Verbruggen, Liefoghe, & Vandierendonck, 2006, for a related design). In all cases, participants could prepare for the relevant task, but the relevant stimulus-response (SR) rule was only selected and executed on go trials. Therefore, if $n-2$ repetition costs reflect an inhibitory mechanism targeting response selection/execution aspects of a trial structure, then $n-2$ repetition costs should be absent from an ABA sequence where the task for $n-1$ was a no-go trial. In this instance, the SR rule for task A should not be inhibited as no SR selection occurred for the intermediate trial. Schuch and Koch (2003) found significant $n-2$ repetition costs, but only when trial $n-1$ was a go trial (i.e. response selection & execution occurred). When a no-go trial was present on $n-1$, $n-2$ repetition costs were

absent. These results (see also Verbruggen et al., 2005, 2006) strongly support the hypothesis that BI targets response related processes in task-switching³.

However, the study of Schuch and Koch (2003) left open the question whether it was the absence of response selection or response execution that led to the reduction of $n-2$ repetition costs. To disentangle these processes, Philipp, Jolicoeur, et al. (2007) used a "go-signal" paradigm in conjunction with a typical BI design. In this paradigm, participants are presented with a cue, followed by the stimulus, but uniquely, participants must not respond to the stimulus until a go signal is presented (a high tone). Similar to Schuch and Koch (2003), a small proportion of trials presented a no-go signal (a low tone). The critical difference between this manipulation and that of Schuch and Koch (2003) is that on some trials the go/no-go signal is presented up to a 1,500ms after stimulus onset (unlike Schuch & Koch, where the signal was presented simultaneously with stimulus onset). In cases where the go/no-go signal delay (GSD) is long, participants are able to select a response because the stimulus is presented long before execution is required. However, when the GSD signals a no-go response, no response execution occurs (c.f. Schuch & Koch).

Using these manipulations, Philipp, Jolicoeur, et al. (2007) predicted to replicate the findings of Schuch and Koch (2003), with no $n-2$ repetition costs when there was a no-go signal on $n-1$ in an ABA sequence. The unique prediction was that if inhibition affects response selection processes, then in an ABA sequence, if trial n involves a long go-signal delay, then no $n-2$ repetition costs should be present as the inhibition can be overcome during the GSD when response selection can occur. However, if inhibition affects response execution processes, then a long GSD should not allow overcoming residual inhibition of task A, and $n-2$ repetition costs should remain. The results clearly showed however that with a long GSD indicating a go response on trial n , no $n-2$ repetition costs

³However, on a more theoretical note, one might ask that if no inhibition of task A occurred in an ABA sequence when $n-1$ was a no-go trial, why is there not a significant $n-2$ repetition *prime*, as presumably task A retains its activation levels somewhat and should prime performance when task A is required again. Thus, in the absence of a cost, it remains an open question why there is not a benefit (see e.g., Jonides & Mack, 1983).

were evident (indeed a significant $n-2$ repetition prime was evident). The results therefore support the suggestion that $n-2$ repetition costs reflect persisting inhibition of response selection processes, which can be overcome given enough time to select an appropriate response.

Response selection is likely to induce conflict (and hence, inhibition), as typically multiple tasks are mapped onto the same response sets. For example, a left key press in an experiment can be associated with an "Odd" or "Lower than 5" judgement, depending on the currently relevant task. Indeed, such overlapping response sets have been shown to contribute to $n-2$ repetition costs. For example, Gade and Koch (2007b) had participants switch between four tasks, three of which had overlapping response sets (vocal responses "left" or "right"). The fourth task however did not overlap with the response set for the other three tasks, and required a vocal response of "up" or "down". The overlapping response-set tasks were referred to as "trivalent" (T) tasks, and the single non-overlapping response-set task as a "univalent" (U) task. ABA sequences were constructed to contrast TTT transitions with TUT transitions. During TTT transitions, response-set conflict should occur at $n-1$, triggering inhibition of task A, generating $n-2$ repetition costs. Conversely, a TUT sequence should generate no conflict at $n-1$ as the U task does not conflict with the response set for T tasks, thus no $n-2$ repetition costs should be present. The predicted pattern of results was confirmed, suggesting overlapping response sets contribute to BI (see also Schneider & Verbruggen, 2008, for a related finding). However, it should be noted that this finding stands in contradiction to the observation of significant $n-2$ repetition costs in many of Arbuthnott's studies (Arbuthnott, 2005, 2008b, 2009; Arbuthnott & Frank, 2000; Arbuthnott & Woodward, 2002) despite the fact her tasks all use univalent response-sets.

Inhibition at earlier stages of trial processing

Despite the considerable evidence that inhibition is triggered by response-related processes of the trial structure, there is some evidence that it is

not exclusively tied to these processes. Indeed, response competition should not have played a significant role in generating $n-2$ repetition costs in the original BI study of Mayr and Keele (2000), as response sets were not unique to each task. Participants merely had to respond to the location of the deviant rectangle on each trial, and thus the response-set is constant throughout the whole experiment. Therefore earlier components of the trial structure could be a source of inhibition.

For example, Sdoia and Ferlazzo (2008) have provided some evidence that inhibition can be triggered during stimulus presentation, and critically at a distinct time before response selection (Philipp, Gade, & Koch, 2007). They found $n-2$ repetition costs even when the task for $n-1$ required stimulus encoding for later comparison, but critically no response selection was required.

Stimulus encoding (Sdoia & Ferlazzo, 2008) and response selection (Philipp, Gade, & Koch, 2007) occurs quite late in the typical trial structure, and it is somewhat surprising that conflict during earlier, preparatory processes stages of task performance does not trigger inhibition. This is especially surprising, as some models of task-switching performance suggest that task cues initiate retrieval of task rules from LTM and installation into WM (Mayr & Kliegl, 2003), a process that might benefit from inhibition of the previous contents of WM (Mayr & Keele, 2000; Mayr & Kliegl, 2000). Despite the dominance of a response-locus of BI, there is some evidence that it can be triggered earlier.

Hübner, Dreisbach, Haider, and Kluwe (2003) developed a variant of the BI paradigm with the goal of investigating positive effects of backward inhibition (i.e. reduced interference from the previous task during current task performance) rather than on negative side-effects of inhibition (i.e. $n-2$ repetition costs). They used a version of the Eriksen flanker paradigm (Eriksen & Eriksen, 1974), where the unique stimulus sets were used for each task, and the stimulus for the current task was flanked by either stimuli from the previous task or stimuli from a task not recently completed. The flanker effect refers to slowed RTs when the flanking stimuli are from a different task to the central, relevant stimulus. However, if the

flanking stimuli on trial n are from the task performed on $n-1$, then they should induce less interference because of backward inhibition. Flanking stimuli from a task performed less recently should interfere more as they are less inhibited. As such a finding would suggest stimulus-based inhibition (Sdoia & Ferlazzo, 2008), Hübner et al. (2003) presented participants with pre-cues. In their Experiment 2, the task cue either informed participants of which task was going to be relevant on the next trial (i.e. a task-specific cue), of the cue merely signalled a switch would occur, with no specific information as to which task would be relevant. In the latter case, participants can not prepare for an upcoming task, unlike with task-specific cues.

Hübner et al. (2003) found less interference from flanking stimuli, demonstrating for the first time some of the positive effects of BI. Importantly, they only found reduced flanker interference when participants were presented with a task-specific cue. This suggests that when preparing for a specific task, inhibition of the previous task can occur, unlike when a mere "Switch" cue appears. So although the reduced interference occurred at the stimulus level, this could only be achieved with task-specific preparation. This finding is in agreement with the top-down/bottom-up distinction of Mayr and Keele (2000, Experiment 3), finding $n-2$ repetition costs only when a task was prepared endogenously.

Cue-target translation & inhibition

Recent work in our laboratory has been concerned with the role of inhibition at earlier stages of the trial structure. We have suggested that inhibition should be employed where there is the greatest need to reduce inter-trial conflict (Botvinick et al., 2001), and it is possible that other parts of the trial-structure may also be found to play a role, if the locus of conflict is shifted onto them (Houghton et al., 2009). As the cue signals a change of task-set, we have suggested that there must be some degree of conflict present during cue encoding. Additionally, Houghton et al. (2009) suggested that the manner in which a task is cued should modulate $n-2$ repetition costs, if the nature of the cue affects the

degree of conflict with recent task performance. Thus, if a task-set is installed into WM after cue presentation (Mayr & Kliegl, 2000, 2003), and the mechanism underlying $n-2$ repetition costs serves to clear WM of components from recently performed tasks (Bao et al., 2007; Mayr & Keele, 2000), then the previous task must undergo some form of inhibition during preparatory stages.

The way in which a task is cued has been shown to influence $n-2$ repetition costs (Arbuthnott, 2005; Arbuthnott & Woodward, 2002). The authors contrasted spatial with verbal cues when performing judgment tasks on number stimuli. The spatial cues consisted of a row of asterisks presented at any one of the vertices of a triangle, with each of the three positions cuing a different task. The verbal cues described the task to be performed (e.g. "Odd/Even"), and were presented centrally. While the verbal cues produced robust $n-2$ repetition costs, the spatial cues did not. Arbuthnott attributed lack of BI with the spatial cues to increased discrimination between the three tasks category-response rules, and also proposed that in the spatial cueing condition, competing task-sets remain active to some degree during task performance (Arbuthnott, 2005, Experiment 2). However, in the spatially cued condition, the stimuli appeared at the cued location and hence changed position from trial to trial (unlike in the verbally cued condition). Thus one must be wary of attributing the attenuation of BI to differences in the cues alone (but see Arbuthnott, 2008b, 2009).

However, Arbuthnott's (2005) study does present a challenge to the suggestion that BI is exclusively generated by the use of overlapping response sets (Gade & Koch, 2007). In particular, Experiment 1 of Arbuthnott (2005) utilized univalent response sets and trivalent stimuli (i.e. a single digit that affords all three possible tasks), a condition which should not generate $n-2$ repetition costs according to Gade and Koch (2007b). This may be construed as evidence that components of the task-set other than response processes are targeted by inhibitory mechanisms when the locus of conflict is placed upon them.

To further investigate the role of task cues in generating $n-2$ repetition costs,

Houghton et al. (2009) used a paradigm very similar to that of Mayr and Keele (2000). Participants were required to respond to the location of a relevant target. Targets were four ovals presented with one centralised to each quadrant of the screen. Ovals differed on visual properties: one was angled, one had a thick border, one was shaded in, and one was neutral. Responses were spatially compatible to the location of the correct oval on the screen (four keys, top-left [D], top-right [J], bottom-left [C], & bottom-right [N]). Participants knew which oval was relevant on a given trial due to a valid pre-cue. In one condition, the cues were verbal, describing which oval to search for (e.g. "Border", "Angled", and "Shaded"). In this instance, Houghton et al. (2009) suggested that participants needed to translate the cue into an active WM representation of the target to search for (a process they called "cue-target translation"). When a cue signals a switch (e.g. "Border"-*"Shaded"*), participants must engage in a new episode of cue-target translation. Instantiation of the new target representation in WM should generate conflict due to the still-active representation from the previous trial. Thus, inhibition is required to overcome this conflict, leading to $n-2$ repetition costs in an ABA sequence (see figure 3 for an example trial sequence).

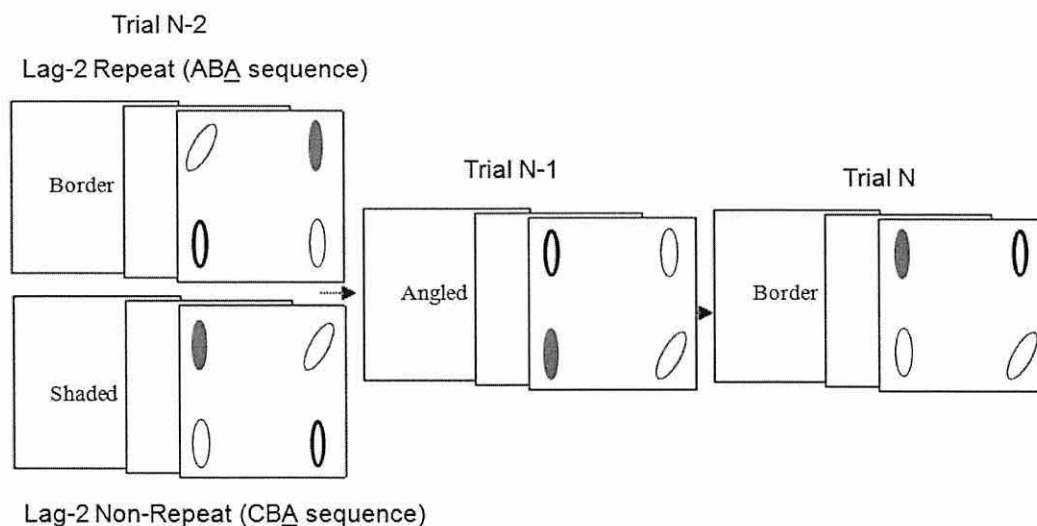


Figure 3. Example of ABA and CBA sequences with word cues from Houghton et al., (2009; Experiment 1).

However, Houghton et al. (2009) suggested that if the cue eased the burden

on WM during cue-target translation, then less conflict should be present during switching as less work is being done by WM. Easing cue-target translation was accomplished by using so-called iconic cues, which were rectangles that provided a relevant sample of the to-be-attended oval. For example, to cue the target with a thick border, the cue was a rectangle with a thick border (see figure 4. In this instance, cue-target translation is less involved than the word cues (which provided no sample of the relevant target). Houghton and colleagues predicted that this scenario would produce no $n-2$ repetition costs, as less conflict is present in WM during cue-target translation when the relevant target switches.

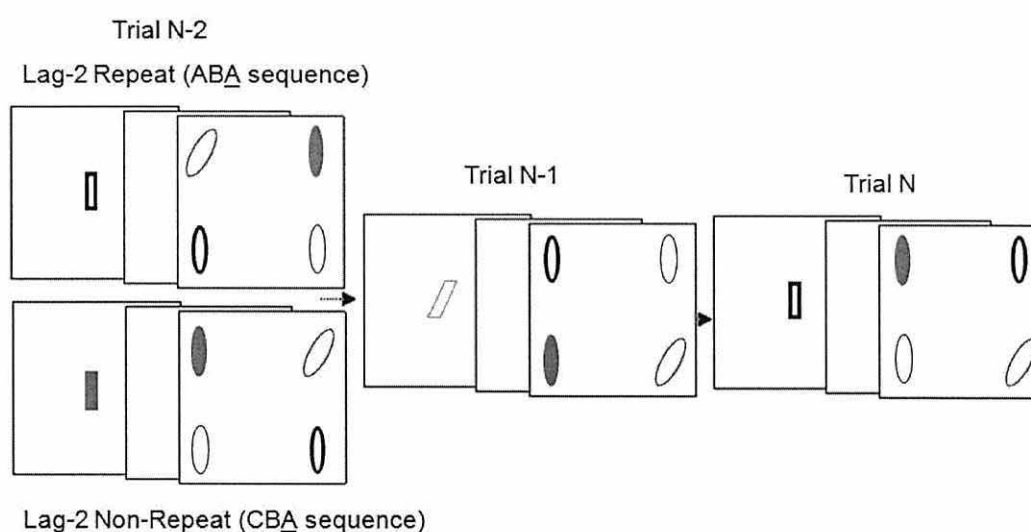


Figure 4. Example of ABA and CBA sequences with iconic cues from Houghton et al., (2009; Experiment 1).

Houghton et al. (2009) found the predicted pattern, with significant $n-2$ repetition costs for word cues, but none for iconic cues. Importantly, this effect cannot be explained due to primary task-difficulty (see their Experiment 2). In their Experiment 3, Houghton and colleagues used arbitrary iconic cue-target relationships (for example a triangle cue indicated to search for the angled oval). In this instance, cue-target translation is even more difficult than for the word cues, and thus should induce greater inter-trial conflict when a new cue-target translation process is required. Indeed, the experiment showed greater $n-2$ repetition costs for arbitrary cue-target relationships than for word cues, further

supporting the cue-target translation hypothesis of BI.

This difference in inhibition due to cue-transparency (Arbuthnott & Woodward, 2002; Grange & Houghton, in pressa; Logan & Schneider, 2006a) is important theoretically, as for all cue-types, stimulus display and response-processes are identical. Additionally, responses were required on every trial, which requires response selection and execution (c.f. Schuch & Koch, 2003). Thus the difference in $n-2$ repetition costs between cue-types can only be explained by cue-based preparatory processes (c.f. Gade & Koch, 2007b; Koch et al., in press; Philipp & Koch, 2005, 2006; Schuch & Koch, 2003; Sdoia & Ferlazzo, 2008). Indeed, from trial to trial, all that changes is the relevant cue-target relationship. By holding all aspects of the trial structure constant in this manner, apart from the component of interest (cue-processing), is a powerful tool in determining the role of that component in switching performance (see also Cooper & Mari-Beffa, 2008; Schneider & Logan, 2007).

It must be noted that the finding of Houghton et al. (2009) does not automatically assume that cue-translation processes are subject to inhibition in other BI paradigms. This is especially true for paradigms where the greatest source of inter-trial conflict is response selection (i.e. when tasks share response-sets; Gade & Koch, 2007b; Philipp, Gade, & Koch, 2007; Schuch & Koch, 2003). In this instance, cue-transparency might have little effect on $n-2$ repetition costs, as reducing interference during cue-processing may have little effect on the interference still inherent during response selection (although this needs to be empirically demonstrated). As response processes are the major source of inter-trial conflict in these studies, it is unsurprising that manipulation of response difficulty effects BI in these studies (see e.g. Koch et al., 2004; Philipp et al., 2007; Schuch & Koch, 2003).

Based on these findings, we suggest that backward inhibition is an active cognitive control mechanism that serves to suppress those aspects of the trial structure that generates the greatest inter-trial conflict (Grange & Houghton,

2009, in pressb; Houghton et al., 2009).

Goals of the PhD

The reviewed literature suggests that all is not bleak when considering using the explicitly-cued task-switching paradigm to measure cognitive control processes. The present PhD aims to elucidate in more detail what these processes are and how they affect sequential switching behaviour. Specifically, this thesis is concerned with the WM representations (i.e. "task-sets") that participants activate in order to perform a given task, and how new representations are implemented when a switch is required. The formation of a WM representation is likely to differ when cue-transparency is manipulated, as more informative cues provide more of the relevant representation to guide behaviour (Grange & Houghton, 2009, in pressb, in pressa; Houghton et al., 2009). Therefore, cue-transparency appears to be an effective way of manipulating the degree of cognitive control required for task performance.

As the primary interest of this PhD is the effect of cue-transparency on switching performance, all other aspects of the task-set will remain constant by using the paradigm of (or slight modifications of) Houghton et al. (2009). This paradigm is able to constrain response processes and stimulus display between all manipulations. All that changes on a trial-to-trial basis is the relevant cue-target pairing.

The experimental chapters of this PhD are either published articles (chapters 2, 4, and 5), or are manuscripts that are due to be submitted (chapters 3 and 6). As such, there is inevitably some overlap between the methods and introductions, as they are all addressing similar questions within the task-switching literature. At the end of each experimental chapter, there is a summary that serves to link each chapter to the next. Sometimes these summary sections will include additional analysis that was not included in submitted papers, but mainly serve to provide a link between chapters, which might not be immediately obvious. These sections are deliberately more conversational, trying to emphasise my line of

reasoning for how the research programme developed.

Overview of chapters

This thesis is generally in two parts. For the first part, I focus on the effects of cue-target translation on backward inhibition. This work extends the report of Houghton et al. (2009) considerably. In the first experimental chapter (Chapter 2), I definitively show that $n-2$ repetition costs in the Houghton et al. paradigm is due to conflict at the cue-processing stage of task performance by utilising a negative transfer paradigm, where cue-target pairings are re-assigned halfway through the experiment. This manipulation modulates $n-2$ repetition costs whilst maintaining identical cue-target sets, something that was not possible in Houghton et al.

Chapter 3 provides supporting evidence that inhibition targets the activated WM representation of which target to attend to, rather than inhibition of the cue itself. This is achieved by using a 2:1 mapping in conjunction with a BI paradigm (see also Altmann, 2007; Gade & Koch, 2008; Mayr & Kliegl, 2003). Experiment 5 within this chapter also provides some evidence that inhibition may be occurring in the iconic-cue condition of Houghton et al. (2009), but that using a transparent cue in some way bypasses this suppressed representation.

Chapter 4 is a response to a recent article from Druey and Hübner (2007) who suggest that in order to observe $n-2$ repetition costs, the cue and the target for the task must remain on the screen together (i.e. cue-target overlap). They found that when the cue disappeared during the CSI, no $n-2$ repetition costs were found. This is important to investigate as in all of our BI paradigms (this thesis and Houghton et al., 2009) we remove the cue during the CSI. Thus, lack of observable $n-2$ repetition costs for transparent iconic cues might have been caused by no temporal cue-target overlap, rather than differential burdens on WM, as we suggested (Houghton et al., 2009). To anticipate these results, no effect of cue-target overlap was found.

For the second part of the thesis, I adapt the Houghton et al. (2009) paradigm to investigate the role of cue-transparency in a typical task-switching

design (i.e. switch vs. repeat). In backward inhibition studies, the task switches on every trial to ensure sufficient critical trials (ABA vs. CBA), so the effects of cue-transparency on the switch cost could not be observed. In Chapter 5, I separate cue-encoding processes from task-switching by using a 2:1 mapping between cues and targets. Using this manipulation, I was able to provide evidence that the cue-switch cost (cue-switch RT minus cue-repetition RT) emerges from active control processes of forming an active WM representation of which target to attend to, rather than passive priming of the cue itself (as suggested in the models of e.g. Logan & Bundesen, 2003; Schneider & Logan, 2005). Additionally, I provide a behavioural dissociation between cue-switch costs and task-switch costs, further suggesting that task-switching does not reduce to cue-switching (c.f. Logan & Bundesen, 2003). This chapter also briefly discusses results of model fitting (using Logan's models) to the experimental data, and suggests that this model is not complete.

For Chapter 6, I use an extended runs design with an instructional cue being followed by four target displays, on each of which the cued target must be located. This manipulation allows separation of cue-related processes (on trial 1) and target-related processes (trials 2-4). Specifically, I was interested in whether cue-transparency affects target processing, or whether the transparency effects are only localised to the cued first trial. This also allowed investigation of cue-transparency on restart costs, which are assumed to reflect cue-encoding processes running. Despite this assumption making its way into explicit models of task-switching (Altmann & Gray, 2008), it has never been tested directly.

Chapter 7 ties all of the experimental chapters together, linking it back to the literature reviewed in this first chapter. I discuss what the thesis has contributed to our understanding of cognitive control processes in task-switching, and more importantly, what questions remain unanswered. I strongly advocate the use of explicit computational modeling of the assumptions that we have built in to our explanation of cue-target translation effects on backward inhibition, constraining

the processes that we assume to be running, and approaching these processes with a more fine-grained analysis. I end with ideas for future research, including investigations of individual differences in $n-2$ repetition costs, and RT distribution analysis, stimulated by some post-hoc analysis of Experiment 1.

Chapter 2

Backward Inhibition & Negative Transfer⁴

⁴This chapter has been published as Grange and Houghton (in press), and is reproduced with permission of the copyright holder. Thanks to Ulrich Mayr, Darryl Schneider, Paul Verhaeghen, and an anonymous reviewer for their constructive comments on a previous version of this article.

CHAPTER 2: BACKWARD INHIBITION & NEGATIVE TRANSFER

Abstract

$N-2$ repetition costs are a performance cost caused by returning to a task after one (vs. more than one) intervening trial (ABA vs. CBA sequences), and may reflect the inhibition of task-set components during switching. In three experiments we support the theory that inhibition can target cue-based preparatory stages of a task. Participants performed a cued target-localization task previously shown to produce $n-2$ repetition costs.

In Experiment 1, re-assignment of arbitrary cue-target pairings midway through the experiment doubled the size of $n-2$ repetition costs, though cue, target and response sets remained unchanged. Experiment 2 controlled for effects of order of conditions or simple change of cue meaning. Experiment 3 demonstrated that the effect depends on re-pairing members of the same cue and target sets.

The results are attributed to heightened conflict (and hence greater inhibition) during cue-target translation when a previously learned cue-target mapping is remapped.

Literature Review & Introduction

The growing research on task-switching aims to understand the mechanisms that enable maintenance and flexible switching of cognitive processes (Monsell, 2003). One process thought to facilitate switching between tasks is inhibition of the just-utilised task set. Evidence for such inhibition mainly comes from the Backward Inhibition (BI) paradigm (Koch et al., in press; Mayr & Keele, 2000; Mayr & Bryck, 2007). In this paradigm, participants switch between three tasks, signalled by a valid pre-cue (e.g. the word "Parity" to judge whether a number is odd or even). Participants are slower and less accurate to return to a recently performed task after one intervening trial (ABA sequence, an $n-2$ repetition) compared to returning to a less-recently performed task (CBA sequence, an $n-2$ switch). These $n-2$ repetition costs are thought to reflect persisting inhibition of a task when it is disengaged to perform another; in an ABA sequence, the inhibition of task A persists over two trials and hinders its reactivation relative to a CBA sequence in which it has had more time to recover.

The question arises as to precisely what is inhibited when a task switches. In a typical BI design (as in task-switching generally), a trial consists of a number of components: first the task cue appears, via which the participant activates a task or target set; then occurs the stimulus display to which the task set is applied to reach a decision; finally the decision is translated into a response, typically via an arbitrary (and recently learned) stimulus-response set. In principle, $n-2$ repetition costs might be observed if any (or any combination) of these processes is inhibited.

There is evidence that inhibition can target response processes when they produce inter-trial conflict, as is the case with overlapping response sets (Gade & Koch, 2007b; Schneider & Verbruggen, 2008). These occur frequently in task-switching designs, where a given response has more than one meaning, depending on the current task (e.g. "Odd", "Higher than 5"). Additionally, Schuch and Koch (2003) found no $n-2$ repetition costs when a response was withheld on trial $n-1$, suggesting that the response set from trial $n-2$ did not

require inhibition (Koch et al., 2004; Philipp, Jolicoeur, et al., 2007).

Recently Houghton et al. (2009) proposed that BI reflects a flexible control process active at the point of greatest inter-trial conflict, and that conflict should occur during preparatory, cue-based, stages of task performance, as this is when a switch of task is initiated. For instance, Mayr and Kliegl (2000) propose that the cue affords retrieval of task rules into working memory (WM), a process which would benefit from the inhibition of the current contents of WM. To test this, Houghton et al. used a design based on that of Mayr and Keele (2000) in which there is only one response set. Participants responded to the location of a target shape appearing along with three non-targets. Each of the shapes (ovals) had a unique visual property, for instance a thickened outline, or being filled in. The target changed on each trial and was signalled by a cue (see Experiment 1 of the current chapter for the design of a typical trial). However, there was only one response set (four key presses, spatially congruent with target location), removing response-set conflict as a factor. Instead, it was proposed that any inter-trial conflict would be centered on cue processing, as participants would have to translate the cue into a WM representation capable of enabling target detection (a process we have called *cue-target translation*, Houghton et al., 2009).

It was further hypothesised that a cue that facilitates cue-target translation should reduce the conflict in WM during a switch, and with it, the need for inhibition. This was tested by manipulating the degree of relatedness between the cue and target, referred to as the *cue-transparency* (Logan & Schneider, 2006a). More strictly, this may be defined as "*the degree to which the cue exogenously provides or directly stimulates the relevant WM representations required to perform the task*" (Grange & Houghton, in pressa). In Houghton et al. (2009), maximally transparent cues provided a sample of the target feature, e.g., if the target shape had a thick border, then the cue had a thick border. A relatively *less* transparent cue for the same target would be the word "Border", as it does not supply a sample of the target visual feature. Nevertheless, the word's meaning in long-term

memory (LTM) is appropriate its use as a cue, and was expected to help direct attention to the relevant visual feature (thickened border). Finally, a minimally transparent cue (e.g. a square) would have an arbitrary relationship with the target with no LTM support. Previous work had found no influence of cue-transparency on $n-2$ repetition costs (e.g. Arbuthnott, 2005), but these studies used tasks which produce response conflict (see Houghton et al., 2009, for discussion of this issue).

Houghton et al. (2009) found the predicted effects of cue-transparency on $n-2$ repetition costs: Maximally transparent cues produced no $n-2$ repetition costs, while the arbitrary cues produced the greatest cost. Verbal cues with an existing LTM association to the target property produced intermediate levels of $n-2$ repetition cost (see also Grange & Houghton, 2009). The authors took this as evidence that the processes underlying BI were not inherently linked to the control of response sets, but could be flexibly directed to that aspect of the task structure producing greatest inter-trial conflict.

The present study

The purpose of the present study was to further test the hypothesis that cue-based conflict can generate $n-2$ repetition costs. One potential problem with the Houghton et al. (2009) studies is that they contrast different sets of cues to the same set of targets, for example iconic cues vs. word cues to icon targets (Grange & Houghton, 2009). This was done to make sure that difficulty of target detection per se was controlled across cueing conditions. This leaves open the possibility that the different cue sets might contribute independently to the effect. For instance, if cue discrimination were harder for one set of cues than for another, this might lead to greater inter-trial interference in cue processing, and consequently more cue inhibition. Such an effect would be independent of the cue-target relationship. In the ideal case, difficulty of cue-to-target translation should be manipulated while holding the cue and target sets constant. This is the aim of the work reported here, in which a negative transfer paradigm (Gade &

Koch, 2007a) is used with the non-transparent icon cues used in Houghton et al. (2009, Experiment 3). In the first half of the experiment, participants become practiced with fixed, but arbitrary, cue-target pairings (e.g. a square cue indicates locate a "shaded" oval). Half-way through the experiment, new cue-target pairings are introduced, but importantly *the same cues and targets are used* (only the learned pairings are changed; for example, a square cue indicates locate an "angled" oval). Therefore the perceptual properties of the cue and target displays, and the relationships within cue and target sets, are identical between halves of the experiment. However, in the second half of the experiment, new cue-target pairings will be competing with the better practiced pairings established in the first half. This increased conflict in WM when the target switches should produce a greater need for inhibitory control, reflected in greater $n-2$ repetition costs. If this is so, the effect can only be attributed to the cue-target relationship, as nothing else has changed.

Experiment 1

Method

Participants

32 participants were recruited from Bangor University through the School of Psychology participant panel. 2 additional participants were removed due to failure to maintain a session-wise accuracy over 90%.

Apparatus & Stimuli

Stimuli were presented on a 17" monitor, connected to a PC running E-Prime experimental software (Psychology Software Tools, 2002); responses were made on a standard QWERTY keyboard. Stimulus display consisted of four ovals (6cm in height; 3 had widths of 2.3cm and one had width of 3.5cm), each differing on a visual property (one was shaded, one had a thick border, one was angled; the remaining oval was always present as a distractor and never required a response).

The current cue was a square, a triangle, or an octagon (all cues had a height and width of 4cm). Cues and stimuli were presented in grayscale shading on a white background. See Figure 5 for examples of cues and targets used.

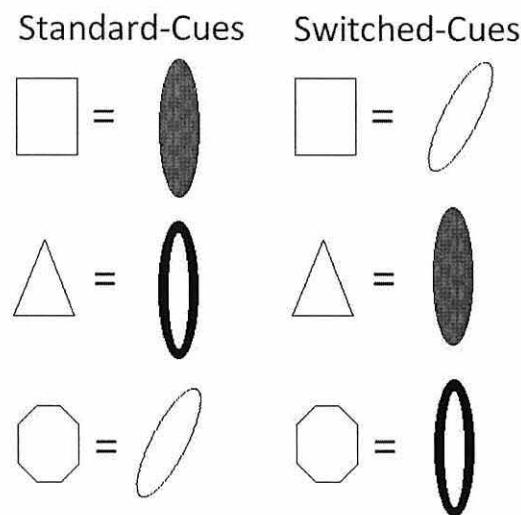


Figure 5. Cues and targets used in Experiment 1. Note, images are not to scale.

Procedure

The experiment was split into two halves. Each half consisted of 10 blocks of 42 trials, with rest screens after each block. A trial began with a single cue centred on the screen for 500ms followed by a blank screen lasting 250ms (Grange & Houghton, 2009). After this, the stimulus display appeared with one oval centred within each quadrant of the screen. There were three potential targets, and one neutral distractor on each stimulus display, and the location of the stimuli was randomised on each trial. The participant's task was to signal the location of the target oval (specified by the cue they had just seen) by making a spatially congruent key press using the index and middle fingers of both hands. Response keys were assigned as follows (target location = Key:Finger): Upper left = D:Left middle ; Lower left = C:Left index; Lower right = N:Right index; Upper right = J:Right middle. Once a response was registered, the screen went blank for a response-cue interval of 500ms, before the cue for the next trial appeared.

Cue-target pairings for the first half of the experiment were as follows: a

square cue indicated search for the "shaded" oval, a triangle cue indicated the "border" oval, and the octagon indicated the "angled" oval. In the second half of the experiment, participants had to learn new cue-target associations: square was now paired with the "angled" oval, triangle with "shaded", and octagon with "border". Cue-pairings were counterbalanced across participants (i.e. half received the square-angled, triangle-shaded, octagon-border pairings in the first half of the experiment). Across all participants, we refer to the cue-target pairings in the first half of the experiment as the standard-cues, and the pairings in the second half of the experiment as the switched-cues.

Before each half of the experiment, the participants were shown the cue-target pairings before proceeding with a brief practice session. The importance of accuracy was emphasised, and practice was terminated by the experimenter once the participant had demonstrated they understood the instructions. The practice sessions were thus quite short, and helped ensure that any effects of negative transfer were captured within the experimental trials (Houghton & Marí-Beffa, 2005).

Design

The experiment manipulated two factors in a 2 x 2 repeated measures design: *cue-pairing* (standard-cues vs. switched-cues) and *sequence* (ABA vs. CBA). The crucial measure was the interaction of the two factors, with the effect of sequence being predicted to be greater for switched- versus standard cues. The dependant variables throughout were RT (ms) and percentage error.

Results

The first two trials of each of the ten sub-blocks were removed from data analysis, as were errors and the two trials following errors (Mayr & Keele, 2000). Correct RT was further trimmed to exclude RTs 2.5 standard deviations above each participant mean. Total data trimming retained 86% of the raw data to be submitted to further analysis. Trimmed RT and percentage errors were submitted

Table 1: Mean reaction time (RT in ms), standard errors (in parentheses), and errors (%) for ABA & CBA sequences in Experiment 1, for standard- and switched-cues.

Cue-pairing	Sequence				ABA-CBA
	ABA		CBA		
	RT	Errors	RT	Errors	
Standard-cues	734 (40)	3.31	714 (41)	3.24	20
Switched-cues	775 (48)	3.81	720 (42)	4.21	55

to separate two-way repeated measures analysis of variance (ANOVA), with the factors and levels as described in Design. Mean RTs, standard errors, and errors are shown in Table 1.

Of the two factors, only *sequence* produced a significant main effect [$F(1,31) = 50.12, p < .001, \eta_p^2 = .62$] due to slower RTs to ABA trials (754ms) than to CBA trials (717ms). For *cue-pairing*, there was a non-significant numerical trend [$F(1,31) < 1$] towards slower RTs to switched-cues (748ms) than to standard-cues (724ms).

Most importantly, the *cue-pairing* by *sequence* interaction was significant [$F(1,31) = 6.39, p < .05, \eta_p^2 = .17$], reflecting greater $n-2$ repetition costs for the switched- versus the standard-cues (55ms and 20ms respectively). $N-2$ repetition costs were significant for both the standard-cues [$t(31) = 2.64, p < .05$] and the switched-cues [$t(31) = 5.38, p < .001$].

For the error analysis, there was a main effect of *cue-pairing*, with more errors in the switched-cue condition (4%) than the standard-cues condition (3.3%), $F(1,31) = 5.45, p < .05, \eta_p^2 = .15$. The main effect of *sequence*, and the interaction did not reach significance.

Is conflict cue-specific?

To test whether the increased $n-2$ repetition costs in the second half of the experiment were due to cue-specific conflict or more general conflict, we carried out the following analysis⁵. In the first half of the experiment, if target "A" is assigned with cue "1", target "B" by cue "2", and target "C" by cue "3", and these pairings are re-assigned in the second half to "A2", "B3", and "C1", there are now two types of ABA sequence possible in the second half of the experiment: *Type I* (A2 - B3 - A2) and *Type II* (A2 - C1 - A2).

