Examining the Role of Retrieval Processes in Set-Alternation Costs

by

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Abstract

The goal of the experiments was to evaluate an explanation of set-alternation costs based on episodic memory principles. The assumption is that performance of any task is a consequence of memory retrieval processes that involve representations of specific prior experiences (Kolers, 1976; Leboe, Whittlesea, & Milliken, 2005; Neill & Mathis, 1998; Tenpenny, 1995; Whittlesea, 1997; Whittlesea & Jacoby, 1990). When the Event 1 and 3 targets mismatch the retrieval of the Event 1 memory episode is not entirely appropriate for performing the Event 3 task. The interference due to a partial match between Events 1 and 3 might be the source of set-alternation costs. Results of Experiment 1 revealed larger costs in the high probability set-alternation condition. The high probability set-alternation condition encouraged retrieval of Event 1. However, because the targets of Event 1 and 3 mismatched the retrieval of Event 1 interfered with the processing of Event 3's task-set. In other words, the interference due to a match in task-sets but a mismatch in targets generated costs. If set-alternations costs originate from a partial match between Events 1 and 3, increasing the amount of overlapping information between these events should reduce costs. The findings of Experiments 2 and 3 showed reduced set-alternation costs when there was a target identity match between Events 1 and 3. Lastly, Experiment 4 showed that set-alternation costs are larger when the retrieval of the Event 1 memory episode is obstructed. That is, costs were larger when there was a combination of obstructed Event 1 retrieval and a partial match between Events 1 and 3.

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Chapter 1: Summary and Rationale for the Experiments

Task switching studies reveal that switching from one task to another can result in significantly slower responding and a greater frequency of incorrect responses than repeating the same task (Allport, Styles, & Hsieh, 1994; Jersild, 1927; Meiran, 1996; Monsell, 2003; Monsell, Yeung, & Azuma, 2000; Rogers & Monsell, 1995; Spector & Biederman, 1976). Such impairments during a change in task are referred to as *task switching costs*.

As an early example for how task switching costs have been investigated in the laboratory, Jersild (1927) required that participants either add 3 to a list of 2-digit numbers or subtract 3 from a list of 2-digit numbers. Those placed in the *task repetition* condition only performed one of the two tasks in response to the list of numbers. Those placed in the *alternate* condition, however, alternated between adding 3 and subtracting 3 in response to each number in the list. Jersild's results indicated that switching between the adding and subtracting tasks led to significantly slower response times and more errors than when participants simply repeated the same task throughout the experiment. The size of task switching costs in this type of experiment is typically computed by subtracting participants' mean response times on task switching trials from their mean response times on task repetition trials.

More recently, researchers have been concerned with identifying the performance costs associated with an alternating task sequence in which there is a switch back to a recently abandoned task-set (Arbuthnott, 2005; Arbuthnott & Frank, 2000; Koch, Gade, & Philipp, 2004; Mayr & Keele, 2000; Schuch & Koch, 2000). Studies show that during set-alternation sequences with the general form Task A – Task B – Task A, switching from Task A to another task and then returning to Task A again results in a *set-alternation cost*, defined as slower response times on set-alternation trials compared to no-alternation sequences. No-alternation sequences have the general form Task C – Task B – Task A, providing a baseline for measuring performance of Task A when the participant did not recently switch from that task (Mayr & Keele, 2000).

Often, set-alternation costs are explained in terms of inhibitory control processes (Arbuthnott & Frank, 2000; Hubner, Dreisbach, Haider, & Kluwe, 2003; Koch, Gade, & Philipp, 2004; Mayr & Keele, 2000; Mayr & Kleigl, 2003; Schuch & Koch, 2003). By this view, it is the activation level of mental representations that control the initiation of specific task sets that produces set-alternation costs. In a set-alternation sequence (Task A – Task B – Task A), performance of the first Task A depends on heightened activation of the task set representation corresponding to Task A. This task set representation then remains in a heightened state of activation during one's efforts to perform Task B. Successful performance of Task B is thought to depend, in part, on inhibiting the previously activated task set representation for Task A. Participants must then overcome that inhibition of Task A to perform the third task in the sequence. Thus, it is the carryover of previous inhibition of Task A that impairs responses on set-alternation trials, as compared to no-alternation trials for which no recent inhibition would have been applied to Task A.

As an alternative to this inhibitory account, the current study aims to evaluate an approach to set-alternation costs based on episodic memory principles. An episodic orientation toward set-alternation costs would not make reference to the activation or inhibition of underlying task set representations. Instead, the assumption of episodic approaches to cognitive phenomena is that performance of any task is a consequence of memory retrieval processes that involve representations of specific prior experiences (or episodes), rather than abstract task-set representations that are not tied to any specific prior experience (Kolers, 1976; Leboe, Whittlesea, & Milliken, 2005; MacLeod, Dodd, Sheard, Wilson, & Bibi, 2003; Neill & Mathis, 1998; Tenpenny, 1995; Whittlesea, 1997; Whittlesea & Jacoby, 1990). The root of this approach is that memory's primary function is to preserve every experience in a person's life. In turn, these representations have the potential to influence the performance of future tasks (e.g., Kolers & Ostry, 1974).

The influence of any one memory episode on current performance depends on its likelihood of being retrieved from memory during performance of the present task. The likelihood of retrieving an episode depends on (at least) three factors: 1) the extensiveness of the processing that took place during that episode (Craik & Lockhart, 1972; Hunt, 1995; Hunt & Smith, 1996; Underwood & Freund, 1968), 2) the extent to which the processes engaged in during that episode overlap with those associated with performance of the current task (Eysenck, 1978; Morris, Bransford, & Franks, 1977; Tenpenny, 1995; Tulving & Osler, 1968; Tulving & Thompson, 1973), and 3) the recency of prior processing (MacLeod et al., 2003; Roediger & Crowder, 1976).

Applying an episodic approach, there may be a way to explain set-alternation costs without requiring the assumption that such effects arise from the inhibition of abstract task-set representations. To illustrate, suppose that responding to Task A carries over to interfere somewhat with performance of Task B. In part, successful completion of Task B requires not generating a response based on the stimulus operation used to perform

Task A. Treating processes associated with the recent completion of Task A as a source of interference ought then to form part of the memory episode for successful completion of Task B. In turn, when participants must again complete Task A, set-alternation costs would depend on the extent that conditions support retrieval of the recently created episodic representation for completing Task B. A recent experience that required treating performance of Task A as a source of distraction might impair subsequent performance of that task. This example provides one possible way in which episodic memory principles might account for set alternation costs and it is possible that there are others.

The goal of the present study was more general, although the outcome of my experiments does permit ruling out some variants of episodic explanation for set alternation costs, including the one described above. The broader objective was to test whether the size of set-alternation costs originates from a partial match between Events 1 and Event 3. In the next section, I provide a brief summary of the design and purpose of my four experiments and a description of the key outcomes.

Experiment 1

This was aimed at determining whether set-alternation costs depend on memory retrieval processes in a very general sense. Specifically, Experiment 1 was designed to test whether the size of set-alternation costs depends on the presence of a cue that is predictive of a requirement to return to the task performed during Event 1 (as in the ABA sequence) or that is predictive of a requirement to perform a task not performed during the preceding two events (as in the CBA sequence). Target stimuli consisted of well-known animal names (e.g., TIGER). Participants completed one of three conceptual tasks in response to every word presentation. They categorized animal names according to typical habitat (Land: Yes vs. No), relative size (Big: Yes vs. No), or whether they are a mammal (Yes vs. No). I also manipulated the probability that participants were required to complete a set-alternation sequence vs. a no-alternation sequence as a function of the location of the Event 3 target stimulus. Specifically, that target appeared either above the vertical mid-point of the computer screen or below it.

For one group of participants, set alternation sequences occurred on 75% of trials in which the Event 3 target appeared at the higher location on the screen, whereas no-alternation sequences occurred on only 25% of trials for which the target appeared at that upper location. I reversed this contingency for another group of participants: Event 3 targets appearing at the upper location completed a no-alternation sequence on 75% of trials and a set alternation sequence on 25% of trials, whereas I reversed this contingency for Event 3 targets appearing at the lower location. The goal of this experiment was simply to test whether the collection of episodic representations created during the experimental session would influence the magnitude of set alternation costs. I expected that the location of the Event 3 target might cue participants to be more or less prepared to complete either a set alternation sequence or a no-alternation sequence, depending on the cross-trial probability of completing those types of sequences *as a function of the Event 3 target's location*.

To illustrate, suppose that the current Event 3 target appeared at the upper location of the screen. My expectation was that this feature of the display would cue prior episodes for which the Event 3 target appeared at this same location. If most of the instances in which the target appeared in the upper location of the screen completed a set-alternation sequence, participants might be encouraged to engage in processes associated with completion of that type of sequence. That is, performance during the high probability set-alternation condition might involve greater retrieval of the Event 1 memory episode than during the high probability no-alternation condition. However, if the Events 1 and 3 targets differ, retrieval is not entirely appropriate for responding to the Event 3 task. Instead, the retrieval might produce interference that generates set-alternation costs.

Showing an influence of Event 3 target's location on set-alternation costs would implicate a role for episodic retrieval processes in the observation of set-alternation costs. Features of Event 3 that modulate the observation of set-alternation costs can be explained with reference to participants' accessing and relying upon memory episodes generated on previous trials. This experiment represents an initial test as to whether the size of set-alternation costs might depend on representations of prior episodes, rather than solely reflecting the degree of inhibition that participants must apply to the Event 1 task on set alternation trials. This experiment supports my expectation that a contingency between Event 3 target location and the requirement to complete a set alternation sequence contributes to the size of set alternation costs.

