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Heightened Conflict in Cue-Target Translation Increases Backward Inhibition in Set Switching

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Backward inhibition (BI) is a performance cost that occurs when an individual returns to a task after 1 (vs. more than 1) intervening trial, and it may reflect the inhibition of task-set components during switching. In 3 experiments, we support the theory that inhibition can target cue-based preparatory stages of a task. Participants performed a cued target-localization task that had been previously shown to produce BI. In Experiment 1, reassignment of arbitrary cue-target pairings midway through the experiment doubled the size of BI, though cue, target, and response sets remained unchanged. In Experiment 2, we controlled for effects of order of conditions or simple change of cue meaning. In Experiment 3, we 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.

Keywords: Task switching, backward inhibition, working memory, cue processing

The growing research on task switching reflects investigators' desire 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-utilized task set. Evidence for such inhibition mainly comes from the backward inhibition (BI) paradigm (Mayr & Keele, 2000; see Koch, Gade, Schuch, & Philipp, in press; Mayr, 2007, for reviews). In this paradigm, participants switch among three tasks, signaled 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 (AB*A* sequence) compared with when they return to a less recently performed task (CB*A* sequence). This BI cost[1] is thought to reflect persisting inhibition of a task when an individual disengages from it to perform another; in an AB*A* sequence, the inhibition of Task A persists over two trials and hinders its reactivation relative to a CB*A* sequence in which an individual has had more time to recover.

The question arises as to precisely what is inhibited when an individual switches tasks. 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 the stimulus is displayed to which the task set is applied to reach a decision; and finally the decision is translated into a response, typically via an arbitrary (and recently learned) stimulus–response set. In principle, BI 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 intertrial 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 BI when a response was withheld on Trial $n - 1$, suggesting that the response set from Trial $n - 2$ did not require inhibition (Koch, Gade, & Philipp, 2004; Philipp, Jolicoeur, Falkenstein, & Koch, 2007).

Recently Houghton, Pritchard, and Grange (2009) proposed that BI reflects a flexible control process active at the point of greatest intertrial 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) proposed 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. (2009) 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 nontargets. Each of the

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[1] We use the term *BI* for conciseness. The more theoretically neutral term *n − 2 repetition cost* is sometimes preferred (Koch et al., in press), but we use BI throughout this article.

shapes (ovals) had a unique visual property, for instance, a thickened outline or being filled in. The target changed on each trial and was signaled by a cue (see Experiment 1 of the current article 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 intertrial conflict would be centered on cue processing, as participants would have to translate the cue into a WM representation capable of enabling target detection (cue-target translation).

It was further hypothesized 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 manipulation of the degree of relatedness between the cue and target, referred to as the *cue transparency* (Logan & Schneider, 2006). 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 press). 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 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 to 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 shown no influence of cue transparency on BI (e.g., Arbuthnott, 2005), but in those previous studies, the tasks produced response conflict (see Houghton et al., 2009, for discussion of this issue).

Houghton et al. (2009) found the predicted effects of cue transparency on BI: Maximally transparent cues produced no BI, while the arbitrary cues produced the greatest cost. Verbal cues with an existing LTM association to the target property produced intermediate levels of BI (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 intertrial conflict.

The Present Study

In the present study, we sought to further test the hypothesis that cue-based conflict can generate BI. One potential problem with the studies by Houghton et al. (2009) is that they contrasted different sets of cues to the same set of targets, for example, iconic cues versus 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, leaving open the possibility that the different cue sets might contribute independently to the effect. For instance, if cue discrimination was harder for one set of cues than for another, this might lead to greater intertrial 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 the cue and target sets are held constant. This was our aim in the work reported here, in which a negative transfer paradigm (Gade & Koch, 2007a) was used with the nontransparent 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 the participant should locate a shaded oval). Halfway through the experiment, new cue-target pairings are introduced, but it is important to note that the same cues and targets are used (only the learned pairings are changed; for example, a square cue indicates that the participant should 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 compete with the previously 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 BI. If this is so, the effect can only be attributed to the cue-target relationship, as nothing else has changed.

Experiment 1

Method

Participants. We recruited 32 participants through the Bangor University participant panel. Two additional participants were removed due to sessionwise accuracy under 90%.