Type II ABA sequences have an intermediate target (C) with a cue (1) that has a learned association with the target "A" from the first half of the experiment. Thus, this extra cue-conflict may lead to an increased measure of $n-2$ repetition costs due to extra inhibition required to overcome this specific cue-related conflict. Alternatively, Type I ABA sequences have an intermediate target (B) with a cue (3) that has a learned association with target C, and thus should not lead to extra inhibition due to no cue-specific conflict.

This analysis is also possible with CBA sequences. For example, Type II CBA sequences (e.g. C1 - B3 - A2) may lead to slower RTs at trial A2 than Type I CBA sequences (e.g. B3 - C1 - A2), as the intermediate trial for Type II sequences uses a cue that was previously associated with task A.

To analyse this possibility, the RT data from the second half of the experiment were re-coded into Type I and Type II ABA and CBA sequences, and were re-analysed in a 2 (*sequence*: ABA vs. CBA) x 2 (*trial-type*: Type I vs. Type II) repeated measures ANOVA. *Sequence* was significant [$F(1,31) = 32.13, p < .001, \eta_p^2 = .51$], but there was no main effect of *trial-type*, $F(1,31) = 2.3, p = .14, \eta_p^2 = .07$ and no interaction with *sequence*, $F(1,31) = 0.04, p > .8$. Therefore the results from Experiment 1 seem not to be due to cue-specific conflict, but rather reflect a more general increase in inter-trial conflict.

⁵We thank Darryl Schneider for suggesting this analysis

Discussion

The results confirmed our prediction that negative transfer of cue-target pairings would increase $n-2$ repetition costs. We suggest this novel finding is due to increased inhibitory control required following the change in cue-target pairings, as cue-target translation must contend with the well practiced, but irrelevant, pairings from the standard-cues condition (Gade & Koch, 2007a). With respect to previous findings (Houghton et al., 2009), these results provide unequivocal support for the idea that it is the process of cue-target translation that matters. As we were able to keep cue, target and response sets completely invariant throughout the experiment, none of these in themselves can explain the effect. The relationship between target location and response also never varied, and hence the finding provides additional support for the proposal that inhibition can target aspects of a task other than conflict arising at the response stage (c.f. Schuch & Koch, 2003).

A final result of note is that while the change in cue-target pairings did increase $n-2$ repetition costs, it did not significantly increase overall RTs. This provides a useful dissociation between increase in overall RT and increase in $n-2$ repetition costs (see also Houghton et al., 2009, Experiment 2).

The above discussion supposes that it is the re-assignment of the cue-target pairings (negative transfer) halfway through the experiment that is crucial to explaining the findings. However, the switched pairings inevitably always occurred in the second half of the experiment. It is possible therefore that inhibition may increase with any similar change in the experimental procedure halfway through. We address this possibility in Experiment 2.

Experiment 2

In Experiment 2 we test whether inhibition is increased following any change in the meaning of cues, but crucially, not involving re-assignment of the pairings of the same cue and target sets. Participants performed the unrelated cue-target

condition of Experiment 1 in conjunction with a condition in which the same cues were used but now paired with related targets (e.g. a square cue indicates search for a square target). We hypothesise that re-pairing the cues with a quite different set of targets within an experiment should not increase the degree of within-set competition during cue-target translation, and hence should not increase $n-2$ repetition costs.

The order of the two cue-target conditions was manipulated between groups, with half of the participants receiving the unrelated pairings first, and the other half receiving them second. If simply occurring in the second half of an experiment of this type (following a change of cue meaning) can potentiate inhibition, then $n-2$ repetition costs in the unrelated condition should be greater in the group which receives this condition second. A similar prediction can be made for the related cue-target pairings. When cues and targets are visually similar, we do not expect to find $n-2$ repetition costs (Houghton et al., 2009), but if any change to the meaning of cues during an experiment leads to increased inhibition then we might expect it to appear when the related condition occurs second. Our hypothesis however predicts in this case that $n-2$ repetition costs will be unaffected by order, resulting in no costs for the related cues, and $n-2$ repetition costs for the unrelated cues of similar magnitude to that found in Experiment 1 for the standard-pairing condition.

Method

Participants

32 new participants were recruited from the same pool as Experiment 1. 16 were randomly assigned to each between-subjects group, based on the order in which they received the cue-target pairings (*Related-Unrelated* group or *Unrelated-Related* group).

Apparatus & Stimuli

For the *unrelated* cues we used the square-angled, triangle-shaded, octagon-border cue-target pairings from Experiment 1. For the *related* cue-target pairings, the target display now consisted of a square, a triangle, and an octagon as potential targets, with the same neutral oval distractor from Experiment 1. See figure 6 for cues and targets used in Experiment 2.

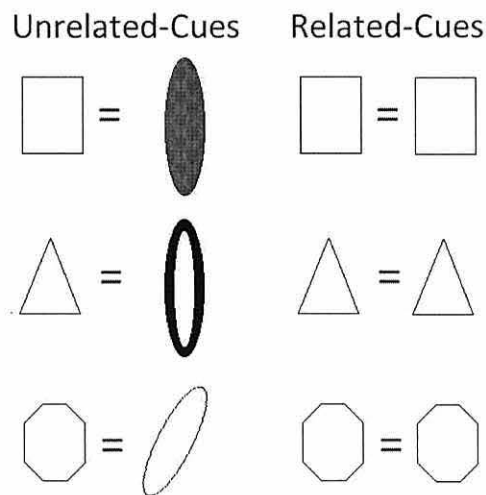


Figure 6. Cues and targets used in Experiment 2. Note, images are not to scale.

Procedure

The procedure was similar to Experiment 1. However, for the related condition, participants were required to locate the target that was the same shape as the cue presented. For example, if a square cue was presented, they must locate the square target.

Additionally, cue-ordering was manipulated between groups. One group received the related cues first, and the unrelated cues second; the second group were presented with the reverse order. Block length, trial structure, and practice protocol were identical to Experiment 1.

Design

Three factors were manipulated in a mixed factorial design: *cue-pairing* (related-cues vs. unrelated-cues) and *sequence* (ABA vs. CBA) were within-group variables, whilst *order* (Related-Unrelated vs. Unrelated-Related) was manipulated between groups.

Results

Data trimming was identical to Experiment 1, and retained 87% of trials for further analysis. The trimmed data was analysed with a mixed factorial ANOVA, with the factors as described in *Design*. Mean RT and errors are shown in table 2.

For the RT analysis, we found no significant effect of the factor *order*, $F(1,30) < 1$, and, most importantly, no interaction involving this factor was significant. For the other main effects: *cue-pairing* showed faster RTs to related cues ($M=567$) than to unrelated cues ($M=715$), $F(1,30) = 36.17$, $p < .001$, $\eta_p^2 = .55$; and *sequence* produced a reliable main effect due to slower responses to ABA trials ($M=648$) than to CBA trials ($M=634$), $F(1,30) = 9.00$, $p < .01$, $\eta_p^2 = .23$. This effect was qualified by a significant interaction with *cue-pairing*, $F(1,30) = 8.81$, $p < .01$, $\eta_p^2 = .23$, driven by greater $n-2$ repetition costs for unrelated cues (27ms) than for related cues (3ms). As noted, these effects did not depend on the order in which the conditions were received (see table 2). The $n-2$ repetition effect (27ms) was similar in magnitude to that found in Experiment 1 for the standard cues condition (20ms), but only half that found for the switched cues condition (55ms).

For the error analysis, the main effect of *order* was significant, with more errors in the Related-Unrelated group (3.67%) than the Unrelated-Related group (2.20%), $F(1,30) = 12.72$, $p < .01$, $\eta_p^2 = .30$. No other main effect or interaction was significant.

Table 2: Mean reaction time (RT in ms), standard errors (in parentheses), and errors (%) for ABA & CBA sequences in Experiment 2, for related- and unrelated-cues for both groups.

Group	Cue-pairing	Sequence				ABA-CBA
		ABA		CBA		
		RT	Errors	RT	Errors	
Related-Unrelated	Related-cues	596 (24)	3.10	593 (24)	2.99	3
	Unrelated-cues	720 (44)	4.64	694 (43)	3.95	26
Unrelated-Related	Related-cues	541 (24)	1.45	538 (24)	1.73	3
	Unrelated-cues	736 (44)	3.18	709 (43)	2.42	27

Discussion

The results show that a simple change in the meaning of a set of cues cannot account for the results from Experiment 1. $N-2$ repetition costs were completely unaffected by the order in which a given cue-target pairing was received. In the Related condition, inhibition was never evident (confirming previous results), while in the Unrelated condition, $n-2$ repetition costs did not increase when the condition was received following a change in cue meaning. Moreover the size of the unrelated $n-2$ repetition effect was the same as in the Standard (i.e., pre-switch) condition in Experiment 1.

Together, Experiments 1 and 2 define a pair of boundary conditions: In Experiment 1 we find a doubling of the size of $n-2$ repetition costs when cue-target mappings are re-paired within the same experimental session, while in Experiment 2 we observed no effect whatsoever of a change in cue meaning that does not involve the same targets. However, we have previously argued that

maximally-transparent cue-target pairings do not require an act of translation to retrieve a target description; participants can use the visual information provided directly by the cue to locate the target. If the nature of the process of target retrieval is quite different in the related and unrelated cases, then the differences between Experiments 1 and 2 might be accounted for by supposing that there is only a carry-over effect from one *translated* cue-target mapping to another. On this account, no modulation of $n-2$ repetition cost was found in Experiment 2 because there was only one translated (non-transparent) mapping used.

Our account however is that the increase in $n-2$ repetition cost in Experiment 1 is specifically due to the same cues and targets being used, which increases within-set conflict during cue-translation in the switched-cues condition. We do not predict that the effect should be found when the same cues are used with two *different* sets of unrelated targets, even though endogenous and effortful cue-target translation will be required for both sets. If the two target sets share no members then there should be no increase in within-set conflict following a switch of target. This prediction is tested in the final experiment of this chapter, in which we pair the same cues with two non-overlapping sets of targets. For both sets of targets the cue-target relationship is non-transparent, and should produce reliable $n-2$ repetition costs. However, we predict that whether participants receive a given cue-target set first or second should have no effect on this cost (i.e., the pattern of results will be the same as in Experiment 2, and different from Experiment 1).

Experiment 3

Method

Participants

32 new participants were recruited from the same pool as Experiment 1.

Apparatus & Stimuli

For one half of the experiment participants saw the standard-cues and targets from Experiment 1, and for the other half, they saw the same three cues but paired with a new set of targets (see figure 7). The new targets were designed to be visually distinct from the standard set, while at the same time offering a very similar kind of target detection task (visual feature search amongst a set of related geometric shapes). They consisted of four rectangles with the same height and width as the ovals from Experiment 1.

Each rectangle differed on a visual property: one was vertical with a dotted outline; another was vertical with horizontal lines evenly spaced within the rectangle; the final target was plain but was aligned horizontally. The fourth rectangle was a distractor, and was plain but aligned vertically. See figure 7 for cues and targets used in Experiment 3.

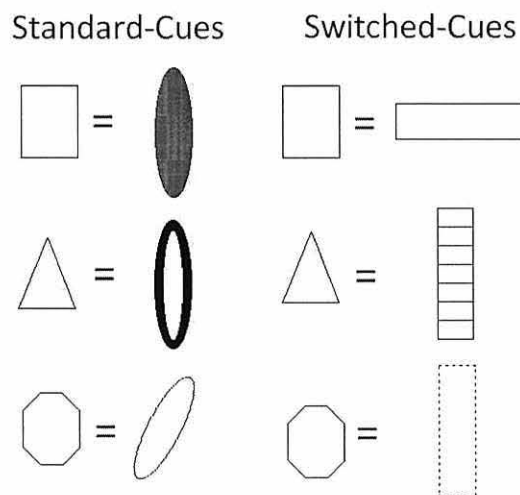


Figure 7. Cues and targets used in Experiment 3. Note, images are not to scale.

Procedure

One half of the experiment utilised the cue-target pairings from Experiment 1, and the other half the same cues with the new target set described above. The order of presentation of these cue-target sets was counterbalanced across participants.

As in Experiment 1, we refer to the first half of the experiment for all participants as *standard-cues*, and the second half as *switched-cues*. All trial timings, practice, and experiment length were identical to Experiment 1.

Design

The experiment manipulated two factors in a repeated measures design: *cue-pairing* (standard-cues vs. switched-cues) and *sequence* (ABA vs. CBA). The crucial measure was the interaction between the two factors.

Results

Data trimming was identical to Experiment 1, and trimmed data (89% of raw trials) was submitted to a two-way repeated measure ANOVA, with the factors as described in *Design*. Mean RT and errors are shown in table 3.

In this experiment, there were no significant main effects or interactions for the error analysis, so we report only the RT results in detail. For the RTs, there was no main effect of *cue-pairing*, despite there being numerically slower RTs for switched-cues ($M=683$) than for standard-cues ($M=660$), $F(1,31)=1.69$, $p>.2$, $\eta_p^2=.05$. There was a main effect of *sequence*, with slower RTs to ABA sequences ($M=680$) than to CBA sequences ($M=662$), $F(1,31)=20.49$, $p<.001$, $\eta_p^2=.4$. Critically, the interaction did not reach significance, $F(1,31)=0.21$, $p>.6$. $n-2$ repetition costs were 20ms for standard-cues and 17ms for switched-cues. Both costs were significant, $t(31)=4.8$, $p<.001$ for standard-cues and $t(31)=2.89$, $p<.01$ for switched-cues.

Although it was not a factor in the design, we also analysed the data with respect to the two cue-target sets shown in figure 7, as we wished to confirm that the new targets introduced in this Experiment behaved like the standard set. For this purpose we conducted a 2-way repeated measures ANOVA with factors *target set* (ovals vs. rectangles) and *sequence* (ABA vs. CBA). Only the main effect of *sequence* reached significance [$F(1,31)=20.49$, $p<.001$, $\eta_p^2=.4$], and both target

Table 3: Mean reaction time (RT in ms), standard errors (in parentheses), and errors (%) for ABA & CBA sequences in Experiment 3, for standard- and switched-cues.

Cue-pairing	Sequence				ABA-CBA
	ABA		CBA		
	RT	Errors	RT	Errors	
Standard-cues	670 (35)	2.92	650 (34)	2.55	20
Switched-cues	691 (32)	2.92	674 (30)	2.96	17

sets produced significant $n-2$ repetition costs: ovals [22ms, $t(31)=3.64$, $p<.001$], and rectangles [15ms, $t(31)=3.3$, $p<.01$].

Experiment 1 & 3 between-experiment analysis

As Experiment 3 has exactly the same structure as Experiment 1 (with no between groups factor), we were able to compare the results. The main benefit of doing so is that it affords a direct comparison of the $n-2$ repetition cost in the second half of the two Experiments.

RT data from Experiments 1 and 3 were analysed in a between subjects ANOVA, with the within-subject factors *cue-pairing* (standard-cues vs. switched-cues) and *sequence* (ABA vs. CBA), and *Experiment* as a between subject factor. Specifically, we were interested to compare the increase in $n-2$ repetition cost due to negative transfer in Experiment 1 with the switched cues of Experiment 3, which provided a suitable control condition for comparison.

There was no main effect of *Experiment*, $F(1,62) = 1.62$, $p>.2$. Critically, the three-way interaction was significant, $F(1,62) = 6.25$, $p<.05$, $\eta_p^2 = .09$. To ensure the three-way interaction was driven by greater $n-2$ repetition costs for switched cues in Experiment 1, two separate two-way ANOVAs measuring these costs were calculated between experiments (one for each cue-type, *standard-cues* vs.

switched-cues).

The first ANOVA for standard-cues had the factors *sequence* (ABA vs. CBA) and *Experiment* (1 vs. 3). In this analysis, we would expect to see no interaction of *sequence* and *Experiment*, which is indeed what we found, $F(1,62) < 1$. The main effect of *Experiment* was also not significant, $F(1,62) < 1$.

The second ANOVA for switched-cues had the same factors as above. In this analysis, we expect there to be an effect of *Experiment* on *sequence* due to larger $n-2$ repetition costs for negative transfer cues. This interaction was significant, $F(1, 62) = 10.2$, $p < .01$, $\eta_p^2 = .14$. Importantly, there was no main effect of *Experiment*, $F(1,62) = 1.47$, $p > .2$.

Discussion

The results of Experiment 3 were unambiguous: $n-2$ repetition costs were found in both halves of the experiment (unlike Experiment 2), and for both cue-target sets, but was completely unaffected by whether the cue-target mapping came first or second in the experiment. In a between experiment analysis with Experiment 1, the only reliable difference was the magnitude of the $n-2$ repetition cost following a change of cue-target mapping. When this involved re-pairing the same cues and targets (Experiment 1) the cost was twice as great as when the same (non-transparent) cues were paired with a new target set (Experiment 3).

General Discussion

The present set of studies provide further evidence that processes involved in cue-based stages of the trial structure can generate inhibition, as measured by $n-2$ repetition costs, even when conflict at the response level is kept constant (Houghton et al., 2009). In Experiment 1 we combined a negative transfer manipulation with a backward inhibition paradigm. Gade and Koch (2007a) have previously shown such a manipulation to affect switch costs, and proposed that this reflects increased inhibitory control following the switching of cue-task pairings. We reasoned therefore that a related manipulation should also increase

$n-2$ repetition costs in the cued target-search paradigm used by Houghton et al. (2009), as these authors have argued that $n-2$ repetition costs in this design reflect the level of inhibitory control required to resolve conflict during cue-to-target translation.

Experiment 1 showed the predicted effect: $n-2$ repetition costs were substantially increased following a re-pairing of the same sets of cues and targets. Experiment 2 demonstrated that this result cannot be explained as due to (i) comparing conditions in the first versus second half of the experiment (ii) a "harder" condition following an easier condition, or (iii) a simple change in cue meaning. Experiment 3 further demonstrated that it is not due to a change from one non-transparent cue-target mapping to another, even when the same cues are paired with both target sets.

This is the first time that such an effect has been reported and adds support to the views put forward both by Houghton et al. (2009) regarding cue-based backward inhibition, and by Gade and Koch (2007b) regarding the role of inhibitory control in generating switch costs. With respect to the Houghton et al. (2009) studies (see also Grange & Houghton, 2009), Experiment 1 also has the important property that different levels of $n-2$ repetition cost were found while the cue and target sets were kept constant. This is the first time this has been shown, as in the previous reports, $n-2$ repetition costs were compared across conditions in which only the targets were unchanged. It was therefore possible that the reported modulation of such cost was due to differences within the cue sets themselves, rather than the transparency of the cue-to-target relationship.

We conclude therefore that the re-pairing of cue-target sets introduces additional conflict in the cue-translation process, as translation must now contend with interference from the well practiced but irrelevant pairings previously utilised. This increased inter-trial conflict results in the need for greater inhibition of the previous cue-target relation when switching takes place, producing larger $n-2$ repetition costs.

Chapter Summary

The data presented in this chapter is important theoretically, as it is the first time $n-2$ repetition costs (and thus, inhibition) have been shown to be modulated purely by manipulation of conflict during cue-target translation. As noted in the introduction to this chapter, the effects found in Houghton et al. (2009) could potentially be caused by using different sets of cues. Using this negative transfer (Experiment 1), we have demonstrated that it is the conflict during cue-processing, independent of the type of cue used, that leads to $n-2$ repetition costs in our paradigm. Therefore we have been able to separate the underlying process away from the type of cue used, making the negative transfer paradigm a potentially important tool for investigating $n-2$ repetition costs and inhibition further.

One application of this paradigm is to neuro-imaging research, especially investigating the electrophysiology of backward inhibition. When utilising event-related potentials (ERPs) to study underlying neural responses to cognitive processes, it is important to keep low-level perceptual properties constant between conditions of interest, and ensure that it is only the endogenous demands on the participant that changes (see for example the Hillyard principle⁶, cited in Luck (2005)). This is because perceptual differences can evoke different neural responses, independent of the underlying cognitive processing. By comparing neural responses to $n-2$ repetition costs between the standard-cue and switched-cue condition of Experiment 1, all that differs will be the demands on inhibitory control, as perceptual differences are controlled.

The experiments presented in this first chapter suggest that it is conflict when a new WM representation is being formed that generates inhibition of the previous WM representation. However, it remains possible that in this chapter, and the work of Houghton and colleagues (Grange & Houghton, 2009; Houghton et al., 2009), that inhibition is merely targeting the cue itself, rather than some WM representation. Therefore for the next chapter, I address this question directly.

⁶"Always compare ERPs elicited by the same physical stimuli, varying only the psychological conditions."

Chapter 3

Backward Inhibition with 2 Cues per Task

CHAPTER 3: BACKWARD INHIBITION WITH 2 CUES PER TASK

Abstract

$N-2$ repetition costs in task-switching studies are a reaction time deficit in returning to a task recently performed (i.e. an ABA sequence) compared to a task not recently performed (a CBA sequence). These costs are thought to reflect persisting inhibition of recently executed tasks, and are therefore important in the study of cognitive control processes.

Although predominantly thought to reflect inhibition of response processes, $n-2$ repetition costs have recently been shown to be modulated by cue-based preparatory stages of task performance. However, in these instances it is not clear whether inhibition suppresses task representations or the cue itself.

In two experiments, we use a target-detection attention-switching paradigm with two cues per target, thus separating possible cue-inhibition from inhibition of target representations. Across both experiments we provide evidence that $n-2$ repetition costs reflect persisting inhibition of target representations, rather than inhibition of the cue itself. Thus, $n-2$ repetition costs remain an important empirical effect in studying the nature of task representations and cognitive control processes.

Literature Review & Introduction

The concept of inhibitory mechanisms to establish control and manipulation of mental representations has become of great interest to researchers over the past decade or so (Gorfein & MacLeod, 2007; Houghton & Tipper, 1996; MacLeod et al., 2003; Tipper, 1985, 2001). One experimental paradigm that is believed to involve inhibition of mental representations is the task-switching paradigm (Koch et al., in press; Logan, 2003; Mayr & Keele, 2000; Mayr, 2007; Meiran, in press; Monsell, 2003). Within this paradigm, participants have to switch between simple cognitive tasks (e.g. judge whether a number presented is odd/even, higher/lower than 5, or presented in red/green font), with the currently relevant task being signalled by a task cue (e.g. Parity, Magnitude, Colour, respectively). To perform a given task, it has been suggested that the relevant task has to be activated (Altmann & Gray, 2008; Meiran, 1996; Rogers & Monsell, 1995), and that irrelevant tasks must be inhibited (Mayr & Keele, 2000; Schuch & Koch, 2003). Inhibition in task-switching is inferred from so-called $n-2$ repetition costs, where it is more costly in both reaction time and error to return to a task recently performed (e.g. an ABA sequence) compared to returning to a task not recently performed (e.g. a CBA sequence). This $n-2$ repetition cost is thought to reflect the persisting backward inhibition (BI) of task A when it was switched away from two trials ago ($n-2$), hindering its reactivation on the current trial (n). $N-2$ repetition costs are a promising marker for cognitive inhibition, as so far they cannot be explained by non-inhibitory accounts (Mayr, 2002, 2007).

It has been suggested that inhibition targets response processes of task-switching performance (Koch et al., in press; Philipp, Jolicoeur, et al., 2007; Schneider & Verbruggen, 2008; Schuch & Koch, 2003), possibly caused by overlapping response sets, as typically all tasks require the same response keys. For example, a left response can mean "odd", "lower than 5" and "red", depending upon the currently relevant trial (Gade & Koch, 2007b). This finding suggests that $n-2$ repetition costs reflect an inhibitory mechanism that targets components

of task performance that generates inter-trial conflict. However, response processes occur quite late in the typical trial structure (cue-stimulus-response), and it remains possible that earlier elements of the trial structure could be targeted by inhibition, if between-trial conflict is present at these earlier stages.

Of the earlier task elements, the task cue could generate conflict (and thus inhibition), as it is on the basis of the cue that participants begin preparing for task performance. Such preparation might benefit from the inhibition of the current contents of working memory (Mayr & Keele, 2000; Mayr & Kliegl, 2000, 2003), as cue-based preparation has been suggested to require retrieval of task rules from long-term memory and their installation into working memory (Mayr & Kliegl, 2000).

Houghton and colleagues (Grange & Houghton, in press, 2009; Houghton et al., 2009) have provided evidence that inhibition can target earlier, cue-based stages of task performance. They utilised an attention-switching paradigm wherein participants were required to search for, and make a spatially compatible response to, the location of a relevant oval target on a screen consisting of four perceptually distinct ovals. Participants knew which target was relevant on a given trial by way of a valid cue. In one condition, the cue was a word that verbally described the characteristics of the target to search for (e.g. "Border", "Angled", and "Shaded" - one of the four ovals was neutral to serve as a distractor, and so was not paired with a cue). In this instance, Houghton et al. (2009) suggested that participants must translate the cue into an active working memory (WM) representation of which target to search for, a process they called *cue-target translation*. When the relevant target switched, participants must engage in a new episode of cue-target translation, which meets with conflict from the still-active WM representation used for the previous trial. In this instance, Houghton and colleagues suggested that the previous WM representation is subject to inhibition, which persists across an ABA sequence, and thus generates the $n-2$ repetition cost.

However, if a cue was so informative as to supply the relevant WM

representation directly, no cue-target translation should be required. In this instance, a switch of relevant target would not generate any conflict in WM, as no cue-target translation for the current trial or the previous trial exists. To test this, Houghton et al. (2009) contrasted word cues with iconic cues, which were rectangles that displayed the relevant target property directly (e.g. a rectangle with a thick border cues the target with the thick border). Houghton and colleagues found that iconic cues such as these produced no $n-2$ repetition costs, which suggests no inhibition was occurring when a new target became relevant.

This result is important, as in both cuing conditions, response processes were identical. Therefore, reduction of $n-2$ repetition cost cannot be explained by extant theories of inhibition in task-switching (Gade & Koch, 2007b; Philipp, Jolicoeur, et al., 2007; Schneider & Verbruggen, 2008; Schuch & Koch, 2003). Instead, Houghton et al. (2009) suggested that $n-2$ repetition costs reflect a flexible cognitive control mechanisms that serves to inhibit those aspects of the trial structure that generate the greatest inter-trial conflict, rather than being tied to response processes exclusively.

However, in the studies of Houghton and colleagues (Grange & Houghton, in pressb, 2009; Houghton et al., 2009), the presence of conflict during cue processing could trigger inhibition of the cue itself, rather than inhibition of WM representations as they have suggested. This possibility arises as only one cue per target is used in their paradigms (whether the cue is a word or an icon). In this instance, an ABA sequence presents a repeat of the cue for task A across the sequence as well as a repetition of the WM representation for this task. This cue-repetition tied together with WM representation repetition makes it impossible to distinguish if their results are caused by cue-inhibition or representation-inhibition. This distinction is important theoretically, as cue-inhibition would imply that $n-2$ repetition costs are not a suitable measure for endogenous cognitive control processes, but rather reflect passive inhibition of perceptual properties of tasks.

Evidence against cue-inhibition

Prima facie, cue-inhibition may appear unlikely as it would have to explain why no $n-2$ repetition costs are found for matching iconic cues (as per Houghton et al., 2009, Experiment 1). In this instance, however, it is difficult to ascertain that no inhibition is occurring purely due to the lack of an observable $n-2$ repetition cost. Indeed, if no inhibition of task A occurs during an ABA sequence, there should emerge an $n-2$ repetition benefit rather than a cost, as task A's activation should persist somewhat and prime performance on trial n . Therefore in the iconic cue condition of Houghton and colleagues, there may indeed be inhibition occurring, but much less than that for less transparent word cues. During an ABA sequence, it is possible that the persisting activation of task across an ABA sequence is somewhat muted by minimal inhibition, thus balancing the $n-2$ repetition cost to zero⁷. Therefore, lack of $n-2$ repetition costs for transparent iconic cues does not necessarily provide evidence against cue-inhibition.

However, there is some evidence in the extant task-switching literature that inhibition may not be cue-dependant. Using two cues per task, Mayr and Kliegl (2003) had participants switch between three different judgements of a presented object: its size (small/large), colour (red/blue) or shape (circle/square). Letters with no pre-experimental association to the tasks were used as cues (for example, "D" and "R" cued the colour task). Mayr and Kliegl found significant $n-2$ repetition costs when the cue for task A switched across an ABA sequence (a sequence henceforth referred to as an A'BA sequence). However, no $n-2$ repetition costs were evident for ABA sequences where the cue for task A repeated over the sequence (henceforth, still referred to as an ABA sequence). This is surprising, as of course an ABA sequence is the trial structure that generates the $n-2$ repetition cost in typical one-cue task-switching paradigms (Mayr & Keele, 2000). To

⁷It is a general problem of many inhibitory paradigms that measurement of inhibition happens after it has occurred, and researchers are actually measuring the *aftereffects* of inhibition rather than inhibition *itself*. Thus this lack of a direct measure of inhibition as it is occurring makes it impossible to confirm that no inhibition occurred merely from the absence of observable aftereffects. I will return to the discussion of this important issue in the general discussion of this thesis.

explain this finding, Mayr and Kliegl (2003) suggested that when a cue repeats across an ABA sequence, positive priming of the cue cancels out the $n-2$ repetition cost (see earlier footnote). Cue priming, they suggest, is a possible explanation as two cues per task enables distinct memory traces to be formed for each cue. However, this explanation is post hoc, and contrary to earlier findings.

Gade and Koch (2008) attempted to reduce any possible role of cue-priming by reducing the cue-stimulus interval (CSI) to 100ms (Mayr and Kliegl, 2003, had used a CSI of 800ms), as less exposure to the cue should reduce any effects of priming on the $n-2$ repetition cost. With this reduction, Gade and Koch found equivalent $n-2$ repetition costs for ABA and A'BA sequences, contra to Mayr and Kliegl.

These studies suggest that inhibition in task-switching does not target the task cue. However, both of these experiments used non-transparent cues associated to each task (Grange & Houghton, in pressa; Logan & Schneider, 2006a). In such situations, Logan and Schneider (2006a) have suggested that cue encoding results in a mediator task name, which can then be used as a cue itself to perform the task. For example, the cues "D" and "R" in the Mayr and Kliegl study could retrieve the mediator "Colour". Therefore, across ABA and A'BA sequences, although the cues themselves do not repeat, the mediator does repeat across the $n-2$ repetition. Therefore, equivalent $n-2$ repetition costs for ABA and A'BA sequences may reflect inhibition of the mediator. Such a process could explain $n-2$ repetition costs without appealing to inhibition of WM representations.

To address this possibility, Altmann (2007) used more transparent cues which had direct associations to the tasks. For example, Altmann had participants judge the colour, magnitude, or parity of presented number stimuli. The cues used for the colour task were "Colour" and "Red Blue". For cues such as these, Logan and Schneider (2006a) suggested that no mediator is required. With these cues, Altmann found equivalent $n-2$ repetition costs for ABA and A'BA sequences, thus ruling out mediator inhibition as source of the effect.

Thus, the evidence reviewed suggests that $n-2$ repetition costs do not reflect inhibition of the cue, but rather target task-specific WM representations (Altmann, 2007). However, such findings do not necessarily suggest that the $n-2$ repetition costs found in the paradigms of Houghton and colleagues do not reflect cue-inhibition (Grange & Houghton, in pressb, 2009; Houghton et al., 2009), as the paradigms of Mayr and Kliegl (2003) Gade and Koch (2008), and Altmann (2007) additionally have substantial conflict during response-selection stages of task performance, as the tasks share overlapping response sets (Koch et al., in press; Philipp, Jolicoeur, et al., 2007; Schneider & Verbruggen, 2008; Schuch & Koch, 2003). As response conflict is not thought to be present in the paradigm of Houghton and colleagues, it remains unanswered whether $n-2$ repetition costs within such a paradigm that has inter-trial conflict centered on cue-processing reflects inhibition of the cue itself or inhibition of WM representations. Addressing this issue is the purpose of the present experiments.

Experiment 4

For Experiment 4 we utilised the word cues from Experiment 1 of Houghton et al. (2009), as these have been shown to reliably produce modest $n-2$ repetition costs. Additionally, although less transparent than matching iconic cues, word cues should not require a mediator (Logan & Schneider, 2006a), which could possibly have explained the results of Gade and Koch (2008) and Mayr and Kliegl (2003; Altmann, 2007).

Method

Participants

51 participants were recruited from the same pool as the previous experiments. One participants data was removed due to session-wise accuracy below 90%.

Apparatus & Stimuli

Stimuli were presented using E-Prime experimental software (Psychology Software Tools, 2002) on a PC with a 17" monitor. Viewing distance was approximately 60cm. Responses were collated using a standard QWERTY keyboard. Target display consisted of four ovals (6cm in height; 3 with widths of 2.3cm and one with width of 3.5cm) presented in the center of each quadrant of the screen. Each target differed visually, with three being potential targets and one neutral distractor (see figure 14). The background for the target display was white.

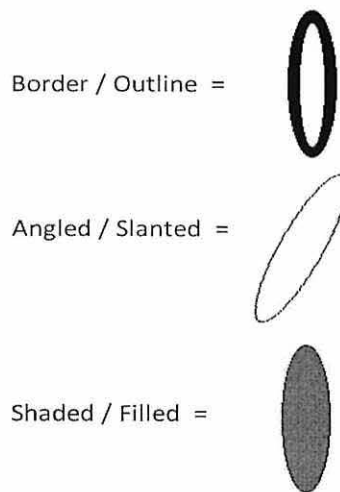


Figure 8. Cues and targets used in Experiment 4. Note, images are not to scale.

Each target was paired with two word cues that described the characteristic of the relevant target for the current trial: 'Border' and 'Outline'; 'Angled' and 'Slanted'; 'Shaded' and 'Filled'. Cues were presented in Times New Roman 15 font in black on a white background. One cue only appeared on each trial, centred on the screen.

Procedure

Participants were tested individually in a session lasting approximately 30 minutes. The session consisted of two experimental blocks of 130 trials, preceded by a practice block of 26 trials.

A trial consisted of a cue presented centrally for 500ms, after which time the cue disappeared and a blank screen followed for 250ms (cue-stimulus interval; CSI). Cue selection was random with the constraint that it could not signal the relevant target from the previous trial (i.e. there were no target repetitions). After the CSI, four oval targets appeared positioned randomly on the screen. The participants task was to make a spatially compatible response to the cued targets location with the following keys (location = response): upper-left = D; lower-left = C; upper-right = J; lower-right = N. Responses were asked to be as fast and as accurate as possible and were made with the index and middle fingers of each hand. After the response, the screen went blank for the response-cue interval (RCI) of 500ms, after which time the cue for the next trial appeared.

There were two blocks of 130 experimental trials, preceded by a practice session of 26 trials. There were two cues per task which allowed three types of transitions: CBA, ABA, and the new sequence, A'BA, where the cue for task A switches over the triplet (e.g. Border, Angled, *Outline*). Cue selection was randomised on each trial, so that given an $n-2$ repetition, there was equal probability of an ABA and a A'BA sequence occurring. Target repetitions did not occur.

Design

The experiment utilised one factor, with three levels: *Sequence* (CBA vs. A'BA vs. ABA). RT (ms) and Error (%) served as the dependant variables.

Results

Data was trimmed to exclude the first 2 trials from each block. Errors, and the two trials following an error were also removed, as were RTs faster than 200ms or slower than 2,500ms. Data trimming retained 89.4% of raw data to be submitted to further analysis. Trimmed RTs and errors were submitted to separate one way ANOVAs with the factor as described in *Design*. Error rates produced no significant effects or interactions, so I will focus on RT. The mean

RTs and errors are shown in figure 9.

The main effect of *sequence* was significant, $F(2,98)=5.95$, $p<.01$, $\eta_p^2 =.11$. RT to CBA sequences ($M=622$) was faster than to A'BA sequences ($M=632$), which in turn was faster than to ABA sequences ($M=645$). Fischers least significant difference (LSD) comparisons showed that the difference between CBA and A'BA sequences was not significant ($p=.098$), and the difference between A'BA and ABA sequences were also not significantly different ($p=.1$). The difference between CBA and ABA sequences however was significant ($p<.01$).