Experiment 2

Based on the specific explanation of set-alternation costs within an episodic memory framework described above, I suggest that the source of conflict that causes set-alternation costs originates from retrieval of a memory episode. One of the most potent sources of enhanced memory retrieval is high overlap between the features of an episode stored in memory and the features of a current event (Eysenck, 1978; Morris, Bransford, & Franks, 1977; Tenpenny, 1995; Tulving & Osler, 1968; Tulving & Thompson, 1973). Experiments 2 and 3 examined whether the degree of overlap within the three-task sequences used to measure set alternation costs will influence the magnitude of those costs.

Similar to Experiment 1, in Experiments 2A and 2B, participants completed one of three conceptual tasks requiring them to categorize animal names according to typical habitat (Land: Yes vs. No), relative size (Big: Yes vs. No), or a third task required that they categorize animal names according to a mammal judgment (Mammal: Yes vs. No). For both set-alternation and no-alternation sequences, in Experiment 2A, I manipulated overlap in target stimuli such that, for half of the trials, the target displayed in Event 3 matched the target identity displayed in Event 2. On the other half of trials, the targets displayed in the three event sequence were all different. The procedure of Experiment 2B was the same except that the target displayed in Event 3 matched the target displayed in Event 1 on half of the trials. As in Experiment 2A, the targets displayed on the remaining half of trials were all different.

These two experiments allowed for a test as to whether an overlap between features of Event 3 and either Event 1 or Event 2 in the sequence would influence the size of set-alternation costs. A central presumption of an episodic processing orientation (Eysenck, 1978; Morris et al., 1977; Tenpenny, 1995; Tulving & Osler, 1968; Tulving & Thompson, 1973) is that overlap of a present task with an episodic memory representation will facilitate retrieval of that previous episode. Consequently, if the size of set-alternation costs depends on the target overlap between Event 3 and either of the two preceding events, it would implicate a contribution of retrieval processes to the generation of set-alternation costs. For example, if an overlap in the Event 3 and Event 2 targets yields higher set-alternation costs than when the Event 3 and Event 2 targets differ, it would be reasonable to suggest that the observation of set-alternation costs depends, at least in part, on participants' retrieval of Event 2 processes.

Results indicated that for Experiment 2B, but not 2A, the overlap between features influenced set-alternation costs. In other words, modulation of set-alternation costs occurred when there was overlapping feature information between Events 3 and 1, but not when there was feature overlap between Event 3 and Event 2.

Experiment 3

Experiment 3 was similar to Experiment 2B except that the goal was to test the role of overlapping target category, instead of the role of an overlap in target identity, in determining the magnitude of set-alternation costs. Two categories of target items were used in this experiment (animal words and animal pictures). Otherwise, participants performed the same three tasks that they performed in the preceding experiments. The inclusion of words and pictures in the stimulus set permitted overlapping responses for items that differed in their visual features. The expectation was that retrieval of processes engaged in during Event 1 would be more accessible to the extent that there is higher overlap in the quality of visual features used to construct Event 1 and Event 3 targets. On all trials, the identities of the targets in the three-event sequence always differed. However, on some of the trials, the Event 1 and Event 3 targets were both pictures or were both words, whereas on other trials the Event 1 and Event 3 targets differed in stimulus format. If the retrieval of episodic memory representations can play a role in the generation of set alternation costs, the expectation was that those costs would be lower as a function of the overlap in the form of Event 1 and Event 3 targets. The experiment supported my expectation that the size of set-alternation costs would depend on an overlap of target category between Events 1 and 3. Specifically, the set-alternation costs were significantly lower when the target category of Events 1 and 3 overlapped compared to when they did not overlap.

Experiment 4

The goal of Experiment 4 was to examine another prediction that could be inspired by an episodic approach to set-alternation costs. Experiences that are recent (MacLeod et al., 2003; Roediger & Crowder, 1976) and involve relatively elaborate processing are known to be more accessible from memory in the future (Craik & Lockhart, 1972; Hunt, 1995; Hunt & Smith, 1996; Underwood & Freund, 1968). As a result, supposing that set alternation costs depend on the likelihood of retrieving the episodic representation for Event 2 during participants' efforts to complete the Event 3 task, those costs ought to be largest when the Event 2 task requires relatively elaborate processing. In this experiment, I manipulated the elaborateness of processing the Event 2 task.

Tasks that require an easy surface-level, perceptual judgment involve less extensive processing in comparison to tasks that require more difficult meaning-based categorization judgments. Using the same animal names that I used in the previous experiments, those names appeared either in red or green font. Participants in one group completed trials requiring them to perform the same three conceptual tasks that they performed in my previous experiments. Another group of participants completed trials requiring them to switch from a conceptual task in Event 1 to a perceptual task that merely required them to identify the colour of animal names in Event 2, followed by a conceptual task in Event 3. For both conditions, half of the trials involved set-alternation sequences and the other half involved no-alternation sequences.

Elaborative processing is widely accepted as generating a more distinctive episodic representation in memory, resulting in greater subsequent accessibility of that episode (Craik & Lockhart, 1972; Hunt, 1995; Hunt & Smith, 1996; Underwood & Freund, 1968). Thus, episodes arising from the completion of difficult conceptual tasks (habitat and size) should be more accessible than episodes arising from the completion of relatively easy perceptual tasks (colour font). Thus, with respect to Event 2, completion of a conceptual task should generate a more accessible episodic representation during Event 3 than completion of an easier perceptual task for Event 2. If so, greater set alternation costs should occur on Event 3 when Event 2 requires completion of a conceptual task than when it requires completion of a perceptual task.

The outcome of Experiment 4 supported my prediction that set alternation costs would be larger when the Event 2 task involved elaborative processing rather than when it involved less elaborative processing. In particular, there were significantly larger set alternation costs when Event 2 required completion of a conceptual task rather than a perceptual task.

Experiments 1-4 were conducted to test whether factors that are often presumed to rely on episodic retrieval would contribute to the magnitude of set alternation costs. In the next section, I provide a more detailed summary of the task-switching literature, highlighting existing research aimed at resolving the current theoretical controversy as to whether task-switching costs are caused by an inhibitory control mechanism or by episodic retrieval processes.

Chapter 2: Literature Review

In general, people are slower at performing a task following the completion of some other task than after performing the same task (Allport et al., 1994; Jersild, 1927; Monsell, 2003; Monsell, Yeung, & Azuma, 2000; Rogers & Monsell, 1995; Spector & Biederman, 1976). Studies that have employed the Stroop colour identification/word identification task provide a clear example of such task-switching costs. In the classic studies, Stroop (1935) presented colour-words printed in an incongruent colour ink (e.g., the colour-word RED printed in green ink). Specifically, participants were presented with the words RED, BLUE, GREEN, BROWN, and PURPLE. These colour words were presented to participants in red, blue, green, brown or purple ink. Additionally, participants were presented with squares that were either red, blue, green, brown, or purple in colour. Participants were required to name the colours as quickly and accurately as they could. The outcome was that participants were significantly slower at naming the ink colour of incongruent colour words compared to naming the colour of squares.

Allport et al. (1994) applied this methodology to an investigation of task switching costs. In their study, participants were presented with several lists of incongruent Stroop stimuli (consisting of the words *red*, *green*, *blue*, *yellow*, or *brown* printed in a mismatching colour). On “repeat” trials, participants were instructed either toread the sequence of words aloud or to name the font colour of the words. On “alternate” trials, participants switched between the word-reading and colour-naming tasks for each successive word. Participants were informed before being presented with an alternate list which task they were required to perform first. The key result was that response times on alternate trials were significantly slower than response times on repeat trials. This demonstrates that performance of a task suffers after having performed a different task a moment before.

The degree of impairment in responding after a task change appears to be dependent, in part, on preparation for the upcoming task. When participants are given the opportunity to prepare for responding to an upcoming alternate task, switch costs are reduced (Rogers & Monsell, 1995; Logan & Bundesen, 2003). For example, Rogers and Monsell (1995) found that switch costs were smaller when the preparation interval in advance of performing a different task was longer and when participants were given pre-cues as to the need to perform a task switch. However, providing participants with such opportunities to prepare for an upcoming task switch does not completely eliminate switch costs (Allport et al., 1994; Fagot, 1994; Meiran, 1996, 2000; Meiran, Chorev, & Sapir, 2000; Rogers & Monsell, 1995). For instance, Allport et al. (1994, Experiment 5) presented participants with runs of two trials consisting of Stroop colour words and requested that they either name the ink colour or read the colour word. Prior to each run of trials, participants were informed which task to perform on the first trial (prime event) and whether the task to be performed on the later trial (probe event) was the same or different from the prime event. Furthermore, the authors varied the response-stimulus interval (RSI; the interval between the prime response and the probe stimulus) from 20 msec to 1,100 msec. Results indicated that switching costs persisted even at the longest RSI.

The residual costs that remain after allowing for preparation of the upcoming task indicate that task-switching might involve two stages. The first stage of task-switching might involve endogenous control, in which preparatory effects take place. However, the second stage of task-switching might not commence until the target stimulus appears (Merian, 1996; Rogers & Monsell, 1995). Thus, when given time to prepare for an upcoming task, task-set reconfiguration begins but is not completed until the efforts to complete the second task begin. During this second stage of task-switching, it might be that the presentation of the stimulus carries with it information that impedes performance. This information could be residual inhibition of the current task set applied during recent efforts to respond to an alternative task. According to a task-set inhibition approach, completion of a current task requires suppression of a competing, but presently irrelevant, task set (e.g., Goschke, 2000; Meiran, 1996; Meiran et al., 2000; Rogers & Monsell, 1995). This inhibition may persist until that formerly irrelevant task set becomes the task that a person is currently required to perform, and may not be fully eliminated until a person can perform that task in response to some stimulus. The outcome is that task-switch costs can occur even when participants have a significant amount of time and forewarning that would allow them to prepare for the requirement to perform a different task. Alternatively, during this second stage of task-switching, the presentation of the stimulus might cue retrieval of a recent episode. When a current task is different from a preceding task, retrieval of processes engaged during completion of the preceding task will be inappropriate for completion of the current task. This inappropriate transfer may then impair current performance.