Apparatus and stimuli. Stimuli were presented on a 17-in. (43.18-cm) monitor, connected to a PC running E-Prime software (Psychology Software Tools, Pittsburgh, PA). The stimulus display consisted of four ovals (6 cm in height; three had widths of 2.3 cm, and one had a width of 3.5 cm), each differing on a visual property (Figure 1). The cue was a square, a triangle, or an octagon (height and width 4 cm). Cues and stimuli were presented in grayscale shading on a white background.

Procedure. The experiment was split into two halves, each consisting of 10 blocks of 42 trials, with rest screens after each block. A trial began with a central cue visible for 500 ms, followed by a 250-ms blank screen. The stimuli then appeared with one oval centered within each quadrant of the screen. There were three possible targets and one neutral distractor on each stimulus dis-
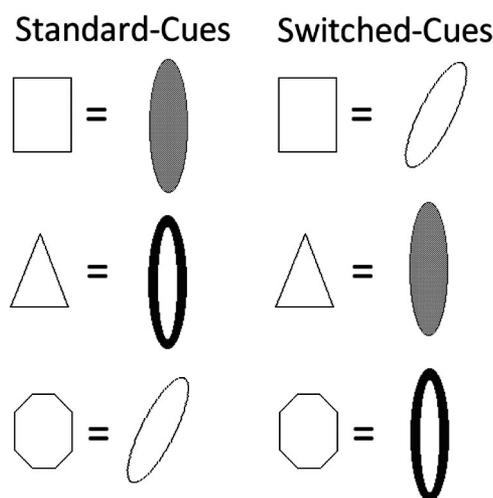

Figure 1. Cue-target pairings in Experiment 1 (images not to scale).

play, and the location of all stimuli was randomized. 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 500 ms, before the cue for the next trial appeared.

Two disjointed sets of cue-target pairings were used, as follows (cue = target): For Set 1, a square = shaded oval, a triangle = border oval, and an octagon = angled oval. For Set 2, a square = angled oval, a triangle = shaded oval, and an octagon = border oval. The order of the pairings was counterbalanced across participants (i.e., half received Set 1 followed by Set 2, and half vice versa). Across participants, we refer to the cue-target pairing in the first half of the experiment as the standard cues and to the pairing in the second half as the switched cues.

Before each half of the experiment, participants were shown the cue-target pairing followed by a brief practice session. The importance of accuracy was emphasized, and practice was terminated once the participant had demonstrated that he or she understood the instructions.

Design. The experiment had a 2 × 2 repeated measures design with factors Cue Pairing (standard cues vs. switched cues) and Sequence (AB*A* vs. CB*A*). 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 were reaction time (RT; in milliseconds) and percentage error.

Results

Initial analysis. The first two trials of each block were removed. For the RT analysis, errors and the two trials following an error were excluded, along with trials with RTs 2.5 standard deviations above each participant mean, leaving 86% of trials. Trimmed RTs and percentage of errors were submitted to separate two-way repeated measures analysis of variance (ANOVA), with the factors and levels as described in Design. Mean RTs, standard errors, and percentage of errors are shown in Table 1.

The factor Sequence produced a significant main effect, $F(1, 31) = 50.12$, $p < .001$, $\eta_p^2 = .62$, due to slower RTs to AB*A* trials (754 ms) than to CB*A* trials (717 ms). For Cue Pairing, there was a nonsignificant trend, $F(1, 31) < 1$, toward slower RTs to switched cues (748 ms) than to standard cues (724 ms).

It is important to note that the Cue Pairing × Sequence interaction was significant, $F(1, 31) = 6.39$, $p < .05$, $\eta_p^2 = .17$, reflecting greater BI for the switched cues (55 ms) versus the standard cues (20 ms). BI 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, only the main effect of Cue Pairing was significant, with more errors in the switched-cue condition (4.0%) than in the standard-cues condition (3.3%), $F(1, 31) = 5.45$, $p < .05$, $\eta_p^2 = .15$.

Is conflict cue-specific? To test whether the increased BI in the second half of the experiment was due to cue-specific conflict or more general conflict, we carried out the following analysis.[2] In

the first half of the experiment, if target "A" is assigned to cue "1," target "B" to cue "2," and target "C" to cue "3," and these pairings are reassigned in the second half as "A2," "B3," and "C1," there are now two types of AB*A* sequences possible in the second half of the experiment: Type I (A2–B3–A2) and Type II (A2–C1–A2).