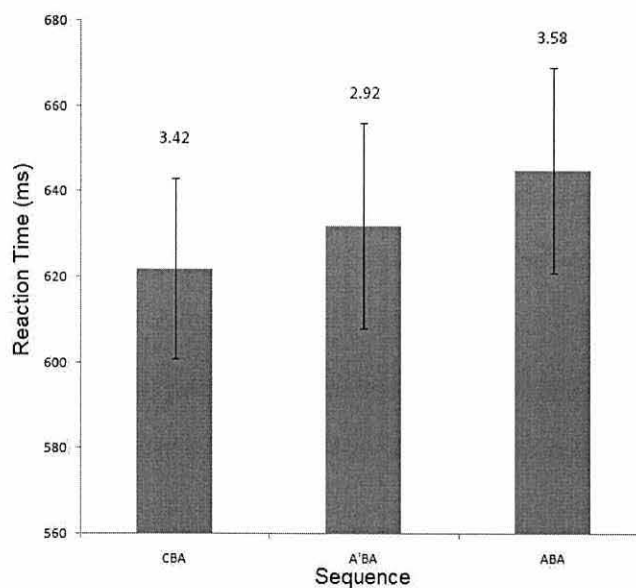


Figure 9. Reaction time from Experiment 4. Error bars denote 1 standard error around the mean. Numbers above error bars represent Errors (%).

The preliminary analysis provides an ambiguous picture, as RTs to A'BA sequences do not differ from either CBA or ABA sequences, so it is not possible to state which sequence it is closer related to. To overcome this, I decided to calculate individuals traditional $n-2$ repetition costs (measured by subtracting CBA RT from ABA RT) and perform a median split on the data. Based on this split, I was able to organise participants scores into those who show $n-2$ repetition costs (BI group), and those that do not (No BI group). The levels of the factor *Sequence* were then analysed for each group separately. For errors, the interaction

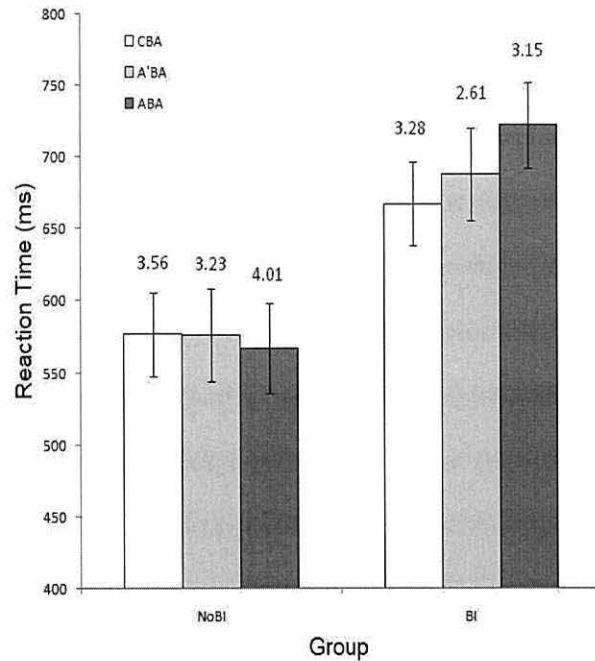


Figure 10. Reaction time data from Experiment 4 after median split. Error bars denote 1 standard error around the mean. Numbers above error bars represent Errors (%).

was not significant, so I focus on RTs. Mean RTs and errors are shown in figure 10.

This data was re-analysed using a mixed factorial repeated measures design, with group as a between subjects factor (BI vs. No BI). Again there was a main effect of *sequence*, $F(2,96)=7.89$, $p<.01$, $\eta_p^2=.14$. The main effect of group was also significant, $F(1,48)=7.87$, $p<.01$, $\eta_p^2=.14$. Importantly, the two interacted, $F(2,96)=16.91$, $p<.001$, $\eta_p^2=.26$.

This interaction was investigated by performing two separate one-way ANOVAs, one for each group. For the No-BI group, the main effect of *sequence* was not significant, $F(2,23)=1.46$, $p=.24$. The main effect of *sequence* for the BI group was significant, $F(2,23)=17.35$, $p<.001$, $\eta_p^2=.42$. LSD comparisons showed that CBA RTs were significantly faster than A'BA RTs ($p<.05$), and that A'BA RTs were significantly faster than ABA RTs ($p<.01$).

Discussion

Preliminary results from this experiment were unclear, as A'BA sequences were not significantly different from either CBA or ABA sequences. However, a

median split on traditional $n-2$ repetition costs was able to differentiate between the sequences. For the group who show no traditional $n-2$ repetition cost, no differences between the three types of sequence was evident. However, for those who did show a traditional $n-2$ repetition cost, CBA sequences were faster than A'BA sequences, which in turn were faster than ABA sequences. This suggests that cue-inhibition does not explain the $n-2$ repetition cost completely in those participants who show effects of inhibition, as a significant $n-2$ repetition cost is evident even when the cue for task A switches (A'BA). Therefore some representation over and above that of the cue is inhibited when switching (Altmann, 2007; Gade & Koch, 2008; Mayr & Kliegl, 2003). However, the significant difference between A'BA and ABA sequences does provide novel evidence that when the inter-trial conflict centers on cue processing, the cue is inhibited to some degree. This finding is counter to the suggestion provided by (Mayr & Kliegl, 2003) as to why they did not find $n-2$ repetition costs in an ABA sequence with their 2:1 mapping paradigm. They suggested that positive priming of the cue persists over the sequence and counteracts the negative effects of inhibition at trial n . This interpretation is not compatible with our finding, as this scenario would predict greater $n-2$ repetition costs for A'BA sequences than for ABA sequences, the opposite of what we found. As the current experiment is the third reported finding of significant $n-2$ repetition costs with a 2:1 mapping when the cue repeats (ABA sequence), it appears likely that the finding of Mayr and Kliegl is a paradigm-specific peculiarity, and we are unable to offer any alternative explanation.

For Experiment 5, we sought to generalise and extend the findings from Experiment 4.

Experiment 5

As we have provided evidence for inhibition of WM representations during switching, it remains an open question whether WM representations are required (and hence, subject to inhibition) even when transparent iconic cues are used (c.f.

Houghton et al., 2009; Experiment 1). Additionally, if WM representations are inhibited in this situation, why are $n-2$ repetition costs not evident?

It is a possibility that a WM representation is inhibited during switching for transparent iconic cues, but that the cue is somehow able to bypass the inhibited representation, thus removing the $n-2$ repetition cost. Inhibition during switching in the transparent cue condition would remain an effective strategy for the cognitive system to avoid perseveration tendencies, as on every trial a response is required to a different target than what was relevant on the previous trial. Indeed the hypothesis of Houghton et al. (2009) does not necessarily preclude inhibition occurring for iconic cues. For example, the suggested absence of a cue-target translation process for transparent iconic cues does not automatically assume no WM representation is utilised in the icon condition. What it does suggest is that this representation is not endogenously formed, as the cue provides the necessary representation directly. Indeed, some WM representation must be required, as the cue disappears before target onset, and thus participants cannot passively match a central cue to its relevant target. When the trial switches, a representation of the previous target is likely still active to some degree, and may therefore require inhibition. $N-2$ repetition costs, by this hypothesis, may arise from trying to endogenously activate an inhibited WM representation. If no endogenous formation of an inhibited representation is required, responses are just as fast as when a non-inhibited representation is required.

To test this latter hypothesis, for Experiment 5 we combine transparent iconic cues and less-transparent word cues in one experimental condition. The cue switches between icons and words every four trials (e.g. icon, icon, icon, icon, word, word, word, word, icon...), although the relevant target always switches on every trial, allowing comparison of ABA and CBA sequences. This manipulation allows $n-2$ repetition trials where all three cues of the sequences are the same, as per Houghton et al. (2009; Experiment 1), for example, word - word - *word*. Additionally, this new paradigm allows $n-2$ repetitions where the cue at $n-2$ is

different to that on n , for example, the sequences *icon - icon - word* and *word - word - icon*. The critical conditions are those in which the cue for n and $n-2$ are different. Below we outline the differing potential outcomes for these $n-2$ repetition sequences.

Critical sequences

- $A_{word} - B_{word} - A_{icon}$: For this sequence, switching from A to B should induce inhibition of A, much like in the word-cue condition of Houghton et al. (2009). If iconic cues are able to bypass inhibited representations, then no $n-2$ repetition cost should be evident in this sequence. However, if iconic cues do not bypass inhibited representations, significant $n-2$ repetition costs should be evident for this sequence.

- $A_{icon} - B_{icon} - A_{word}$: For this sequence, finding an $n-2$ repetition cost would suggest that some inhibition of A took place when it switched to B, even though both cues were iconic (a scenario Houghton et al., 2009, Experiment 1, have suggested no inhibition occurs). Such a finding would suggest that inhibition is occurring during switching between iconic cues, but that it is not found in *icon - icon - icon* sequences as the cue is able to bypass the inhibited representation.

The generalised prediction is that no $n-2$ repetition costs should be evident if the current trial is an iconic-cued trial, as any inhibited representation will be able to be bypassed. If the current trial is a word-cued trial, then $n-2$ repetition costs should be evident regardless of what the cues were on $n-1$ and $n-2$, as endogenous formation of an inhibited representation is required.

Method

Participants

36 new participants were recruited from the same pool as the previous experiments. Four participants data were removed due to session-wise accuracy below 90%.

Apparatus & Stimuli

The target display was identical to that of Experiment 4. The cues alternated between word- and iconic-cues every 4 trials. The word cues were the three words 'Border', 'Angled', and 'Shaded' with the same font as Experiment 4. The iconic cues were rectangles which displayed the visual property of the relevant target for the current trial (see figure 11).

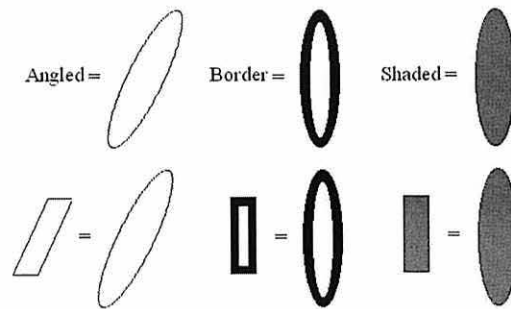


Figure 11. Cues and targets used in Experiment 5. Note, images are not to scale.

The icon cues were presented with a height of 4cm and a width of 1.4cm in greyscale shading on a white background.

Procedure

The experiment presented both cue types within the same experimental block. The experiment consisted of 4 blocks, each consisting of 168 trials. Trials were grouped into runs; a run consisted of 4 trials of one cue type (e.g. word, word, word, word, or icon, icon, icon, icon). The cue-type switched between words and icons every 4 trials. Again the relevant target switched on each trial. The first cue-type presented to each participant in the experiment was counterbalanced across participants.

A trial had the same temporal parameters as Experiment 4. Each block was followed by a self-paced rest screen. The experimental block was preceded by a brief practice session, terminated when the participant demonstrated they understood the task (Houghton & Mari-Beffa, 2005).

Design

The experiment manipulated 3 factors within subjects: *cue-type* (icon or word on current trial); *sequence* (ABA vs. CBA), and trial in run (1-4). RT and error again served as the dependant variables.

Results

The first 8 trials (2 runs) from each block were removed as warm-up trials. The remainder of error and RT trimming was identical to Experiment 4. RT and errors were submitted to separate three-way repeated measure ANOVAs with the factors as described in *Design*. The RT data is shown in figure 12, and the errors are shown in table 4.

RT analysis

For the RT analysis, there was a main effect of *cue-type*, with faster RTs if the current trial was cued by an icon ($M=480$) than if it was cued by a word ($M=563$), $F(1,31) = 42.95$, $p < .001$, $\eta_p^2 = .58$. There was also a main effect of *sequence*, with slower RTs to ABA sequences ($M=526$) than to CBA sequences ($M=517$), $F(1,31) = 5.62$, $p < .05$, $\eta_p^2 = .15$. The main effect of *trial* was not significant, $F(3,93) = 0.18$.

Cue-type interacted with sequence, with smaller $n-2$ repetition costs for iconic cues (-1ms) than for word cues (19ms), $F(1,31) = 6.93$, $p < .05$, $\eta_p^2 = .18$. No other interactions were significant (largest $F = 1.35$).

To test the generalised predictions outlined in the introduction, trials were organised into whether the current trial had the same cue-type as $n-2$ or a different cue-type. The data was thus re-analysed with a 3-way ANOVA with the levels *current-cue* (icon vs. word), *sequence* (ABA vs. CBA) and *previous-cue* (same vs. different). The critical result is the interaction of previous-cue on sequence, and the three-way interaction.

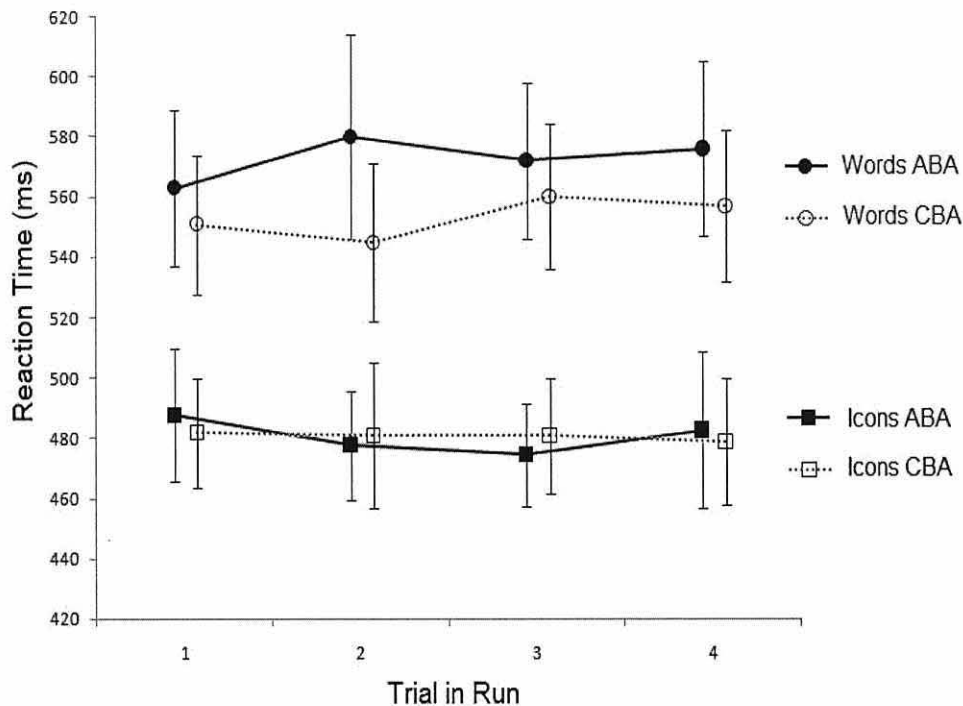


Figure 12. Reaction time data from Experiment 5. Error bars denote 1 standard error around the mean.

There was no interaction of previous-cue on sequence, $F(1,31) = 1.23$, $p > .27$. The three-way interaction was also not significant, $F(1,31) = 0.3$, $p > .5$. Therefore, the $n-2$ repetition costs found for word cues occurred regardless of whether the cue on $n-2$ was a word or an icon. Conversely, no $n-2$ repetition costs are observed if the current trial is an icon.

Error analysis

The errors showed marginally more errors for word cues ($M=3.83$) than for iconic cues ($M=3.26$), reflected by a marginal main effect of *cue-type*, $F(1,31) = 2.96$, $p = .095$, $\eta_p^2 = .09$. The main effect of *sequence* was significant, with more errors being made on ABA trials ($M=4.01$) than on CBA trials ($M=3.05$), $F(1,31) = 10.08$, $p < .01$, $\eta_p^2 = .25$. No interactions were significant (largest $F = 1.23$).

Discussion

For the present experiment we sought to extend the findings from Experiment 4 that $n-2$ repetition costs reflect persisting inhibition of WM representations

Table 4: *Percent errors for ABA & CBA sequences in Experiment 5, across cue-types and trial.*

Current Cue	Trial	Sequence	
		ABA	CBA
Icon	1	3.72	3.05
	2	4.50	3.00
	3	3.41	2.68
	4	2.74	2.71
Word	1	4.64	2.80
	2	4.32	3.60
	3	4.77	3.42
	4	3.98	3.12

rather than cue-specific inhibition. This pattern was again found for this Experiment, as $n-2$ repetition costs were found across sequences when the cue for $n-2$ was a different type to that on n . However, such costs were only found if the cue for trial n was a word cue, requiring endogenous formation of a WM representation. This pattern of results was true regardless if all trials in the ABA sequence were words ($A_{word} - B_{word} - A_{word}$), or whether $n-2$ and $n-1$ were icons $A_{icon} - B_{icon} - A_{word}$. If inhibition had targeted just the cue, then an $A_{word} - B_{word} - A_{word}$ sequence would be more costly than an $A_{icon} - B_{icon} - A_{word}$ sequence, as it is only the case in the former sequence that the cue is the same on $n-2$ and n .

We have also provided initial evidence that inhibition does occur when switching in the iconic-cued condition of Houghton et al. (2009). Specifically, we found $n-2$ repetition costs in the $A_{icon} - B_{icon} - A_{word}$ sequences. If inhibition did not occur when switching between iconic-cued tasks, then no inhibition of A should occur when switching to B in this sequence (i.e. $A_{icon} - B_{icon}$). However, the $n-2$ repetition cost found at n suggests some inhibition did occur.

The lack of an observable $n-2$ repetition cost when n was an iconic-cued trial

suggests that iconic cues are able to bypass an inhibited representation. This result suggests that $n-2$ repetition costs reflect inhibition of WM representations that are only detected when the inhibited representation is to be activated endogenously. Of course, such a suggestion is crudely defined, and rigorous modeling of the processes that lead to an activated WM representation is essential for elucidating which aspect generates $n-2$ repetition costs (but see figure 13 for an overview of the stages that more fine grained modeling would need to address).

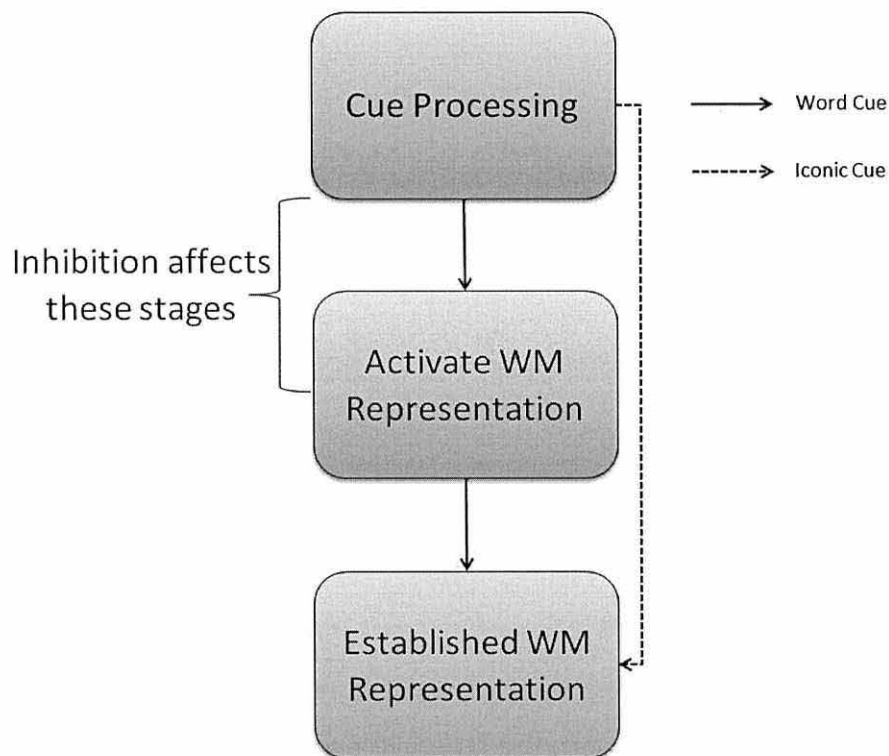


Figure 13. A processing model for $n-2$ repetition costs. Arrows denote different cue processes.

General Discussion

Across two experiments, we have provided evidence that inhibition in task-switching targets WM representations over and above that of the cue, even in a paradigm where inter-trial conflict is centered on cue processing (Altmann, 2007; Gade & Koch, 2008; Grange & Houghton, in pressa; Houghton et al., 2009). This extends the findings of Altmann (2007) and Gade and Koch (2008), who found cue-independent inhibition, but using tasks with conflict centered on response

processes (Schuch & Koch, 2003). In such scenarios, equivalent $n-2$ repetition costs for A'BA and ABA sequences could have occurred due to inhibition of a shared response selection process (e.g. cues "magnitude" and "high/low" require the SR mapping of left key = low, right key = high).

The results presented are important, as Houghton et al. (2009) have recently shown that $n-2$ repetition costs can be modulated by difficulty of cue processing. In such situations, $n-2$ repetition costs may have reflected inhibition of task cues themselves, independent of any WM representation.

In Experiment 4, $n-2$ repetition costs were found to be independent from cue-repetitions (e.g. A'BA sequences); however, despite this, some cue inhibition was present. Both of these costs were only found in participants who showed traditional $n-2$ repetition costs (ABA-CBA), suggesting that individual differences plays a significant role in the effects found. For those who adopt an inhibitory strategy⁸, inhibition of the cue and inhibition of a WM representation occurs. However, it remains an open question what is occurring in those participants who show no $n-2$ repetition costs. If the inhibitory theory of task-switching is correct, then how are these participants able to switch tasks? This question is difficult to address, as $n-2$ repetition costs reflect the aftereffects of inhibition, and do not measure inhibition directly. Therefore it is unclear whether the lack of $n-2$ repetition costs reflect a failure of inhibition to deploy at $n-2$, or rather reflect a superior ability of these participants to *overcome* inhibition on the current trial. The latter is a distinct possibility, as overall RTs were faster for those who showed no $n-2$ repetition costs. These questions are important to address, as a lack of $n-2$ repetition costs have been consistently taken to reflect a lack of inhibition occurring (Grange & Houghton, in pressa; Houghton et al., 2009; Mayr, Diedrichsen, Ivry, & Keele, 2006), which is not necessarily the case.

Experiment 5 provided some evidence that inhibition does occur in situations that typically do not generate $n-2$ repetition costs. Specifically, in Houghton et al. (2009), iconic cues have shown to produce no $n-2$ repetition cost (e.g. $A_{icon} - B_{icon}$

⁸At this stage, no assumptions will be made whether this strategy is intentional, or automatic.

- A_{icon}). However, when trial n was a word cue ($A_{icon} - B_{icon} - A_{word}$), $n-2$ repetition costs occurred. This suggests some inhibition occurred when switching between two iconic cued trials ($A_{icon} - B_{icon}$), a scenario suggested not to require inhibition in Houghton et al.

However, these results must be viewed cautiously. In this paradigm, word-cues and iconic-cues were combined within the same block. In this situation, the cognitive system may be treating the iconic cues differently than to when iconic cues are presented within a block on their own (as in Houghton et al., 2009). Specifically, the system might engage in cue-target translation, even though it is not strictly required. A constant cue-target translation strategy would be more efficient than a constant strategy of no cue-target translation (as then word cues would not be able to be processed), and would likely be more efficient than switching between cue-target translation for word cues, and no cue-target translation on iconic cues (which would require a strategy shift every four trials). However, there is evidence against the suggestion the system may be treating the iconic cues in a similar fashion to that of the word cues. Specifically, RTs were significantly faster if the current trial was cued by an icon. If cue-related processes were similar for word cues and iconic cues, RTs should be equivalent. Additionally, $n-2$ repetition costs were absent if the current trial was an icon, even when the preceding two trials were word-cued trials (e.g. $A_{word} - B_{word} - A_{icon}$). This suggests some stage of processing is bypassed (or dealt with differently) when the system is presented with an iconic cue (see figure 13). As already suggested, rigorous modeling of the processing stages following cue presentation are important for elucidating which aspect is targeted by inhibition.

Chapter Summary

The present chapter presented evidence against a possible alternative account of our findings in Houghton et al. (2009). Cue-inhibition was a likely alternative explanation of these results, and so direct empirical evidence that cue-inhibition could not explain our results exclusively was required.

Another alternative explanation of the findings of Houghton et al. (2009) was highlighted during the course of these studies, when a paper was published that suggested $n-2$ repetition costs could only be found in situations where the task cue remained on the screen during stimulus presentation. As the experiments of Houghton et al. (and the current experiments of this thesis) removed the cue before target onset, the lack of $n-2$ repetition costs for transparent iconic cues could be caused by the lack of "cue-target overlap" (Druey & Hübner, 2007). For the next chapter, I investigated this possibility directly.

Chapter 4

Cue-Target Overlap & Backward Inhibition⁹

⁹This chapter has been published as Grange and Houghton (2009), and is reproduced with permission of the copyright holder. Thanks to Katherine Arbuthnott, Ronald Hübner, Nachshon Meiran, W. Trammell Neill, and an anonymous reviewer for their constructive comments on a previous version of this article.

CHAPTER 4: CUE-TARGET OVERLAP & BACKWARD INHIBITION

Abstract

$N-2$ repetition costs are a performance cost observed when subjects return to a task after just one intervening trial of a different task, compared to returning after a longer interval (ABA vs. CBA sequences, where A, B, C are tasks). This effect is thought to reflect the need to overcome inhibition applied specifically to task A during disengagement at trial $n-1$. Druet and Hübner (2007) have suggested that employment of such a specific inhibitory mechanism relies upon the cue and the target of the task overlapping temporally.

We provide evidence across three experiments (including a direct replication attempt) that this is not the case, and that the presence of task-specific inhibition relies to some extent on the need to translate the cue-target relationship into working memory. Additionally, we provide evidence that faster responses in no overlap conditions are driven by low-level perceptual differences between target displays across overlap conditions.

We conclude that inhibitory processes in task switching are an effective sequential control mechanism, employed equally in cases of temporally overlapping and temporally separated cues and targets.

Literature Review & Introduction

The task-switching paradigm has become an increasingly popular approach to studying the mechanisms of cognitive control which permit us to change the way we respond to a given environment to produce sequences of actions (Monsell, 2003). One mechanism frequently postulated to aid fluent shifts of action-based attention is inhibition (Houghton & Tipper, 1996). In a task-switching context, Mayr and Keele (2000) proposed that when subjects expect to switch task on every trial, they will inhibit a just completed task-set to aid switching to the next one. In line with this idea, they found that response times (RTs) are increased when a task repeats after just one intervening trial (an ABA sequence), compared to when it doesn't (a CBA sequence). Mayr and Keele proposed that this $n-2$ repetition cost reflects an inhibitory mechanism which serves to suppress the activation levels of a task during its disengagement, aiding a new task-set to become dominant. As a result, re-activation of a task-set is impaired if it has been recently abandoned (inhibited). This inhibitory mechanism has been labeled Backward Inhibition (BI).

Recent work in the BI paradigm has proposed that the effect depends upon response selection stages of task performance (Schuch & Koch, 2003), in particular on the use of tasks with overlapping response sets (Gade & Koch, 2007b). In contrast, the role of the task-cue (the way in which the subject is instructed as to which task to perform on a given trial) has received much less attention (though see Arbuthnott, 2005). This is surprising as all BI studies to date have utilised valid task-cues, and it is only on the basis of the cue that the subject knows which task to prepare for. Mayr and Kliegl (2000) suggested that the task cue affords retrieval of the associated task set from long term memory and its installation into working memory (WM), and one might well anticipate that this process of "cue-to-task translation" would be aided by the inhibition of the current contents of WM.

In this vein, a recent paper by Druey and Hübner (2007) suggested that $n-2$ repetition costs only occur when the cue and the imperative stimulus (target) of

the task are temporally overlapping (i.e. when the cue remains on the screen during stimulus presentation). In their experiments, $n-2$ repetition costs were only found when cues and targets temporally overlapped; when the cue disappeared before the target display, no such costs were found (Experiment 1). In addition it was suggested that the temporal overlap of cue and target at the $n-2$ trial was essential for observation of the $n-2$ repetition cost (Experiment 2). On this basis, the authors suggest that in cases of non-overlapping cues and targets, selective inhibition of the preceding task does not occur. They propose that when cues and targets are separated temporally, the current task is less salient and thus harder to discriminate; therefore "...in order to select the correct task set with reasonable accuracy, interference from *both* competing task sets must actually be reduced" (Druey & Hübner, 2007, p.753, emphasis added).

We found this suggestion intriguing as recent work in our laboratory provides further evidence that the way task cues operate to activate the task in WM can modulate $n-2$ repetition costs (Houghton et al., 2009). We suggest that in order for task retrieval to happen, some translation must occur in WM between the task-cue and the task it represents. When the task-set is installed into WM based on this translation, it meets with competition from the still-active cue-to-task translation used on the previous trial. This latter representation is subject to inhibition to clear WM (Mayr & Keele, 2000). Therefore, we argued that using a cue-to-task relationship that minimised the need for translation should result in reduced conflict in WM from trial to trial. This should reduce the need for inhibition and consequently the $n-2$ repetition cost should be reduced. We tested this by having subjects respond to the location of a target object accompanied by 3 non-targets (Houghton et al., 2009; see the method section of the current article for an example of this paradigm). On each trial, a preceding cue specified a distinguishing feature of the target object and the target changed on every trial. In a series of experiments we manipulated the cue-target relationship in terms of its "transparency"; that is, how directly the visual information provided by the

cue specified what was to be looked for. For instance, if the stimuli were 4 ovals, each distinguished from the rest by some visual feature (see figure 14), then a transparent cue to a target would be a shape (a rectangle) possessing the target feature. A (relatively) non-transparent cue would be a word referring to the distinguishing feature. To summarise the results of this study, we found that the size of the $n-2$ repetition effect varied inversely with the degree of transparency of the cue-target relationship, and was altogether absent for the most transparent cues (i.e. those most directly representing the target feature). However, in other conditions we found robust $n-2$ repetition costs across four experiments, and, most pertinently in the present context, in every condition cues and targets were temporally separated (with a cue stimulus interval of 250ms).

In addition to our findings, there are published articles reporting significant BI effects with no temporal overlap of cue and target (Altmann, 2007; Kuhns et al., 2007; Sinai et al., 2007), suggesting that overlap might not be essential to detect $n-2$ repetition costs. However, given the Druey and Hübner (2007) findings, it may be that $n-2$ repetition costs are simply reduced when cues and targets do not overlap. If the condition being used does not, for whatever reason, lead to very large or reliable BI effects, then the effect of lack of overlap could be enough to make them statistically unreliable. Thus in the case of our own studies referred to above, the complete absence of a $n-2$ repetition cost when the cue-to-target relationship was maximally transparent may have been caused by the fact we did not utilise temporally overlapping cues. The purpose of the present article is to investigate the relationship of temporal cue-target overlap and BI with our target detection paradigm, thus testing its generality (Experiment 6). Experiment 7 addresses the nature of $n-2$ repetition costs in both overlap conditions over varying response-cue intervals (RCIs), and also investigates a possible mechanism driving the observed faster RTs for temporally separated cues and targets (see also Druey & Hübner). In Experiment 8, we attempt a direct replication of Druey and Hübner's study. To anticipate the results, across all experiments we found no

evidence that $n-2$ repetition costs are modulated by cue-target overlap.

Experiment 6

The purpose of the first experiment was twofold: i) To directly test Druey and Hübner (2007)'s proposal of the dependence of cue-target overlap on observing $n-2$ repetition costs in a target detection paradigm, thus examining its generality. While, as mentioned above, there are published reports of $n-2$ repetition costs without temporal overlap of cue and target, these studies do not examine whether such overlap affects the magnitude of these costs. ii) To investigate whether the lack of $n-2$ repetition costs found in the transparently-cued conditions of Houghton et al. (2009) was due to the absence of temporal overlap of cue and target, rather than the absence of the need to translate the cue-target relationship. To do this we adapted Experiment 1 of Houghton et al. in which target visual icons were cued either transparently by visually similar icons (a condition within which we found no $n-2$ repetition costs), or non-transparently by words referring to the target feature (a condition that produces robust $n-2$ repetition effects). We crossed this contrast with whether the cues were allowed to temporally overlap with the target stimuli or not. If $n-2$ repetition costs are enhanced by temporal overlap then we should find larger effects in the overlap condition; in particular, such costs may appear where we have previously failed to observe them.

Using this paradigm allows two opportunities to measure any modulation of $n-2$ repetition costs with cue-target overlap: specifically, in the first instance such costs may now occur with the iconic transparent cues, a scenario that we have reported generates none; secondly, the overlap of cue and target may increase measures of $n-2$ repetition costs within a manipulation we have reported it to be present before, i.e. utilising the word cues (Houghton et al., 2009). Therefore this paradigm affords a powerful and critical test of Druey and Hübner's (2007) conclusions.

Method

Participants

28 undergraduates (23 female; mean age = 21.1 years) from Bangor University were recruited from the participant pool run by the school of psychology in exchange for partial course credit.

Apparatus & Stimuli

Stimuli were presented on a 17" CRT monitor, from a viewing distance of approximately 60cm, and responses were made on a standard QWERTY keyboard, both connected to an IBM-compatible PC. Stimulus display consisted of four ovals (6cm in height; 3 had widths of 2.3cm and one had width of 3.5cm) presented with one to the centre of each of the four quadrants on the screen (assigned randomly on each trial). Three of the ovals were potential targets whilst one was always present as a distractor. Each of the three targets had distinguishing features and the distractor was visually neutral from the targets (see figure 14). The target display was preceded by either a word cue or an iconic cue, dependent upon the current condition. In the word-cue condition, three linguistic cues were used: 'Border', 'Angled' and 'Shaded'. Only one cue appeared in each trial centred in the PC monitor in black Times New Roman 15 font on a white background. The iconic-cue condition utilised three rectangular cues. During presentation, each iconic task cue was held constant at 4cm in height, with a width of 1.4cm presented in greyscale shading centred on the PC monitor with a white background. Each experimental cue in the word and iconic cue blocks were paired to a relevant target. For the word-cue block, each cue verbally described the visual properties of the target it was paired with, whereas the iconic-cues displayed visual properties of the target it was paired with. These cue-target pairings remained fixed throughout the experiment.

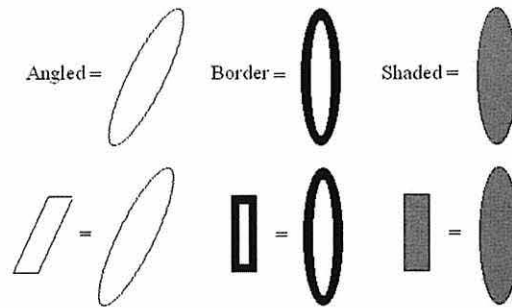


Figure 14. Cues and targets used in Experiment 6. Note, images are not to scale.

Procedure

The experimental session lasted approximately 30 minutes. Participants were presented with four blocks of 126 trials each: 1) word cues with temporally overlapping cues and targets; 2) word cues with no temporal cue-target overlap; 3) iconic cues with temporally overlapping cues and targets; and 4) iconic cues with no temporal cue-target overlap. Each experimental block was preceded by a practice block of 26 trials. Block presentation was counterbalanced across participants.

A trial began with a blank screen for 500ms, followed by either a word or iconic cue (dependent upon the current block). The cue was presented for 500ms followed by a cue-stimulus interval (CSI) of 250ms. In the no-overlap block, the CSI consisted of a blank screen; in the overlap block, the cue remained on the screen for the duration of the CSI. After the CSI, there followed the target display, with one oval to the centre of each quadrant of the screen. In the overlap block, the cue remained present in the centre of the screen; in the no-overlap block, only the targets were displayed. The participant's objective was to locate the target oval that was paired with the cue presented in the previous screen, and to respond to its location by pressing the appropriate key on the keyboard as quickly and as accurately as possible. Each quadrant of the screen was assigned a spatially related key on the keyboard, with which to respond when the correct target was identified. If the relevant target appeared in the top-left of the screen, participants were instructed to respond with the 'D' key; if it appeared in the top-right,

participants responded with the 'J' key; if it appeared in the bottom-left, the 'C' key; and if it appeared in the bottom-right of the screen, participants responded with the 'N' key. Responses were made with the index and middle finger of each hand, which were rested on the keys at all times. Incorrect responses generated no feedback from the program. After the response, the screen went blank for 500ms (response-cue interval — RCI), after which time the cue for the next trial appeared. No task repetitions occurred.

Design

The experiment manipulated three factors, each with two levels, in a fully-crossed, repeated measures design: *cue type* (icons vs. words), *cue-target overlap* (overlapping vs. non-overlapping) and *task sequence* (ABA vs. CBA). Error rates (%) and reaction time (RT) in milliseconds (ms) served as the dependent variables.

Results

The first 2 trials from each block were removed from data analysis. Trials in which an error occurred, and the 2 trials immediately following an error, were also excluded. Response times faster than 200 milliseconds (ms) or slower than 2,500ms were removed as outliers. Data trimming left 87.07% of the raw data to be submitted for further analysis

Trimmed reaction times (RT) and error data were submitted to separate three-way repeated measures Analysis of Variances (ANOVAS), with the factors as described above in *Design*. In the Error analysis only the main effect of cue type was reliable, $F(1,27) = 9.87$, $p < .01$, $\eta_p^2 = .27$, with more errors being committed with word cues (4.47%) than iconic cues (3.44%). Therefore, the results will focus on RT data. Means by condition, errors, and $n-2$ repetition costs, are shown in table 5.