An episodic retrieval approach has been useful in explaining how a recent episode can influence current performance on a task. For instance, episodic retrieval principles are useful in describing the phenomenon of asymmetric switch costs, that is, the findings of larger costs when switching to a dominant task set (Allport et al., 1994). This phenomenon was first described by Allport and colleagues (1994). Participants in their study performed a colour-word Stroop task involving the presentation of colour words printed in congruent and incongruent ink colours. The researchers found that participants were equally fast at word-reading congruent and incongruent items, whereas colour-naming was significantly slower for incongruent items compared to congruent items. This suggested that the word-reading task was the more dominant task of the two. The researchers found that switching from the nondominant colour-naming task to the dominant word-reading task resulted in larger costs compared to switching to the nondominant colour-naming task. Allport et al. (1994) proposed that the dominant word-reading task-set involves stronger task-set inertia or persisting activation. To overcome this source of interference during completion of the colour-naming task, strong inhibition must be applied to the persisting task set. Thus, when switching to the dominant task, one is required to recover from the inhibition that was applied to it during completion of the nondominant task set. That is, the asymmetric switch cost reflects the time it takes to overcome the inhibition previously applied to the dominant task.

Applying an episodic approach to explain asymmetric switch costs, Wong and Leboe (2009) suggested that the amount of switch costs might depend not on the time it takes to overcome persisting inhibition applied to a dominant task set but, instead, on the amount of time it takes to overcome the interference caused by the retrieval of inappropriate information from a prior episode. The key factor they identified was the elaborateness of processing required by the task participants were switching from, rather than the requirement for participants to switch from a dominant task to a nondominant task. In their study, participants were presented with well-known animal names (e.g., RABBIT) that were presented in red or green and in large or small font size. In two experiments, participants completed tasks that required conceptual judgments about the animal names, involving judgments about the typical habitat or size of the animals. They also completed perceptual tasks that required judgments about either the word size or font colour of the animal names. Although none of the tasks could be properly considered as dominant over the others in the same way as the contrast between Stroop colour naming versus colour word reading, the two perceptual tasks were easier to perform and participants tended to complete them more quickly and more accurately than the two conceptual tasks. The critical finding of these experiments was that the size of switch costs depended almost entirely on the difficulty of the task participants were required to switch from. That is, regardless of the ease or difficulty of the task that participants were required to switch to, the need to switch *from* a relatively difficult conceptual task generated much larger switch costs than the need to switch *from* a relatively easy perceptual task. The implication is that switching costs can be reasonably seen as a form of proactive interference that is more difficult to overcome when performance of a preceding task requires more extensive processing. Given this plausible role of episode-based interference in determining the size of switching costs, in general, such costs ought to be sensitive to manipulations that enhance the accessibility of the episode associated with completion of the preceding task.

Similarly, Leboe, Whittlesea and Milliken (2005) found larger costs switching from an elaborate nondominant task set. In their study, words and nonwords were presented in red or green. Participants were required to complete a colour-name task or a lexical decision task. Half of the trials involved task repetition (e.g., Lexical Decision – Lexical Decision) and the remaining half of the trials involved switching between the two tasks (e.g., Lexical Decision – Colour Identification). The results of their study revealed asymmetric switch costs, that is, greater costs when switching from the relatively more difficult lexical decision task to the colour identification task compared to when participants were required to perform the reverse task switch. The authors suggested that the more elaborate processing that is associated with performing a lexical decision task generates a highly retrievable memory episode. During a task switch sequence, inappropriate processes associated with the lexical decision task set were either more readily retrieved or represented a greater source of interference when retrieved, causing a more potent source of interference when participants were required to perform the colour identification task.

In this same study, the authors varied the amount of stimulus information that overlapped between the prime and probe events. The prime and probe stimuli varied along the dimensions of colour, lexical status, and orthography, such that the prime and probe targets matched or mismatched. The expectation was that the overlap of information would cue the retrieval of prior processes engaged during the prime task. When the retrieved processes were inappropriate for completing a different probe task, this would result in large switch costs. Results of their study revealed particularly high switch costs when participants performed a lexical decision task followed by a colour naming task when the prime and probe targets also matched in colour. The authors concluded that the colour match between the prime and probe targets cued the retrieval of inappropriate processes which then further enhanced interference of participants’ efforts to generate a response to the colour identification task. This previous study oriented to the more traditional method for measuring switch costs provided another motive to seek a role for episodic retrieval influences on the size of set alternation costs.

Alternating Task Effects

A study by Mayr and Keele (2000) demonstrated the alternating task effect (i.e., set alternation costs). In their study, participants were presented with 4 rectangular blocks displayed in the four corners of a computer screen. All items varied along the dimensions of colour, orientation, and movement. Moreover, each dimension was assigned a neutral attribute (the colour blue, an upright orientation, and no movement). Stimuli that varied on the colour dimension were either purple or pink; stimuli that varied along the orientation dimension were either tilted right or left; and stimuli that varied along the movement dimension moved either horizontally (right – left) or vertically (up – down). During each trial, only one of the four stimuli deviated along a particular dimension. For example, only one out of the four stimuli was pink, while the remaining three stimuli were blue (neutral). Preceding each trial, participants were provided with verbal instructions that corresponded to three possible tasks. Specifically, participants were instructed to identify the block that deviated along either the colour dimension, the orientation dimension, or the movement dimension by making a key press corresponding to the location of the deviant block. [Participants performed either a no-alternation sequence of trials (e.g., Colour – Orientation – Movement) or a set-alternation sequence (e.g., Colour – Orientation – Colour)]. Results revealed slower performance on set-alternation sequences of trials compared to no-alternation sequences.

From a task-set inhibition approach, task-sets are organized in a network carrying activation weights and signals for communication with other units in memory. This activation is thought to initiate a task-set after reaching some threshold level and activation persists after performance of that task. In a traditional task-switch sequence (Task A – Task B), the activation of task set A persists onto Task Set B and interferes with responding, leading to a switch cost. However, to complete Task Set B, one would have to somehow overcome the persistent activation from the preceding Task Set A. Mayr and Keele (2000) proposed that it is an active inhibitory process that allows a person to disengage from the preceding task set. They referred to this process as *backward inhibition*. Inhibition of an earlier task set allows the current task set to take precedence. The result is that it should be more difficult to revert back to a task set that was recently switched from than to a task set that was abandoned less recently (Mayr & Keele, 2000). In other words, there should be greater costs associated with a set-alternation sequence (ABA) than with a no-alternation sequence (CBA). This is because inhibition applied to Task Set A would be stronger when performing it at the end of an ABA sequence then when performing it at the end of a CBA sequence. Thus, the concept of backward inhibition can account for Mayr and Keele's observation of set alternation costs.

Mayr and Keele's (2000) observation of backward inhibition appeared to be unaffected by stimulus features, in contrast to what would be expected if episodic retrieval principles were involved in modulating set-alternation costs. Specifically, Mayr and Keele (2000) varied the overlap of target information between events such that colour, orientation, or movement of the Event 1 and Event 3 targets matched or mismatched. According to an episodic account, if Event 3 shares similar information with Event 1, this should cue retrieval of Event 1 processes that are appropriate for completing the same Event 3 task and result in smaller costs compared to when there is no overlap of information. However, the results of their study showed that an overlap of low-level feature information between targets of Events 1 and 3 had little influence in set alternation costs. They concluded that episodic retrieval processes did not have a role in the costs associated with alternating task sequences.

Mayr and Keele's (2000) suggestion that episodic retrieval processes are unlikely to account for the alternating task effect received support from another study by Arbuthnott and Frank (2000). In their experiment, participants were presented with stimuli (digits, letters, symbols) and were required to make semantic categorization judgments (even/odd, consonant/vowel, text/math). Displayed on a computer screen were three stimuli from each of the three stimulus categories. The stimuli were positioned in a column. On some occasions, the target position repeated across the trials. [Participants completed a five-trial task sequence consisting of (Task 1 – Task 1 – Task 2 – Task 3 – Task 2). Thus, within a sequence of trials, the participant performed a task repetition, a 1-switch no-alternation sequence, and a set alternation]. Similar to Mayr and Keele's (2000) study, an alternating task effect was observed. In addition, factors associated with retrieval of a previous episode were ineffective at modulating alternation costs. Specifically, target location repetition across no-alternation and set-alternation sequences of trials did not influence the alternating task effect. From this, the authors stated that it was unlikely that set-specific episodic retrieval underlies the alternating task effect. They proposed that a more likely explanation for the costs associated with reverting to a previously abandoned task set is that it reflects the amount of time it takes to resolve the inhibition that was applied to an earlier competing task set that becomes the to-be-completed task set on the next trial.

Given that both the persisting inhibition account and the episodic framework propose that costs reflect some type of interference, it would be sensible to further investigate the source of this interference. According to the inhibition view, persisting inhibition interferes with current responding and the time it takes to resolve this interference reflects the costs. The approach that I have adopted when conducting my dissertation research is that the proposal to reject a role for interference owing to reinstatement of a prior episode is premature. My goal was not to establish that episodic retrieval is definitively the cause of set alternation costs. Rather, my goal was to determine whether the size of set alternation costs would depend on factors known to be important for heightening the accessibility of episodic memory representations.