Type II AB*A* 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 BI due to extra inhibition required to overcome this specific cue-related conflict. Alternatively, Type I AB*A* sequences have an intermediate target (B) with a cue (3) that has a learned association with the target "C" and thus should not lead to extra BI due to no cue-specific conflict.

This analysis is also possible with CB*A* sequences. For example, Type II CB*A* sequences (e.g., C1–B3–A2) may lead to slower RTs at Trial A2 than Type I CB*A* sequences (e.g. B3–C1–A2), as the cue used in the intermediate trial for Type II sequences was previously associated with the target "A."

To analyze this possibility, we recoded the RT data from the second half of the experiment into Type I and Type II AB*A* and CB*A* sequences and reanalyzed them in a 2 (Sequence: AB*A* vs. CB*A*) × 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 to a more general increase in intertrial conflict.

Discussion

The results confirmed our prediction that negative transfer of cue-target pairings would increase BI. 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 (cf. Schuch & Koch, 2003).

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

In the prior discussion, the reassignment of the cue-target pairings (negative transfer) halfway through the experiment is supposed crucial to the explanation of the findings. However, the switched pairings inevitably occurred in the second half of the experiment. It was possible, therefore, that BI may be increased by

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[2] We thank Darryl Schneider for suggesting this analysis.

Table 1
Mean Reaction Times, Standard Errors, and Percentage of Errors for ABA and CBA Sequences Across Cue Pairings for Experiment 1

| | Sequence | | | | | | |
| | ABA | | | CBA | | | Backward inhibition |
Cue pairing	M (ms)	SE (ms)	Errors (%)	M (ms)	SE (ms)	Errors (%)	(ABA–CBA) (ms)
Standard cues	734	40	3.31	714	41	3.24	20
Switched cues	775	48	3.81	720	42	4.21	55

Note. Ms = milliseconds.

any similar change in the experimental procedure halfway through. We addressed this possibility in Experiment 2.

Experiment 2

In Experiment 2, we tested whether BI is increased following any change in the meaning of cues but not involving reassignment 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 were paired with related targets (e.g., a square cue indicates a search for a square target). We hypothesized 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 BI.

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 having the unrelated pairs occur in the second half of an experiment of this type (following a change of cue meaning) can potentiate BI, then BI in the unrelated condition should be greater in the group that 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 BI (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 predicted in this case that BI would be unaffected by order, resulting in no BI for the related cues and a BI effect for the unrelated cues of similar magnitude to that found in Experiment 1 for the standard-pairing condition.

Method

Participants. We recruited 32 new participants from the same pool as in Experiment 1. Sixteen were randomly assigned to each between-subjects group on the basis of the order in which they received the cue-target pairings (related–unrelated group or unrelated–related group).

Stimuli and apparatus. For the unrelated cue-target pairings, we used Set 2 from Experiment 1. For the related pairings, the target display consisted of a square, a triangle, and an octagon as potential targets, with the same neutral oval distractor from Experiment 1 (Figure 2).

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, the participants were to 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 was presented with the reverse order. Block length, trial structure, and practice were identical to those of 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, while Order (related–unrelated vs. unrelated–related) was manipulated between groups.

Results

Data trimming was identical to that performed in Experiment 1; we retained 87% of trials for further analysis. The trimmed data were analyzed with a mixed factorial ANOVA, with the factors as described in Design. Mean RTs, standard errors, and percentage of errors are shown in Table 2.
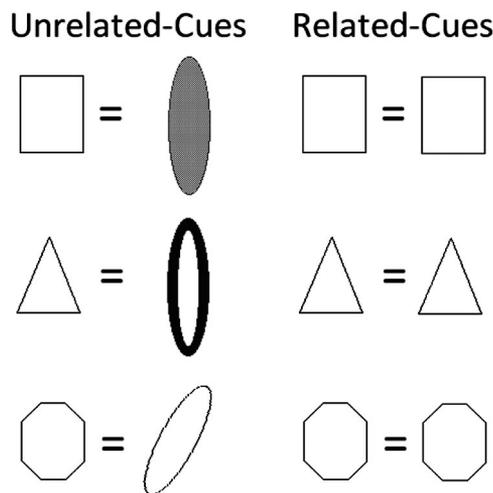
Figure 2. Cue-target pairings in Experiment 2 (images not to scale).