In the RT analysis, all three factors produced significant main effects: *cue-type*, icon-cued trials ($M=501\text{ms}$; $SE=15\text{ms}$) were faster than word-cue trials

Table 5: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for ABA & CBA sequences in Experiment 6 across both cue types and cue-target overlap (CTO)

		Sequence				
		ABA		CBA		ABA-CBA
Cue		RT	Errors	RT	Errors	
Icons	CT Overlap	520 (93)	3.25	519 (78)	3.46	1
	No CT Overlap	481 (95)	3.16	483 (96)	3.90	-2
Words	CT Overlap	636 (108)	4.04	614 (96)	4.59	22
	No CT Overlap	584 (98)	4.97	564 (91)	4.29	20

($M=599$; $SE=17$), $F(1,27) = 64.99$, $p < .001$, $\eta_p^2 = .71$; cue-target overlap, RTs in the non-overlapping condition ($M=528$; $SE=16$) were faster than the overlapping condition ($M=572$; $SE=16$), $F(1,27) = 19.96$, $p < .001$, $\eta_p^2 = .43$; task sequence, ABA sequences ($M=555$; $SE=16$) produced slower RTs than CBA sequences ($M=545$; $SE=15$), $F(1,27) = 6.05$, $p < .05$, $\eta_p^2 = .18$.

The main effects were accompanied by a significant interaction of cue-type and sequence type, $F(1,27) = 12.13$, $p < .01$, $\eta_p^2 = .31$. This interaction was investigated using planned comparison paired t-tests, which showed that $n-2$ repetition costs were only apparent for word cues: ABA sequences ($M=610$; $SE=18$) were significantly slower than CBA sequences ($M=589$; $SE=16$), $t(27)=3.39$, $p < .01$. For iconic cues, ABA sequences ($M=500$; $SE=16$) did not differ from CBA sequences ($M=501$; $SE=15$), $t(27) = -0.13$, $p = .8$, thus replicating the findings of our earlier work (Houghton et al., 2009). Of most

importance, the overlap factor did not interact with sequence type, nor was there a three-way interaction of overlap, sequence and cue. In summary, $n-2$ repetition costs were only found when targets were cued verbally, and this was not affected in any way by whether cues overlapped with targets or not (table 5).

*Bayes Factor Analysis*¹⁰

As the key result of this study is the lack of an interaction between cue-target overlap and $n-2$ repetition costs (i.e. a null result), some alternative analysis is required. Statistical null-hypothesis significance testing (NHST) does not allow researchers to provide evidence for a null hypothesis (Gallistel, 2009), rather they can only be rejected. Failure to reject a null hypothesis during NHST is not evidence in favour of the null result, rather it is seen as a state of ignorance (Rouder, Speckman, Sun, Morey, & Iverson, 2009).

A solution to this problem has been recommended by Rouder et al. (2009) using Bayes factors which allow evidence to be assessed both in favour of the null and the alternative hypothesis. The Bayes factor that is calculated compares the probability of the obtaining the observed data given the null hypothesis with the probability of obtaining the observed data given the alternative hypothesis. The resulting Bayes factor statistic (denoted B_{01}) is an odds-ratio, which is directly interpretable in terms of the odds of the null being true compared to the alternative.

For this analysis I only used the word-cue data, as no $n-2$ repetition costs were found for icon-cues. To calculate the Bayes factor to investigate the null effect of cue-target overlap on $n-2$ repetition costs, I first calculated individual $n-2$ repetition costs for all participants across both levels of overlap. This allows comparison of $n-2$ repetition costs across both levels of overlap with a paired t-test (which is one of the two possible statistics that can be used to calculate the Bayes factor).

Results of the analysis produced a t-statistic of 0.17. The calculation of the

¹⁰This analysis was not included in Grange and Houghton (2009)

Bayes factor analysis requires the t-statistic and sample size¹¹. Results showed $B_{01} = 6.76$, which suggests that the null hypothesis is almost 7 times more likely given the observed data than the alternative hypothesis. This result provides positive support for the null hypothesis that there is no interaction of cue-target overlap on $n-2$ repetition costs.

Discussion

The results from Experiment 6 clearly challenge the proposal that specific inhibition of a recently abandoned task during task-switching is dependent upon the cue and target overlapping temporally, as suggested by Druey and Hübner (2007). Additionally, $n-2$ repetition costs were only present when some degree of translation was required between the cue and the target it represented (i.e. the word cue condition), thus replicating our recent work (Houghton et al., 2009). If task specific BI depended on cue-target overlap alone, then we should have observed $n-2$ repetition costs for the iconic-cues within this manipulation; this was not the case (see also Altmann, 2007; Kuhns et al., 2007; Sinai et al., 2007). Our paradigm is a powerful test of the effect of overlap on $n-2$ repetition costs in that it allows two scenarios within which their modulation with temporal cue-target overlap can appear: it was possible that $n-2$ repetition costs may have appeared where we had previously failed to find it with the iconic cues; secondly, within the word cue manipulation (a scenario where we have found $n-2$ repetition costs previously; Houghton et al., 2009) $n-2$ repetition costs may have been greater when the cue and target overlapped. However, both of these scenarios failed to find any modulation with overlap. Druey and Hübner's suggestion that both irrelevant tasks are inhibited when the cues and target are separated cannot explain our findings, and as such we suggest that task-specific BI does occur in cases of overlapping and non-overlapping cues and targets.

It remains to address possible reasons why Druey and Hübner (2007) did not

¹¹The values are entered into an online calculator, which can be found here: <http://pcl.missouri.edu/bayesfactor>

find such a $n-2$ repetition effect when the cue and target did not overlap temporally. An important difference is that Druey and Hübner used a relatively large RCI of 1,000ms, whereas we used one of 500ms. It has been shown that the $n-2$ repetition cost decreases as the RCI increases (Mayr & Keele, 2000). Gade and Koch (2005) proposed that this is due to decay of the activation levels of competing tasks during the RCI, which in turn reduces the level of inhibition required to fully suppress them. The extended RCI may have altered the as yet unexplored dynamics of activation levels and subsequent inhibition between cue overlap conditions. For Experiment 7 we addressed this question by repeating the word cue condition from Experiment 6, with the addition of manipulating the RCI between blocks. If our finding of $n-2$ repetition costs with separated cues and targets was specifically caused by a peculiarity tied to shorter RCIs, then we should not find such costs at longer response cue intervals. This manipulation is also important theoretically, as to our knowledge no study has manipulated the RCI with temporally separated cues and targets in a BI paradigm.

A further change was made for Experiment 7 to address a possible cause for the reduced RTs for non-overlapping cues and targets (see also Druey & Hübner, 2007). Druey and Hübner suggested possible reasons for this reduction, including the need to divide attention between cue and target in the overlapping condition, and a possible rechecking process to activate the correct task. Another possible cause is that the removal of the cue before target onset encourages advanced task preparation, as subjects cannot simply wait until the target arrives before deciding what they should do, as they can when the cue remains visible throughout a trial (DeJong, 2000; Verbruggen et al., 2007). Such higher-level influences are feasible, but a low-level perceptual difference between overlapping and separated conditions may also affect performance. The target display within our paradigm and Druey and Hübner differs significantly between the two overlap conditions at a perceptual level, as there is no cue visible in the separated condition. This low-level difference may be significant, as it has been shown in the perception

literature that saccades (and spatially compatible manual responses) to peripheral targets are speeded if a central fixation stimulus is removed 200ms before target onset as compared to when the fixation remains visible throughout a trial. This "Gap-effect" (Gómez, Atienza, Vázquez, & Cantero, 1994) has, among other mechanisms, been attributed to the relative ease of disengaging covert attention from the centre of a display to the peripheral target in gap conditions (Fischer & Weber, 1993) resulting in so-called express-saccades. This "attentional release" is especially relevant in our paradigm as participants must disengage covert attention from the central cue to search for peripheral targets. Therefore, in Experiment 7 during the no-overlap condition, instead of presenting a blank screen in between the cue and target display, the cue was replaced with a mask consisting of a string of x's which remained visible during target display; thus the low-level features of target display are now matched between overlap conditions. If the speeded RTs in the no-overlap condition were due to the need for dividing attention between target and meaningful cue, a rechecking process, or enhanced preparation, then the presence of the mask should not affect the main effect of overlap. However, if the speeded RTs are caused by an "attentional release" generated by low-level presence of a central stimulus akin to the gap effect, the two overlap conditions should now produce equivalent RTs.

Experiment 7

Method

Participants

28 undergraduates (18 female; mean age = 24.1 years) from Bangor University were recruited from the same pool as experiment 1. None had participated in Experiment 1.

Apparatus & Stimuli

The apparatus, word cues, and target displays were identical to Experiment 6.

Procedure

In this experiment, Cue-Target Overlap (CTO, 2 levels) was crossed with Response-Cue Interval (RCI, 2 levels). The levels of CTO were as in Experiment 1 (overlap, no overlap); the levels of RCI were 100ms (short), and 900ms (long). This produced four conditions: (1) overlap, short; (2) overlap, long; (3) no overlap, short; (4) no overlap, long. The cue presentation times and CSI were unchanged from Experiment 1. However, during the CSI in the no overlap condition, the cue was replaced by a central string of 6 X's in the same font as the word cues. This mask remained present during target display. Each condition was presented in its own block of 130 trials, each block being preceded by a practice block of 26 trials. Block presentation was counterbalanced across participants, but with the constraint that overlap only changed from block 2 to block 3 (i.e. blocks 1-2 then 3-4; blocks 2-1 then 4-3; blocks 3-4 then 1-2; or blocks 4-3 then 2-1).

Design

The experiment manipulated three factors with two levels each in a repeated measures design: *RCI* (short vs. long); *cue-target overlap* (overlap vs. no overlap) and *task sequence* (ABA vs. CBA). All three factors were manipulated within participants. Error rates (%) and RT (ms) served as the dependant variables.

Results

Data trimming (see Experiment 6) left 92.2% of the raw data to be submitted to further analysis. Trimmed RT and error (%) were submitted to separate three-way repeated measures ANOVAs, with the factors as described in *Design*. There were no significant effects in the error rates, so the results section focuses on RT. Means by condition, errors, and $n-2$ repetition effects are shown in table 6.

There was a main effect of *RCI*, with RTs in the short RCI ($M=650$; $SE=31$) producing slower RTs than in the long RCI condition ($M=608$; $SE=30$), $F(1,27) = 16.94$, $p < .001$, $\eta_p^2 = .39$. There was also a main effect of *Task Sequence*, with ABA sequences ($M=640$; $SE=30$) producing slower RTs than CBA sequences

Table 6: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for ABA & CBA sequences in Experiment 7 across both RCIs and cue-target overlap (CTO)

RCI		Sequence				
		ABA		CBA		ABA-CBA
		RT	Errors	RT	Errors	
100ms	CT Overlap	672 (190)	3.07	644 (186)	2.54	28
	No CT Overlap	663 (154)	3.30	620 (146)	2.50	43
900ms	CT Overlap	612 (158)	1.95	608 (174)	1.99	4
	No CT Overlap	613 (158)	2.64	599 (155)	2.95	14

($M=618$; $SE=30$), $F(1,27) = 10.28$, $p < .01$, $\eta_p^2 = .28$. There was no main effect of *Overlap*, $F(1,27) = 1.09$, $p > .3$, showing that the masking slowed the RTs in the no-overlap conditions to be equivalent to the overlap conditions.

Task sequence interacted with RCI, $F(1,27) = 12.49$, $p < .01$, $\eta_p^2 = .32$, replicating the finding of reduced $n-2$ repetition costs at longer RCIs (Gade & Koch, 2005). This interaction was investigated using planned comparison paired t-tests. At the short RCI, ABA sequences ($M=668$; $SE=32$) were significantly slower than CBA sequences ($M=632$; $SE=31$), $t(27) = 3.86$, $p < .01$. At the long RCI, ABA sequences ($M=612$; $SE=29$) did not differ significantly from CBA sequences ($M=603$; $SE=30$), $t(27) = 1.43$, $p > .16$.

Most importantly for the current context, $n-2$ repetition costs were again not modulated by cue-target overlap, nor was any other interaction involving overlap significant.

Bayes Factor Analysis

Again, the Bayes factor was calculated to find support for the null hypothesis. Short RCI trials only were used, as this produced the greatest level of $n-2$ repetition cost, thus giving every opportunity to find an effect of overlap on these costs. The short RCI data was submitted to a 2 (overlap) \times 2 (sequence) repeated measures ANOVA, which produced no significant interaction, $F(1,27) = 0.37$. To calculate the relevant t -statistic for this interaction (i.e. comparison of $n-2$ repetition costs between overlap), the square-root of the F statistic was taken, giving $t(27) = 0.61$. Given a sample size of 28, $B_{01} = 5.73$, providing positive support that the null is almost 6 times more likely than the alternative hypothesis given the observed data.

Discussion

As in Experiment 6, we found no interaction of overlap on $n-2$ repetition costs. As a further test of the effect of cue-target overlap on BI, we manipulated the RCI, which showed the usual pattern of reduced $n-2$ repetition costs at longer RCIs for both overlap conditions (Gade & Koch, 2005), thus supporting our argument that backward inhibition is equivalent in nature in both overlap conditions.

Furthermore, we found that the speeded RTs in the no overlap conditions of Experiment 6 were likely due to low-level perceptual differences between target displays across overlap conditions, with the presence of a central stimulus in the overlap condition perhaps impeding attentional disengagement from cue to target display, an effect akin to the gap effect (Fischer & Weber, 1993; Gómez et al., 1994). Such an effect presents more evidence against the hypothesis that cue-target overlap can affect high-level mechanisms, such as BI (Druey & Hübner, 2007).

Across two experiments we have demonstrated that the conclusions of Druey and Hübner (2007) do not generalise to our target detection paradigm. For purposes of clarification whether the failure to generalise these conclusions is specific to our paradigm, or a more general replication failure, for Experiment 8

we sought to replicate the relevant aspects of Druey and Hübner's (2007) original design of their Experiment 1.

Experiment 8

Method

Participants

28 participants (18 female; mean age = 23.4years) from Bangor University were recruited from the same pool as Experiments 6 and 7; none had participated in either of these experiments.

Apparatus & Stimuli

Stimuli were presented and responses collected by the apparatus from Experiment 6. The task stimuli were the numbers 1 to 9, excluding 5, and were presented centrally in white on a black background. Task cues were shapes, with one cue per task: square, diamond, and a circle (each approximately 6cm in height and width). Shapes were also presented in white.

Procedure

Participants were asked to classify the central digit according to one of three possible tasks, dependant upon the cue presented on the current trial: a square indicated a parity judgement (odd/even), a diamond indicated a magnitude judgement (higher/lower than 5), and the circle indicated a position judgement (position on the number line of 1 to 9: central [3,4,6,7]/peripheral: [1,2,8,9]). Of the four possible blocks in Druey and Hübner (2007), we only manipulated cue-target overlap. In their Experiment 1 they also manipulated whether the stimuli were spatially integrated within the cue (i.e. central) or non-integrated (i.e. one stimulus either side of the cue). Results found significant $n-2$ repetition costs only with centrally placed, spatially integrated stimuli. Therefore we utilised integrated, central, stimuli throughout. This is also the strategy that Druey and Hübner used for their Experiment 2.

Responses were made on the QWERTY keyboard, using two keys: the 'V' key was pressed for "even", "less than 5" and "central", and the 'N' key was pressed for "odd", "higher than 5" and "peripheral" responses.

A trial began with the presentation of a central cue, selected randomly with the restriction of no task-repetitions being allowed. In the no-overlap condition, the cue was presented for 400ms, before being removed for 100ms (blank screen) after which the stimulus appeared on its own. The stimulus remained onscreen until a response was registered, after which a blank screen was presented for 1,000ms (response-cue interval). In the overlap condition, the cue was presented for 500ms, after which time the stimulus appeared centrally within the cue. The cue remained onscreen during stimulus display. Both cue and stimulus disappeared after a response was registered. The RCI was also 1,000ms for this condition.

Cue-target overlap was manipulated between blocks in the same experimental session, with the order of condition presentation counterbalanced across participants. Druey and Hübner (2007) repeated the experimental procedure over two sessions for each participant. However, all BI studies to date have only utilised one experimental session for each participant, so in order to simplify generalisation, we only ran one session for each participant. Additionally, across just one session there are ample trials per $n-2$ condition (approximately 190 per overlap condition). For each overlap condition, participants were first exposed to two practice blocks of 48 trials each, after which four experimental blocks were presented, consisting of 96 trials each. The experimental session lasted approximately 60 minutes.

Design

The experiment was a 2x2 repeated measures design, with the factors *cue-target overlap* (overlap vs. no-overlap), and *sequence* (ABA vs. CBA). Reaction time (ms) and error rates (%) served as the dependant variables.

Table 7: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for ABA & CBA sequences in Experiment 8 across cue-target overlap (CTO)

	Sequence				
	ABA		CBA		ABA-CBA
	RT	Errors	RT	Errors	
CT Overlap	865 (205)	3.92	858 (195)	3.23	7
No CT Overlap	838 (221)	4.13	837 (222)	3.59	1

Results

The first two trials from each experimental block were removed, as were errors and trials following errors. The RT trimming differed to that of Experiments 6 and 7 to align with analysis of Druey and Hübner (2007), which consisted of removing the fastest 5% and the slowest 5% of RTs from each condition (R. Hübner, personal communication, August 29, 2008). Trimmed RT and error (%) were submitted to separate two-way repeated measures ANOVAs, with the factors as described in Design. Means by condition, errors, and $n-2$ repetition costs are shown in table 7.

Error rates showed no main effect of *overlap*, $F(1,27) = 1.05$, $p > .3$. There was a main effect of *sequence*, $F(1,27) = 4.49$, $p < .05$, $\eta_p^2 = .14$, with more errors being committed on ABA sequences (4.02%) than CBA sequences (3.41%). Critically, there was no interaction of overlap on sequence, $F(1,27) < 1$, $p > .7$.

RT analyses showed no main effect of *overlap*, $F(1,27) = 0.4$, $p > .53$ or *sequence*, $F(1,27) = .35$, $p > .55$. Most importantly in the present context, cue-target overlap did not interact with task sequence, $F(1,27) = 0.33$, $p > .56$. For the overlap condition, the $n-2$ repetition cost was 7 ms, $t(27) = 0.77$, $p = .45$; for the non-overlap condition, the cost was 1ms, $t(27) = 0.2$, $p = .85$. To investigate

the null interaction further, we calculated individual $n-2$ repetition costs for each participant for both cue-target overlap manipulations. If cue-target overlap does produce larger $n-2$ repetition costs, then we would expect to see this pattern within the individual $n-2$ repetition scores, even though the group means did not reach significance.

However there was no clear pattern of overlap affecting the $n-2$ repetition costs. Nine participants showed positive $n-2$ repetition costs in both overlap conditions. Regarding the interaction of overlap on the $n-2$ repetition costs, 13 participants showed numerically greater $n-2$ repetition costs for overlapping cues and targets than for separated cues and targets as would be expected by the hypothesis of Druey and Hübner (2007). However, 13 participants showed a trend in the opposite direction, with numerically greater $n-2$ repetition costs for separated cues and targets than for overlapping cues and targets. Such distributional trends cannot be accounted for by the hypothesis proposed by (Druey & Hübner, 2007).

Bayes Factor Analysis

Analysis to calculate B_{01} was similar to Experiment 7, in that I used the F statistic of the interaction to obtain a t-statistic, which was $t(27) = 0.57$. This provides a $B_{01} = 5.86$, again providing positive support for the null hypothesis, it being almost 6 times more likely than the alternative hypothesis.

Discussion

The present experiment aimed to directly replicate that of Druey and Hübner (2007) to determine if the findings from Experiment 6 and 7 indicated a failure to generalise their findings to a new paradigm, or a more general replication failure. However, we failed to replicate their critical results. Indeed, the finding of no $n-2$ repetition cost for RT in either overlap condition suggests that the experimental procedure used does not produce reliable BI effects. If the procedure does not produce such reliable 'baseline' BI effects, then any manipulation added to it

(such as cue-target overlap) is susceptible to producing effects that are equally unreliable.

It is possible that our replication failure may be due to only exposing participants to one experimental session, whereas Druey and Hübner (2007) used two. We only used one session as this has been the norm within the BI literature to date, and as such we wanted to retain consistency allowing generalisation to this literature. It remains possible that one can obtain the interaction of Druey and Hübner under such narrow, albeit atypical, conditions. We see no theoretical reason why adding a second session should introduce this interaction, although this may be an avenue for future research.

The finding of no interaction of overlap on task sequence here suggests that our findings from Experiment 6 and 7 are more than a generalisation failure to other paradigms. This experiment, taken together with the reported findings of significant $n-2$ repetition effects with no cue-target overlap from other laboratories, strongly supports our assertion that cue-target overlap is not essential to observe $n-2$ repetition costs in task-switching.

General Discussion

The present set of results has directly challenged the proposal of Druey and Hübner (2007) that the cue and target must be co-present in order to observe task-specific inhibition (BI, as measured by $n-2$ repetition costs). Experiment 6 found $n-2$ repetition costs for both temporally overlapping and non-overlapping cues and targets. Furthermore, we extended our previous findings of no $n-2$ repetition costs with maximally transparent cue-target relationships in cases of temporally overlapping cues and targets (Houghton et al., 2009). Experiment 7 demonstrated that $n-2$ repetition costs showed the same pattern of being reduced at longer RCIs (Gade & Koch, 2005). Experiment 8 attempted a direct replication of critical features of their paradigm, but again the $n-2$ repetition cost was not modulated by cue-target overlap. Taken together we suggest that the difference in experimental design is not the cause of our failure to generalise Druey and

Hübner's findings to a new paradigm, and that the necessity of cue-target overlap for task-specific BI to occur can be seriously called into question.

Additionally we have taken steps in addressing the possible mechanisms behind faster RTs in no overlap conditions. Experiment 7 suggests that the speeded RTs are possibly due to passive low-level differences between overlap displays, and thus "release" of attention from a central stimulus thus facilitating redeployment of attention to the target display. Such a result suggests that the facilitation in no overlap conditions is not caused by higher level functions such as rechecking or advanced preparation. However, it is slightly problematic for this line of reasoning that our replication of Druey and Hübner's (2007) study found no main effect of overlap; however, we note that there was a slight trend for faster RTs in the no-overlap condition (table 7). One difference is apparent between the presented paradigms that may account for this, that being our target-detection paradigm requires a shift of attention from a central cue to peripheral stimuli (as in saccade tasks producing the "Gap-effect" Fischer & Weber, 1993; Gómez et al., 1994), whereas Druey and Hübner's (2007) paradigm requires withdrawing attention from the outline of a larger central cue to a smaller central stimulus, which could be considered withdrawing attention from a peripheral cue to a central target (see also Verbruggen et al., 2007). Future research might attempt to manipulate the direction of attentional disengagement empirically to determine its necessity to observe speeded RTs.

In conclusion, the absence of modulation of $n-2$ repetition costs and the finding of equivalent RTs when low-level perceptual differences are controlled provide strong evidence against the hypothesis of Druey and Hübner (2007) that temporal cue-target overlap influences such a high-level mechanism as backward inhibition.

Chapter Summary

This chapter (and the previous) served to address alternative explanations of the lack of observable $n-2$ repetition costs when cue-target relationships are maximally transparent. In the current chapter, no $n-2$ repetition costs could have been caused by a lack of cue-target overlap. Thus, taken together, chapters 3 & 4 provide converging evidence that the absence of $n-2$ repetition costs for transparent cues is caused by reduced conflict during cue-target translation.

Since the publication of this chapter, one other study has addressed the role of cue-target overlap in backward inhibition. Mayr (2009, footnote 1) also found no modulation of $n-2$ repetition costs. However, this issue may warrant more attention as it is often desirable to remove the cue before target onset so as to force participants to engage in advanced preparation for the task (DeJong, 2000; Verbruggen et al., 2007). It is not entirely clear why Druey and Hübner (2007) found the pattern of results they did, and why we (Grange & Houghton, 2009) and Mayr (2009) have been unable to detect an effect. Aside from the issues raised in the general discussion to this chapter, I am unable to offer any alternative explanation.

The work presented thus far has highlighted the dominant role cue-transparency has in generating $n-2$ repetition costs. However, a BI design such as this requires a switch of relevant target on every trial to allow sufficient ABA and CBA sequences for comparison. Therefore, with the current paradigm we are unable to ascertain the effects of cue-transparency on more "traditional" task-switching phenomena, such as the switch cost and its reduction with advanced preparation. The next section of the thesis focusses more on this issue, providing converging evidence on the role of WM representations in guiding task behaviour. Specifically, I use two cues per target to separate cue-encoding processes from task-switching processes (see general introduction; Logan & Bundesen, 2003). In doing so, I was able to contrast two competing hypotheses regarding cue-switch costs (where the task repeats, but the cue presented switches

from the one used on $n-1$), and show that cue-switch costs arise from cognitive control processes that establish a WM representation.

Chapter 5

Cue-Transparency & Switch Costs¹²

¹²Experiments 10 & 12 of this chapter have been published as Grange and Houghton (in press), and components are reproduced with permission of the copyright holder. Thanks to Erik Altmann, Katherine Arbuthnott, Kate Arrington, Bernard Hommel, and Iring Koch for their detailed and constructive comments on a previous version of this article. These Experiments were also presented at British Psychological Society 26th Cognitive Section Conference, September 2009, University of Hertfordshire, UK. by James A. Grange.

CHAPTER 5: CUE-TRANSPARENCY & SWITCH COSTS

Abstract

In the explicitly-cued task-switching paradigm, two cues per task allow separation of costs associated with switching cues from costs of switching tasks. Whilst task-switch costs have become controversial, cue-switch costs are robust. The processes that contribute to cue-switch costs are under-specified in the literature: They could reflect perceptual priming of cue properties, or priming of control processes that form relevant working memory representations of task demands.

The present paper manipulated cue-transparency in an attention-switching design to test the contrasting hypotheses of cue-switch costs, and shows that such costs emerge from control processes of establishing relevant working memory representations, rather than perceptual priming of the cue itself. When the cues were maximally transparent, cue-switch costs were eradicated.

The results are discussed in terms of recent theories of cue-encoding, and provide a formal definition of cue-transparency in switching designs and its relation to working memory representations that guide task performance.

Literature Review & Introduction

A central element of cognitive control is the ability to switch from one task to another in an environment which affords both, for instance switching from using a word processor to an email program on a computer. Interest in the experimental study of task-switching has grown considerably in recent years. In a typical design, participants are required to shift frequently between two (sometimes more) tasks on stimuli that afford both (e.g. switching between making parity and magnitude judgements on numbers). In such designs, the currently relevant task is signalled by a valid pre-cue (e.g. the word "Magnitude"; Meiran, 1996, in press; Monsell, 2003). When participants have to switch tasks between trials (BA task sequence) they show increases in reaction time (RT) and errors compared to repeating a task (AA sequence). This difference is known as the switch cost, and is typically reduced at extended preparation intervals (Meiran, 1996; Rogers & Monsell, 1995, though see Altmann, 2004a; 2004b; Koch, 2001).

When the desired task is explicitly cued in this manner, a task-repetition (AA) implies a repetition of the previous task cue, while a task-switch implies a change from the previous cue. Consequently some or all of the switch cost may be due to differences in cue processing in the two conditions. To try to separate costs of switching cue from costs of switching task per se, investigators have used two cues per task (Logan & Bundesen, 2003; Mayr & Kliegl, 2003). In this case, three types of sequence are possible: *Cue-Repeat* (both cue and task repeat, e.g. Magnitude-Magnitude), *Cue-Switch* (task repeats, but cue switches, e.g. Magnitude-High/Low), and *Task-Switch* (both cue and task switch e.g. Magnitude-Parity). The "cue-switch cost" is measured by comparing performance on cue-switch trials with cue-repetition (in neither case does the task switch). Whilst cue-switch costs have been shown to be substantial (Logan & Bundesen, 2003; Mayr & Kliegl, 2003), task-switch costs (now measured as task-switch RT minus cue-switch RT) have become more controversial: Some studies find that task-switches show a cost over and above that which can be explained by

switching cues (Altmann, 2006; Arrington et al., 2007; Jost et al., 2008; Mayr & Kliegl, 2003; Monsell & Mizon, 2006) while others report that task-switches are no more costly than cue-switches alone (Arrington & Logan, 2004b; Logan & Bundesen, 2003, 2004; Logan & Schneider, 2006b; Schneider & Logan, 2005).

Research in the two-cues per task (hereafter 2:1 mapping) paradigm has been focussed on the presence (or lack thereof) of task-switch costs, and as such there has been a certain neglect of the factors that contribute to the cue-switch cost. In particular, Meiran (in press) has noted it is still an undecided issue whether the cue-switch cost itself reflects priming of control mechanisms (Arrington et al., 2007; Mayr & Kliegl, 2003, e.g. instantiating an active task representation in WM) or whether it reflects priming of perceptual cue properties (Logan & Bundesen, 2003; Schneider & Logan, 2005).

This is a very important issue, as in the former case results obtained with the paradigm remain directly relevant to the study of cognitive control, while in the latter case their importance is largely limited that of being a control condition.

The purpose of the present article is to investigate the factors contributing to the cue-switch cost further. Using an attention-switching design (Houghton et al., 2009), we provide evidence that cue-switch costs arise from priming of control processes that activate a representation of the relevant attentional target in WM, rather than from perceptual priming of the cue itself. Furthermore, we provide evidence of a behavioural dissociation between cue-switching and attention-switching. We first present a brief overview of relevant theories of cue encoding in task-switching, before describing in more detail the rationale and design of the current experiments.

Cue encoding

In the explicitly-cued task-switching paradigm, the cue must activate in working memory (WM) a representation that is sufficient to allow successful performance of the correct task, a process known as *cue encoding* (Altmann & Gray, 2008; Arrington et al., 2007; Logan & Bundesen, 2003; Logan & Schneider,

2006a, 2006b; Mayr & Keele, 2000; Mayr & Kliegl, 2003; Schneider & Logan, 2005). The nature of this representation is generally under-specified in the literature (though see Arrington et al., 2007), but is of theoretical importance if we are to understand how it can guide behaviour in multi-task environments.

Mayr and Kliegl (2000; 2003) have suggested that cue encoding requires retrieval of task rules from long-term memory (LTM) and installation into WM. Mayr and Kliegl (2000, Experiment 3) suggested that such rules are specific SR mappings (e.g., if stimulus is small animal then press left, if large animal then press right) rather than more abstract task specifications (e.g., respond according to stimulus size). In this model, cue-switch costs arise as a new cue requires use of a different retrieval path to obtain task rules from LTM; cue-repetition trials benefit of priming of retrieval route due to recency. Altmann and Gray (2008) suggest that cue encoding involves activating a task code in episodic memory (see also Altmann, 2002; 2003; 2004a; 2004b; Altmann & Gray, 2002). From this code, the meaning of the task is gleaned (i.e. "even/odd"); once stimulus onsets, the task meaning and the stimulus combine to retrieve the correct response.

To test the representations produced by cue encoding, Arrington et al. (2007) had participants make overt responses to the cues to try and separate the effects of cue encoding from the effects of processing the stimuli. The logic of the design assumes that cue and target processing are serial: If the type of response made to the cue is completed after successful cue encoding, then all cue-switch costs should only appear in cue-RT, with no cue-switch costs in target-RT. Therefore, one can assess the final representation gained from cue encoding by comparing cue responses that resulted in successful separation to cue responses that were not successful.

Across experiments, two cues were used for each task, and the type of response required for the cue varied between experiments, either indicating which cue was presented (i.e. a separate response for each cue, resulting in a 1:1 mapping of cues to responses) or which task was presented (one response for each

task, resulting in a 2:1 cue-response mapping). The results showed that with a 1:1 cue-response mapping, cue switch effects were still apparent in target RT, suggesting cue encoding had not been separated from target processing. However, a successful separation did occur when a 2:1 response-cue mapping was utilised, suggesting that cue encoding results in a semantic representation of the task to be performed, and not a representation of the cue itself. This finding supports the view that cue-switch costs reflect priming of the control processes of forming a semantic representation that guides task performance.

In contrast to the above literature, Logan and colleagues have proposed that when task-switches are explicitly cued no endogenous act of control is needed to change task (Logan & Bundesen, 2003; 2004; Logan & Schneider, 2006a; 2006b; Schneider & Logan, 2005; 2009). On every trial the participant encodes the cue (e.g. "Magnitude") and the stimulus (e.g. "8") jointly, employing them as a compound cue to retrieve the correct response (e.g. the cue Magnitude and the stimulus 8 retrieves the response High) by probing semantic, as well as episodic, memory (Arrington & Logan, 2004b). Repetition of cues on non-switch trials facilitates cue encoding due to the recency of cue traces in short-term memory, whereas when the cue switches, it must be encoded anew from long-term memory, causing a cost (Schneider & Logan, 2005). On this view, the cue-switch cost results from perceptual priming of the cue rather than priming of control processes (c.f. Arrington et al., 2007; Mayr & Kliegl, 2003).

The present study

The ability to distinguish between the control- and perceptual-priming hypotheses of cue-switch costs relies on implementing a paradigm that directly contrasts the two hypotheses' predictions. Specifically, the control-hypothesis suggests that cue switch costs arise from priming of the processes that enable forming a WM representation with a new cue, whereas the perceptual-hypothesis suggests it is merely the change in cue itself, regardless of the WM representation formed by it. Therefore, if the cues were to provide the relevant WM

representation directly, then during a cue-switch transition (in a 2:1 mapping paradigm), the control-hypothesis would predict there should be reduced (or no) cue-switch cost as no WM representation has to be generated endogenously. As such, there are fewer encoding processes to prime (either the route of long-term memory retrieval, e.g. Mayr & Kliegl, 2003, or the formation of a semantic representation of what to do, e.g. Arrington et al., 2007). In contrast, the perceptual-hypothesis claims that it is the change in cue itself that is important, irrespective of how informative it may be with respect to components of the target task. This latter variable is known as *cue transparency* (Arbuthnott & Woodward, 2002; Logan & Schneider, 2006a): transparent cues are clear as to their task meaning (e.g. "Odd/Even" for a parity task), whereas a non-transparent cue is less meaningful (e.g. the letter "G" for a parity task).

To test the competing hypotheses, we utilised a target detection paradigm based on that used by Houghton et al., (2009; see also Mayr & Keele, 2000), wherein participants are required to search for - and respond to the location of - a target oval among a set of four ovals which each differ in some visual property. This paradigm measures the effects of switching attention between WM representations in isolation of switching between differing response processes (as they stay the same throughout). The target on a given trial is determined by a valid cue, which is either a word that describes its distinctive property (e.g. "Border" to signal an oval with a thick border), or an iconic shape, that displays the visual property to search for (e.g. a rectangle with a thick border). In this paradigm, we consider the iconic cue to be maximally *transparent*, as it provides the necessary WM representation (target description) directly. In contrast, the word cue is relatively *non-transparent*, as it in no way resembles the target and serves only to constrain the endogenous activation of the relevant visual target in WM. We should note here that, in the task-switching literature, words having a pre-existing (LTM) semantic relationship to the task have been considered the most transparent type of cue (e.g. Arbuthnott & Woodward, 2002; Logan &

Schneider, 2006a). However, we use the term transparency to refer to the degree to which the cue provides the relevant target information, and as such our iconic cues are *relatively* more transparent than the word cues (as they look like the target). We return to the issue of cue transparency and its meaning in the general discussion.

The distinction between the control- and perceptual-hypothesis of cue-switch costs can be directly tested with this paradigm. During cue-switch transitions for word cues (e.g. Border - *Outline*), both hypotheses would predict a cue-switch cost. On the control-hypothesis, a WM representation has to be formed endogenously based on the new cue (Arrington et al., 2007; Mayr & Kliegl, 2003), and on the perceptual-hypothesis the new cue is perceptually different from the previous cue. In contrast, for the iconic cues (e.g. bordered rectangle - *bordered triangle*), the perceptual-hypothesis would predict a cue-switch cost due to their different perceptual properties, while the control-hypothesis would predict no such cost, as no endogenous formation of a WM representation needs to be instantiated.

