ORIGINAL ARTICLE

Cue-switch costs in task-switching: cue priming or control processes?

James A. Grange · George Houghton

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 (WM) representations of task demands. Across two experiments we manipulated cue-transparency in an attention-switching design to test the contrasting hypotheses of cue-switch costs, and show that such costs emerge from control processes of establishing relevant WM representations, rather than perceptual priming of the cue itself. When the cues were maximally transparent, cue-switch costs were eradicated. We discuss the results in terms of recent theories of cue encoding, and provide a formal definition of cue-transparency in switching designs and its relation to WM representations that guide task performance.

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, 2009; Monsell, 2003). When participants have to switch tasks between trials (B<u>A</u> task sequence) they show increases in reaction time (RT) and errors compared to repeating a task (A<u>A</u> 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, b; Koch, 2001).

When the desired task is explicitly cued in this manner, a task-repetition (A<u>A</u>) 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, Logan, & Schneider, 2007;

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Jost, Mayr, & Rösler, 2008; Mayr & Kliegl, 2003; Monsell & Mizon, 2006) while others report that task-switches are no more costly than cue-switches alone (Arrington & Logan, 2004; 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 [e.g. instantiating an active task representation in working memory (WM); Arrington et al., 2007; Mayr & Kliegl, 2003] 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 to 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 (Grange & Houghton, 2009; Houghton, Pritchard, & Grange, 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 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, b; Mayr & Kliegl, 2000, 2003; Schneider & Logan, 2005). The nature of this representation is generally underspecified 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 S–R 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, b; 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, b; Schneider & Logan, 2005). 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. "Magnitude and 8 = high") by probing semantic, as well as episodic, memory (Arrington & Logan, 2004). 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 LTM, 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 (cf. 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 LTM 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 (see Fig. 1 of this paper for an example). 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.
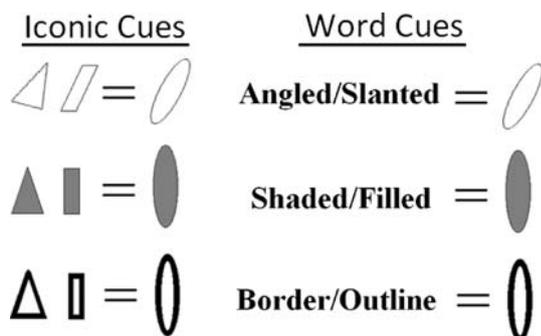

Fig. 1 Cues and stimuli for Experiment 1 (images not to scale)

Experiment 1

Method

Participants

A total of 33 participants were recruited from the participant panel run by the School of Psychology at Bangor University in exchange for partial course credit. One participant was removed due to session-wise accuracy below 90%.

Apparatus and stimuli

All experiments were presented on a PC running E-Prime (Psychology Software Tools, 2002) with a 17″ (43.18 cm) CRT monitor. Viewing distance was approximately 60 cm. Responses were registered by a standard QWERTY keyboard. Stimulus display consisted of four ovals (6 cm in height; three had widths of 2.3 cm and one had width of 3.5 cm), 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 (see Fig. 1). 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 4 cm and a width of 1.4 cm; triangular cues were displayed with a height of 4 cm and a width of 2 cm. 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 Fig. 1 for cue-target pairings). Cue-target pairings remained constant throughout the experiment.

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 100 or 900 ms, 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 1,000 ms, 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 RCI's of 1,000 ms or more allow for any carryover effects from the previous trial to dissipate (Meiran, Chorev, & Sapir, 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 .5.[1] 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*).

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[1] This 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 1 and Experiment 2 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

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

Results

The first trial from each sub-block of 110 trials from both conditions was removed from data analysis. Trials in which an error was committed and the trial immediately following an error were also removed, as were RTs faster than 200 ms or slower than 2,500 ms. Data trimming left 93.7% of the raw data to be submitted to further analysis. RT and errors (%) were submitted to separate three-way repeated measures analysis of variances (ANOVAs), with the factors as described above in "Design". Mean RT, standard deviations, and errors are shown in Table 1.

RT analysis

Reaction time 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.

The CSI-by-sequence interaction reached significance, $F(2,62) = 45.61$, $p < .001$, $\eta_p^2 = .6^2$ 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 (59 ms) than in the iconic cue condition (−1 ms), $F(1,31) = 49.45$, $p < .001$, $\eta_p^2 = .62$. The magnitude of the attention-switch cost did not differ between cue-types, with the attention-switch cost being 32 ms for the word cues and 41 ms 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 1).

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 1 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

Table 1 Mean reaction time (ms) for cue-repetition, cue-switch, and attention-switch sequences in Experiment 1 across both cue-types

Sequence	Word cues		Iconic cues	
	Short CSI	Long CSI	Short CSI	Long CSI
Cue-repetition	692	537	587	520
	(113)	(90)	(88)	(83)
	2.22	*2.26*	*1.99*	*2.54*
Cue-switch	779	569	587	519
	(145)	(100)	(89)	(79)
	3.24	*3.12*	*1.93*	*2.49*
Attention-switch	838	573	653	534
	(151)	(117)	(115)	(85)
	5.74	*4.05*	*4.33*	*2.81*
Cue-switch cost	97	32	0	−1
Attention-switch cost	59	4	66	15

Standard deviations in parenthesis, errors (%) in italics

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[2] Mauchley's test indicated that the CSI × sequence interaction violated the assumption of sphericity, $\chi^2(2) = 7.55$, $p < .05$. Estimates of sphericity using Greenhouse–Geisser correction ($\varepsilon = .71$) did not alter the significance of the interaction, so this interaction (and others that do not alter the significance) is (are) reported with uncorrected *df*s.