In seeking a role for episodic retrieval influences, I also investigated set alternation costs with conceptual tasks that require somewhat more elaborate, meaning-based processing than the fairly simple tasks that researchers of set alternation costs tend to rely upon. My rationale was that it could be more difficult to detect episodic retrieval influences originating from the overlap between the features of a current and previous event when performance of a task is relatively easy. In other contexts, support for episodic memory influences on priming effects tend to emerge mainly when the tasks required of participants involve deeper conceptual engagement (Hughes & Whittlesea, 2003; Joordens & Becker, 1997; Leboe et al., 2005; Whittlesea & Jacoby, 1990). With this strategy, success in observing a role for episodic memory principles might motivate future studies aimed at sorting out the relative contributions of inhibitory control processes *versus* episodic retrieval processes to the generation of set alternation costs.

Chapter 3: Present Study

Experiment 1

Results of Stroop studies indicate that participants are slower and less accurate at naming the colour of an incongruent colour word than at naming the colour of a congruent colour word. For example, a congruent colour word might be the word RED printed in red font colour whereas an incongruent colour word might be the word RED printed in green font colour (see MacLeod, 1991, for a review). Interestingly, this impairment in responding to incongruent colour words is greater when participants complete a larger proportion of congruent colour naming trials during an experiment (Logan, & Zbrodoff, 1979; Lowe & Mitterer; 1982; Tzelgov, Henik, & Berger, 1992). This finding suggests that because the congruent Stroop colour items can be successfully completed by word-reading, the participants will tend to rely more on engaging automatic word-reading processes to respond. However, a reliance on word-reading processes will interfere with naming the font colour on incongruent trials. Interestingly, the proportion manipulation can be item-specific. That is, specific Stroop items can be associated with mostly congruent or incongruent trials. As an example, Jacoby, Lindsay, and Hessels (2003) presented colour words (e.g., YELLOW and GREEN) in a congruent colour on 80% of trials and other colour words (e.g., BLUE and RED) in a congruent colour on only 20% of trials. Results revealed greater impairment in responding to incongruent trials involving mostly congruent colour words than to incongruent trials involving mostly incongruent colour words.

Extending the work of Jacoby et al. (2003), Crump, Gong, and Milliken (2006) found that the stimulus location could be an effective cue for demonstrating item-specific proportion congruency. Unlike in Jacoby et al.'s study (2003), the proportion congruent manipulation was not dependent on the Stroop item. Instead, it was based on the location in which the Stroop item appeared. On one section of the computer screen (e.g., the upper half), most of the items that appeared were congruent, whereas on another section of the computer screen (e.g., the lower half), most of the items that appeared were incongruent. Results revealed greater impairment in responding to incongruent trials involving Stroop items that appeared in the mostly congruent, upper half location.

Extending this work to the task-switching paradigm, Leboe, Wong, Crump, and Stobbe (2008) manipulated item-specific congruency based on location during a task switch. Participants were assigned to one of two conditions that differed based on the probability of being required to complete a task repetition across trials (mostly task-repeated trials vs. mostly task-switch trials). Each display consisted of the presentation of an animal name. In response to each display, participants completed one of two tasks. They either identified the size of the animal as big or small or they identified the habitat of the animal as typically land or sea by making a button-press. Participants knew which task to perform by the presentation of rows of symbols above and below the location of the animal name presented during each display. For instance, if the animal name was accompanied by rows of pound signs (######), that cued the participant to identify the size of the animal, whereas rows of asterisks (******) required identification of the animal's typical habitat. Manipulation of the proportion of task-repeated trials was based on the location of the probe word. For example, in one condition, on 75% of the trials for which the probe target was displayed at the upper location of the screen, the participants were required to perform the same task that they performed in response to the earlier prime display. In this condition, the probe task differed from the prime task on only 25% of the trials. The likelihood of performing the same versus a different task in response to the probe display was reversed for probe displays presented at the lower location of the screen.

The outcome of this study was that greater switching costs occurred when the probe location indicated a high likelihood of a task repetition. This result is consistent with an episodic retrieval view in that current processing is affected by the retrieval of earlier processing episodes. When task repetitions more frequently occurred when the target display appeared in the upper location of the screen, the appearance of a target at that location would make contact with those earlier experiences and engage processes that occurred during previous trials. In consequence, participants were most prepared to repeat a task for probes that appeared in the upper location, resulting in an increased cost on the few trials that required a task switch. For target displays that appeared at a lower location, participants' experiences on previous trials prepared them to perform a different task than the one they completed when responding to the prime display. As a result, the cost of switching tasks was smaller in magnitude than when the location of the probe display was mainly associated with task repetitions.

Similar to the Leboe et al. (2008) study, for my Experiment 1, I expected that the size of the alternating task effect would depend on a location-based manipulation of the proportion of set-alternation versus no-alternation sequences. The target location of Event 3 became associated with either a higher proportion of set alternation sequences or a higher proportion of no-alternation sequences. In other words, the display for Event 3 was associated with processes necessary for a switch back to the task associated with Event 1 or a switch to an alternate task-set. I expected that when presented with a task in a mostly set-alternation location, that feature of the current display would cue retrieval of memory episodes for previous trials in which the Event 3 stimulus appeared at that location. However, because the targets of Event 1 and 3 mismatch, retrieval would not be entirely appropriate for performing Event 3's task. The partial match between Events 1 and 3 would produce greater amounts of interference that would be the source of large set-alternation costs. In contrast, if most of the instances for which the Event 3 stimulus appeared at the upper location involved no-alternation sequences, processes needed to complete a no-alternation sequence would be engaged. There would be less retrieval of the Event 1 memory episode and thus, less interference produced as a result of retrieving an associated task-set paired with a different target.

Participants. Students enrolled in an Introduction to Psychology course at the University of Manitoba were recruited for participation in exchange for course credit. Twenty-three of the participants completed trials in which the upper location of the screen was associated with mostly task unrepeated trials (Task C – Task B – Task A). Twenty-five participants completed trials in which the upper location of the screen was associated with mostly task repeated trials (Task A – Task B – Task A). All participants were required to be under the age of 40, have normal vision (or corrected to normal vision), normal colour vision, and English as a first language. There were 53 participants; however, data for participants who failed to respond accurately for at least one trial for each condition of the design (i.e., they incorrectly responded to one event in the three-event sequence that defined each trial) were omitted from subsequent analysis. This policy resulted in the omission of 5 participants, resulting in the analysis of responses from the remaining 48 participants.

Apparatus and stimuli. For this experiment and the additional three experiments described below, participants were seated in front of a 15-inch colour monitor connected to a Dell Personal Computer. E-Prime software (MacWhinney, St. James, Schunn, Li, & Schneider, 2001) was used to present stimuli and to record the speed and accuracy of participant responses.

Stimuli for Experiment 1 consisted of 8 well-known animal names, chosen such that an equal number of words corresponded to the categories of animal habitat (land vs. water), relative size (big vs. small), and mammal (mammal vs. non mammal). Specifically, one name represented a big, land, mammal (TIGER), one represented a big, water, mammal (WHALE), one represented a big, land, non-mammal (OSTRICH), one represented a big, water, non-mammal (SHARK), one represented a small, land, mammal (RABBIT), one represented a small, water, mammal (BEAVER), one represented a small, water, non-mammal (LOBSTER), and one represented a small, land, non-mammal (PIGEON). On each trial, words appeared in red, blue, or green font colour; The font colour indicated which task to complete. Words printed in red indicated that a mammal judgment was to be made. Words printed in blue indicated that a size of animal judgment was required. Lastly, words printed in green indicated a habitat judgment. Times New Roman font size of 30 was used. The letters subtended 1.36^{o} of visual angle, horizontally, and 3.41^{o} of visual angle, vertically.

Design and procedures. There was 1 practice block composed of 8 sequences of three-event trials and 1 test block composed of 160 sequences of three-event trials. The Event 3 displayed in the upper location of the screen was either associated with mostly set-alternation or mostly no-alternation sequences. Participants were required to complete three conceptual tasks in succession. For instance, they either categorized animal names according to their typical habitat (Land: Yes vs. No), relative size (Big: Yes vs. No), or whether the animal is a mammal (Mammal: Yes vs. No). Every combination of these three conceptual tasks contributed equally to the set-alternation and no-alternation sequences. The two task sequence conditions (no-alternation sequence vs. set-alternation sequence) were contingent on the location of Task 3. When the Task 3 display appeared at one location (i.e., above the vertical mid-point of the computer screen), participants completed a set-alternation sequence on 75% of those trials and a no-alternation sequence on the remaining 25% of trials. When the Task 3 display appeared at another location (i.e., below the vertical mid-point of the computer screen), participants completed a no-alternation sequence on 75% of those trials and a set-alternation sequence on the remaining 25% of trials.

Detailed verbal instructions pertaining to the types of tasks to be completed were given to participants at the start of the experiment. Additionally, participants were informed that they would be given an opportunity to take breaks after every three-event sequence. Participants were instructed to sit a comfortable reading distance away from the computer. Furthermore, they were asked to respond as quickly and accurately as possible by making a button press on a keyboard. Participants responded by pressing keys “x” for yes and “m” for no.

Each three-event sequence began with the request “Press b to Begin” displayed in the middle of the screen. Following the pressing of the “b” key, a fixation cross appeared centered on the screen for 500 msec was replaced with the task display for Events 1 and 2. The task display for Event 3 appeared either above the vertical mid-point of the screen or below the vertical mid-point of the screen. The task display for Events 1, 2, and 3 appeared with a target printed in red, blue, or green font colour. If a response to a task was incorrect, error feedback consisting of the presentation of the word “WRONG!” appeared for 500 msec in the prior location of the target display. Furthermore, if participants took longer than 5000 msec to respond, the word “INCORRECT” appeared for 500 msec in the prior location of the target display. Otherwise, a correct response initiated the disappearance of the event’s target and task cue. The response-stimulus interval (RSI) between Events 1 and 2 and between Events 2 and 3 was 50 msec whereas separating each of the three trial sequences was a 500 msec delay followed by the instruction, “Press b to Begin” (see Figure 1).