This distinction is important theoretically as each hypothesis suggests something different about how participants deal with cues in task-switching environments: the perceptual-hypothesis suggests that all a participant requires is a perceptual representation of the cue, which combines with a perceptual representation of the stimulus, being jointly used to retrieve the correct response (Schneider & Logan, 2005). The control-hypothesis suggests that it is an active representation over and above that of the cue that guides behaviour (e.g. semantic "rules", Arrington et al., 2007; Mayr & Kliegl, 2003). Therefore it is essential to investigate which is more likely to be the case, as an understanding of cue-encoding representations will constrain theories and models of task-switching performance.

Experiment 9

Before contrasting the two theories of cue-switch costs by adapting the paradigm of Houghton et al. (2009), it is important to first establish whether this

paradigm produces typical "task"-switching effects. Specifically, it is important to demonstrate the adapted paradigm of Houghton and colleagues produces switch costs that reduce given extended preparation intervals. Providing this foundations is important, as without it any effects of cue-transparency on cue-switch costs may be uninterpretable, due to uncertainty that this paradigm produces typical effects. Therefore for this experiment, I utilised word and iconic cues from Houghton et al., allowing immediate task-repetitions. Additionally, I manipulated the cue-stimulus interval to vary preparation time.

Method

Participants

17 participants were recruited from the same pool as previous experiments. One participant was removed due to session-wise accuracy below 90%.

Apparatus & Stimuli

All experiments were presented on a PC running E-Prime (Psychology Software Tools, 2002) with a 17" (43.18cm) CRT monitor. Viewing distance was approximately 60 cm. Responses were registered by a standard QWERTY keyboard. Stimulus display consisted of four ovals (6cm in height; three had widths of 2.3cm and one had width of 3.5cm), with one oval presented in each quadrant of the screen. Stimulus position was randomized on each trial. Three of the stimuli were always potential targets, and one was a neutral distractor. Each potential target differed from the others by means of a visual property. The target display was preceded by a valid cue, presented in the centre of the screen. The cues and stimuli were identical to that from Experiment 7 (icons and words).

Procedure

Word cues were presented in a separate block from iconic cues, and presentation order of cue manipulation was counterbalanced across participants. Participants were presented with three experimental blocks for each cue type, with

112 trials within each block. For sake of brevity, only the word cue condition will be described; the iconic cued condition was identical with the exception of the type of cue used.

A trial began with the presentation of the cue, which appeared in centre of the screen. After a variable cue-stimulus interval¹³ (CSI) of either 100ms or 900ms (randomly selected on each trial), the four oval target stimuli appeared with one centred in each quadrant of the screen. The cue remained in the center of the screen during target presentation to allow for very short CSIs (as otherwise the participant may not have seen the cue). The targets remained visible until a response from the participant had been registered. Response criteria was identical to Experiment 7. After a response had been registered, a blank screen served as a RCI, with a fixed duration of 1,000ms for both CSI types. After the RCI, the cue for the next trial appeared.

The relevant target (among a possible three) for each trial was selected randomly, with no constraints; thus $p(\text{task-switch})=.66$. Trials were categorised as "Switch" or "Repeat" post hoc based on the relevant target on the preceding trial.

Design

The experiment manipulated three independent variables within subjects: *cue-type* (icons vs. words), *CSI* (short vs. long) and *sequence* (switch vs. repeat). Mean RT (ms) and error rates (%) served as dependent variables.

Results

The 1st trial from each block, trials in which an error was committed, and trials immediately following an error were removed from data analysis. Response times faster than 200ms or slower than 2,500ms were also removed. Data trimming left 92.5% of the raw data to be submitted to further analysis. RT and errors (%) were submitted to separate three-way repeated measures ANOVAs,

¹³Typically, CSI refers to a blank screen inbetween presentation of the cue and the stimulus, and stimulus onset asynchrony (SOA) is used to describe stimulus onset if the cue remains on the screen. However, the task-switching literature typically uses the term CSI in place of SOA, which I also do in this chapter to retain consistency.

Table 8: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for Switch & Repeat sequences in Experiment 9 across both cue-types and cue-stimulus intervals (CSIs)

		Sequence				
Cue	CSI	Repeat		Switch		Switch cost ^a
		RT	Errors	RT	Errors	
Words	100ms	729	3.3	784	5.29	55
		(93)		(117)		
	900ms	567	2.8	561	3.2	-6
		(80)		(66)		
Icons	100ms	611	3.0	635	2.6	24
		(92)		(97)		
	900ms	524	2.3	530	3.1	6
		(90)		(77)		

^aSwitch cost = Switch RT - Repeat RT

with the factors as described above in *Design*. Mean RT, standard deviations, and errors are shown in table 8.

The RT analysis showed all three main effects were reliable: *cue-type*, iconic cued trials ($M=575$) were faster than word cued trials (660ms), $F(1,15) = 30.82$, $p < .001$, $\eta_p^2 = .67$; *CSI*, RT to the short CSI (690ms) was slower than to the long CSI (545ms), $F(1,15) = 225.89$, $p < .001$, $\eta_p^2 = .94$; *Sequence*, RT to switch trials (628ms) were slower than to repeat trials (608ms), $F(1,15) = 8.02$, $p < .05$, $\eta_p^2 = .35$.

The cue-by-CSI interaction was also reliable, $F(1,25) = 18.3$, $p < .01$, $\eta_p^2 = .55$, which was driven by a greater reduction in RT at long CSI for word cues than for iconic cues (see table 8). There was also a sequence-by-CSI interaction, indicative of a reduction in switch cost at the longer CSI, $F(1,15) = 8.16$, $p < .05$, $\eta_p^2 = .35$. Surprisingly, the cue-by-sequence interaction was not reliable ($F < 1$), however the three-way interaction was marginally significant, $F(1,15) = 4.02$, $p = .06$, $\eta_p^2 = .21$.

The three-way interaction was investigated using planned comparison paired t-tests to investigate the switch cost at every CSI level for both cue types. For the word cued trials, the switch cost of 55ms at short CSI was reliable [$t(15)=2.71$, $p<.05$], but was not reliable at the long CSI ($t<1$). For iconic cued trials, the switch cost of 24ms at short CSI approached significance [$t(15)=2.07$, $p=.053$], but the comparison at long CSI was not reliable ($t<1$).

For the error data, there was a main effect of *cue-type*, with more errors being committed for word-cued trials (3.6%) than for iconic-cued trials (2.8%), $F(1,15) = 4.87$, $p<.05$, $\eta_p^2 = .25$. The main effect of *CSI* was also reliable, with more errors being committed at the short CSI (3.6%) than at the long CSI (2.9%), $F(1,15) = 4.81$, $p<.05$, $\eta_p^2 = .24$. The main effect of *sequence* was not reliable, and neither were any interactions.

Discussion

The present experiment served as a pilot study to ensure that the adapted paradigm of Houghton et al. (2009) produced typical task-switching effects. A robust switch cost was found, which decreased with longer preparation intervals, mirroring typical task-switching results (Logan, 2003; Monsell, 2003). Also of note was the (almost significant) difference in switch cost between the cue-types, being almost half for iconic cues. This effect is uninterpretable due to the confound of cue-switching with task-switching using just one cue per task in this experiment. The reduction could be due to smaller "true" task-switch costs (or, more correctly, attention-switch costs as our paradigm involves no switch in task; Mayr & Kliegl, 2003), or an effect of differential cue-priming between cue-types (Logan & Bundesen, 2003). Therefore, we now move on to the experiments addressing the cause of the cue-switch cost, as outlined in the introduction.

Experiment 10

This experiment is very similar to that of Experiment 9, except two cues per target are utilised to separate cue-switching from attention-switching processes.

Method

Participants

33 new participants were recruited from the same pool as previous experiments. One participant was removed due to session-wise accuracy below 90%.

Apparatus & Stimuli

All experiments were presented on a PC running E-Prime (Psychology Software Tools, 2002) with a 17" (43.18cm) CRT monitor. Viewing distance was approximately 60 cm. Responses were registered by a standard QWERTY keyboard. Stimulus display consisted of four ovals (6cm in height; three had widths of 2.3cm and one had width of 3.5cm), with one oval presented in each quadrant of the screen. Stimulus position was randomized on each trial. Three of the stimuli were always potential targets, and one was a neutral distractor. Each potential target differed from the others by means of a visual property. The target display was preceded by a valid cue, presented in the centre of the screen.

Dependent upon the condition, the cue was either a word or an iconic shape. For the word cue condition, one of six possible cues were used: "Angled", "Slanted", "Border", "Outline", "Shaded", or "Filled", and were presented in black Times New Roman with a font size of 15. The iconic cues consisted of three rectangular shapes, and three triangular shapes. Rectangular cues were presented with a height of approximately 4cm and a width of 1.4cm; triangular cues were displayed with a height of 4cm and a width of 2cm. Only one cue was presented on each trial.

All cues and targets were presented in grayscale shading on a white background. Two cues were paired to each of the three targets in both the word- and iconic-cue conditions: in the word cue condition, the cue verbally described the characteristic of the target to search for, whereas in the iconic cue condition, the visual property of the relevant target was apparent in the cue (see figure 1 15).

Cue-target pairings remained constant throughout the experiment.

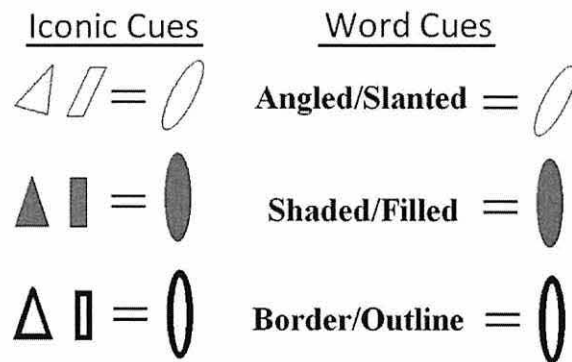


Figure 15. Cues and targets used in Experiment 10. Note, images are not to scale.

Procedure

Word cues and iconic cues were presented in separate blocks, with block presentation counterbalanced across participants. Each block consisted of 550 trials (split into smaller 110 trial sub-blocks with a self-paced rest screen between each), preceded by a practice session consisting of 26 trials.

A trial began with a central cue, followed by the target display. The cue-stimulus interval (CSI) was either 100ms or 900ms, with equal probability. During target display, the cue remained visible in the centre of the screen, with one oval in each quadrant of the screen surrounding the cue. The participant was required to locate the oval target that was paired with the presented cue, and make a spatially compatible response to its location utilizing one of four keys: if the relevant target was in the top left quadrant, the "D" key served as the correct response; if the target was in the top-right, "J" was the correct response; if the target was in the bottom left, "C" was correct; and if in the bottom right quadrant, "N" was correct. Responses were asked to be made using the index and middle finger of each hand, which were rested on the keys at all times. Participants were asked to respond as quickly and as accurately as possible. After a response was registered by the program, the screen went blank for a fixed time of 1,000ms, which served as the response-cue interval (RCI). A fixed RCI with varying CSI

also varies the response-stimulus interval (RSI) from trial to trial. However, it has been shown that RCIs of 1,000ms or more allow for any carry-over effects from the previous trial to dissipate (Meiran, et al., 2000; see also discussion in Logan, Schneider, & Bundesen, 2007). After the RCI, the cue for the next trial appeared.

Target and cue selection was manipulated in a pseudo-random fashion in the following manner. The program first selected the relevant target for the current trial with the probability of a switch from the previous target manipulated between subjects. One group of 16 received a target-switch probability of .33 whilst the other group received a probability of .5¹⁴. After the target was selected, the cue was chosen with an equal probability for both possible cues.

Trials were organized into three types of sequences post-hoc based on a comparison of the relevant cue and target on the previous trial with the cue and target on the current trial: *Cue-Repeat* (both target and cue repeats, e.g. Border - Border); *Cue-Switch* (target repeats, but cue switches, e.g. Border - Outline); and *Attention-Switch* (both cue and target switches, e.g. Border - Angled).

Design

The experiment manipulated three independent variables in a three-way repeated measures design: *cue-type* (icons vs. words), *CSI* (short [100ms] vs. long [900ms]), and *sequence* (cue-repetition vs. cue-switch vs. attention-switch). Error rates (%) and reaction time (RT) in milliseconds (ms) served as dependent variables.

Results

Data trimming (see Experiment 9) left 93.7% of the raw data to be submitted to further analysis. Reaction time (RT) and errors (%) were submitted to separate three-way repeated measures ANOVAs, with the factors as described above in

¹⁴This manipulation was included because some researchers have suggested that the task-switch cost is modulated by the probability of a task switch (Monsell & Mizon, 2006). Analysis for Experiment 10, 11, and 12 showed that switch probability had no main effect or interactions with any other factor for either RTs or errors. Therefore for the analysis in this article we collapsed across probability, without further discussion.

Design. Mean RT, standard deviations, and errors are shown in table 9 (see also figure 16¹⁵).

RT Analysis

RT analysis showed all three main effects were significant: *cue-type*, RT to iconic cued trials ($M=567$) was significantly faster than to word-cued trials ($M=665$), $F(1,31) = 59.99$, $p < .001$, $\eta_p^2 = .66$; *CSI*, RT to the short CSI ($M=689$) was slower than RT to the long CSI ($M=542$), $F(1,31) = 446.45$, $p < .001$, $\eta_p^2 = .94$; *sequence*, RT to cue-repetitions ($M=584$) was faster than to cue-switches ($M=614$), which in turn was faster than attention-switches ($M=650$), $F(2,62) = 69.2$, $p < .001$, $\eta_p^2 = .69$. Planned contrasts showed that the cue-switch contrast [i.e. cue-repetition vs. cue-switch RT; $F(1,31) = 36.07$, $p < .001$, $\eta_p^2 = .54$] and the attention-switch contrast [i.e. cue-switch vs. attention-switch; $F(1,31) = 45.6$, $p < .001$, $\eta_p^2 = .6$] were both significant.

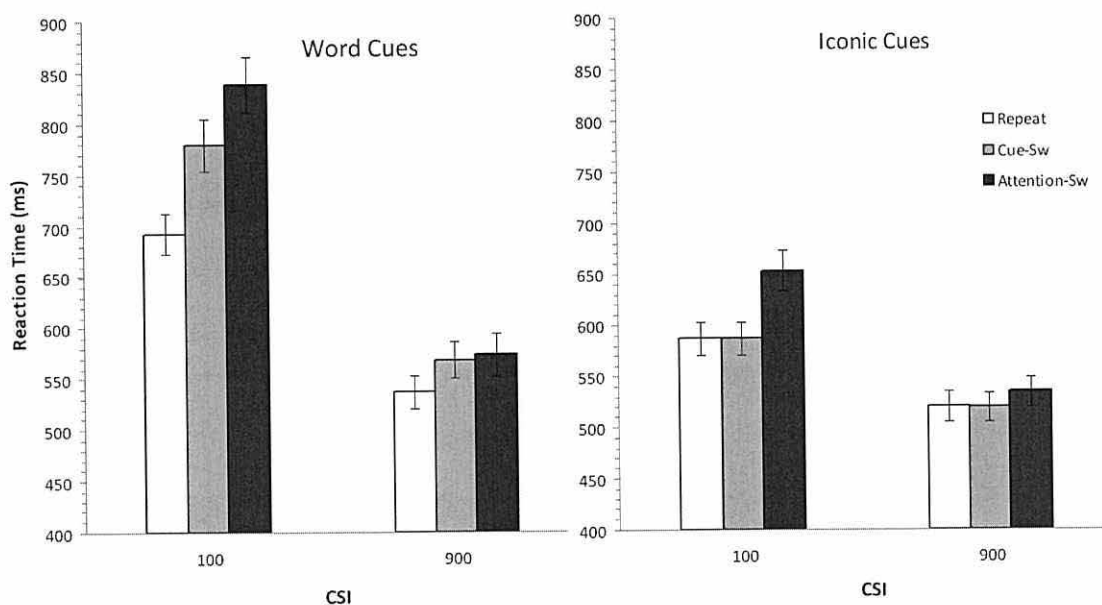


Figure 16. Mean reaction time for all sequences across both cue-types from Experiment 10. Note error bars denote one standard error around the mean.

The CSI-by-sequence interaction reached significance, $F(2,62) = 45.61$,

¹⁵Despite the apparent redundancy of a figure with the presence of a detailed table, the higher-level interactions are easily interpretable from the graph.

Table 9: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 10 across both cue-types

		Sequence							
		Cue-Repeat		Cue-Switch		Attention-Switch			
Cue	CSI	RT	Error	RT	Error	RT	Error	C-sc ^a	A-sc ^b
Words	100ms	692	2.22	779	3.24	838	5.74	97	59
		(113)		(145)		(151)			
	900ms	537	2.26	569	3.12	573	4.05	32	4
		(90)		(100)		(117)			
Icons	100ms	587	1.99	587	1.93	653	4.33	0	66
		(88)		(89)		(115)			
	900ms	520	2.54	519	2.49	534	2.81	-1	15
		(83)		(79)		(85)			

^aC-sc = Cue-switch cost

^bA-sc = Attention-switch cost

$p < .001$, $\eta_p^2 = .6$ which reflected a reduction in cue-switch- and attention-switch-cost at longer CSIs. There was also a cue-by-CSI interaction, $F(1,31) = 136.25$, $p < .001$, $\eta_p^2 = .82$, indicative of a greater reduction in RT at longer CSIs for word cues than for iconic cues.

Of most interest to the current experiment, there was a cue-by-sequence interaction, $F(2,62) = 26.75$, $p < .001$, $\eta_p^2 = .46$. Planned contrasts investigated this interaction and showed that the cue-switch cost was greater in the word-cue condition (59ms) than in the iconic-cue condition (-1ms), $F(1,31) = 49.45$, $p < .001$, $\eta_p^2 = .62$. The magnitude of the attention-switch cost did not differ between cue types, with the attention-switch cost being 32ms for the word-cues and 41ms for the iconic cues, $F(1,31) = 1.66$, $p > .2$. The three-way interaction also reached significance, $F(2,62) = 10.29$, $p < .001$, $\eta_p^2 = .25$. This interaction reflected a greater reduction of the cue-switch cost for the word cues than for the

iconic cues at the long CSI compared to the short CSI, $F(1,31) = 16.42$, $p < .001$, $\eta_p^2 = .19$. The reduction in attention-switch cost was similar in magnitude between cue-types, $F(1,31) < 1$ (see table 9).

Error Analysis

Error analysis mostly reflected the RT analysis, with the following exceptions: the main effect of *CSI* was not significant, $F(1,31) = 1.96$, $p > .17$, nor was the cue-by-CSI interaction significant, $F(1,31) = 1.11$, $p > .3$. The three-way interaction also did not reach significance, $F(2,30) < 1$.

Discussion

The results from Experiment 10 are clear: whilst the cue-switch cost was significant in the word-cue condition, it was eradicated for the iconic cues, thus supporting the view that cue-switch costs reflect active control processes of forming a WM representation from a newly presented cue; the perceptual-hypothesis cannot explain these results. Additionally, Experiment 1 shows that maximal cue-transparency removed the cue-switch cost for iconic cues compared to word cues, but the attention-switch cost was equivalent between cue types. This clearly shows that cue- and attention-switching are dissociable, and thus arise from different underlying processes (c.f. Logan & Bundesen, 2003).

However, when a cue-switch trial occurred for iconic cues, the new cue is visually very similar to the cue on the previous trial. Thus, participants may be treating each pair of cues as just one cue, somehow ignoring its overall shape (e.g. if you see a bordered cue, *regardless of its shape*, search for the bordered target). This *cue-collapsing* (Monsell & Mizon, 2006) could explain the lack of cue-switch cost for iconic cues without appealing to control processes. We investigate this possibility in the next 2 experiments.

Experiment 11

To rule out cue-collapsing as a complete explanation of our finding in Experiment 10, we changed the iconic cues so that they did not have the visual features of the target to search for. However, each pair of cues that were paired to a certain target still looked similar to each other (see figure 17), so some degree of visual cue-collapsing could still occur (i.e. if cue is a rectangle *-regardless of orientation-* search for the angled oval), but the relationship between the pair of cues and the targets were of an abstract nature, that is they were non-transparent.

If our results from Experiment 11 were due to cue-collapsing, then we would expect to find reduced cue-switch costs again for iconic cues. If however the reduced cue-switch costs for iconic cues in Experiment 10 were due to no endogenous formation of a WM representation needing to be instantiated, the non-transparent nature of the iconic cues in Experiment 11 should require cue-target translation, thus mimicking the cue-switch cost found for word cues.

Method

Participants

33 new participants were recruited from the same pool as the previous experiments. One participant was removed due to session-wise accuracy below 90%.

Apparatus & Stimuli

The experiment utilized the same apparatus as Experiment 10. The word-cue condition remained identical to that of Experiment 10. However, for the iconic cues, the cue-target relationship was made more abstract for each pairing (see figure 17 for these pairings). A rectangle (either vertically or horizontally aligned) was used to cue the angled target, a square or diamond (rotated square) was used to cue the shaded target, and a triangle (regular or inverted) was used to cue the border target.

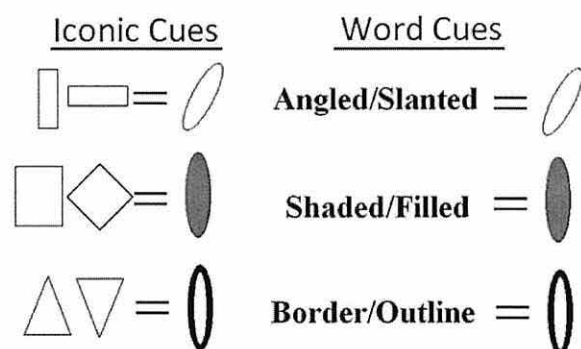


Figure 17. Cues and targets used in Experiment 11. Note, images are not to scale.

Procedure

The procedure of the experiment was identical to that of Experiment 10.

Design

The design was identical to Experiment 10.

Results

Data trimming (see Experiment 10) left 92.4% of the raw data to be submitted to further analysis. RTs and errors were submitted to separate three-way repeated measures ANOVAs, with the factors as described above in *Design*. Mean RT, standard deviations, and errors are shown in table 10 (see also figure 18).

RT Analysis

The main effect of *cue-type* did not reach significance, $F(1,31) = 2.13, p > .1$. There was however a main effect of *CSI*, with slower RTs to the short CSI ($M=814$) than the long CSI ($M=609$), $F(1,31) = 552.77, p < .001, \eta_p^2 = .95$. The main effect of *sequence* also reached significance, RT to cue-repetitions ($M=654$) was faster than to cue-switches ($M=704$), which in turn was faster than attention-switches ($M=777$), $F(2,62) = 86.67, p < .001, \eta_p^2 = .74$. Planned contrasts showed that the cue-switch contrast [$F(1,31) = 71.85, p < .001, \eta_p^2 = .7$] and the attention-switch contrast [$F(1,31) = 62.95, p < .001, \eta_p^2 = .67$] were both significant.

Table 10: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 11 across both cue-types

		Sequence							
		Cue-Repeat		Cue-Switch		Attention-Switch			
Cue	CSI	RT	Error	RT	Error	RT	Error	C-sc ^a	A-sc ^b
Words	100ms	716	2.88	822	2.79	874	5.79	106	52
		(130)		(152)		(172)			
	900ms	566	2.69	612	2.45	621	4.35	46	9
		(100)		(125)		(139)			
Icons	100ms	750	2.73	783	3.90	942	5.51	33	159
		(135)		(138)		(176)			
	900ms	584	2.61	598	3.33	672	3.83	14	74
		(106)		(113)		(159)			

^aC-sc = Cue-switch cost

^bA-sc = Attention-switch cost

The CSI-by-sequence interaction reached significance, $F(2,62) = 43.94$, $p < .001$, $\eta_p^2 = .59$ again reflecting a reduction of cue- and attention-switch costs at longer CSIs.

There was a cue-by-sequence interaction, $F(2,62) = 20.23$, $p < .001$, $\eta_p^2 = .4$. Planned contrasts investigated this interaction and showed that the cue-switch cost was greater in the word-cue condition (76ms) than in the iconic-cue condition (23ms), $F(1,31) = 32.5$, $p < .001$, $\eta_p^2 = .51$. Conversely, the attention-switch cost was greater for the iconic-cues (117ms) than for the word-cues (30ms), $F(1,31) = 35.0$, $p < .001$, $\eta_p^2 = .53$. The three-way interaction did not reach significance, $F(2,62) = 2.76$, $p > .07$, $\eta_p^2 = .08$.

This cue-by-sequence interaction was investigated further by collapsing across CSI and performing two One-Way ANOVAs (one for each cue type) with the factor *Sequence*. For word cues, the main effect was significant, $F(2,62) = 58.13$,

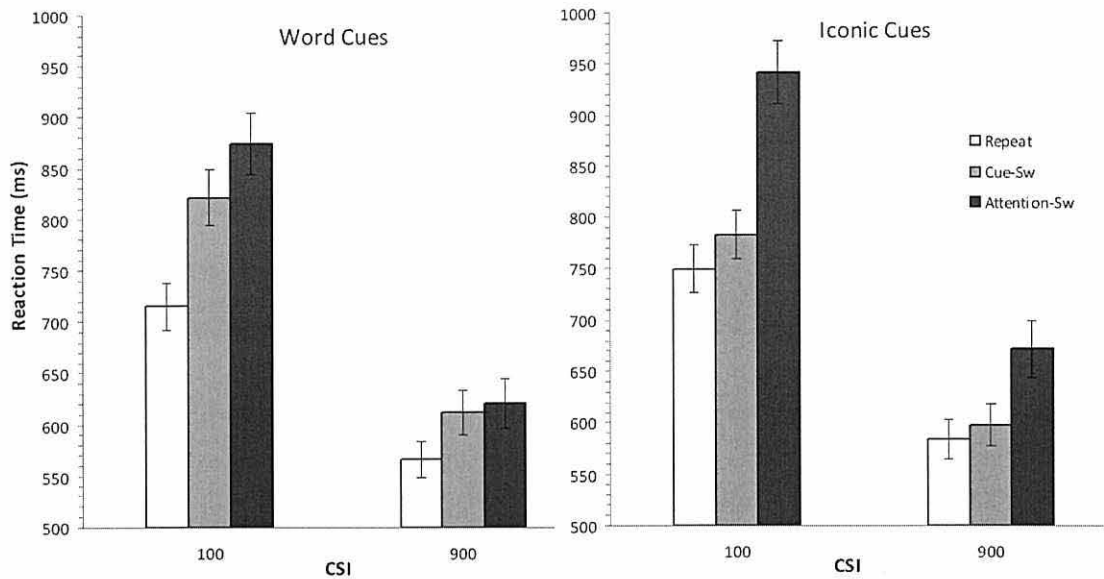


Figure 18. Mean reaction time for all sequences across both cue-types from Experiment 11. Note error bars denote one standard error around the mean.

$p < .001$, $\eta_p^2 = .65$, driven by significant cue-switch costs [$F(1,31) = 88.83$, $p < .001$, $\eta_p^2 = .74$] and attention-switch costs [$F(1,31) = 9.77$, $p < .01$, $\eta_p^2 = .24$]. For iconic cues, the main effect was also significant, $F(2,62) = 67.02$, $p < .001$, $\eta_p^2 = .71$, also driven by significant cue-switch costs [$F(1,31) = 11.67$, $p < .01$, $\eta_p^2 = .27$] and attention-switch costs [$F(1,31) = 73.7$, $p < .001$, $\eta_p^2 = .7$].

Error Analysis

Error rates largely reflected that of the RTs, with the exception that the cue-by-sequence interaction was only marginally significant, $F(2,62) = 2.74$, $p < .08$ and the three way interaction of cue, sequence, and CSI was not significant, $F(2,62) < 1$.

Discussion

The present experiment attempted to address whether the visual similarity between iconic cue-types in Experiment 10 lead participants to collapse each pair of cues into a single cue based on its low-level perceptual properties. The results suggest that when cue-target translation is required for iconic cues, some cue-switch cost is apparent even when participants are able to collapse across cues,

but that it was still much smaller than the cue-switch cost in the word condition, suggesting that some degree of cue collapsing can occur for visually similar cues (Monsell & Mizon, 2006; an idea similar to that of semantically related cues priming each other, Logan & Schneider, 2006b,). As different cues were used in the iconic cue conditions between Experiment 10 and 11, and given the presence of a cue-switch cost in Experiment 11 for icons that was smaller than for word cues, it is unclear whether Experiment 11 was successful at determining whether the reduction of cue-switch cost in Experiment 10 was due to cue-collapsing or removing the need to translate the cue into a WM representation. Therefore, for Experiment 12, we directly addressed the role of cue-collapsing for the iconic cues in Experiment 10.

Experiment 12

To rule out cue-collapsing as a complete explanation of our findings, for Experiment 12 we only utilized the iconic cues from Experiment 10 in two conditions. In the first condition, the *Matching Cues* condition, the iconic cues were paired with the same targets as in Experiment 10. Thus, in this instance the cue-target relationship is maximally transparent. However, in the second condition, the *Unrelated Cues* condition, the same iconic cues were now paired with shapes that did not share any visual properties with the cues. For example, if participants saw a shaded rectangle or a shaded triangle, they must search for the pentagon (see figure 19 for the cue-target pairings); thus the unrelated cues were non-transparent.

With this manipulation, we were able to ascertain the role of control processes generating cue-switch costs directly without the concern of cue-collapsing as an alternative explanation. It is important to highlight that participants were able to collapse cues equally in this experiment, as in both conditions the same cues were used. Therefore, if the reduction of cue-switch costs in Experiment 10 were due to cue-collapsing, we should find equally reduced cue-switch costs in both the matching- and unrelated-cue condition. If however the reduced cue-switch costs

for iconic cues in Experiment 10 were due to bypassing or reducing the need for endogenous control processes, the non-transparent nature of the unrelated cues in Experiment 12 should require activating a WM representation endogenously, thus mimicking the cue-switch cost found for word cues.

This experiment is also a more powerful test of the comparison between control- and perceptual-priming explanations of cue-switch costs, as a cue-switch trial now controls for perceptual differences (as identical cues are used).

Method

Participants

34 new participants were recruited from the same pool as the previous experiments. Two were removed for session-wise accuracy below 90%.

Apparatus & Stimuli

The experiment was presented on the same apparatus as Experiment 10. There was a change to the targets used for part of this experiment, but we utilized only the iconic cues from Experiment 10 in two conditions. In the first condition (*matching*), the targets were identical to those of Experiment 10, allowing a transparent relationship between cue and target. For the second condition (*unrelated*), the iconic cues were paired with geometrical shapes (octagon, pentagon, and a square) having no pre-experimental association with the cues (see figure 19 for the cue-target relationships). The new targets had an approximate height and width as that of the ovals, and throughout we utilized the same neutral oval from Experiment 10 as a distractor.

Procedure

The procedure of the experiment was identical to that of Experiment 10.

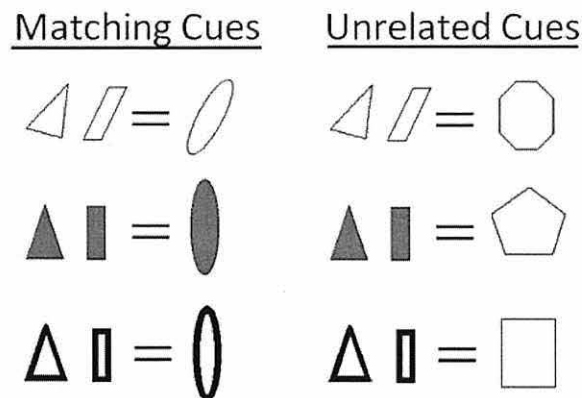


Figure 19. Cues and targets used in Experiment 12. Note, images are not to scale.

Design

The independent variables were identical to Experiment 10, except the factor *cue-type* now had the levels matching vs. unrelated.

Results

Data trimming utilised the same criteria as Experiment 10, and left 93% of the raw data to be submitted to further analysis, consisting of separate three-way repeated measures ANOVAs with the factors as described above in *Design*. Mean RT, standard deviations, and errors are shown in table 11 (see also figure 20).

RT Analysis

RT analysis showed all three main effects were significant: *cue-type*, RT to matching cue trials ($M=545$) was significantly faster than to unrelated cue trials ($M=877$), $F(1,31) = 506.42$, $p < .001$, $\eta_p^2 = .94$; *CSI*, RT to the short CSI ($M=779$) was slower than RT to the long CSI ($M=643$), $F(1,31) = 238.67$, $p < .001$, $\eta_p^2 = .89$; *sequence*, RT to cue-repetitions ($M=672$) was faster than to cue-switches ($M=699$), which in turn was faster than attention-switches ($M=762$), $F(2,62) = 70.19$, $p < .001$, $\eta_p^2 = .69$. Planned contrasts showed that the cue-switch contrast [$F(1,31) = 30.9$, $p < .001$, $\eta_p^2 = .5$] and the attention-switch contrast [$F(1,31) = 59.94$, $p < .001$, $\eta_p^2 = .66$] were both significant.

The CSI-by-sequence interaction reached significance, $F(2,62) = 27.89$,

Table 11: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 12 across both cue-types

Cue	CSI	Sequence							
		Cue-Repeat		Cue-Switch		Attention-Switch		C-sc ^a	A-sc ^b
		RT	Error	RT	Error	RT	Error		
Unrelated	100ms	882	3.24	955	3.66	1083	4.92	73	128
		(115)		(134)		(157)			
	900ms	751	2.58	768	2.80	823	3.20	17	55
		(120)		(126)		(154)			
Matching	100ms	559	2.10	571	1.11	625	2.78	12	54
		(75)		(86)		(124)			
	900ms	497	1.63	502	2.78	516	2.86	4	14
		(63)		(69)		(78)			

^aC-sc = Cue-switch cost

^bA-sc = Attention-switch cost

$p < .001$, $\eta_p^2 = .47$ which reflected a reduction in cue-switch- and attention-switch-cost at longer CSIs. There was also a cue-by-CSI interaction, $F(1,31) = 105.24$, $p < .001$, $\eta_p^2 = .77$, indicative of a greater reduction in RT at longer CSIs for unrelated cues than for matching cues.

Again there was a cue-by-sequence interaction, $F(2,62) = 37.97$, $p < .001$, $\eta_p^2 = .55$. Planned contrasts investigated this interaction and showed that the cue-switch cost was greater in the unrelated cue condition (44ms) than in matching cue condition (8ms), $F(1,31) = 14.08$, $p < .01$, $\eta_p^2 = .31$. The magnitude of the attention-switch also differed between cue types, being greater for the unrelated cues (92ms) than for the matching cues (35ms), $F(1,31) = 23.45$, $p < .001$, $\eta_p^2 = .43$. This interaction was followed up by collapsing across CSI and submitting the data to two separate one-way ANOVAs (one for each cue-type)

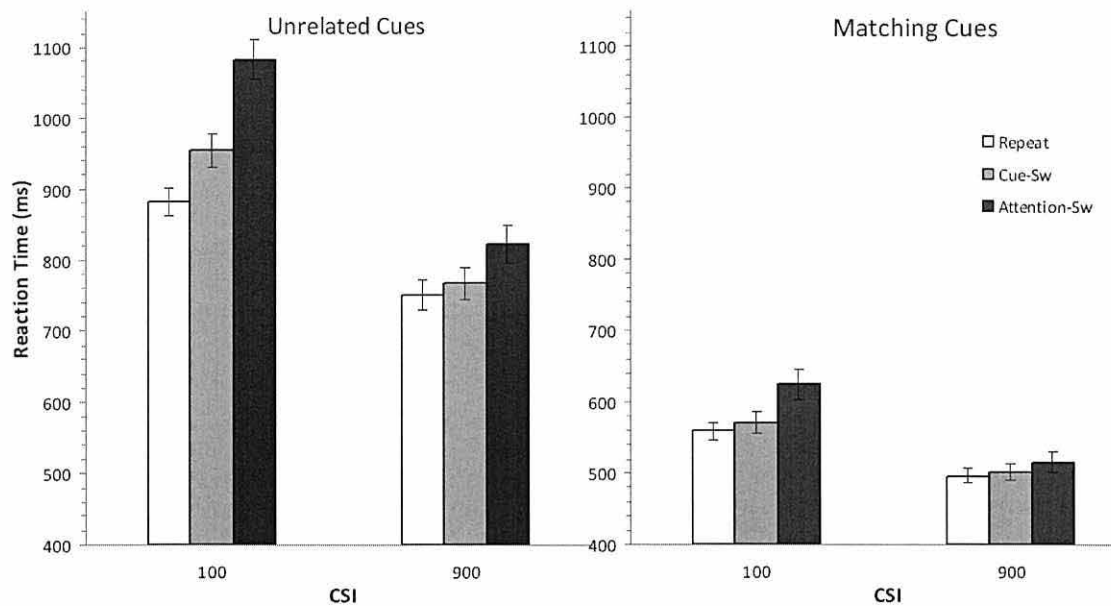


Figure 20. Mean reaction time for all sequences across both cue-types from Experiment 12. Note error bars denote one standard error around the mean.

with the factor *Sequence* (as in Design). The ANOVAs showed that the 8ms cue-switch cost for matching cues was actually significant¹⁶, $F(1,31) = 6.39$, $p < .05$, $\eta_p^2 = .17$, as was the 44ms cue-switch cost for unrelated cues, $F(1,31) = 24.46$, $p < .001$, $\eta_p^2 = .44$. The attention-switch cost was significant for both matching cues [35ms; $F(1,31) = 19.00$, $p < .001$, $\eta_p^2 = .38$] and for the unrelated cues [92ms; $F(1,31) = 59.82$, $p < .001$, $\eta_p^2 = .66$].