Table 2
Mean Reaction Times, Standard Errors, and Percentage of Errors for ABA and CBA Sequences Across Cue Pairings for the Unrelated–Related and Related–Unrelated Groups From Experiment 2

	Sequence						
	ABA			CBA			Backward inhibition (ABA–CBA) (ms)
Group/cue pairing	*M* (ms)	*SE* (ms)	Errors (%)	*M* (ms)	*SE* (ms)	Errors (%)	
Unrelated–related							
Related cues	541	24	1.45	538	24	1.73	3
Unrelated cues	736	44	3.18	709	43	2.42	27
Related–unrelated							
Related cues	596	24	3.10	593	24	2.99	3
Unrelated cues	720	44	4.64	694	43	3.95	26

Note. Ms = milliseconds.

For the RT analysis, we found no significant effect of Order, $F(1, 30) < 1$, and, most important, no interaction involving this factor was significant. For the other main effects: Cue Pairing showed faster RTs to related cues (567 ms) than to unrelated cues (715 ms), $F(1, 30) = 36.17$, $p < .001$, $\eta_p^2 = .55$, and Sequence produced a reliable main effect due to slower responses to ABA trials (648 ms) than to CBA trials (634 ms), $F(1, 30) = 9.00$, $p < .01$, $\eta_p^2 = .23$. 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 BI for unrelated cues (27 ms), $t(31) = 3.34$, $p < .01$, than for related cues (3 ms), $t(31) = 0.76$, $p > .4$. As noted, these effects did not depend on the order in which the conditions were received. BI (27 ms) was similar in magnitude to that found in Experiment 1 for the standard-cues condition (20 ms) but only half that found for the switched-cues condition (55 ms).

For the error analysis, the main effect of Order was significant, with more errors in the related–unrelated group (3.7%) than in the unrelated–related group (2.2%), $F(1, 30) = 12.72$, $p < .01$, $\eta_p^2 = .30$. No other main effect or interaction was significant.

Discussion

The results show that a simple change in the meaning of a set of cues cannot account for the results from Experiment 1. BI was completely unaffected by the order in which a given cue-target pairing was received. In the related condition, BI was never evident (confirming previous results), while in the unrelated condition, BI did not increase when the condition was received following a change in cue meaning. Moreover, the size of the unrelated BI effect was the same as in the standard (i.e., preswitch) condition in Experiment 1.

Together, Experiments 1 and 2 define a pair of boundary conditions: In Experiment 1, we found a doubling of the size of BI 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 the supposition that there is only a carryover effect from one translated cue-target mapping to another. On this account, no modulation of BI was found in Experiment 2 because there was only one translated (nontransparent) mapping used.

Our account, however, is that the increase in BI 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 would 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, in which we pair the same cues with two nonoverlapping sets of targets. For both sets of targets, the cue-target relationship is nontransparent and should produce reliable BI. 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. We recruited 32 new participants from the same pool used in the previous experiments.

Stimuli and apparatus. 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 (Figure 3). The new targets were designed to be visually distinct from the "standard" set, while at the same time offering a similar target localization task (visual feature search among 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 had a unique visual property: one had a dotted outline, and another contained horizontal lines, while the last was aligned horizontally. The distractor rectangle was plain and aligned vertically.
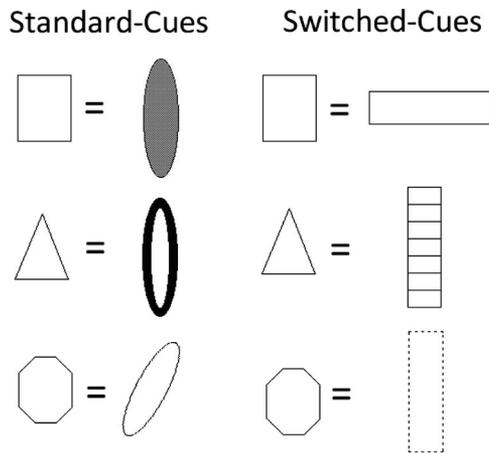
Standard-Cues   Switched-Cues

Figure 3. Cue-target pairings in Experiment 3 (images not to scale).

Procedure. In one half of the experiment, we utilized the cue-target pairings from Experiment 1, and in the other half, we used the same cues with the new target set described earlier. 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 those in Experiment 1.

Design. For this experiment, we manipulated two factors in a repeated-measures design: Cue Pairing (standard cues vs. switched cues) and Sequence (AB*A* vs. CB*A*). The crucial measure was the interaction between the two factors.