Figure 1. Details of Experiment 1 Procedure

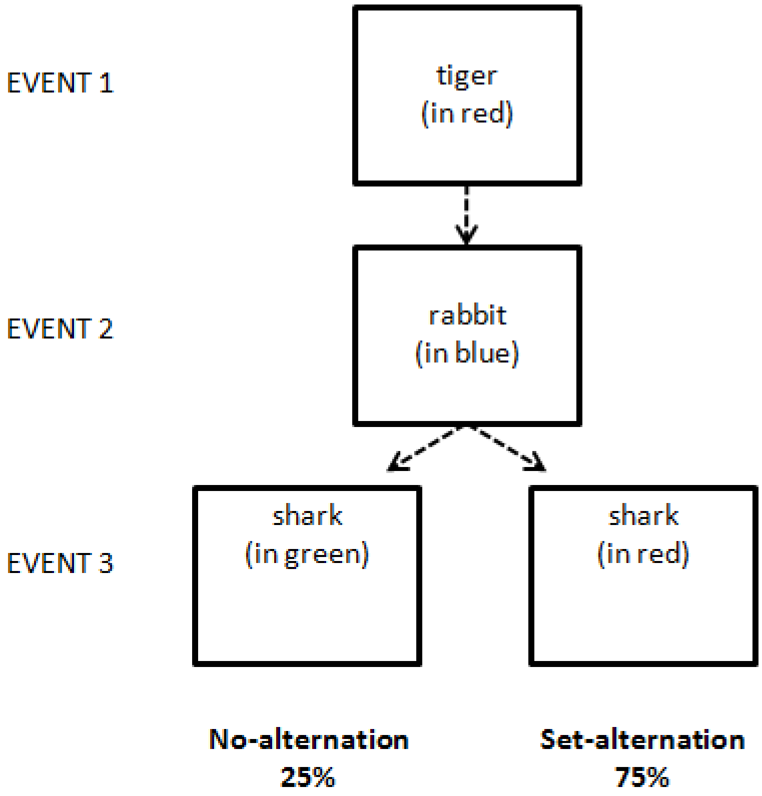

Figure 1: Example of possible no-alternation sequence and set-alternation sequence when the upper location is associated with mostly set-alternation sequences of trials. The colour of the target acted as a task cue (red = mammal task, blue = size task, green = habitat task). The figure does not represent exact scaling.

Analysis. Since the main goal of all 4 experiments was to examine factors that underlie the alternating task effect, it is not theoretically critical to discuss the results of the Event 3 task conditions. Thus, for all 4 experiments, the responses to the Event 3 tasks were collapsed. Any interaction effects with the Event 3 task condition are listed in the Appendix.

For Experiment 1, the first 40 three-event experimental trials were omitted from analysis. This was done to examine RT data that may be influenced by the proportion switch manipulation. That is, the first 40 three-event trials provided an opportunity for participants to learn that the Event 3 target location was associated with the likelihood of a no-alternation or a set-alternation sequence. The omission of the first 40 three-event trials resulted in the omission of 25% of experimental trials.

Within each of the combinations of Set Alternation Probability (mostly set-alternation location vs. mostly no-alternation location) X Task Sequence (set-alternation vs. no-alternation), trial sequences in which an error was made in response to the target item during Events 1, 2, or 3 were not included in the analysis of RTs. This resulted in the elimination of 19% of trials. Specifically, there were errors made in response to the target item on 8% of Event 1 and 9% of Event 2 trials and errors made on 7% of Event 3 trials. After eliminating these trials, for each of the conditions of interest, mean correct RTs and proportion of errors in responding to the target for Event 3 were computed for each participant. Set-alternation costs were computed as the difference between participants' average correct response times on set-alternation trials and no-alternation trials. The data were then submitted to a 2 (set alternation probability: mostly set-alternation location vs. mostly no-alternation location) X 2 (task sequence: set-alternation vs. no-alternation) repeated-measures analysis of variance (ANOVA). The mean correct RTs and proportion of incorrect responses during Event 3 can be seen in Table 1 and Figure 2.

Table 1

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 1, as a function of set-alternation probability (mostly no-alternation vs. mostly set-alternation) and task sequence (no-alternation vs. set-alternation)

	Mostly no-alternation		Mostly set-alternation	
Task sequence condition	RT	ERR	RT	ERR
No-alternation	1338	.063	1334	.048
Set-alternation	1306	.059	1372	.069
AT effects	32	.004	-38	-.021
SE	20	.018	21	.019

AT Effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Figure 2. Event 3 Response Times and Proportion of Errors

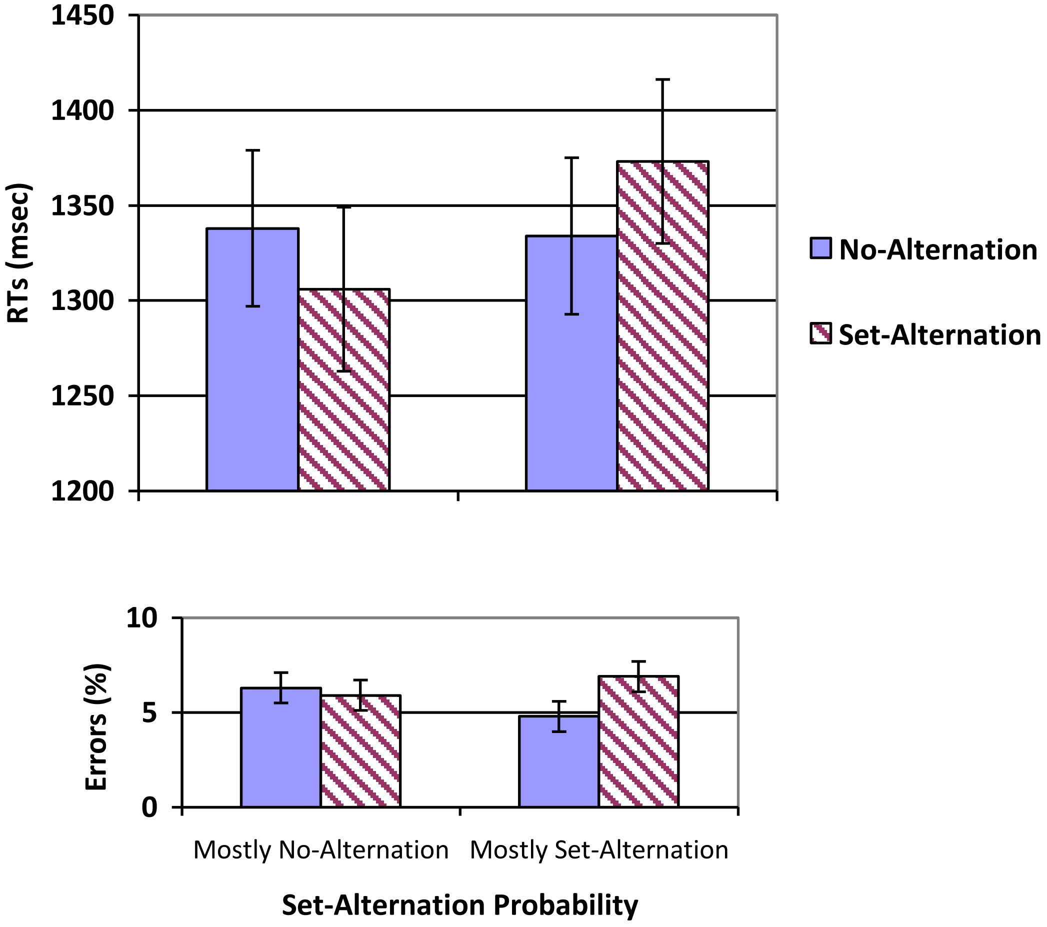

Figure 2. Mean correct response times (RTs) of the Event 3 tasks as a function of set-alternation probability (mostly no-alternation, mostly set-alternation) and task sequence (no-alternation, set-alternation). The error bars show standard error of the no-alternation trials and set-alternation trials.

For all experiments, an alpha level of .05 was used for all statistical tests. Furthermore, Event 3 response times were automatically excluded from further analysis if they were less than 200 msec or greater than 3000 msec. Overall, exclusion of RTs on this basis resulted in the elimination of less than 3% of observations.

The purpose of Experiment 1 was to test whether low-level task-irrelevant features, such as target location, would affect the magnitude of set-alternation costs. As discussed above, an inhibitory control processes account of the alternating task effect involves executive control mechanisms that operate on abstract task-set representations. These mechanisms do not permit influences on set alternation costs that depend on features of the Event 3 stimulus (Mayr & Keele, 2000) or their associations with prior processing episodes. Costs are thought to be the result of persisting inhibition that was applied prior to Event 3. Thus, any manipulation of set-alternation probability that occurs at the time of Event 3 should not modulate the amount of inhibition that was applied earlier.

Results and discussion.

***Response times*.** Response time analysis yielded a significant main effect of set alternation probability, *F*(1, 47) = 4.57, *MSE* = 27,586.98, *p* =.038. That is, RTs during the Event 3 task were 32 msec faster for the mostly no-alternation location than for the mostly set-alternation location (1321 vs. 1353 msec). There was no main effect of task sequence, *F* < 1.

There was a significant interaction between the task sequence and set alternation probability conditions, *F*(1, 47) = 6.45, *MSE* = 27,586.98, *p* < .05. This effect arose from particularly slow RTs during set-alternation trials within the mostly set-alternation condition. That is, within the set-alternation condition, RTs were about 66 ms slower during mostly set-alternation trials than on mostly no-alternation trials (1372 vs 1306 msec), *F*(1, 47) = 9.54, *MSE* = 11150.14, *p* < .003. Within the no-alternation condition, RTs during mostly no-alternation trials (*M* = 1338, *SD* = 293) and mostly set-alternation trials (*M* = 1334, *SD* = 292) were not significantly different, *p* > .05.