The three-way interaction also reached significance, $F(2,62) = 10.63$, $p < .001$, $\eta_p^2 = .25$, again reflecting a greater reduction of the cue-switch cost for the unrelated cues than for the matching cues at the long CSI compared to the short CSI, $F(1,31) = 7.09$, $p < .05$, $\eta_p^2 = .19$. The reduction in attention-switch cost was similar in magnitude between cue-types, $F(1,31) = 2.74$, $p > .1$ (see table 11).

Error Analysis

Error analysis mostly reflected the RT analysis, with the following exceptions: The cue-by-sequence interaction was not significant, $F(2,62) = 1.51$, $p > .23$, nor

¹⁶To test whether this 8ms cue-switch cost for the matching cues differed from the 0ms cost for iconic cues in Experiment 10, we analysed the cue-switch costs in a $2 \times 2 \times 2$ mixed ANOVA, with *CSI* (100ms vs. 900ms) and *sequence* (cue-repeat vs. cue-switch) as within-subject variables, and *experiment* as a between subject variable. The sequence-by-experiment interaction was not quite significant, $F(1,62) = 3.4$, $p = .07$, $\eta_p^2 = .05$. The three-way interaction was not significant.

was the CSI-by-sequence interaction significant, $F(2,62) = 1.47, p > .24$. The three-way interaction also did not reach significance, $F(2,62) = 1.59, p > .21$.

Discussion

The results from Experiment 12 have removed any doubt as to the alternative explanation of the lack of cue-switch cost in Experiment 10 due to cue-collapsing as participants were now able to collapse across cues equally. Additionally, in Experiment 12 perceptual differences during a cue-switch transition were identical between the matching and unrelated conditions (therefore being a tighter control for cue-collapsing than Experiment 11). Despite this, we again found that cue-switch costs were reduced when the cues were maximally transparent, thus supporting our findings from Experiment 10.

The finding of a significant 8ms cue-switch cost for matching cues in this Experiment is not counter to our conclusions; the more important finding is the significant cue-switch interaction in both experiments, being greater when the cue is less transparent.

General Discussion

Across three experiments we have sought to investigate whether the cue-switch cost in task-switching arises from active control processes or perceptual priming of the cue itself. Based on our results, we suggest that cue-switch costs arise from active control processes that form a WM representation (Arrington et al., 2007; Mayr & Kliegl, 2003) rather than from perceptual priming of the cue itself. When the representation is provided by the cue directly (i.e. transparent iconic cues), the cue-switch cost is greatly reduced (or removed completely). These results provide an important constraint on theories of cue encoding, as they provide additional evidence that cue encoding results in an activated WM representation of what to do, rather than merely a visual representation of the cue itself. The strongest evidence against the perceptual-hypothesis of cue-switch costs comes from Experiment 12, where perceptual changes during cue-switch

transitions were controlled between conditions; with this controlled for, greater cue-switch costs were still found when a WM representation of which target to attend to had to be formed endogenously.

On the final product of cue encoding

The question arises as to what the final product of cue encoding actually is. Arrington et al. (2007) suggested that cue encoding results in a semantic categorical representation of the task to be performed. As our paradigm does not involve task-switching in the usual sense, we suggest it is unlikely that participants utilised a semantic code to guide behaviour. Specifically, on the basis of the cue the participant can be certain what the target should look like, and therefore we suggest that a visual representation is the final product of cue encoding (the "target description" in models of selective attention, e.g., Houghton & Tipper, 1996). This contention is supported by the speeded RTs for iconic cues, which provide this relevant visual representation exogenously. For the word cues, although we used words semantically related to the visual target (to facilitate the formation of cue-target associations), it is still possible some (non-visual) semantic code is utilised as a mediator (Logan & Schneider, 2006a). This possibility might be investigated by looking interference effects from, for instance, Articulatory Suppression (AS), which has been shown to inflate the task-switch cost under some circumstances (Emerson & Miyake, 2003; Miyake, Emerson, Padilla, & Ahn, 2004; but see Bryck & Mayr, 2005). More generally, the idea that the final product of cue encoding is flexible and depends on the exact nature of the tasks involved is an important one. Across the task-switching literature, a vast array of different tasks has been used, and the representations resulting from cue encoding are likely to differ between studies, and may contribute to differences in findings.

Cue-transparency

The manipulation of cue-transparency in the task-switching literature has normally used word cues as the most transparent cues (e.g. Arbuthnott &

Woodward, 2002; Logan & Schneider, 2006a). In our Experiment 10, word cues were used as less transparent cues than the more transparent iconic cues. As one reviewer of this paper pointed out, this raises important issues that require clarification for future studies wishing to manipulate cue transparency:¹⁷ Is cue-transparency to be determined only in relation to other cues, or is transparency based on the relationship between the features of the cue and the target? We believe that these issues can be resolved by formalising our definition of cue transparency. As we have argued in this paper, cue encoding results in an activated representation that allows successful performance of the current task. We have also argued that the representation formed can be different between studies, and is determined by the nature of the task to be performed. We thus define cue transparency as *the degree to which the cue exogenously provides or directly stimulates the relevant WM representations required to perform the task*. For instance, suppose participants are engaged in switching between responding to the colour or the shape of targets (Cooper & Marí-Beffa, 2008), and that the possible colours are *blue* and *red* mapped to left and right key presses. There are many levels of cue transparency possible, e.g., in increasing transparency for the colour task, (i) an arbitrary letter code ("A" = colour task), (ii) the word "Colour", (iii) the words BLUE and RED arranged vertically (specifying the response set), (iv) BLUE - RED horizontally aligned (left, right) with their associated manual responses (providing the stimulus-response, SR, mapping), and (v) Blue and Red coloured patches, aligned congruently to the SR mapping. One would predict decreasing switch costs with each increase in transparency, as the cue more directly stimulates the relevant WM representations (including the SR mapping).

Cue transparency, by this definition, is not a fixed property of a cue or cue type: a transparent cue used in one study is not necessarily a transparent cue in a different context. In our paradigm, successful performance depends on having a visual representation of the relevant target, and hence the iconic cues are more transparent according to the above definition. However, if the task situation

¹⁷We thank Iring Koch for pointing out these issues.

involves semantic categorisation (as, e.g., Arrington et al's., 2007 study does), the representation required is of a semantic nature; in this instance, word cues would be more transparent than iconic cues. Experiment 12 of this paper demonstrates this well. In both conditions, we utilised identical iconic cues. However, in the matching condition, the cues provide the WM representation directly, and can be considered transparent. The iconic cues in the unrelated condition, although identical to those in the matching, are not transparent as they do not provide the relevant WM representation, and the participant must activate the representation endogenously.

Conclusion

In sum, we have provided evidence that the cue-switch cost in task-switching arises from active control processes, rather than merely priming of the cue itself. This is important theoretically as the cued task-switching paradigm gained its popularity in the hope of that the task-switch cost reflects component control processes; further investigation of the cue-switch cost should provide a fruitful avenue for researchers hoping to understand control processes.

Chapter Summary

The present set of results serve to strengthen the notion that the task-switching procedure measures cognitive control processes. Most researchers to date have been concerned with the presence/absence of task-switch costs, as these were thought to be reflective of reconfiguration processes (Mayr & Kliegl, 2003; Monsell & Mizon, 2006). Instead, I focussed on what drives cue-switch costs, and have provided evidence that these are caused by cognitive control processes that activate a WM representation of what to do (Arrington et al., 2007).

One aspect of the findings that has been underrated in these experiments is the consistent finding of significant attention-switch costs even when the cue-switch cost was null for matching cues and targets (Experiments 10 and 12). This provides a potential behavioural dissociation between cue-switching and attention-switching (c.f. Arrington & Logan, 2004b; Logan & Bundesen, 2003; Schneider & Logan, 2005). Attention-switching is an important task-set parameter (Logan & Gordon, 2001), so this provides important converging evidence that not all task-switching effects can be explained by cue-switching (Altmann, 2006). Additionally, Altmann (Altmann, 2004a, 2004b) has reported that task-switch costs reduce with extended preparation intervals only when the CSI is manipulated within-subjects (see also Koch, 2001). Altmann suggests that the system needs to be exposed to varying preparation intervals so as to appreciate the benefits of (and thus engage in) advanced preparation; thus when this is not possible in a between-subjects design, there is no interaction of CSI on the switch cost. However, in a further study, I have found that the attention-switch costs found in Experiment 10 of this chapter do still reduce given longer CSIs, even when CSI is manipulated between subjects (see Appendix A for full method & results). Therefore, the reduction of attention-switch cost at longer CSIs in Experiments 10-12 do not appear to be caused solely by the within-subjects manipulation of CSI. It remains an open question for future research why attention-switch costs reduce at long CSIs between subjects, whereas the "task"-switch cost does not.

Model Fitting

To provide converging evidence that the present results cannot be explained entirely by cue-switching effects, Logan's models (Arrington & Logan, 2004b; Logan & Bundesen, 2003, 2004) were fit to the data from Experiments 10-12. I provide the model equations (from which RT predictions for each type of sequence are estimated) here again for ease of exposition, and discuss the fitting procedure below. Full explanations of the differences between models can be found in the general introduction.

Model 1

Model 1 assumes no cue-encoding benefits, but does assume an endogenous reconfiguration processes that runs on task-switch trials. Thus, the model predicts the same RT for cue-repetitions and cue-switches:

Cue-repetitions cue-switches:

$$RT = RT_{Base} + \mu_c \cdot \exp\left(\frac{-CSI}{\mu_c}\right) \quad (15)$$

Attention-switch trials are assumed to include an extra reconfiguration process, and is thus defined as:

Attention-switch:

$$RT = RT_{Base} + \exp\left(\frac{-CSI}{\mu_c}\right) \cdot (\mu_c + \mu_s) + \frac{\frac{1}{\mu_c}}{\frac{1}{\mu_c} - \frac{1}{\mu_s}} \cdot \left[\exp\left(\frac{-CSI}{\mu_s}\right) - \exp\left(\frac{-CSI}{\mu_c}\right) \right] \cdot \mu_s \quad (16)$$

Model 2

Model 2 assumes no reconfiguration, but explains all transition effects by the degree of cue-priming from n-1 to n. Thus, the model makes the same prediction for cue-switch trials and attention-switch trials:

Cue-repetitions:

$$RT = RT_{Base} + \mu_r \cdot \exp\left(\frac{-CSI}{\mu_r}\right) \quad (17)$$

Cue-switch & attention-switch:

$$RT = RT_{Base} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (18)$$

Model 2+1

Model 2 + 1 combines the previous two models, assuming a cue-repetition benefit and a time-consuming reconfiguration process. It has separate equations to predict RTs for each sequence:

Cue-repetitions:

$$RT = RT_{Base} + \mu_r \cdot \exp\left(\frac{-CSI}{\mu_r}\right) \quad (19)$$

Cue-switch:

$$RT = RT_{Base} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (20)$$

Attention-switch:

$$RT = RT_{Base} + \exp\left(\frac{-CSI}{\mu_a}\right) \cdot (\mu_a + \mu_c) + \frac{\frac{1}{\mu_a}}{\frac{1}{\mu_a} - \frac{1}{\mu_s}} \cdot \left[\exp\left(\frac{-CSI}{\mu_s}\right) - \exp\left(\frac{-CSI}{\mu_a}\right) \right] \cdot \mu_s \quad (21)$$

Model 3

Model 3 is the Mayr and Kliegl (2003) model, which assumes a cue-repetition benefit. On attention-switch trials, new rules need to be applied to the target display, and thus the estimate of RT_{Base} shifts for attention-switches compared to cue-switches.

Table 12: *Model parameters & fits for Experiment 10*

Cue Type	RT_{Base}	μ_c	μ_s	r	RMSD	
Model 1						
Icons	523	132	87	.981	13	
Words	542	276	117	.973	32	
	RT_{Base}	μ_r	μ_a	r	RMSD	
Model 2						
Icons	392	240	346	.759	79	
Words	520	254	375	.980	27	
	RT_{Base}	μ_r	μ_a	μ_s	r	RMSD
Model 2+1						
Icons	523	131	172	52	.959	23
Words	547	223	346	54	.982	29
	$RT_{BaseRep}$	$RT_{BaseAlt}$	μ_r	μ_a	r	RMSD
Model 3						
Icons	510	547	146	165	.982	15
Words	523	553	251	356	.991	19

Cue-repetitions:

$$RT = RT_{BaseRep} + \mu_r \cdot \exp\left(\frac{-CSI}{\mu_r}\right) \quad (22)$$

Cue-switch:

$$RT = RT_{BaseRep} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (23)$$

Attention-switch:

$$RT = RT_{BaseAlt} + \mu_a \cdot \exp\left(\frac{-CSI}{\mu_a}\right) \quad (24)$$

The formal models of Logan and Bundesen (2003, 2004) were applied to data from Experiments 10-12. The model was fit to the data using the Solver routine in Microsoft Excel, in which the program was informed to reduce the root mean

Table 13: *Model parameters & fits for Experiment 11*

Cue Type	RT_{Base}	μ_c	μ_s	r	RMSD	
Model 1						
Icons	594	251	203	.982	31	
Words	583	268	120	.968	40	
	RT_{Base}	μ_r	μ_a	r	RMSD	
Model 2						
Icons	588	240	363	.938	56	
Words	571	224	366	.980	30	
	RT_{Base}	μ_r	μ_a	μ_s	r	RMSD
Model 2+1						
Icons	587	241	364	57	.938	56
Words	588	203	346	52	.977	32
	$RT_{BaseRep}$	$RT_{BaseAlt}$	μ_r	μ_a	r	RMSD
Model 3						
Icons	557	668	277	333	.987	24
Words	563	596	233	356	.992	20

square difference (RMSD) between model predictions and observed data. To achieve this, Solver estimated the model parameters in the equations stated for each particular model, which allowed best fit to the observed data. Fitting was to individual participants data, rather than averaged data. Once model parameters had been estimated for each participant, the averages of parameter values were calculated.

Model parameters were estimated separately for both cue types. Model estimates and RMSD for each parameter as a function of cue type is shown in tables 12 - 14.

Qualitatively, we can already assume that the models will not fit the data perfectly, due to the dissociation of cue- and attention-switch costs found between cue types across all 3 experiments. Indeed, Model 1 (which assumes an act of reconfiguration on task-switch trials only without appealing to any cue-encoding processes being primed during cue-repetitions) did not fit the data very well for word- and unrelated-cues. This was due to there being considerable cue-switch

Table 14: *Model parameters & fits for Experiment 12*

Cue Type	RT_{Base}	μ_c	μ_s	r	RMSD	
Model 1						
Matching	502	126	83	.987	13	
Unrelated	763	233	190	.967	39	
	RT_{Base}	μ_r	μ_a	r	RMSD	
Model 2						
Matching	502	121	170	.958	21	
Unrelated	755	202	351	.942	54	
	RT_{Base}	μ_r	μ_a	μ_s	r	RMSD
Model 2+1						
Matching	502	121	134	75	.991	10
Unrelated	760	198	276	149	.981	30
	$RT_{BaseRep}$	$RT_{BaseAlt}$	μ_r	μ_a	r	RMSD
Model 3						
Matching	492	525	134	162	.986	13
Unrelated	731	818	232	328	.978	33

costs across all experiments when the cue-target relationship was less transparent (Grange & Houghton, in pressb), which the model does not account for.

Conversely, when the cue-target relationship is transparent, Model 1 becomes the best fit for the data, as in these cases there is little (if any) cue-switch cost.

Model 2, which assumes no act of reconfiguration, and explains all transition effects on the strength of residual priming of the cue in WM from the previous trial, fit the word/unrelated cue data much better than for the iconic/matching cues, for which it did not fit well at all. Again, this is driven by the absence of cue-switch costs for the maximally transparent cues and targets. Model 2+1 surprisingly did not fit the data as well as either of the previous models. Model 3, though, fit all data much better than any of its competitors.

Model 3 was designed to provide a theoretical account put forward by Mayr and Kliegl (2003) that task-switching in the explicitly-cued paradigm was achieved by two stages: cue-driven retrieval of task rules from LTM and their installation into WM (reflected in cue-switch costs) and application of these rules to the target

display (reflected in task-switch costs). This model explains the present data very well as for maximally transparent cues, the task-rules (in this case an endogenous representation of what target to search for) is provided by the cue itself, so no retrieval is required from LTM (and hence, no cue-switch cost). As all trials require applying the rules to the target display (whether the rules were generated endogenously or provided exogenously), there was a task-switch cost in all experiments.

However, there is one problem with the suggestion that Model 3 is the best fit: it has more free parameters with which to fit the observed data than Models 1 and 2. Therefore, one can not say with any certainty whether the better fit of Model 3 is exclusively to do with better underlying assumptions or an emergent property of a more complex model. What can be stated, however, is that Model 3 fits the data better than Model 2+1, which has the same amount of free parameters.

Models 1 and 2 can be discounted to explain the current data set, as the emerging empirical data provides ample evidence that contradicts their assumptions. As cue-encoding processes are obviously primed on cue-repetition trials (as most studies report significant cue-switch costs), then Model 1, with its assumption of no cue-encoding processes, can immediately be discounted. Additionally, as there is now considerable evidence of a significant task-switch cost over and above that of switching cues (Arrington et al., 2007; Jost et al., 2008; Mayr & Kliegl, 2003; Experiments 10-12 of this thesis), Model 2, which assumes all transition effects can be explained by cue-switching, can also be discounted. Therefore, model complexity need not be an issue as the only two theoretically plausible models are Model 2+1 and Model 3.

Conclusion

The present chapter has provided some important constraints on future models of task-switching. Particularly relevant is the apparent dissociation of cue-switching from attention-switching. The next chapter serves to extend these findings by dissociating cue-related processes from target-related processes of task

performance in an extended runs design. This paradigm allows us to address the cause of the attention-switch cost: It could be caused by time-consuming reconfiguration processes (Meiran, 1996; Rogers & Monsell, 1995), or it could emerge from activation processes (Altmann & Gray, 2008) that run on repetition and switch trials that serves to ensure the relevant representation is the most active (a process that is imperative on switch trials as well as repetition trials).

Chapter 6

Cue-Transparency & Extended Runs Design

CHAPTER 6: CUE-TRANSPARENCY & EXTENDED RUNS DESIGN

Abstract

Cue-transparency in task-switching refers to the degree to which a task cue provides the necessary working memory (WM) representation with which task performance can be made: transparent cues provide a sample of the WM representation directly, whereas non-transparent cues are often arbitrary and rely on recently learned cue-task associations. Although transparent cues have been shown to improve task-switching performance, it is not clear whether cue-transparency affects just cue-related processes in task-switching, or whether it also has a carry-over effect onto target processing.

Across two experiments, we separate cue-related processes from target-related processes by using an extended runs design, wherein a task cue is followed by a series of targets, upon which the cued task must be performed. Despite transparency affecting the first (cued) trial, no carry-over effects were found on target processing. Additionally, we provide evidence that the restart cost in task-switching is caused by activating a WM representation, a process aided by transparent cues.

We discuss the results in terms of the importance of formation of - and access to - a stable WM representation in multi-task situations.

Literature Review & Introduction

Every day life requires frequent and effective shifts of tasks to produce goal-directed behaviour. For example, when making a cup of coffee, one must switch between finding a suitable mug, filling it with coffee, filling the kettle, boiling it, and pouring the water into the mug. The mechanisms underlying such shifts of attention and action have been studied utilising the explicitly cued task-switching paradigm (Meiran, 1996; Sudevan & Taylor, 1987; for a review see Monsell, 2003). Within this paradigm, participants must typically perform two or more simple cognitive judgements (e.g. a parity judgement of odd/even; a magnitude judgement of higher/lower than five) on stimuli that afford both tasks (e.g. numbers), with the relevant task being signalled by a valid pre-cue (e.g. the word "Magnitude"). It is a consistent finding within this paradigm that switching tasks (BA) is more costly in both reaction times (RT) and error rates than performing the same task (AA, where 'A' and 'B' are tasks), an effect known as the switch cost. A dominant view of the switch cost until recently was that it reflects a timely reconfiguration process of one's attentional set to deal with changing task demands. Consistent with this proposal, switch cost has been shown to reduce with increased preparation intervals, suggesting much of the reconfiguration can occur in advance (Rogers & Monsell, 1995; but see Altmann, 2004a, 2004b). As such, the attractiveness of the switch cost as a psychological construct is that it offers a window into the temporal dynamics of cognitive control.

A problem within the explicitly cued task-switching paradigm with one cue per task is that task-switches are confounded with cue-switches, and task-repetitions with cue-repetitions. Using two cues per task has allowed researchers to separate the costs associated with switching cues and the costs of switching tasks (Logan & Bundesen, 2003; Mayr & Kliegl, 2003), as three types of sequence are now possible: *Cue-Repeat* (both cue and task repeat, e.g. Magnitude - Magnitude), *Cue-Switch* (task repeats, but cue switches, e.g.

Magnitude-*High/Low*), and *Task-Switch* (both cue and task switch e.g. Magnitude-*Parity*). Costs associated with switching cues (measured by comparing reaction time to cue-switches versus cue-repetitions) have been reliably shown to be substantial, due to residual priming of cue encoding processes during cue-repetitions (Grange & Houghton, in pressa; Logan & Bundesen, 2003; Mayr & Kliegl, 2003); cue-switches do not benefit from direct priming and thus must be encoded fully. Accordingly, when the task-switch cost is measured (task-switch RT minus cue-switch RT, as opposed to the traditional task-switch minus task-repeat), it has been found to be less reliable than previously thought: Some studies find that task-switches show a cost over and above that which can be explained by switching cues (Altmann, 2006; Arrington, Logan, & Schneider, 2007; Jost, Mayr, & Rösler, 2008; Mayr & Kliegl, 2003; Monsell & Mizon, 2006) while others report that task-switches are just as costly as cue-switches (Arrington & Logan, 2004; Logan & Bundesen, 2003, 2004; Logan & Schneider, 2006b; Schneider & Logan, 2005), suggesting that, at least in some cases, task switching is little more than switching cues.

In order for the relevant task to be performed correctly, on the basis of the cue a durable task representation must be activated in working memory (WM) that allows a correct response to be made on the stimulus (Altmann & Gray, 2008; Arrington, Logan, & Schneider, 2007; Logan & Bundesen, 2003; Logan & Schneider, 2006a, 2006b; Mayr & Kliegl, 2000, 2003; Schneider & Logan, 2005). As the focus of the majority of the recent task-switching literature has been on the presence/absence of task-switch costs, there has been a certain neglect of the factors which contribute to the cue-switch cost (but see Arrington et al., 2007). The cue-switch cost is generally accepted to reflect cue-encoding benefits on cue-repetition trials (Logan & Bundesen, 2003; Mayr & Kliegl, 2003), and as such, any processes that contribute to successful cue encoding are primed on cue-repetition trials.

Logan and Bundesen (2003; see also Schneider & Logan, 2005) suggest that

cue encoding results in a relatively passive perceptual representation of the cue in short-term memory (STM). This cue is then combined with an encoded stimulus, and jointly used to retrieve the correct response from long-term memory (LTM). For example, the cue "Parity" and the stimulus "2" uniquely retrieve the response "Even". From this perspective, cue-switch costs (and task-switch costs) arise from perceptual priming of the cue in STM during cue-repetitions. If a cue switches from trial $n-1$ to n (regardless of whether the task changes also), the cue must be encoded anew, which induces a cost.

Conversely, Mayr and Kliegl (2003; see also Mayr and Kliegl, 2000) suggested that the cue-switch cost arises from priming of a cue-specific retrieval route that obtains task rules from LTM and installs them into WM. By this theory, when a cue repeats, the retrieval route is primed, unlike when a cue switches which requires use of a new (and un-primed) retrieval route. Thus, cue-switch costs arise from priming of control processes that establish a WM representation of what to do.

Grange and Houghton (in pressa) sought to distinguish between these competing theories of the cue-switch cost. They varied cue-transparency (Logan & Schneider, 2006b), which can be defined as "*the degree to which the cue exogenously provides or directly stimulates the relevant WM representations required to perform the task*" (Grange & Houghton, in pressa). With relatively non-transparent cues, endogenous control is required to activate the relevant WM representation, unlike transparent cues which provide this representation directly. In an attention-switching paradigm, participants were asked to locate an oval target among a total of four ovals that corresponded with a valid pre-cue. Transparent cues were rectangles or triangles which shared a visual property of the relevant target (e.g. a rectangle/triangle with a thick border cued an oval with a thick border). Relatively less transparent cues were word cues that described the characteristics of the oval to search for (e.g. the words border or outline cued an oval with a thick border).

Using two cues per target, Grange and Houghton (in pressa) found cue-switch costs to be significant for the less transparent word cues, but were absent for the transparent iconic cues. On this basis, the authors suggested that the cue-switch cost in this design reflected the timely formation of a WM representation for the word cues, something not necessary for the transparent iconic cues as the cue provides the necessary representation directly. It is important to note that perceptual-hypothesis of cue-switch costs (Logan & Bundesen, 2003; Schneider & Logan 2005) cannot explain the lack of cue-switch cost for iconic cues, as cue-switches always involved a perceptual switch of cue.

Separating cue-processing from target-processing

Previous studies had found effects of cue-transparency on task-switching. Arbuthnott and Woodward (2002) found that non-transparent cues produced larger task-switch costs than transparent cues. Logan and Schneider (2006a) suggested that non-transparent cues required retrieval of a task mediator to perform the task. For example, if the cue for a red/blue judgement is a square, then the cue must first activate a mediator (e.g. the word "colour"). Transparent cues do not require this mediator retrieval, hence resulting in faster responding.

However, these studies (and that of Grange & Houghton, in pressa) investigate cue-related processes in paradigms where such processes are inextricably conflated with target processing. Specifically, reaction time on a particular trial is a combination of cue-related processing and target related processing. Although cue-processes can be separated from target processing theoretically (see for example the mathematical models of Logan & Bundesen, 2003), empirical separation is more convincing.

The work of Altmann has championed one such empirical paradigm that allows such separation (Altmann, 2002, 2006, 2007; Altmann & Gray, 2002, 2008). This *extended runs* paradigm presents a cue that signals which of two (or more) tasks are relevant for a given run or trials. On this run of trials, the cue is only present for the first stimulus; after this, stimuli are presented in isolation, and the

relevant task must be maintained in memory (e.g. $C_AAAAA C_BBBBB$ etc., where C_B , for example, is the cue for task B). Cues can either indicate a repetition of the previously relevant task (e.g. $C_AAAAA - C_AAAAA$) or a switch from the previous task (e.g. $C_AAAAA - C_BBBBB$). Using this paradigm, only trial one confounds cue-processing with target processing, whereas un-cued trials only reflect target processing. Using subtractive logic, cue-related processes can be estimated by comparing cued trials to un-cued trials.

Typical findings from this paradigm mostly reflect that found in the standard cuing-paradigm on trials one of the run (i.e. the cued trial), for example switch RTs are slower than repetition RTs. However, there are some findings that are unique to this paradigm. For example, if the cue signals a repetition of the previous task, trial one RT is much larger than RT for cue-less trials, indicating a substantial cost of processing the cue independent of any switch of task. This "restart cost" (Allport & Wylie, 2000; Altmann, 2002, 2006, 2007; Altmann & Gray, 2002, 2008; Gopher, Armony, & Greenspan, 2000; Poljac et al., 2009) is thought to reflect the time the cognitive system needs to re-activate task representations that may have decayed since the last cue presentation. The restart cost is important theoretically, as it suggests that encoding and activation processes run on repetition trials as well as switch trials, a view not compatible with the reconfiguration view of a dedicated set of processes that run on switch trials only (Meiran, 1996; Rogers & Monsell, 1995). It suggests the cognitive system faces the same problem on switch trials *and* repetition trials of ensuring that the relevant task is the most active among competing representations.

A related finding is that RTs slow steadily over a run of cue-less trials (Altmann, 2002, 2006, 2007; Altmann & Gray, 2002, 2008), which is reported to reflect the system attempting to access a decaying task representation (which becomes more difficult over time due to passive decay, and hence slows responses; but see Altmann, 2009; Lewandowsky, Oberauer, & Brown, 2009a, 2009b). This process has been called "within run slowing", and the restart cost is thought to

reflect the re-activation of the representation that has slowed within the run.

Another full-run effect is *Full Run Error Switch Costs* (FRESC; Altmann & Gray, 2008). This is the finding of increased errors on task-switch trials for the whole run, not just on the first trial following a switch. Altmann and Gray state that FRESC are caused by the presence of incongruent stimuli, i.e. those associated with different responses on the two tasks (e.g., for parity and magnitude judgments on numbers, if both "Odd" and "Lower than 5" demand left key presses, then 3 is congruent and 4 incongruent). Importantly, if a participant responds to the "wrong" dimension of a congruent stimulus then the response will still be scored as correct, whereas for an incongruent stimulus it will produce an error. FRESC arise due to encoding processes which aim to execute the most active task representation. On switch runs, the now irrelevant task is still relatively active, and thus sometimes intrudes on selection. On repetition trials, representations from the previous run will not interfere with the current run, as they are compatible. Thus, FRESC reflect memory intrusions from recent episodes, a phenomenon the reconfiguration hypothesis cannot account for (Meiran, 1996; Rogers & Monsell, 1995). Specifically, they predict that on switch trials, all switch effects (RT and error) should be localised to reconfiguration processes running on trial 1. Trials 2 and on should not include any switch effects, as the system should be "reconfigured" already.

Does cue-processes spill into target processing?

One question that has remained relatively under-specified in the task-switching literature is to what extent manipulations of cue transparency affect target processing. For example, the results of Grange and Houghton (in pressa) and Arbuthnott and Woodward (2002) demonstrate that cue-transparency can have a large effect on task performance. It is unclear however, whether the cue-transparency is uniquely affecting cue-related processes (such as establishing a WM representation of which task to perform, Grange & Houghton) or whether it additionally affects target processing. Such effects on target processing could be

altered by cue-transparency as target performance requires access to the WM representation formed on the cued trial (Altmann & Gray, 2008; Grange & Houghton, *in pressa*). It is possible that manipulating cue-transparency results in different WM representations of the task (Dreisbach & Haider, 2008, 2009), which could later effect target processing if different WM representations are retrieved.

The present study

In the experiments of Grange and Houghton (*in pressa*), the authors suggested that the same WM representation was required for both iconic cues and word cues. However, it is possible that the WM representation activated for the iconic cues was much more superficial and transient than that for the word cues, as it was not endogenously created. Therefore, in an extended runs design, performance on cue-less trials may be impaired for transparent cued conditions as the system is trying to access a superficial WM representation. Alternatively, if the same WM representation is utilised for iconic and word cues, then cue-less trial performance should be equivalent between cue-types. The purpose of the present study was to investigate these effects of cue-transparency on extended runs designs. Additionally, we were interested to investigate, using the 2:1 mapping of Grange and Houghton (*in pressa*), whether adding a run of cue-less trials after cue-processing affects performance on the cued-trial. It is possible that the lack of cue-switch cost for transparent iconic cues was due to differential effects on target-processing between the cuing conditions, rather than the effects of cue-processing, as we suggested.

Additionally, adding a run of cue-less trials after an initial cued trial allowed us to attempt to address the cause of the attention-switch costs found in all experiments in this Grange and Houghton (*in pressa*), even with maximally transparent cues. From the Experiments thus far, we cannot definitively distinguish between any of the dominant interpretations of this cost. Such costs can be caused by a time consuming reconfiguration process (Meiran, 1996; Monsell & Mizon, 2006; Rogers & Monsell, 1995), application of retrieved task rules to the

target (Mayr & Kliegl, 2000; 2003), priming of cue encoding (Logan & Bundesen, 2003), or encoding and activation processes that run on both switch and repeat trials, resulting in repetition priming on repetition trials (Altmann & Gray, 2008). There is some suggestion from the results of Grange and Houghton that support Altmann and Gray's interpretation; specifically, increased preparation time (CSI) reduced RT for all sequences, not just switch trials.

By introducing a run of cue-less trials, we were able to address some predictions from Altmann and Gray's (2008) encoding/activation account of task-switch costs. Specifically, if encoding processes run on repetition trials as well as switch trials, we should find a restart cost on trial 1 (cued trial) in a run, even if this cue signals a task repetition (Allport & Wylie, 2000; Altmann, 2002; 2006; 2007; Altmann & Gray, 2002; Gopher et al., 2000; Poljac et al., 2009). The reconfiguration account of task-switch costs would suggest that if the cue signalled a repetition, nothing needs to be done by the cognitive system, resulting in no cost for repetitions. Our account of cue-target translation in activating a task-representation in WM produces a novel prediction for the restart cost: Specifically, if the restart cost reflects re-activation of task representations in WM that may have decayed over the previous run (Altmann, 2002; 2006; 2007; Altmann & Gray, 2002; Poljac et al., 2009), and formation of task representations can be aided by maximally transparent cues (Grange & Houghton, *in press*), then the restart cost should be reduced for more transparent cues compared to less transparent word cues.

A second prediction we are able address from the encoding/activation account is that of FRESC (Altmann & Gray, 2008), caused by previous tasks intruding on memory which manifest on incongruent trials. In this sense, the stimulus displays used in Grange and Houghton (*in press*) can be considered incongruent as it always contains all possible targets, and if the participant selects the wrong one it will produce an error. On switch runs, the relevant target from the previous run will always afford a different response to that of the now-relevant target. Finding

a FRESC would provide converging evidence that the attention-switch cost is caused by encoding/activation processes rather than a switch-specific reconfiguration process.

Experiment 13

For this experiment, Experiment 1 from Grange and Houghton (in pressa, Experiment 10 of this thesis) was adapted to include a set of cue-less trials following cue presentation.

Method

Participants

34 new participants were recruited from the same pool as previous Experiments. Data from two subjects were removed for failure to meet session-wise accuracy of 90%.

Apparatus & Stimuli

The experiment utilised the same stimuli and apparatus as Experiment 10, again utilising the word and iconic cues between blocks, with two cues per target.

Procedure

The main procedural difference from Experiment 12 was the experimental blocks were grouped into runs of four trials, with the relevant task for that run being indicated by a cue on trial 1. The cue was manipulated between blocks. The cue was presented for 100ms, after which it was surrounded by the four oval targets (as per Experiment 12). When a response was registered, the stimulus display was removed for a fixed period of 100ms (response-stimulus interval, RSI) after which time a new target display appeared, initiating trial two in the run. After the response to the fourth target display in the run, the screen was blank for 100ms, after which time the cue for the next run was presented. The probability of a task switch was fixed at $p(\text{switch}) = .33$.

The experimental session lasted approximately 60 min. Word cues and iconic cues were presented in separate halves of the experiment, with presentation order counterbalanced across participants. Each half of the experiment consisted of four blocks, each with 80 runs, totalling 320 runs for each cue-type. Each half of the experiment was preceded by a practice session consisting of 16 runs.

Design

The experiment manipulated three independent variables, in a fully-crossed repeated measures design: *cue-type* (icons vs. words), *sequence* (cue-repetition vs. cue-switch vs. attention-switch), and *position* of trial in a run (trial 1 vs. trial 2 vs. trial 3 vs. trial 4). Error rates (%) and reaction time (RT) in milliseconds (ms) again served as dependent variables.

Results

Data trimming was identical to that of Experiment 12, and left 91.8% of the raw data to be submitted to further analysis, consisting of two separate three-way repeated measures ANOVAs, with the factors as described in *Design*. Mean RTs and errors are displayed in figure 21. For the RT data, I first present analysis of the full data set (*full-run analysis*), followed by analysis of just the cued-trials (i.e. trial 1, *cued-trial analysis*). After this, the restart cost between cue-types is analysed.