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 that cue- and attention-switching are dissociable, and thus arise from different underlying processes (cf. 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 Experiment 2.

Experiment 2

To rule out cue-collapsing as a complete explanation of our findings for Experiment 2 we only utilized the iconic cues from Experiment 1 in two conditions. In the first condition, the *Matching Cues* condition, the iconic cues were paired with the same targets as in Experiment 1. 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 Fig. 2 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 1 were due to cue-collapsing, we should find equally reduced cue-switch costs in both the matching- *and* unrelated cue

Fig. 2 Cues and stimuli for Experiment 2 (images not to scale)

condition. If, however, the reduced cue-switch costs for iconic cues in Experiment 1 were due to bypassing or reducing the need for endogenous control processes, the non-transparent nature of the unrelated cues in Experiment 2 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

A total of 34 participants were recruited from the same pool as the previous experiments. None had participated in any of the previous experiments. Two were removed for session-wise accuracy below 90%.

Apparatus and stimuli

The experiment was presented on the same apparatus as Experiment 1. There was a change to the targets used for part of this experiment, but we utilized only the iconic cues from Experiment 1 in two conditions. In the first condition (*matching*), the targets were identical to those of Experiment 1, 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 Fig. 2 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 1 as a distractor.

Procedure

The procedure of the experiment was identical to that of Experiment 1.

Design

The independent variables were identical to Experiment 1, except the factor *cue-type* now had the levels matching versus unrelated.

Results

Data trimming utilised the same criteria as Experiment 1, and left 93% of the raw data to be submitted to further
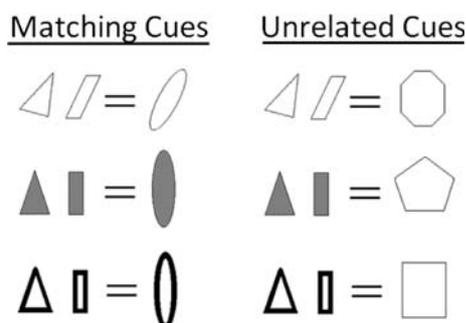
analysis, consisting of separate three-way repeated measures ANOVAs with the factors as described above in "Design". Mean RT, SD, and error are in Table 2.

RT analysis

Reaction time 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, $F(2,62) = 27.89$, $p < .001$, $\eta_p^2 = .47$ 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 (44 ms) than in matching cue condition (8 ms), $F(1,31) = 14.08$, $p < .01$, $\eta_p^2 = .31$. The magnitude of the attention-switch also differed between cue-types, being greater for the unrelated cues (92 ms) than for the matching cues (35 ms),

Table 2 Mean reaction time (ms) for cue-repetition, cue-switch, and attention-switch sequences in Experiment 2 across both cue-types

Sequence	Unrelated cues		Matching cues	
	Short CSI	Long CSI	Short CSI	Long CSI
Cue-repetition	882	751	559	497
	(115)	(120)	(75)	(63)
	3.24	*2.58*	*2.10*	*1.63*
Cue-switch	955	768	571	502
	(134)	(126)	(86)	(69)
	3.66	*2.80*	*1.11*	*1.77*
Attention-switch	1,083	823	625	516
	(157)	(154)	(124)	(78)
	4.92	*3.20*	*2.78*	*2.86*
Cue-switch cost	73	17	12	4
Attention-switch cost	128	55	54	14

Standard deviations in parenthesis, errors (%) in italics

$F(1,31) = 23.45$, $p < .001$, $\eta_p^2 = .43$. This interaction was followed up by collapsing across CSI and submitting the data to two separate one-way ANOVAs (one for each cue-type) with the factor *Sequence* (as in "Design"). The ANOVAs showed that the 8 ms cue-switch cost for matching cues was actually significant, $F(1,31) = 6.39$, $p < .05$, $\eta_p^2 = .17$, as was the 44 ms cue-switch cost for unrelated cues, $F(1,31) = 24.46$, $p < .001$, $\eta_p^2 = .44$.

To test whether this 8 ms cue-switch cost for the matching cues differed from the 0 ms cost for iconic cues in Experiment 1, we analysed the cue-switch costs in a $2 \times 2 \times 2$ mixed ANOVA, with CSI (100 vs. 900 ms) 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.

The attention-switch cost was significant for both matching cues [35 ms; $F(1,31) = 19.00$, $p < .001$, $\eta_p^2 = .38$] and for the unrelated cues [92 ms; $F(1,31) = 59.82$, $p < .001$, $\eta_p^2 = .66$].

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, $F(1,31) = 2.74$, $p > .1$.

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 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 2 have removed any doubt as to the alternative explanation of the lack of cue-switch cost in Experiment 1 due to cue-collapsing as participants were now able to collapse across cues equally. Additionally, in Experiment 2 perceptual differences during a cue-switch transition were identical between the matching and unrelated conditions. Despite this, we again found that cue-switch costs were reduced when the cues were maximally transparent, thus supporting our findings from Experiment 1.

The finding of a significant 8 ms 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 two 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 2, 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, 1994). 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; 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 1, 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[3]: 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 (see, e.g. 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, (1) an arbitrary letter code ("A" = colour task), (2) the word "Colour", (3) the words BLUE and RED arranged vertically (specifying the response set), (4) BLUE–RED horizontally aligned (left, right) with their associated manual responses (providing the stimulus–response, SR, mapping), and (5) 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 S–R 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

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[3] We thank Iring Koch for pointing out these issues.

hence the iconic cues are more transparent according to the above definition. However, if the task situation 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 2 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.

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