Errors.

Examination of the error data revealed no significant main effects or interactions ($p >$ .05, in all instances).

Summary. I predicted that the manipulation of set alternation probability based on location would modulate set-alternation costs by generating higher costs in the mostly set-alternation condition. My idea was that encountering mostly set-alternation sequences on those trials would prepare participants to complete three-task sequences with an Event 3 task that was the same as the Event 1 task. Performance on set-alternation trials was significantly slower when the probability that the Event 3 task would match the Event 1 task was high. That is, participants were more impaired in completing the Event 3 task when the location of the Event 3 target was predictive of the requirement to complete a set-alternation sequence.

The probability manipulation did succeed in introducing a bias for participants to expect that the Event 3 task would match the Event 1 task. That is the expected consequence of a probability manipulation like the one I used in Experiment 1. In other priming studies, increasing the probability of a relation between the present task and a preceding task tends to increase the contribution of the preceding task to current performance (Crump, Gong, & Milliken, 2006; Crump & Milliken, 2009; Kane et al., 1997; Logan & Zbrodoff, 1979; Marczinski, Milliken & Nelson, 2003).

I considered two possible sources of interference that might be enhanced when the probability of set alternation is high. First, following an inhibitory account of set alternation costs, processes occurring at the time of Event 2 might be most critical, since that is when participants are thought to impose inhibition on the Event 1 task. In turn, that inhibition is what might generate costs to performance of that task during Event 3. Perhaps task-set inhibition generated during Event 2 influences performance of the Event 3 task only when the probability of set alternation is relatively high.

Second, although the Event 1 and Event 3 tasks may have been the same on set alternation trials, the Event 1 and Event 3 target words always differed on set-alternation trials. That being the case, it is possible that encouraging greater reliance on the Event 1 task by increasing set-alternation probability might actually contribute a source of interference on set-alternation trials. After having performed an Event 1 task in response to one target word, the influence of those processes when performing the same task during Event 3 might conflict with the need to respond to a different target. Other studies have reported similar sources of impairment. For example, there is a long history of studies that reveal impaired performance when participants must repeat a task in response to items from different categories (Bertelson, 1965; Hommel, 1998; Marczinski et al., 2003). Perhaps performance on set-alternation trials was superior in the mostly no-alternation condition because the Event 3 task was unlikely to match the Event 1 task on those trials. On the rare trials for which the Event 3 task matched the Event 1 task in that condition, the memory episode for completion of the Event 1 task may have been less accessible to impair performance in generating a response to Event 3.

Manipulations known to determine the accessibility of episodic representations in memory provide one way to distinguish between these two alternatives, even though proponents of an inhibitory account often argue that set-alternation costs are insensitive to episodic memory influences (Arbuthnott & Frank, 2000). A strong episodic explanation of set alternation costs might emphasize the role of the Event 1 task, as I have done in the previous paragraph. When the Event 1 target differs from the Event 3 target, perhaps heightened accessibility of the memory episode for having performed the Event 1 task is a source of impaired performance on set alternation trials. By contrast, some blend of an episodic and inhibitory account might be a better explanation if it is accessibility of the Event 2 memory episode that has the primary role in generating set-alternation costs. After performing Event 1 in a set alternation sequence, inhibition of the Event 1 task-set that occurs during efforts to respond to Event 2 might well form part of a memory episode. This approach would simply follow previous efforts to reconcile episodic and inhibitory accounts of priming effects, such as in discussions of the negative priming effect (e.g., May**,** Kane, Hasher, & Stoltzfus, 1997**;** Tipper, 2001). If so, one might expect that enhancing the accessibility of the Event 2 memory representation would increase the magnitude of set-alternation costs.

I conducted Experiment 2 for the purpose of distinguishing between these two possibilities. Given that overlap in event features is a factor that is known to enhance the accessibility of events in memory (Eysenck, 1978; Morris, Bransford, & Franks, 1977; Tenpenny, 1995; Tulving & Osler, 1968; Tulving & Thompson, 1973), I tested whether set-alternation costs would depend on the presentation of identical words as targets 1) for the Event 2 and Event 3 tasks or 2) for the Event 1 and Event 3 tasks. According to the rationale provided above, a match in Event 2 and Event 3 targets should *increase* the size of set-alternation costs according to a combined inhibitory/episodic retrieval account of those costs. By contrast, a match in Event 1 and Event 3 targets should *reduce* the size of set-alternation costs according to the episodic account of set-alternation costs described above. When the probability of set-alternation was high, my suggestion was that participants were impaired in performing the Event 3 task because the high probability of a match in the Event 1 and Event 3 task made the Event 1 memory episode more accessible to influence performance. In turn, a cost may have occurred as a result of interference from participants having just performed an identical task in reference to a different target word. This match on one dimension combined with a mismatch on another has been known to generate impaired performance in other priming contexts (see Leboe, J. P., Leboe, L. C., & Milliken, 2010, for a discussion). Experiment 2A tested the role of a match between Event 2 and Event 3 targets, whereas Experiment 2B tested the role of a match between Event 1 and Event 3 targets.

Experiment 2A

In Experiment 2A, similar to the procedure of Experiment 1, participants were required to complete three consecutive events (Event 1→ Event 2 →Event 3) on each of a series of trials. On half of the experimental trials, participants completed a no-alternation sequence of trials. On the remaining experimental trials, they completed a set-alternation sequence. Within both the no-alternation and set-alternation sequences, targets for each of the three tasks differed from each other on 50% of trials, whereas the Event 2 and Event 3 targets were identical on the remaining trials. Event 1 differed from Event 2 and Event 3 targets on every trial.

Based on backward inhibition accounts, set-alternation costs occur because the task set associated with Event 1 is inhibited during the completion of the second task during Event 2. This inhibition carries over to Event 3 and impairs responding to the third task. Thus, backwards inhibition emphasizes the role of inhibition applied to processes engaged at the time of Event 2 on performance during Event 3. Greater amounts of inhibition applied to the no-longer-relevant task of Event 1 allow for successful completion of the second task during Event 2. However, when the inhibited task-set becomes relevant at the time of Event 3, the prior inhibition impairs responding. In other words, set-alternation costs are thought to result because of reverting back to a previously inhibited task set. Proponents of this inhibitory view suggest that most of these costs originate from inhibition applied to the task-set rather than low-level factors such as the features that characterize the target stimulus display. For example, Mayr and Keele (2000) and Arbuthnott and Frank (2000) found that repetition of attributes associated with stimulus displays across the three-event sequence had no influence on set-alternation costs. They suggested that the costs must be caused by inhibition of abstract task-set representations, independent of the stimulus information that participants were using to complete those tasks.

In contrast, an explanation of set-alternation costs that presumed a role for episodic retrieval processes would predict that an overlap in the stimuli presented during Event 3 and Event 2 should make the memory representation generated by Event 2 more accessible for retrieval during participants' efforts to respond to Event 3. Even if it is an inhibitory process at the time of Event 2 that causes set-alternation costs, perhaps expression of that inhibitory process in the form of set-alternation costs depends on accessibility of the memory representation for Event 2. Although previous research has failed to confirm that such overlap can contribute to set alternation costs (Arbuthnott & Frank, 2000; Mayr & Keele, 2000) it is noteworthy that set-alternation studies tend to rely on quite simple tasks and stimuli. The lack of information represented by the sort of stimulus displays that tend to be used by researchers of set-alternation costs might make it difficult to observe changes in the magnitude of set-alternation costs as a function of stimulus overlap across the three-event sequences. That possibility was one motive for my present reliance on more meaningful items and requiring participants to perform conceptual tasks. In other priming studies, increasing stimulus and task complexity in this way makes it easier to observe such contributions of overlap between a current task and a prime event (e.g., Hughes & Whittlesea, 2003). Thus, the expectation is that higher set-alternation costs would occur for task sequences in which the Event 3 and Event 2 targets are the same than for task sequences in which those two targets are different.

Participants. Students enrolled in an Introduction to Psychology course at the University of Manitoba were recruited for participation in exchange for course credit. It was required that all participants were under the age of 40, have normal vision (or corrected-to-normal vision), normal colour vision, and English as a first language. There were 28 participants. However, data for participants who failed to respond accurately for at least one trial for each condition of the design (i.e., they incorrectly responded to one event in the three-event sequence that defined each trial) were omitted from subsequent analysis. This policy resulted in the omission of 1 participant resulting in the analysis of responses from the remaining 27 participants.

Apparatus and stimuli. Stimuli for Experiment 2A consisted of the same 8 well-known animal names as Experiment 1.

Design and procedure. Participants were required to complete 1 practice block consisting of 8 sequences of three-event trials and 1 test block comprised of 160 sequences of three-event trials. They categorized animal names according to a LAND-WATER habitat judgment, a BIG- SMALL size judgment, or a MAMMAL-

NONMAMMAL judgment. Every combination of these three conceptual tasks contributed equally to the set-alternation and no-alternation sequences. The procedure of Experiment 2A was identical to that of Experiment 1 except that within both the set-alternation and no-alternation sequences, targets for each of the three tasks differed from each other on 50% of trials (e.g., Event 1 – TIGER, Event 2 – OSTRICH, Event 3 – SHARK). The target stimulus for Event 2 and Event 3 repeated on the remaining 50% of trials (e.g., Event 1 - TIGER, Event 2 - OSTRICH, Event 3 - OSTRICH) (see Figure 3).