Full-run analysis

For RT, there was a marginal effect of *cue*, with RTs to iconic cued runs (572ms) being faster than to word cued runs (596ms), $F(1,31) = 3.36$, $p = .076$, $\eta_p^2 = .098$. There was a main effect of *sequence*, $F(2,62) = 82.51$, $p < .001$, $\eta_p^2 = .73$. Planned comparisons showed that cue-repetition runs (567ms) were faster than cue-switch runs (581ms), $F(1,31) = 36.08$, $p < .001$, $\eta_p^2 = .54$, which in turn were slower than attention-switch runs (604ms), $F(1,31) = 65.31$, $p < .001$, $\eta_p^2 = .68$.

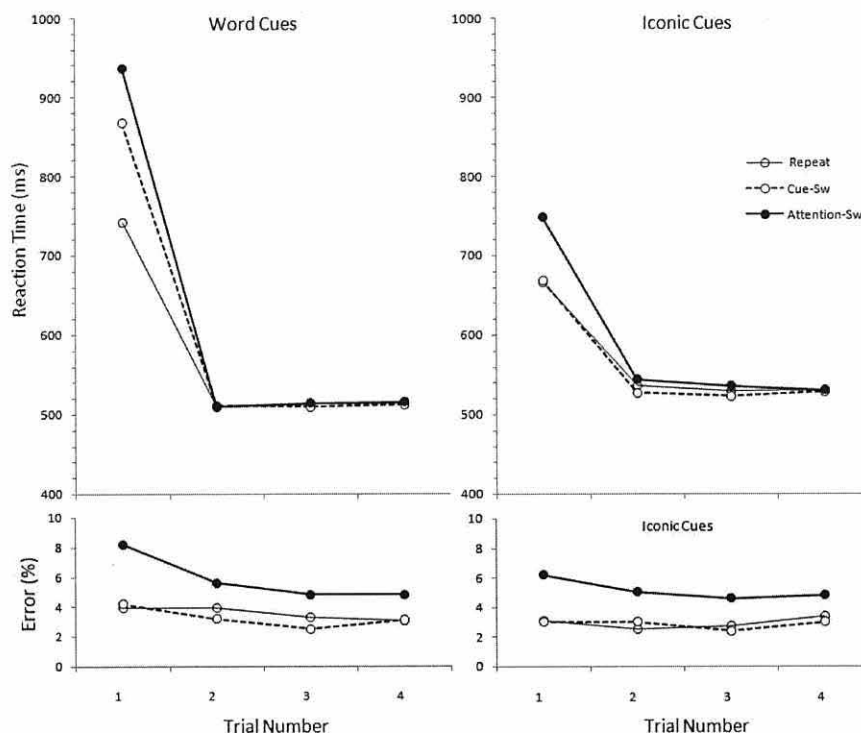


Figure 21. Reaction time and error data from Experiment 13 for each cue-type.

The main effect of *trial* was also significant, $F(3,93) = 274.48$, $p < .001$, $\eta_p^2 = .9$. Planned comparisons showed that RT to trial 1 (771ms) was slower than to trial 2 (523ms), $F(1,31) = 292.24$, $p < .001$, $\eta_p^2 = .9$. Trial 2 RT did not differ from trial 3 (520ms), which did not differ from trial 4 (522ms), all F 's < 1.

There was a cue-by-trial interaction, $F(3,93) = 133.66$, $p < .001$, $\eta_p^2 = .81$. Planned contrasts show this was driven by less reduction in RT between trial 1 and trial 2 for icon cues (159ms reduction) compared to word cues (338ms), $F(1,31) = 146.82$, $p < .001$, $\eta_p^2 = .83$. RT for iconic cues continued to improve from trial 2-3 (6ms reduction), whereas word cues did not (-1ms), which actually proved significant, $F(1,31) = 4.19$, $p < .05$, $\eta_p^2 = .12$.

There was again a cue-by-sequence interaction, $F(2,62) = 31.66$, $p < .001$, $\eta_p^2 = .51$. Planned comparisons showed that the cue-switch contrast (RT for cue-switch runs minus cue-repetition run RT) was smaller for iconic cues (-4ms) than for word cues (31ms), $F(1,31) = 59.51$, $p < .001$, $\eta_p^2 = .66$. The attention-switch contrast (RT for attention-switch runs minus cue-switch run RT)

did not differ between cue-types (28ms & 19ms for icon cues and word cues, respectively), $F(1,31) = 3.03$, $p > .09$.

The three-way interaction also reached significance, $F(6,86) = 35.23$, $p < .001$, $\eta_p^2 = .53$. Planned contrasts showed this was driven by a greater reduction in the cue-switch cost for word cues from trials 1-2 for word cues compared to the iconic cues, as there was no cue-switch cost for iconic cues, $F(1,31) = 69.28$, $p < .001$, $\eta_p^2 = .69$.

Errors

The error analysis showed a main effect of *sequence*, $F(2,62) = 40.34$, $p < .001$, $\eta_p^2 = .57$; planned comparisons showed no difference between cue-repetition (3.3%) and cue-switch runs (3.1%), but a difference between cue-switch and attention-switch runs (5.7%), $F(1,31) = 40.64$, $p < .001$, $\eta_p^2 = .57$. As can be seen by figure 21, increased error rates for attention-switch trials was not limited to trial 1 in the run, suggesting the presence of FRESC (Altmann & Gray, 2008).

There was also a main effect of *trial*, driven by a reduction in errors from trial 1 (4.9%) to trial 2 (3.9%), and from trial 2 to trial 3 (3.4%); trial 3 did not differ from trial 4 (3.7%) $F(3,93) = 16.08$, $p < .001$, $\eta_p^2 = .34$.

The cue-by-sequence was not significant, but there was a significant interaction of cue and trial, $F(3,93) = 3.17$, $p < .05$, $\eta_p^2 = .57$, driven by a greater reduction in error from trials 1-4 for words compared to icon cues. There was also a sequence-by-trial interaction, $F(6,186) = 6.87$, $p < .001$, $\eta_p^2 = .34$; planned contrasts showed this was driven by a greater reduction in error between trials 1 and 2 for the attention-switch contrast (cue-switch vs. attention-switch), $F(1,31) = 8.42$, $p < .01$, $\eta_p^2 = .21$.

Cued-trial analysis

Cued trial RTs (i.e. trial 1 from each condition) were analysed in a 2 (*cue-type*: icons vs. words) x 3 (*sequence*: cue-repetition vs. cue-switch vs. attention-switch) repeated measures ANOVA.

There was a main effect of *cue-type*, with faster RTs to iconic cues (694ms) than to word cues (848ms), $F(1,31) = 52.19$, $p < .001$, $\eta_p^2 = .63$. There was also a main effect of *sequence*; RT to cue-repetitions (704ms) was faster than to cue-switches (767ms), which in turn was faster than attention-switches (843ms), $F(2,62) = 106.64$, $p < .001$, $\eta_p^2 = .78$. Planned contrasts showed that the cue-switch contrast [$F(1,31) = 53.26$, $p < .001$, $\eta_p^2 = .63$] and the attention-switch contrast [$F(1,31) = 87.45$, $p < .001$, $\eta_p^2 = .74$] were both significant.

Of most interest, there was a cue-by-sequence interaction, $F(2,62) = 44.32$, $p < .001$, $\eta_p^2 = .59$. Planned contrasts investigated this interaction and showed that the cue-switch cost was greater in the word-cue condition (126ms) than in the iconic-cue condition (2ms), $F(1,31) = 85.24$, $p < .001$, $\eta_p^2 = .73$. The magnitude of the attention-switch cost did not differ between cue types, with the attention-switch cost being 70ms for the word-cues and 80ms for the iconic cues, $F(1,31) < 1$.

Restart-cost analysis

To investigate the effect of cue-transparency on the restart cost, I performed analysis on cue-repetition runs only, on trials 1 and 2. Mean RTs are re-plotted in figure 22 to show restart costs.

I analysed the data in a 2 (*cue-type*; icons vs. words) x 2 (*trial*; 1 vs. 2) repeated measures ANOVA. There was no main effect of *cue-type*, $F(1,31) < 1$. There was a main effect of *trial*, $F(1,31) = 181.37$, $p < .001$, $\eta_p^2 = .85$. Importantly, there was a cue-by-trial interaction, $F(1,31) = 34.91$, $p < .001$, $\eta_p^2 = .53$.

This interaction was followed up with planned paired-*t* comparisons, which showed that the restart cost for iconic cues [130ms; $t(31) = 9.67$, $p < .001$] was smaller than for word cues [232ms; $t(31) = 12.75$, $p < .001$].

Discussion

The results demonstrated that despite the extra burden of cue encoding to cope with the run of cue-less trials, the cue-switch cost for iconic cues was again

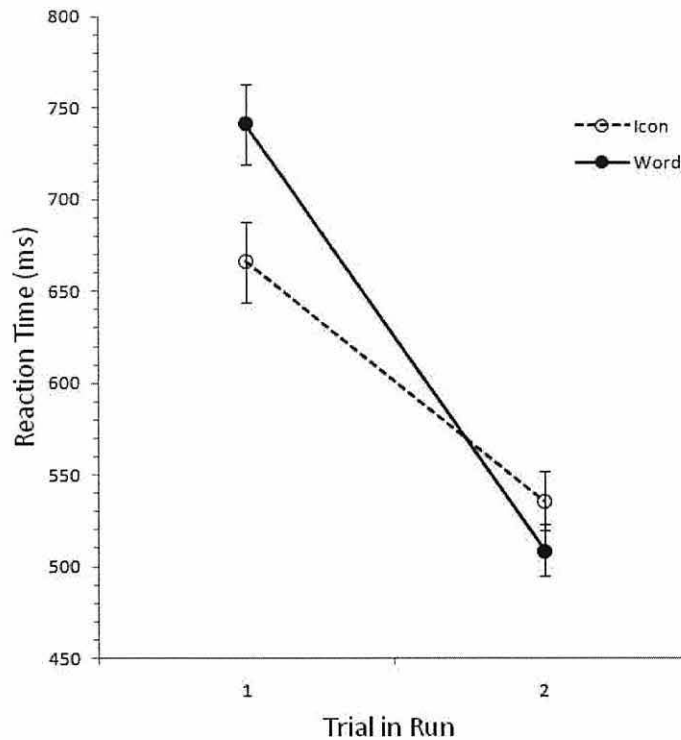


Figure 22. Reaction time restart-costs (Trial 1 RT vs. Trial 2 RT) from Experiment 13 for each cue-type. Error bars denote one standard error around the mean.

drastically reduced compared to word cues, thus replicating and extending the results from Grange and Houghton (in pressa). Therefore the representation afforded by the maximally transparent cues is not superficial and limited to a cued trial; it is as durable as the more effortful endogenous representation obtained by the less transparent word cues.

Our prediction of reduced restart costs for maximally transparent iconic cues was confirmed; the restart cost for iconic cues was smaller than for word cues. This is important theoretically as it provides converging evidence that maximally transparent cues aid cue-target translation, required for formation of task representations in WM. The presence of restart costs in our paradigm shows that encoding/activation processes run on all trials, not just switch trials, and that this activation is aided by transparent cues. This supports Altmann's approach to explaining the switch cost as an emergent property of more well-defined memory processes (Altmann, 2002; 2003; 2004a; 2004b; Altmann & Gray, 2002; 2008).

Additionally, this experiment has provided data enabling tentative

suggestions for the source of the attention-switch costs we have consistently found across experiments in Grange and Houghton (in pressa). The presence of FRESC suggests that on switch runs the activation levels of the previous (now irrelevant) task intrude on selection throughout the whole run. If task-set reconfiguration is confined to the switch trial (Rogers & Monsell, 1995), then it cannot explain this effect. Additionally, the FRESC provides evidence that task-switching does not reduce to switching between compound cues (Logan & Bundesen, 2003), the effects of which should again be limited to the cued trial (trial 1).

Experiment 14

It is still not entirely clear that the restart cost is linked to activation processes (although Altmann & Gray, 2008, argue that it emerges from functional activity). Specifically, it could arise as a form of switch cost, caused by switching attention from cue processing to target processing (Altmann, 2002). This switching may be facilitated for the transparent cues as participants are switching from an iconic cue to an iconic target, which might induce less conflict than switching from a word cue to an iconic target. To address this possibility, we manipulate cue-transparency in Experiment 14 in two iconic-cue conditions, where no switch in modality is required. In one condition, the cues are the transparent rectangular cues used in Experiment 13. The other cues are non-transparent iconic cues used in Experiment 1 of this thesis (standard-cues). If the differential restart cost between cue-types in Experiment 13 was due to differences of modality switching, no difference should be found between the restart costs for the present experiment. However, if the differential restart cost was due to aided activation of WM representations for transparent cues, then again we should find reduced restart costs for transparent iconic cues compared to the less-transparent iconic cues.

Method

Participants

16 new participants were recruited from the same pool as all previous experiments. Data from 2 participants were lost due to computer error during the preparation of this chapter.

Apparatus & Stimuli

This experiment utilised the rectangular iconic cues from Experiment 13, and the non-transparent "standard-cues" from Experiment 1 (see figure 5). The targets were the oval targets used in Experiment 13. The rectangular cues were labeled "matching", and the non-transparent cues were labeled "unrelated". As the primary interest was the restart cost, only one-cue per target was utilised to simplify the analysis.

Procedure

The procedure was identical to Experiment 13 with the exception that only one cue was used per target. Due to this, runs were organised into switch or repeat runs, depending on the relevant target on the previous run. Switch probability was again left at .66.

Design

The experiment manipulated three independent variables, in a fully-crossed repeated measures design: *cue-type* (matching vs. unrelated icons), *sequence* (repeat vs. attention-switch), and *position* of trial in a run (trial 1 vs. trial 2 vs. trial 3 vs. trial 4). Error rates (%) and reaction time (RT) in milliseconds (ms) again served as dependent variables.

Results

Full-run analysis

Data trimming was identical to Experiment 13, and retained 92.3% of raw trials to be submitted to further analysis, consisting of two separate three-way repeated measures ANOVAs, with the factors as described in *Design*. RT analysis showed a main effect of *cue-type*, with faster RTs to matching cues (528ms) than to unrelated cues (594ms), $F(1,13) = 6.87$, $p < .05$, $\eta_p^2 = .35$. There was also a main effect of *sequence*, with faster RTs to attention-repetitions (540ms) than to attention-switches (582ms), $F(1,13) = 19.45$, $p < .01$, $\eta_p^2 = .60$. *Position* also reached significance, with slower RTs to trial 1 (734ms) than to trials 2-4 (502ms, 503ms, 504ms, respectively), $F(3,39) = 14.4$, $p < .001$, $\eta_p^2 = .53$. Contrasts showed that trial 1 differed from trial 2 [$F(1,13) = 15.41$, $p < .01$, $\eta_p^2 = .54$], but trial 2 did not differ from trial 3, which in turn did not differ from trial 4 (all F 's < 1).

Cue-type interacted with sequence, $F(1,13) = 15.09$, $p < .01$, $\eta_p^2 = .54$, driven by greater switch costs for unrelated cues (67ms) than for matching cues (17ms). There was also an interaction of cue on position, $F(3,39) = 8.52$, $p < .01$, $\eta_p^2 = .40$. Contrasts showed this was driven by a greater reduction in RT from trials 1 to 2 for unrelated cues (385ms) than for matching cues (78ms), $F(1,13) = 8.09$, $p < .05$, $\eta_p^2 = .38$. There was also an interaction of sequence on position, $F(3,39) = 14.77$, $p < .001$, $\eta_p^2 = .53$, due to a larger reduction from trial 1 to 2 for switch runs compared to repetition runs [$F(1,13) = 15.58$, $p < .01$, $\eta_p^2 = .55$]. Additionally, switch runs improved from trials 2 to 3, whereas repetition runs slowed slightly; contrasts showed this difference to be significant, $F(1,13) = 4.75$, $p < .05$, $\eta_p^2 = .27$. The three way interaction reached significance, $F(3,39) = 12.54$, $p < .001$, $\eta_p^2 = .49$.

Errors

For the error analysis, only the main effect of sequence reached significance, with more errors committed on switch trials (4.72%) than repetition trials (3.13%), $F(1,13) = 16.70$, $p < .01$, $\eta_p^2 = .56$.

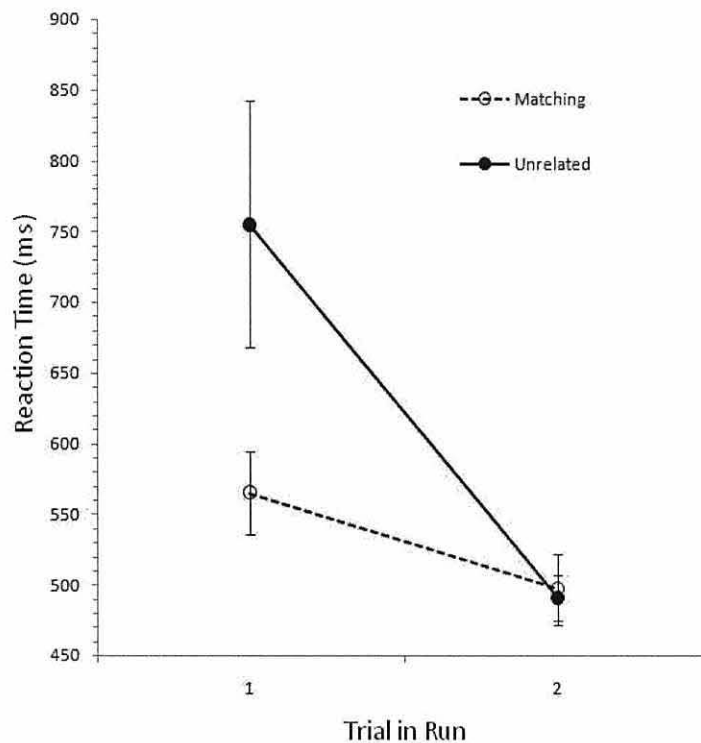
Restart-cost analysis

Figure 23. Reaction time restart-costs (Trial 1 RT vs. Trial 2 RT) from Experiment 14 for each cue-type. Error bars denote one standard error around the mean.

Restart costs were analysed by submitting repetition runs trials 1 and 2 for both matching and unrelated cues into a two-way ANOVA, with the levels cue-type (matching vs. unrelated) and position in trial (1 vs. 2). Mean RTs are re-plotted in figure 23 to show restart costs.

The main effect of *cue-type* [$F(1,13) = 6.53, p < .05, \eta_p^2 = .34$] and *position* [$F(1,13) = 13.77, p < .01, \eta_p^2 = .52$] were both significant. Importantly, cue-type interacted with position [$F(1,13) = 5.92, p < .05, \eta_p^2 = .31$].

This interaction was followed up with paired-*t* comparisons, which showed that the restart cost for matching cues [69ms; $t(13) = 5.62, p < .001$] was smaller than for unrelated cues [263ms; $t(13) = 3.14, p < .01$].

Discussion

The results from this experiment clearly suggest that the reduction of restart cost for transparent iconic cues is due to the aided activation of WM representations. Thus, taken together with the results from Experiment 13, provides novel evidence that the restart cost reflects cue-encoding processes rather than a form of attention-switching between cue and target processing. Additionally, FRESC was again found for this experiment, providing more evidence that the attention-switch cost is caused by encoding/activation processes that run on switch and repetition trials, contra the reconfiguration account of task-switching (Rogers & Monsell, 1995).

General Discussion

Across two experiments, we have sought to investigate the effects of cue-transparency on trial processing in an extended runs design. Specifically, we were interested whether cue-transparency effects the WM representation activated on the cued-trial, which would manifest in differential performance on un-cued trials between cue-types. We found no effect of cue-transparency on un-cued trials, suggesting that the WM representation used is the same between cue-types.

We also extended the findings of Grange and Houghton (in pressa) by showing that the differential cued-trial effects between cue-types was not caused by a superficial handling of the cue, as the effects of cue-transparency on the cued-trial did not have any effect on later target processing. To our knowledge, this is the first evidence that cue-transparency does not affect target processing, when cue- and target processing are separated empirically using an extended runs design.

We have also provided important evidence that the restart cost in task-switching reflects activation processes. It was possible that the restart cost reflects a form of attention-switch cost between cue and target processing; however, such an account is incompatible with the finding of modulation of restart cost with cue-transparency. However, it is still possible that the restart cost may

reflect processes independent of activating a WM representation. Indeed, a modest restart cost remained for transparent cues even though they provided the representation directly. Therefore, the restart cost may reflect a general interruption effect, where it takes time to collate back in mind what to do with the stimulus after being interrupted by a cue (Altmann & Trafton, 2007). Such "interruption-lags" have been shown to be reduced when a cue is informative as to the nature of the to-be-restarted task (Hodgetts & Jones, 2006a, 2006b; Schneider & Dixon, 2009). Alternatively, given the frequency of attention-switch trials in these experiments ($p=.66$), the restart cost may reflect the time required to monitor any change in the cue to determine whether it indicates a change in task or not (sometimes it does, sometimes it does not). In this case, the reduction in the restart cost for iconic cues would indicate that the exogenous provision of target information makes it more rapidly clear whether a switch or repeat of target is needed. Further work is required to distinguish between these competing causes regarding the restart cost.

These results also suggest that the source of the attention-switch cost found in this chapter and the previous chapter are due to encoding/activation processes rather than a time-consuming reconfiguration process. The presence of FRESC (Altmann & Gray, 2008) demonstrates sporadic intrusion of the previously relevant (and thus, incorrect) task during switch runs. This finding is not compatible with the reconfiguration account, as reconfiguration should be complete on trial 1 of a switch run. Altmann and Gray (2008) suggest FRESC is caused by the system always trying to execute the most active task, which sometimes suffers from selection failures as on switch runs the irrelevant task is still active.

The view of activation processes explaining the attention-switch costs here is perhaps not wholly incompatible with the notion of inhibition suppressing irrelevant tasks when switching, which implies an act of reconfiguration (Mayr & Keele, 2000; Mayr & Kliegl, 2003). We have supported this backward inhibition account in studies utilising a similar paradigm to that presented here, but with

one cue per task (Houghton et al., 2009). Our present experiments had frequent task repetitions, unlike Houghton et al., and the presence of task repetitions has been shown to reduce the $n-2$ repetition cost, an empirical marker of inhibition in task-switching (Philipp & Koch, 2006). Philipp and Koch support the view of activation and inhibition being two processes that can be used together to resolve conflict when switching to a new task, but suggest that when task repetitions are frequent, the cognitive system alters the way resolution of conflict occurs, preferring activation rather than inhibition. This bias occurs as when repetitions are frequent, task activation is often beneficial (i.e. to repeat tasks). This suggests that activation and inhibition are flexible processes of control that can be used interchangeably dependent upon the current experimental situation. When switches are more frequent, a bias towards inhibition is more beneficial, inducing the $n-2$ repetition cost. Thus support of activation processes in the present set of experiments is not intended to rule out any role for processes of targeted inhibition, for instance of the sort postulated in models of serial behaviour and the control of attention (Houghton & Tipper, 1996).

In cases of frequent task repetitions (as in our present experiments), another process of conflict resolution has been proposed that is similar to inhibition, but more automatic. Altmann (2002; Altmann & Gray, 2008) proposes that task codes decay in WM across a run of trials so as to prevent a catastrophic build up of highly activated task codes in episodic memory. Such decay can be empirically measured in extended runs designs, and is reflected in a monotonical increase in RT as the run progresses. This "within-run slowing" thus reflects a functional process (akin to inhibition) that reduces activation of a task allowing a subsequent switch. However, in our Experiments, there was no evidence of within-run slowing, which can perhaps be explained by having only 3 runs after the cued trial. In the majority of studies reporting within-run slowing, more than 4 trials were used, and often consisted of 9+ trials (but see Poljac et al., 2008). Indeed, examination of graphical representations of within-run slowing (e.g. Altmann, 2002, figure 3)

show very little, if any, slowing from trials 1-4. Therefore, it is important to extend our findings by replicating our experiments with more cue-less trials to measure if within-run slowing is evident in our paradigm. Such a finding would add strength to the activation account of task-switching. Additionally, manipulating cue-target translation requirements might affect this slowing. This could answer some important questions about how durable representations are that are generated endogenously (i.e. word cues from Experiment 13) and those that are generated exogenously (iconic, transparent, cues). It is also important to try to understand the relationship between decay of tasks and their specific inhibition. As Altmann and Gray (2008) point out, decay and inhibition are not mutually exclusive constructs. However, as is implicit in the very name, decay is generally presented as being a passive, automatic, process that will take place over time as long as nothing prevents it (such as purposeful re-activation). It is moot whether this assumption should be made, and the empirical basis for it needs close examination. It is far from inconceivable that the limited-capacity systems involved in task activation and maintenance, such as prefrontal attentional and working memory networks, have active "decay" mechanisms which promote cognitive flexibility and adaptability in ever changing environments by acting against the continued dominance of strongly activated cognitive and behavioural sets (Houghton & Tipper, 1996).

Chapter 7

General Discussion

CHAPTER 7: GENERAL DISCUSSION

Across five experimental chapters, I have investigated what cognitive control processes are in operation within the explicitly cued task-switching paradigm. The effects found are, on first impression, quite diverse with little relation to each other. I begin this discussion section by giving an overview of the effects found, before proposing an integrated view of how trials are dealt with in our paradigm (Grange & Houghton, in pressa, in pressb, 2009; Houghton et al., 2009).

Overview of findings

The first part of the thesis focussed on backward inhibition, inferred from so-called $n-2$ repetition costs. Chapter 2 provided further evidence that $n-2$ repetition costs can be caused by conflict during cue-target translation processes (Grange & Houghton, 2009; Houghton et al., 2009), demonstrating that the inhibitory mechanism thought to underlie this cost (Mayr & Keele, 2000; Mayr, 2007; Koch et al., in press) is not targeted exclusively at response processes (Schuch & Koch, 2003). Cue-target translation was made more difficult by the negative transfer of learned cue-target pairings, which enabled perceptual properties of cue- and target-displays identical between conditions. The negative transfer doubled the size of $n-2$ repetition costs. Chapter 3 demonstrated that the $n-2$ repetition costs in our paradigm are not caused exclusively by inhibition of the cue itself, but rather the costs emerge from inhibition of WM representations of which target to respond to. This provides further evidence (Altmann, 2006; Gade & Koch, 2008) that backward inhibition targets cue-independent representations of task performance, even in a paradigm where inter-trial conflict is placed on cue-processing. Additionally, this chapter provided evidence that even with transparent cues that typically show no observable $n-2$ repetition costs, some inhibition of WM representations might be occurring. When cues are transparent, the inhibited representation is bypassed in some, as yet, underspecified way. This is important, as previously we have taken the lack of $n-2$ repetition cost as

evidence against inhibition occurring for transparent iconic cues (Houghton et al., 2009), a view that may require alteration. An alternative explanation for the absence of $n-2$ repetition costs for transparent iconic cues was addressed in Chapter 4, namely the absence of cue-target overlap in our paradigm (Druey & Hübner, 2007). However, we found no influence of cue-target overlap on $n-2$ repetition costs.

The second part of this thesis addressed more typical task-switching effects, such as comparisons of repetitions and switches (Chapter 5). Using two cues per task, effects of cue-switches were separated from attention-switches. This manipulation showed significant cue-switch costs, but only when the cue was relatively non-transparent, therefore requiring cue-target translation. When the cue-target relationship was maximally transparent, no cue-switch costs were found. Despite the effects of cue-transparency on cue-switch costs, the attention-switch cost was equivalent in those situations where the target displays were identical between conditions. Based on this, we have suggested that the cue-switch cost reflects timely formation of an active WM representation of what to do, a process that is primed on cue-repetition trials; if the cue is transparent, no timely formation is required as the cue provides the representation (c.f. our formal definition of cue-transparency). Thus, the cue-switch cost is an important effect in studying cognitive control processes in task-switching; this stands counter to Logan and Bundesen's (2003; see also Schneider & Logan, 2005) suggestion that the cue-switch cost reflects passive perceptual priming of cue properties.

For the final chapter (Chapter 6), we demonstrated that cue-transparency affects only cue-processing stages of trial performance. As typically cue- and target-related responses are confounded, it was impossible to discern which aspect cue-transparency was aiding. Despite adding a series of cue-less trials, the same effects of cue-transitions as in chapter 5 were found on the cued-trials in Experiment 13. Additionally, using this extended runs design we were able to address the cause of the attention-switch cost consistently found. By examining

restart costs and full-run error switch costs, we suggest that the attention-switch costs arise from encoding/activation processes rather than by an act of reconfiguration. Therefore, from this perspective the attention-switch cost is seen as a repetition *benefit*, rather than a switch *cost*. The challenge the cognitive system faces is the same on all trials, that of ensuring the relevant task is the most active, and thus is selected.

Cognitive control processes in our paradigm

Given the variety of effects we have explored in this thesis, the challenge remains to integrate them to demonstrate all the effects are related, and arise from functional activity (Altmann & Gray, 2008; Newell, 1973). Doing so will highlight the cognitive control processes I suggest are in operation when participants perform our paradigm. We have compared cue-transparency conditions for all effects, as transparent cues are considered likely to reduce the burden of control processes as the cue is so informative as to what to do. Therefore, for the discussion of how the effects in this thesis are integrated, I will focus on relatively non-transparent cuing conditions (i.e. words or abstract icons), as these will demonstrate all of the processing stages. The processes outlined below can be considered the beginning of a more fine-grained formalisation of our recent reports concerning control processes in task-switching (Grange & Houghton, in pressb, in pressa, 2009; Houghton et al., 2009).

The dominant theme throughout all of the chapters is that successful performance requires a WM representation of what to do (see also Altmann & Gray, 2008; Arrington et al., 2007; Dreisbach & Haider, 2008, 2009; Meiran, Kessler, & Adi-Japha, 2008). Once a task cue has been perceptually encoded (Altmann & Gray, 2008), the representation needs to be activated on the basis of the cue (Altmann & Gray, 2008; for a similar view, see Mayr & Kliegl, 2000; 2003). The more transparent the cue, the less involved this process (Grange & Houghton, in pressa). The required representation for our paradigm is likely a visual representation of target properties, as RTs are speeded when iconic cues provide

the necessary target feature. If the cue is so non-transparent as to have no LTM support (i.e. abstract iconic cues), this process might employ a mediator to access the representation (Logan & Schneider, 2006a), although at this stage in the research process, we have not searched for empirical evidence of employment of a mediator for non-transparent cues.

Activation of the WM representation when a cue is presented runs on switch trials and repetition trials, inferred from the restart cost in Chapter 6, and the benefit of extended preparation intervals on repetition and switch trials in Experiments 10-12. Activation of the representation is aided if the current task is the same as the previous task. If the current task is different from the previous one, the system must ensure the relevant task is activated above and beyond that of the irrelevant task. This can be achieved by inhibiting representations from the previous task, in combination with activation of the relevant task. Inhibitory input may be under strategic control of the system, as in cases of frequent repetitions inhibition may become burdensome (Philipp & Koch, 2006). It remains for future research to address how the system might be able to adapt to varying switch probabilities, in fear of appealing to a homunculus construct (Monsell & Driver, 2000a). One distinction that would be critical is whether the reduction of inhibition usage is due to the system *reacting* to ongoing experience of switch probability, or whether the system is set prior to performance to a "non-inhibitory" setting due to *expectancies* of switch probability. The latter would suggest inhibition is under strategic control (i.e. proactive), whereas the former would suggest inhibition is a more reactive process. There is evidence in the literature that inhibition can be deployed proactively (Hübner et al., 2003; Kuhns et al., 2007), but these experiments do not address adaptation of inhibitory strategies given variable switch probabilities.

When a cue is followed by a run of cue-less trials (Chapter 6), the system needs access to a stable WM representation (Altmann & Gray, 2008). The evidence presented in this thesis suggests that the representation is the same,

regardless of the cue format (see also Chapter 3). This is important, as some researchers have found WM representations greatly affect task performance (Dreisbach & Haider, 2008, 2009), and one reviewer of Chapter 2 suggested that our results between cue-formats could be explained by differing WM representations being utilised. We made the important distinction in Chapter 5 that the nature of the WM representation used to perform a task differs greatly between paradigms (c.f. Arrington et al., 2007). In our paradigm, it is most parsimonious to assume that the representation used to guide behaviour is of a visual nature. Adding dual-task interference would be helpful in tapping the true nature of the representation. For example, participants could be presented with a visual working memory load (e.g. a few distinct, but abstract shapes) that must be retained for future recall, before being presented with a few task-switching trials. If the WM representation used for our paradigm is visual, then performance should be worse given this dual-task interference than if the secondary task involved retention of semantic items (e.g. words). One member of our lab has suggested that the WM representation used is actually verbal in nature (Pritchard, 2009); if this is the case then performance should be worse given the semantic WM load ¹⁸.

Given the overview of trial performance given relatively non-transparent cues, it remains to ask what processes are bypassed given transparent cues. Chapter 3 provided evidence that switching between transparent cues does generate inhibition, but that on trial n the transparent cue is able to bypass the inhibited representation. Thus the process being bypassed is the endogenous formation of the WM representation (which inhibition seems to affect), rather than bypassing the need for a representation per se. For example, Chapter 6 showed that despite enhanced performance on cued trials for transparent cues, this had no detrimental effect to cue-less trials, which must require a WM representation of what to do

¹⁸However, there is little empirical evidence for this assertion. It is possible that given an abstract cue a semantic label is used as a mediator to then access the WM representation (Logan & Schneider, 2006a). This does not imply that the final representation used to perform the task is semantic in nature.

(Altmann, 2002; Altmann & Gray, 2008). Therefore, transparent iconic cues are likely to be able to directly place a representation in WM. Trial processing after a WM representation is in place (regardless of how it got there, endogenously or exogenously provided) is identical between cue-types.

Theoretical implications of research

Given the growth of research on the explicitly-cued task-switching paradigm, it is important to investigate the effects of cue-processing on trial performance in isolation of other processes. This has been the aim of the present research program (including Houghton et al., 2009), where we have utilised a paradigm where participants switch between cue-target pairings, keeping all other aspects of the trial structure constant between trials (Cooper & Marí-Beffa, 2008; Schneider & Logan, 2007). Specifically, response processes were kept constant between trials. Response conflict has been suggested as a target of reconfiguration processes in many task-switching paradigms (Meiran, 2000; Philipp, Jolicoeur, et al., 2007; Philipp & Koch, 2005; Schuch & Koch, 2003; Verbruggen et al., 2005, 2006), but cannot explain our findings. Instead, we have shown modulation of task-switching effects due to cue-transparency when the cue and the attentional target are all that changes from trial-to-trial.

Conversely, it might initially appear that not all task-switching effects can be explained by our account, suggesting the limited scope of our theoretical approach. For example, Koch et al. (in press) raise the issue that Schuch and Koch (2003) found no $n-2$ repetition costs after trials that did not require a response (i.e. a "no-go" trial on $n-1$), even when the cue was non-transparent. However, this concern is borne from an apparent misapprehension of what our conclusions are from our research. We do not claim that our reported modulation of task-switching phenomena explains the presence/absence of equivalent effects in different paradigms. For example, we have already discussed that cue-transparency has been shown not to affect $n-2$ repetition costs in a different backward inhibition paradigm (Arbuthnott & Woodward, 2002). However,

Arbuthnott's paradigm, and that of Schuch and Koch (2003) have conflict at response selection stages of trial performance. Therefore, what we do suggest, is that inhibition targets those aspects of the trial structure that generates the greatest inter-trial conflict. Therefore, the statement of Koch et al. (in press) can be explained by our hypothesis. Specifically, no $n-2$ repetition costs were found, *even with non-transparent cues*, as the greatest source of inter-trial conflict at $n-1$ was removed (i.e. the response selection), and therefore no inhibition occurred. Our research program merely serves to highlight that cue-related processes can affect trial performance if the source of conflict is shifted upon them.

Another finding from the literature that our research does not address at first impression is that switch costs are found even in paradigms with no cue. For example, the alternating runs procedure of Rogers and Monsell (1995) had no explicit cue, so the switch cost can not be a product of cue-target translation processes. Additionally, the voluntary task-switching procedure introduced by Arrington and Logan (2004a) also has no cue. In this paradigm, participants choose on each trial which task to perform. For example, participants are presented with a number from one to nine, excluding five, and must make a magnitude or parity judgement. Each task uses unique response keys (e.g. magnitude - left hand [d,f keys]; parity - right hand [j,k keys]), so that the choice of the participant can be gleaned. In this paradigm, substantial switch costs are found (see also Arrington & Logan, 2005). However, participants may utilise a task cue, but one that is generated endogenously by the participant. Formation of a new endogenous cue to guide behaviour may benefit from inhibition of the previous tasks representation in WM (Mayr & Bell, 2006). Indeed, when voluntarily switching between three tasks, $n-2$ repetition costs have been shown in this paradigm (Lien & Ruthruff, 2008). The voluntary task-switching paradigm, however, has substantial conflict at all stages of the trial structure, due to the limited support from the environment in terms of task cues or predictable sequences. Therefore, it might be important to delineate the contributions of the

different trial components to the voluntary task-switch cost (task selection, response selection, response execution etc.). One could remove response conflict by combining the voluntary task-switching paradigm with our target-location paradigm. Any cost in this paradigm would be due to task selection and attention switching alone.