Results

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

There were no significant effects in the error analysis, so we report only the RT results. There was no main effect of Cue Pairing, although RTs for switched cues were slower (683 ms) than RTs for standard cues (660 ms), $F(1, 31) = 1.69$, $p > .2$, $\eta_p^2 = .05$. The main effect of Sequence was significant, with slower RTs to AB*A* (680 ms) than to CB*A* sequences (662 ms), $F(1, 31) = 20.49$, $p < .001$, $\eta_p^2 = .4$. Critically, the interaction was not significant, $F(1, 31) = 0.21$, $p > .6$. BI costs were 20 ms for standard cues, $t(31) = 4.8$, $p < .001$, and 17 ms for switched cues, $t(31) = 2.89$, $p < .01$.[3]

Experiments 1 and 3 between-Experiment analysis. As Experiment 3 has exactly the same structure as Experiment 1, we were able to compare the results. The main benefit of doing so is that it affords a direct comparison of BI in the second half of the two experiments.

The RT data from Experiments 1 and 3 were analyzed in a between-subjects ANOVA, with Cue Pairing (standard cues vs. switched cues) and Sequence (AB*A* vs. CB*A*) as the within-subject factors and Experiment as a between-subject factor. There was no

main effect of Experiment, $F(1, 62) = 1.62$, $p > .2$. However, the three-way interaction was significant, $F(1, 62) = 6.25$, $p < .05$, $\eta_p^2 = .09$. We therefore performed two separate two-way ANOVAs (one for each cue type), comparing BI across experiments, both with factors Sequence (AB*A* vs. CB*A*) and Experiment (1 vs. 3). For the standard cues (first half of experiment), neither the main effect of Experiment nor the interaction with Sequence was significant (all $Fs < 1$). For the switched cues (second half), there was again no main effect of Experiment but the interaction with Sequence was significant, $F(1, 62) = 10.2$, $p < .01$, $\eta_p^2 = .14$. We thus confirmed that the BI in the switched cues in Experiment 1 was greater than that for the switched cues in Experiment 3.

Discussion

The results of Experiment 3 were unambiguous: BI was found in both halves of the experiment (unlike Experiment 2) and for both cue-target sets, but it 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 BI 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 (nontransparent) cues were paired with a new target set (Experiment 3).

General Discussion

The present set of studies provides further evidence that processes involved in cue-based stages of the trial structure can generate BI, 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 BI 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 BI in the cued target-search paradigm used by Houghton et al., (2009), as these authors have argued that BI in this design reflects the level of inhibitory control required to resolve conflict during cue-to-target translation.

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

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[3] Although not a factor in the design, we analyzed the data with respect to the two cue-target sets shown in Figure 3 to confirm that the new targets introduced in this experiment behaved like the "old" set. For this purpose, we conducted a two-way repeated-measures ANOVA with factors Target Set (ovals vs. rectangles) and Sequence (AB*A* vs. CB*A*). Only the main effect of Sequence reached significance, $F(1, 31) = 20.49$, $p < .001$, $\eta_p^2 = .4$, and both target sets produced significant levels of BI: ovals (22 ms), $t(31) = 3.64$, $p < .001$, and rectangles (15 ms), $t(31) = 3.3$, $p < .01$.

Table 3

Mean Reaction Times, Standard Errors, and Percentage of Errors for ABA and CBA Sequences Across Cue Pairings for Experiment 3

| | Sequence | | | | | | |
| | ABA | | | CBA | | | Backward inhibition (ABA–CBA) (ms) |
Cue pairing	*M* (ms)	*SE* (ms)	Errors (%)	*M* (ms)	*SE* (ms)	Errors (%)	
Standard cues	670	35	2.92	650	34	2.55	20
Switched cues	691	32	2.92	674	30	2.96	17

Note. Ms = milliseconds.

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 BI and by Gade and Koch (2007a) regarding the role of inhibitory control in generating switch costs. With respect to the Houghton et al. studies (see also Grange & Houghton, 2009), Experiment 1 also has the important property that different levels of BI were found while the cue and target sets were kept constant. This is the first time this result has been shown, because in the previous reports, BI was compared across conditions in which only the targets were unchanged. It was therefore possible that the reported modulation of BI 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 utilized. This increased intertrial conflict results in the need for greater inhibition of the previous cue-target relation when switching takes place, producing a larger BI effect.

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