Figure 3. Details of Experiment 2A Procedure

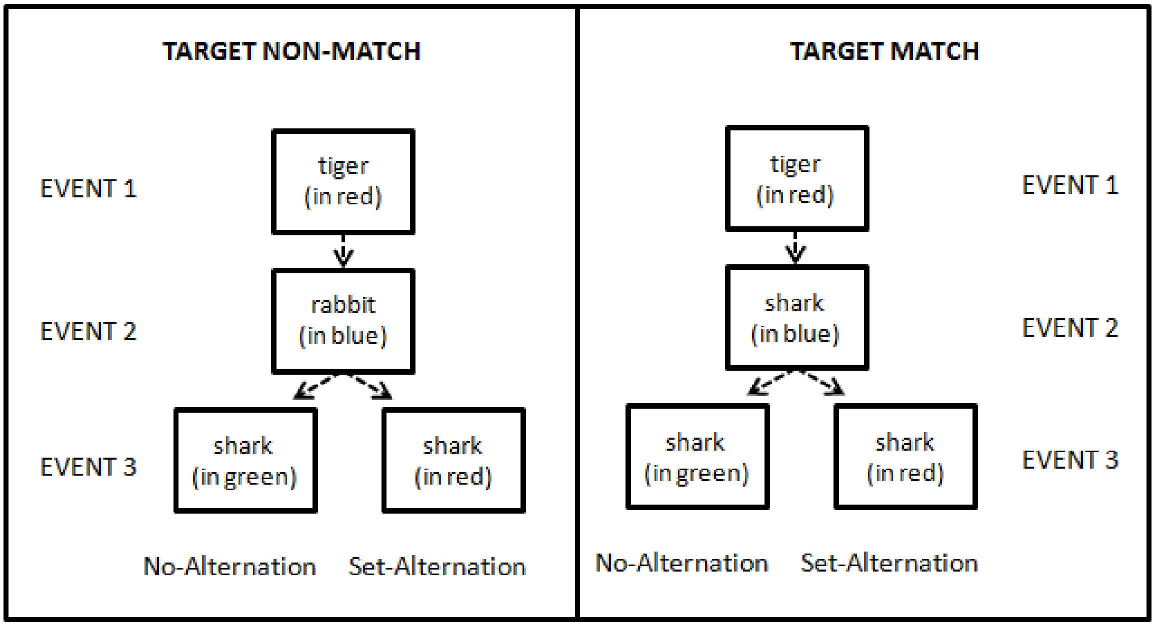

Figure 3: Example of a possible Event 2/Event 3 target identity non-match, no-alternation sequence and set-alternation sequence and an example of a possible Event 2/Event 3 target identity match, no-alternation and set-alternation sequence. The colour of the target acted as a task cue (red = mammal task, blue = size task, green = habitat task). The figure does not represent exact scaling.

Analysis. Within each of the combinations of Event 2/Event 3 Target Identity Match condition (non-match vs. match) X Task Sequence condition (no-alternation vs. set-alternation), trial sequences in which an error was made in response to the target item during Event 1, Event 2, or Event 3 were not included in the analysis of RTs.

Specifically, an error was made on 10% of Event 1 trials and on 10% of Event 2 trials, and an error was made on 11% of Event 3 trials. Elimination of trials in which an error was made resulted in the overall exclusion of 24% of trials from the RT analysis. After eliminating these trials, for each of the conditions of interest, mean correct RTs and proportion of errors in responding to the target for Event 3 were computed for each participant. These data were then submitted to separate 2 (Event 2/Event 3 target identity match) X 2 (task sequence) repeated measures ANOVAs, treating Event 2/Event 3 target identity match condition (non-match vs. match) and task sequence (no-alternation vs. set-alternation) as within-participant factors. Table 2 and Figure 4 displays the mean Event 3 RTs and proportion of incorrect responses in Experiment 2A.

Table 2

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 2A, as a function of target identity (non-match vs. target match), and task sequence (no-alternation vs. set-alternation)

	Non-match		Match	
Task sequence condition	RT	ERR	RT	ERR
No-alternation	1389	.061	1322	.101
Set-alternation	1519	.084	1424	.110
AT effects	-130	-.023	-102	-.009
SE	27	.019	24	.016

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = represents the within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Figure 4. Experiment 2A Response Times and Proportion of Errors

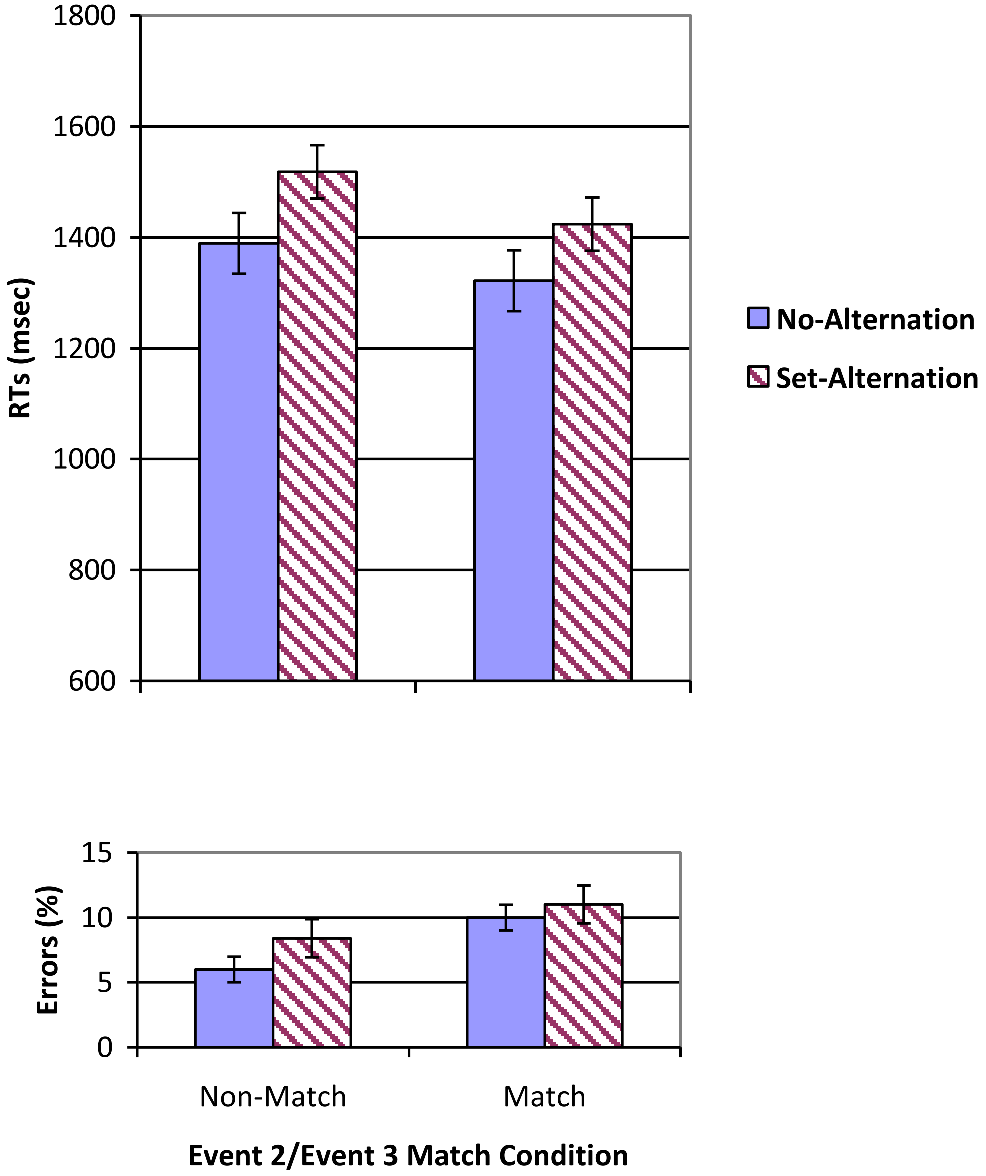

Figure 4. Mean correct response times (RTs) of the Event 3 tasks as a function of Event 2/Event 3 target identity match (non-match, match) and task sequence (no-alternation, set-alternation). Error bars are the standard error of the no-alternation and set-alternation trials.

Results and discussion.

Response times. Analysis of response time data yielded a significant main effect of task sequence condition, $F(1,27) = 25.88$, $MSE = 43{,}692.58$, $p < .001$. In particular, mean Event 3 RTs were 115 msec faster on no-alternation trials than on set-alternation trials (1356 vs. 1471 msec). There was also a significant main effect of Event 2/Event 3 target identity match, $F(1, 27) = 10.97$, $MSE = 50{,}525.33$, $p < .005$. Event 3 RTs during match trials were 81 msec faster than during non-match trials, (1373 vs. 1454 msec). There was no significant interaction between the Event 2/Event 3 target identity match and task sequence conditions, $p > .05$.

Errors. An examination of the proportion of errors revealed a significant main effect of Event 2/Event 3 target identity match condition, $F(1, 27) = 18.70$, $MSE = .005$, $p < .001$. Specifically, participants were 4% more likely to make an Event 3 error during match trials than non-match trials (11% vs. 7%). No other main effects or interactions were significant ($p > .05$, in all instances).

Summary. The outcome of Experiment 2A revealed that a match in target identities between Event 3 and Event 2 did not significantly increase set-alternation costs. Indeed, robust set-alternation costs occurred in both the match and non-match conditions. In set-alternation sequences, it was thought that the similarity between Event 3 and Event 2 should enhance retrieval of processes engaged during Event 2, which might include inhibition of the task-set that was just activated for Event 1. If the Event 2 episode involves both task-set B processes and inhibition of task-set A, the enhanced retrieval of the Event 2 memory episode should act as greater source of interference and result in a greater impairment to performance on set alternation trials. The results of Experiment 2A failed to confirm this expectation.