Limitations of research

Under-specification of task processes

Although holding many task-set parameters constant in our paradigm is beneficial for understanding the contribution of cue-related processes to task-switching, it does make generalisation to other paradigms more problematic. I have discussed suggestions for how to integrate our paradigm and theoretical approach with other paradigms to start generalising our reports, which I shall not repeat here. Generalisation might also be aided by explicit computational modeling of our assumptions, which would allow us to test which of our assumptions can be generalised into an integrated view of task-switching.

Such modeling would also enable a more fine-grained analysis as to what processes are actually involved in the underspecified notion of cue-target translation. Currently, this process is a rather loose construct, although we can be relatively sure that the end product of this process is an activated (likely visual) representation of the target to search for. Modeling within a computational architecture (Newell, 1990) would afford tight constraints on how such representations are formed. For example, if modeling within the architecture of ACT-R (see for example Altmann & Gray, 2008; Sohn & Anderson, 2001, 2003), the latency of the retrieval of a relevant representation from episodic memory is determined by sub-symbolic equations that select the most active "chunk" in episodic memory. A chunk's activation is determined by the number of previous presentations of the chunk, and the time since its last usage. Chunks that are retrieved frequently (or one that was retrieved most recently) are more active, and

are thus more likely to be retrieved if more than one chunk is relevant to the current situation (which is quite frequent in task-switching situations with multivalent stimuli). Retrieval times of chunks are based on biologically plausible temporal occurrences estimated from single-cell recordings (J. R. Anderson, 2007), so the dynamics of reaction time throughout the experimental blocks can be compared to concise model predictions.

N-2 repetition costs & preparation

Another aspect of our theory regarding cue-based causes of the $n-2$ repetition cost that might suggest limitations in its generality is the repeated failure in the literature to find a reduction of $n-2$ repetition costs given extended preparation intervals (Bao et al., 2006; Mayr & Keele, 2000; Mayr, 2002; Gade & Koch, 2008; Schuch & Koch, 2003). Indeed, Koch et al. (in press) states this as one of two incompatible findings in the literature with our account. This discrepancy is superficially quite concerning for our cue-target translation locus of backward inhibition effects. Specifically, if inhibition of the preceding representation and activation of the current representation occurs during cue onset, then extending the CSI should allow for $n-2$ repetition costs to be overcome completely before an overt response is required.

However, there are key differences between our paradigm and that of those in which no effect of preparation on $n-2$ repetition costs was found. Specifically, all of the studies that have manipulated CSI in a BI design have had greatest inter-trial conflict centered at response stages of task performance (Schuch & Koch, 2003). As we have discussed previously (Houghton et al., 2009; see also Chapter 2), in such studies it is likely that inhibition will also be centered on response stages; therefore, manipulation of the preparation interval will have little effect on the $n-2$ repetition costs, as these are only overcome when a response for the new task is to be selected (i.e. long after cue onset; Philipp et al., 2007).

Therefore the current evidence is silent as to whether an extended preparation interval can reduce $n-2$ repetition costs in a paradigm where inter-trial conflict is

centered on cue processing stages. Work is ongoing in our laboratory to address this important issue by manipulating preparation intervals in a negative transfer paradigm (Grange & Houghton, in press). However, there is some initial post-hoc evidence that $n-2$ repetition costs are reduced given adequate preparation time.

I constructed cumulative distribution frequency (CDF) plots of data from Experiment 1 of this thesis. CDFs (Ratcliff, 1979) are constructed by rank ordering all RTs from fastest to slowest for each individual subject for each individual condition. Quantiles are then calculated for each participant and condition. Once this has been performed, the quantiles are averaged across participants. The CDFs are then plotted. The CDFs provide much more detail than average RT alone, as differences between conditions can be examined across the whole RT distribution (from fastest to slowest). The fastest RTs in the distribution are those in which participants were suitably ready for the upcoming trial (DeJong, 2000), whereas the slowest RTs are likely to reflect those proportion of trials in which participants were not prepared. Thus, we can investigate the dynamics of $n-2$ repetition costs in our paradigm between prepared trials (fast) and unprepared trials (slow). The CDFs for "standard" cues and "switched" cues can be found in figures 24 and 25.

For the standard cues where we found a modest $n-2$ repetition cost, it is clearly visible on the CDF that this cost only emerges later in the RT distribution. Quantiles at the faster (prepared) end of the distribution show no difference between ABA and CBA trials. This pattern is even more pronounced for the switched cues CDF. Here, despite a substantial $n-2$ repetition cost at later stages of the RT distribution, again during the prepared trials, very little difference is evident between ABA and CBA sequences. These analyses are promising for the assertion that $n-2$ repetition costs can be overcome given sufficient preparation time in our paradigm (c.f. Koch et al., in press).

This pattern of results, however, is different to that of Schneider and

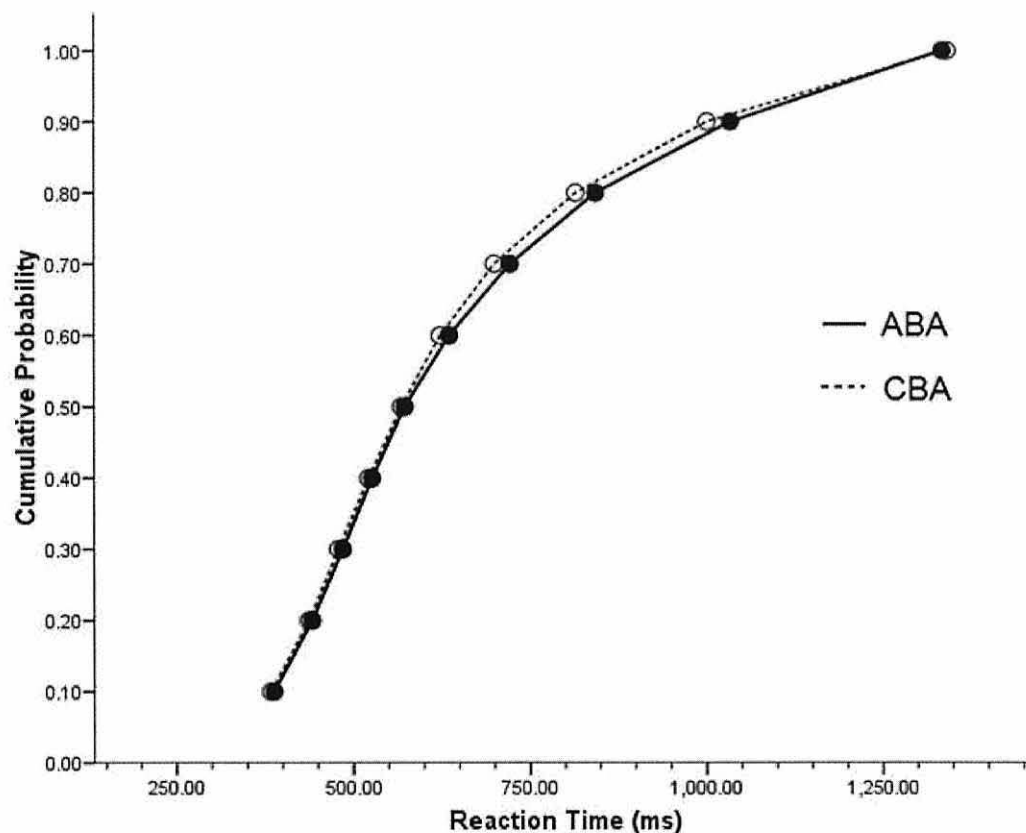


Figure 24. Cumulative distribution frequency plots for standard-cue ABA and CBA sequences from Experiment 1.

Verbruggen (2008), who constructed CDFs of their RTs, and found $n-2$ repetition costs were present throughout the whole distribution. However, their paradigm had considerable conflict centered on response-processes (Philipp, Gade, & Koch, 2007; Schuch & Koch, 2003). Therefore, the finding of Schneider and Verbruggen actually strengthens my suggestion that CSI-reduced $n-2$ repetition costs have not been found due to the fact that conflict in these paradigms has centered on response stages, and are thus impervious to preparation processes. Future work would seek to replicate the CDF of Schneider and Verbruggen and that of Grange and Houghton (in pressb) to see if the differences truly can be explained by different loci of conflict. This future work might also benefit from explicit distribution-fitting analysis of the RT distribution (Heathcote, Brown, & Mewhort, 2002; Heathcote, Brown, & Cousineau, 2004; Steinhauser & Hübner, 2009). Typically, in such analysis an Ex-Gaussian distribution fits best (Heathcote,

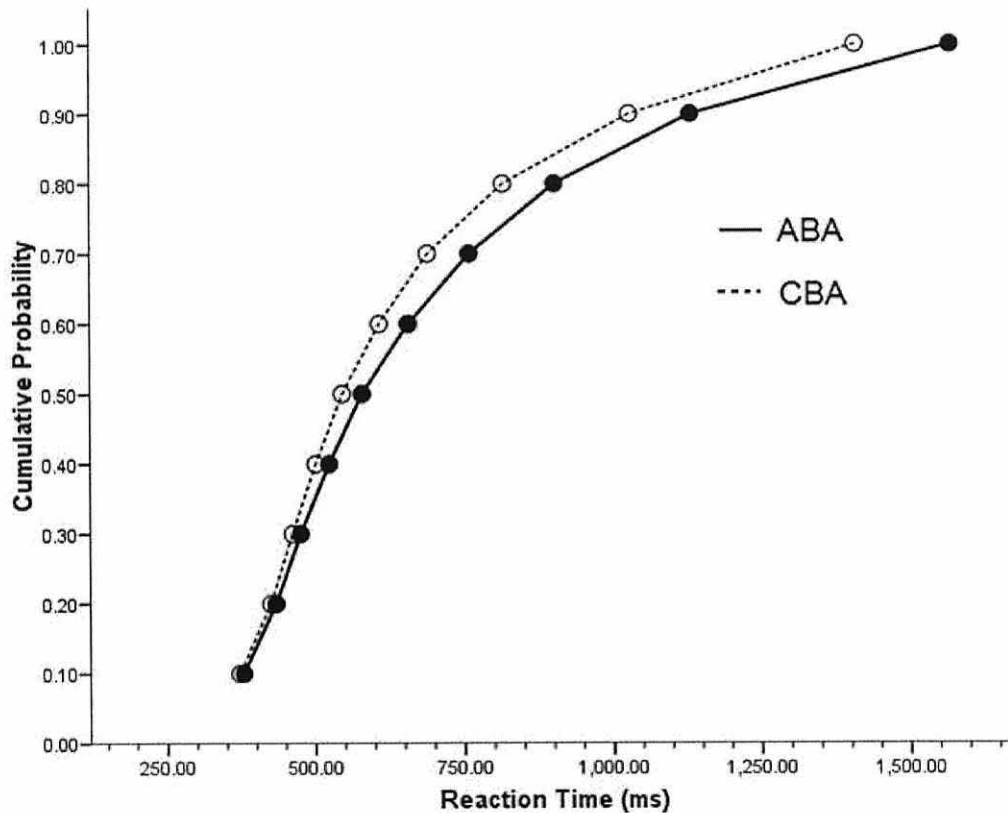


Figure 25. Cumulative distribution frequency plots for switched-cue ABA and CBA sequences from Experiment 1.

Popiel, & Mewhort, 1991). The Ex-Gaussian distribution is a cohesion of a Gaussian (i.e. normal) and an exponential distribution, with the exponential component reflecting the tail end of the RT distribution. Fitting such a distribution estimates three parameters: a mean for the Gaussian distribution, a standard deviation of the Gaussian component, and a mean for the exponential component. These parameters can be treated as dependent variables in statistical analysis. Therefore, such fitting allows one to investigate whether backward inhibition affects the Gaussian or exponential component of an RT distribution. This distinction is important, as the exponential component is thought to be affected by more controlled processes, whereas changes in the Gaussian component are thought to be driven by more automatic processes (see Steinhauser & Hübner, 2009, for a discussion on these issues). Ex-Gaussian analysis could then be conducted on paradigms where the locus of conflict changes (i.e. cue- or response-related) to address which aspect of the distribution is being affected by

$n-2$ repetition costs, and thus inhibition.

Do $n-2$ repetition costs really reflect inhibition?

It might seem unparsimonious to add the assumption of inhibitory processes during task-switching when the model of Altmann and Gray (2008), which has quite simple assumptions, can fit a vast array of task-switching data without inhibition. Therefore, it is imperative to address whether $n-2$ repetition costs really do reflect inhibitory processes. This is important, as inhibition as a psychological construct has received a lot of criticism over recent years (Carr, 2007; Gorfein & MacLeod, 2007; Gorfein & Brown, 2007; MacLeod et al., 2003), with many suggesting non-inhibitory accounts should always be pursued. The approach researchers adopt to the view of inhibition have been categorised by Gorfein and Brown (2007) as that of "inhibitophiles" and "inhibitophobes", with the latter being resistant to the notion of cognitive inhibition.

In relation to this categorisation, Gorfein and Brown (2007) states three questions an inhibitophile must address when postulating inhibition to explain an effect. These three questions can be collapsed into one: can "inhibitory" results be accounted for without inhibition? $N-2$ repetition costs appear to fare rather well against this question, as non-inhibitory accounts thus far have been shortcoming, and have been refuted empirically. For example, one non-inhibitory account that has succeeded in explaining other "inhibitory" paradigms has been episodic-priming (see for example Logan, 1988). Episodic-priming suggests that when selection in a task occurs, all selection-relevant items (i.e. stimulus, stimulus dimension, response, distractors etc.) are stored in episodic memory, akin to a memory "photo". When this task occurs in the future, the previously stored memory "photo" is automatically retrieved. If the selection-relevant items retrieved from memory differ from the selection-relevant items for the current task, a cost arises due to a mismatch between current task-items and retrieved task-items. This approach has been successful in explaining negative-priming effects (Tipper, 1985, 2001) in selective attention, which were previously thought

to solely reflect inhibition of distractors (Neill, 1997). By this notion, $n-2$ repetition costs may occur as task-items from $n-2$ may differ from those on the current trial, thus inducing a mismatch cost. However, Mayr (2002) found equivalent $n-2$ repetition costs when $n-2$ and n contained the same items and when they contained different items. Additionally, and directly related to this thesis, the episodic-priming account would predict greater $n-2$ repetition costs when the cue for task A switches across an ABA sequence; however, Experiment 4 showed that ABA sequences are actually more costly than A'BA sequences.

However, there are two potential causes for concern when using $n-2$ repetition costs to infer inhibitory mechanisms have been deployed; each will be discussed in turn. These do not suggest that inhibition is not occurring, but rather highlight the complications of utilising the $n-2$ repetition cost to infer such inhibitory mechanisms.

Correlated costs and benefit problem (CCB)

M. C. Anderson and Levy (2007) highlighted a critical oversight in the majority of articles reporting inhibitory mechanisms. This problem arises from the fact that inhibitory paradigms always measure the aftereffects of inhibition, rather than inhibition itself. With the $n-2$ repetition cost, we are measuring the aftereffects of inhibition at n that occurred two trials ago. This is akin to measuring the speed of a train long after it has left the station.

Cognitive control mechanisms that might be recruited during a particular process ought to be consistently recruited; if these mechanisms are engaged under one set of circumstances, they should also be engaged in future situations that have similar circumstances. Therefore, a paradigm that induces inhibition at time "T" and later measures behavioural after-effects of this inhibition (e.g. at time "T+1", fail to consider the involvement of inhibition at T+1. This is problematic as inhibition at T+1 will contribute to the recorded measurement, together with the (targeted) residual inhibition from T.

In the context of the backward inhibition paradigm, the standard behavioural

measurement (RT) may be confounded. The standard comparison of RT to task *A* in an ABA and CBA sequence may have more differences than currently identified. The current assumption is that the sequences only differ in terms of activation levels of task *A* at the completion of a triplet: in an ABA sequence, task *A* will be suffering from residual inhibition, and will require more time to activate than task *A* in a CBA sequence, which has no residual inhibition. Looking deeper exposes another critical distinction between the two sequences: returning to a recently abandoned task in an ABA sequence will not only require overcoming the inhibition of *A*, but will also require the inhibition of the now-irrelevant task, *B*. The relative activation levels of tasks *B* and *A* at the time of switching to trial *n* are different in both triplets: in an ABA sequence, as task *A* has been inhibited, the relative activation levels of task *A* (inhibited) and task *B* (active) is great; alternatively, in a CBA sequence, task *A* is at resting level, whereas task *B* is active. The larger difference in activation levels between task *A* and task *B* in an ABA sequence will require *greater* levels of inhibition to be applied to task *B* on completion of the triplet. Therefore, RT in an ABA sequence is an (apparently) inseparable mixture of the time taken to overcome the residual inhibition of task *A*, *and* the extra time needed to inhibit task *B*.

This creates a problem when using *n-2* repetition costs to measure inhibitory input. For example, greater inhibitory control will *benefit* task selection at *n*, and will also cause a *cost* due to persisting inhibition from trial *n-2*. Therefore it is entirely feasible that people with greater inhibitory control will actually have reduced *n-2* repetition costs due to the benefit of faster conflict resolution at *n*. This greater inhibitory input will in turn create greater inhibitory traces at trial *n-2*, and the two may balance out at trial *n* to produce a null *n-2* repetition cost.

Conversely, worse inhibitory control will mean less (or no) persisting inhibition of task *A* across an ABA sequence. However, this might not produce an *n-2* repetition benefit, as at trial *n*, interference from the active task *B* has to be countered with non-inhibitory processes, which might take more time (i.e. await

decay of task B, Altmann & Gray, 2008). Therefore, this scenario too might predict no observable $n-2$ repetition costs.

At present, I am uncertain how to efficiently address the CCB problem. One possible measure is to analyse RTs for task B on ABAB trials compared to CBAB trials. During an ABAB trial, according to the CCB problem, performance of task A at $n-1$ will require greater inhibition of task B than in a CBAB sequence, as task A is suffering from residual inhibition. Because of this residual inhibition, the differential activation levels between task A and B are greater in an ABAB sequence than in a CBAB sequence. Therefore, greater inhibition at $n-2$ in an ABAB sequence will produce slower RTs for completion of the final task B than in a CBAB sequence. Although this analysis could be performed on my current data set, I have not done so as it is a post-hoc investigation, and might require a more thought-out experimental design.

Individual differences

It is an often found (but rarely reported) finding that not every individual shows $n-2$ repetition costs. This was demonstrated in Experiment 4, where a median split on $n-2$ repetition costs demonstrated considerable differences between those who show costs and those who do not. Individual differences in an effect of interest is important for theorising as it constrains models considerably (Underwood, 1974). Specifically, in the current context, if the inhibitory theory of task-switching is correct, and $n-2$ repetition costs reflect inhibition, how are those who show no $n-2$ repetition costs able to switch tasks? Mayr (2006) reported findings from neurological patients, and demonstrated the patients with lesions to the right pre-frontal cortex show no $n-2$ repetition costs, whereas those with lesions to the left pre-frontal cortex did show $n-2$ repetition costs. Based on these findings, Mayr et al. (2006) concluded that inhibitory deficits occur with damage to the right pre-frontal cortex. Yet despite this "inhibitory deficit", such patients were able to switch tasks just as well as those with "intact" inhibition.

This issue is difficult to examine, as absence of $n-2$ repetition costs does not

mean no inhibition occurred during switching. Experiment 5 demonstrated that inhibition might be occurring in situations where we have failed to observe $n-2$ repetition costs in the past. Additionally, in Experiment 4, the no-BI group had faster RTs, suggesting perhaps that lack of $n-2$ repetition cost may reflect ease of *re-activating* an inhibited representation at n , rather than implying no inhibition occurred at $n-1$ (see also the CDFs in figures 24 and 25).

I have discussed in more detail above the problem of using the $n-2$ repetition cost as a pure measure of inhibition (see the section "Correlated costs and benefits problem"). However, let us assume that $n-2$ repetition costs do reflect inhibition. The question remains what is driving the individual differences of observed cost. Individual differences in inhibitory processing has been studied by Conway, Tuholski, Shisler, and Engle (1999) and Engle, Conway, Tuholski, and Shisler (1995). They have suggested that inhibition is a resource dependent process, and that if the demands of the primary task consume WM capacity, there is no WM resources left to deploy inhibition. If however the primary task does not consume WM, resources are free to deploy inhibition. They tested their hypothesis utilising the negative priming paradigm (Tipper, 1985), where RTs are slower to a probe at trial n if it was present as a distractor on $n-1$. This negative priming is thought to be caused by inhibition of the distractor at $n-1$, which persists and hinders its activation as a target at n (Houghton & Tipper, 1996). Conway et al. (1999) and Engle et al. (1995) manipulated WM load by intermittently presenting letters between trials that were to be retained for future recall. Conway et al. (1999) and Engle et al. (1995) found that the negative priming effect decreased as the WM load increased, suggesting that a consumed WM allowed for no free resources to deploy inhibition. By this account, those individuals with larger working memory capacity (WMC) should always have more resources left to deploy inhibition.

In terms of the variation in $n-2$ repetition costs, one possibility is that they are driven by natural variation in individuals WMC. Based on the assumptions of Conway et al. (1999) and Engle et al. (1995), those that show no $n-2$ repetition

cost may have larger WMC (and thus more free resources to deploy inhibition), and those that show no cost may have low WMC (as the primary task may consume all of their WMC, leaving no free resources for inhibition). An alternative prediction might be that those with larger WMC may be able to maintain more than one representation active in WM, thus requiring less inhibition when switching between them (although Mayr & Kliegl, 2000 and 2003 suggest that such co-activation does not occur in WM). Despite these differing predictions, in ongoing work I have been unable to find a correlation between WMC (as measured using the automated operation span task, Unsworth, Heitz, Schrock, & Engle, 2005) and $n-2$ repetition costs (see Appendix B). Future work might pursue the factors contributing to individual differences in $n-2$ repetition costs, and why they are not related to WMC. An alternative approach to correlational analysis might be to actively consume WMC by presenting a WM load before a series of trials, investigating its effects on $n-2$ repetition costs.

Conclusion

Returning to the very beginning of this thesis, I presented the problem of how are humans able to control their ongoing cognitive processes. Additionally, I presented the stability/flexibility dilemma (Goschke, 2000) that the cognitive system faces, that of maintaining a stable representation that is at the same time easily removed when task demands change. I have provided evidence that control of cognitive processes requires formation of a durable working memory representation of task demands, which can be accessed to guide behaviour in multi-task environments. Far from being a homunculan construct, I have suggested that this is achieved by activation processes forming the present WM representation (a process that can be aided with considerable environmental support in the form of transparent cues), together with task-specific inhibitory mechanisms targeting irrelevant representations. It remains for future work to develop modeling of this inhibitory process, enabling a more fine-grained description as to how the cognitive system actually achieves it.

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Appendices

Appendix A — CSI between-subjects manipulation

Appendix B — Working memory capacity & backward inhibition

Appendix A

CSI manipulation between-subjects

Method

Participants

32 participants were selected from the same participant pool as experiments 10-13. None had taken part in any of these previous experiments. 16 participants were assigned randomly to each CSI level.

Apparatus & Stimuli

The experiment utilised the same experimental apparatus and stimuli from Experiment 10.

Procedure

The procedure was very similar to that of Experiment 10. All participants were exposed to two blocks (one for each cue type) which consisted of five sub-blocks of 110 trials. However, each participant was exposed to only one level of CSI for the duration of the experiment (100ms or 900ms). As CSI was now manipulated between subjects, I decided it might be important to keep the overall trial duration constant (as measured by the time for a response on one trial to the presentation of the stimuli on the next trial (the response-stimulus interval). Therefore, RCI was also manipulated between subjects so that the CSI and RCI added to 1 second. Thus, for those with a CSI of 100ms, the RCI was 900ms; this was reversed for those exposed to the 900ms CSI.

Each block for both cue-types was preceded by a practice session consisting of 26 trials. Switch probability was not manipulated for this experiment, and was held constant at .33 to give equal cell sizes for each transition type (cue-repetition, cue-switch, & attention-switch).

Table A1: Mean reaction time (RT in ms), standard deviations (in parentheses), and errors for cue-repeat, cue-switch, and attention-switch sequences in Experiment 12 across both cue-types

Cue	CSI	Sequence							
		Cue-Repeat		Cue-Switch		Attention-Switch		C-sc ^a	A-sc ^b
		RT	Error	RT	Error	RT	Error		
Words	100ms	657	4.24	728	4.37	784	7.52	71	56
		(25)		(25)		(31)			
	900ms	513	3.30	525	4.23	539	5.18	12	14
		(25)		(25)		(31)			
Icons	100ms	539	3.24	541	3.13	586	3.93	2	45
		(22)		(22)		(27)			
	900ms	511	3.78	489	3.69	513	5.37	-22	24
		(22)		(22)		(27)			

^aC-sc = Cue-switch cost

^bA-sc = Attention-switch cost

Design

The experiment manipulated three independent variables in a mixed factorial design: *sequence* (cue-repetition vs. cue-switch vs. attention-switch), *cue-type* (icons vs. words) and *CSI* (100ms vs. 900ms). Cue-type and sequence were manipulated within subjects whereas CSI was manipulated between subjects.

The dependent variables were reaction time (ms) and error (%).

Results

Error rates and RT were submitted to separate mixed factorial repeated measures ANOVAs, with the factors as stated in *Design*. Mean RT and error is shown in table A1.

RT analysis

For the RT analysis, all main effects were significant: *CSI*, RT to the short CSI ($M=639$) was slower than to the long CSI ($M=515$) $F(1,30) = 14.12, p < .001, \eta_p^2 = .32$; *cue-type*, RT to iconic cues ($M=530$) were faster than to word cues ($M=624$) $F(1,30) = 74.54, p < .001, \eta_p^2 = .71$; *sequence*, RT to cue-repetitions ($M=555$) were faster than to cue-switches ($M=571$), which in turn was faster than to attention-switches ($M=605$), $F(2,60) = 42.05, p < .01, \eta_p^2 = .58$. Planned comparisons showed that both the cue-switch contrast [$F(1,30) = 24.11, p < .001, \eta_p^2 = .45$] and the attention-switch contrast [$F(1,30) = 38.31, p < .001, \eta_p^2 = .56$] were both significant.

Cue-type interacted with CSI, $F(1,30) = 44.41, p < .001, \eta_p^2 = .60$. This was due to slower RTs for word cues than iconic cues only for the short CSI. Replicating experiments 10 and 12, there was a cue-by-sequence interaction, $F(2,60) = 19.72, p < .001, \eta_p^2 = .40$. Planned contrasts investigated this interaction and showed that the cue-switch cost was greater for word-cues (41ms) than for iconic-cues (-10ms), $F(1,30) = 33.62, p < .001, \eta_p^2 = .53$. The magnitude of the attention-switch cost did not differ between cue-types, with the attention-switch cost being 35ms for the word-cues and 34ms for the iconic cues, $F(1,30) = 0.01, p > .9$.

The three-way interaction also reached significance, $F(2,60) = 4.6, p < .05, \eta_p^2 = .13$. This interaction again reflected a greater reduction of the cue-switch cost for the word cues than for the iconic cues at the long CSI compared to the short CSI, $F(1,30) = 3.94, p = .056, \eta_p^2 = .12$. The reduction in attention-switch cost at the longer CSI was similar in magnitude between cue-types, $F(1,30) = 1.79, p > .19$ (see table A1).

Error analysis

For the error rates, there was a main effect of *cue-type*, with more errors being committed for word cues ($M=4.81$) than for iconic cues ($M=3.86$), $F(1,30)$

= 4.82, $p < .05$, $\eta_p^2 = .14$. There was also a main effect of sequence, $F(2,60) = 15.8$, $p < .001$, $\eta_p^2 = .35$. Planned comparisons showed that cue-repetitions ($M=3.6$) did not differ from cue-switches ($M=3.9$), $F(1,30) = 0.82$, $p > .3$. However, cue-switches elicited less errors than attention-switches ($M=5.5$), $F(1,30) = 13.96$, $p < .01$, $\eta_p^2 = .32$. There was no main effect of CSI, $F(1,30) = 0.03$, but CSI did interact with cue-type, $F(1,30) = 5.29$, $p < .05$, $\eta_p^2 = .15$. This interaction was generated by more errors being made to word cues than iconic cues only at the short CSI; for the long CSI, there was no difference between errors to icon and word cues (see table A1). No other interactions were significant.

Appendix B

Working Memory Capacity & Backward Inhibition

Method

Participants

75 participants were recruited from the Bangor University participant panel. None had participated in any of the previous experiments. Six participants were removed due to failure to meet the 85% accuracy criteria of the maths portion of the AOSPAN task. Two further participants were removed due to unusually low AOSPAN scores (AOSPAN score of 3).

Apparatus & Stimuli

The same apparatus and stimuli as Experiment 6 were used. Both the BI task and the WMC task were presented and recorded by the computer.

Procedure

For the BI task, I utilised the word cues from Experiment 6 (non-overlapping cues and targets). I used an RCI of 100ms instead of our more usual 500ms as I wanted to create a scenario that produces robust $n-2$ repetition costs (see Experiment 7). There were two blocks of 130 trials, preceded by a practice block of 28 trials.

The AOSPAN is entirely mouse driven. Participants must remember letters that are presented serially, as a recall test follows. Letter presentation is preceded by a simple maths problem (e.g. "[9*2]-5=?"). Participants must solve the maths problem as quickly as possible. This is to prevent active rehearsal of the presented letters. The amount of letters and maths problems presented to the participant is known as the set size, and ranges from 3-7.

An experimental trial can be seen in figure B1. It begins with a maths problem, which must be solved as quickly as possible. Once solved, the participant

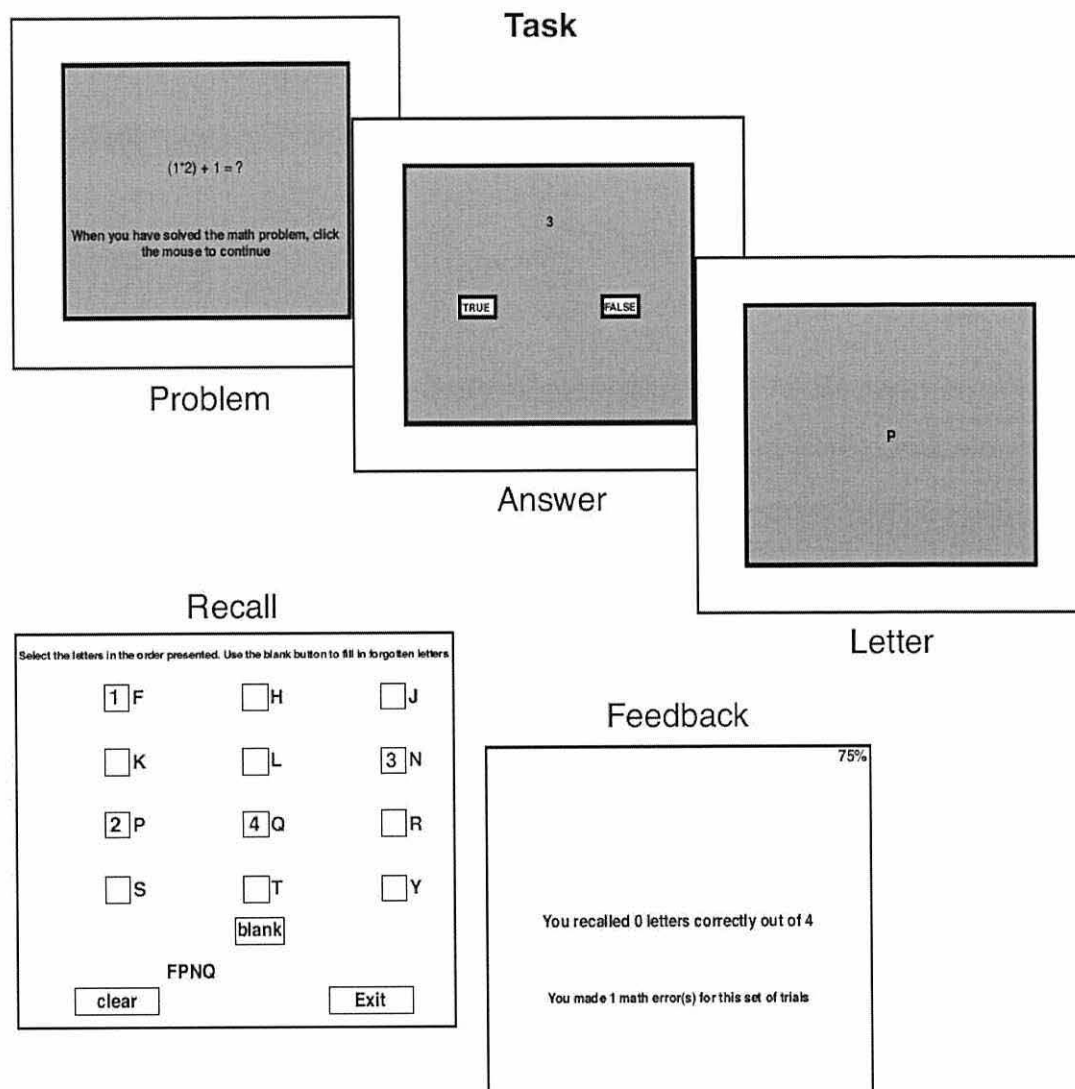


Figure B1. Trial structure from the Automated Operation Span Task (Unsworth et al., 2005).

clicks the mouse button, and the program proceeds to the answer page, on which a number appears. The participant is required to match whether the presented number is the correct solution to the previous maths problem, and click the appropriate box. Once the participant had made a response, a letter (F,H,J,K,L,N,P,Q,R,S,T, or Y) appeared for 800ms. This was the to-be-remembered item. This process could repeat for between 3-7 cycles (depending on the current set size) after which time the letter recall page appeared. The participant was required to select the remembered letters in the correct order they were presented. This portion of the test was not timed. After

they had given their responses, the program would present feedback for how many letters they correctly recalled from the current trial, and feedback on their cumulative percentage of correct responses for the maths problems. Participants were explicitly asked to retain accuracy over 85%; failure to do so would result in removing their data from the final analysis.

There were three sets of each set size, which ranged from 3-7 letters. This made a total of 75 letters and 75 maths trials. The order of set size was totally random to remove any effect of participant expectancy on current set size.

Upon conclusion, the program reported five scores. Ospan scores, Ospan total, math errors, speed errors, and accuracy errors. The Ospan score is a total of all perfectly recalled sets. This uses an all-or-none method. For example, if a participant correctly recalls 3 letters from a set size of 3, 4 letters from a set size of 4, and 3 letters from a set size of 5, their OSPAN score would be 7. The Ospan total score is the sum of all recalled letters in their correct positioning. In the above example, their Ospan total score would be 10. Of the errors reported for the maths problem, they were split into speed and accuracy errors. Speed errors were trials in which the participant did not respond to the maths problem screen within the allocated time. Accuracy errors were trials in which they responded incorrectly to the maths answer display.

Results

For the BI data, data trimming was identical to Experiment 1. No participant made more than 10% errors, so none were excluded on this basis. To begin with, I report the statistical analysis of the BI measure, before proceeding to the correlational analysis.

The error rates for the BI task were analysed utilising a paired *t*-test. Results showed that ABA sequences produced more errors (3.89%) than CBA sequences (2.97%), $t(66)=3.17$, $p<.01$. For the RT scores, ABA sequences were slower (626ms) than CBA sequences (601ms), $t(66)=4.67$, $p<.001$.

For the correlation, a matrix with Ospan score, Ospan total, *n*-2 repetition

Table B1: Correlation matrix from AOSPAN experiment, correlating $n-2$ repetition costs with Ospan scores.

Measure	(1)	(2)	(3)	(4)
(1) Ospan	–			
(2) Ospan Total	.92**	–		
(3) N-2 repetition cost - RT	.01	-.02	–	
(4) N-2 repetition cost - Error	.07	.1	.25*	–

Note. * = $p < .05$, ** = $p < .01$.

cost (RT) and $n-2$ repetition cost (Error) were analysed using two-tailed analysis. The matrix together with Pearson's r values are shown in the table B1.

As can be seen from the table, Ospan scores did not correlate with either measure of BI. BI RT did correlate with BI error, $r(66) = .25$, $p < .05$.