Experiment 2B

The results of Experiment 2A indicated no significant difference in set-alternation costs when Event 3 and Event 2 targets overlapped. Experiment 2B tested the alternative possibility that overlap between the features of Event 1 and Event 3 would have a greater influence on the size of set-alternation costs. An episodic retrieval view of set-alternation costs would predict that an overlap in the stimuli between Event 3 and Event 1 should make the memory representation generated by Event 1 more accessible for retrieval during efforts to respond to Event 3. During a set-alternation sequence in which the Event 3 task is identical to the Event 1 task, a difference in the targets corresponding to those events might contribute a source of impairment to performing the Event 3 task. If so, set alternation costs should be larger when different items serve as Event 1 and Event 3 targets compared to when the targets of Event 1 and Event 3 match.

Participants. Students enrolled in an Introduction to Psychology course at the University of Manitoba were recruited for participation in exchange for course credit. It was required that all participants were under the age of 40, have normal vision (or corrected-to-normal vision), normal colour vision, and English as a first language. There were 32 participants. Data from all 32 participants were used for the analysis.

Apparatus and stimuli. Stimuli for Experiment 2B consisted of the same 8 well-known animal names as Experiment 1.

Design and procedure. Participants were required to complete 1 practice block consisting of 8 sequences of three-event trials and 1 test block comprised of 160 sequences of three-event trials. The procedure of Experiment 2B was identical to that of Experiment 2A except that within both the set-alternation and no-alternation sequences, targets for each of the three tasks differed from each other on 50% of trials (eg., Event 1 – TIGER, Event 2 – OSTRICH, Event 3 – SHARK). The target stimulus for Event 1 and Event 3 repeated on the remaining 50% of trials (eg., Event 1 - TIGER, Event 2 - OSTRICH, Event 3 - TIGER) (see Figure 5).

Figure 5. Details of Experiment 2B Procedure

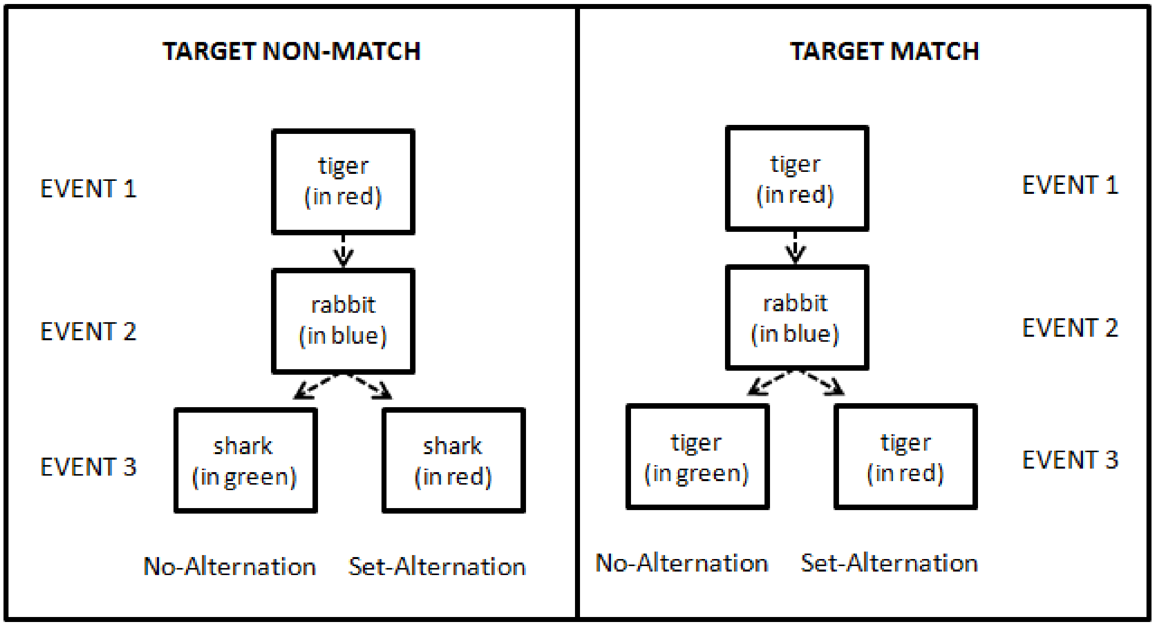

Figure 5: Example of a possible Event1/Event 3 target identity non-match, no-alternation sequence and set-alternation sequence and an example of a possible Event 1/Event 3 target identity match, no-alternation and set-alternation sequence. The colour of the target acted as a task-cue (red = mammal task, blue = size task, green = habitat task). The figure does not represent exact scaling.

Analysis. Within each of the combinations of Event1/Event 3 Target Identity Match condition (non-match vs. match) x Task Sequence condition (no-alternation vs. set-alternation), trial sequences in which an error was made in response to the target item during Event 1, Event 2, or Event 3 were not included. Specifically, an error was made on 8% of Event 1 trials and on 8% of Event 2 trials, and an error was made on 7% of Event 3 trials. Elimination of trials in which an error was made resulted in the overall exclusion of 19% of trials from the RT analysis. After constraining these trials, for each of the conditions of interest, mean correct RTs and proportion of errors in responding to the target for Event 3 were computed for each participant. These data were then submitted to a separate 2 (Event 1/Event 3 target identity match) X 2 (task sequence) repeated-measures ANOVAs, treating Event1/Event 3 target identity match condition (non-match vs. match) and task sequence condition (set-alternation vs. no-alternation) as within-participant factors. Table 3 and Figure 6 display mean correct Event 3 RTs and proportions of errors in Experiment 2B.

Table 3

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 2B, as a function of Event 1/Event 3 target identity match (non-match vs. match), and task sequence (no-alternation vs. set-alternation)

	Non-match		Match	
Task sequence condition	RT	ERR	RT	ERR
No-alternation	1404	.058	1449	.079
Set-alternation	1515	.077	1399	.014
AT effects	-111	-.019	50	.065
SE	28	.012	23	.017

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = represents the within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Figure 6. Experiment 2B Response Times and Proportion of Errors

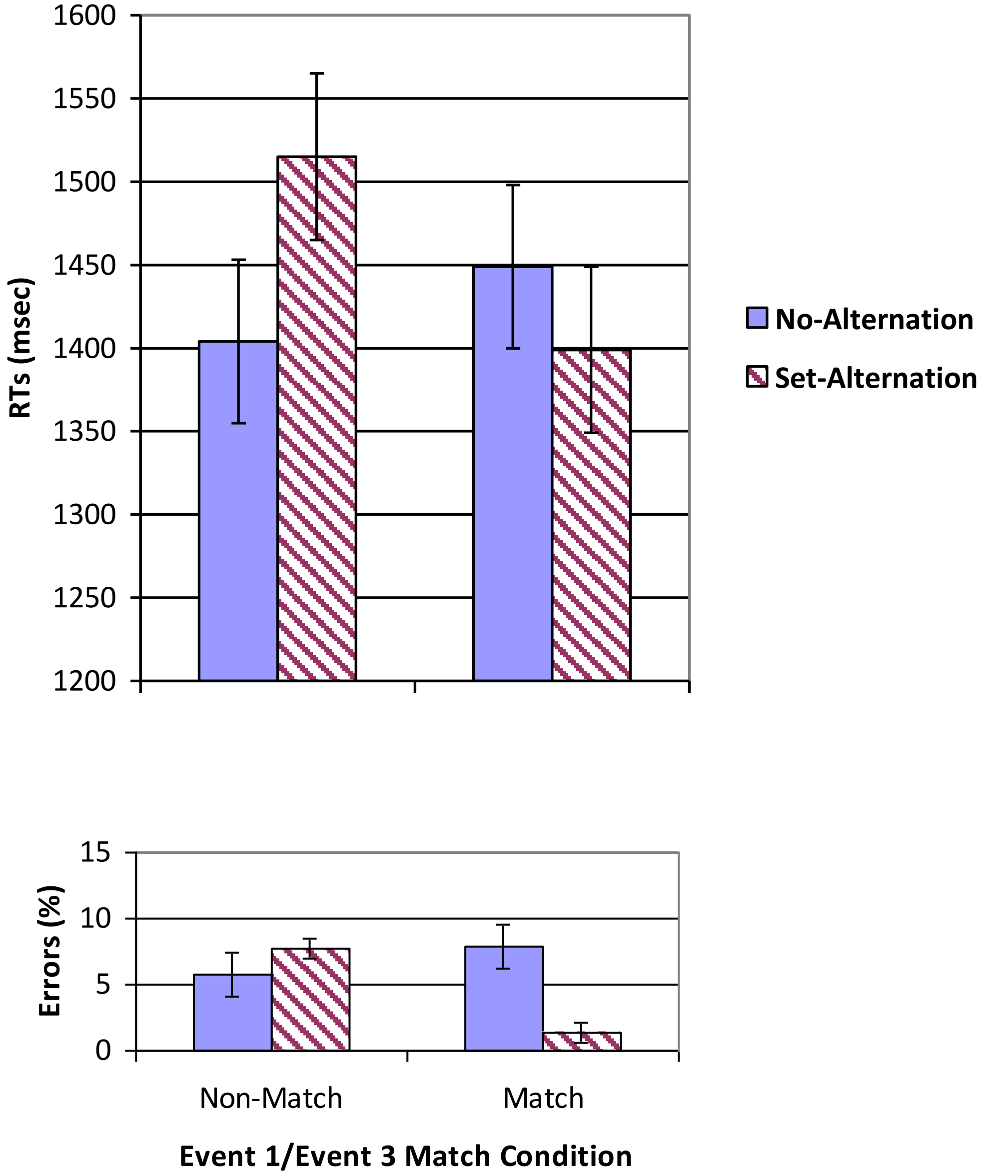

Figure 6. Mean correct response times (RTs) of the Event 3 tasks as a function of Event 1/Event 3 target identity match (non-match, match) and the task sequence (no-alternation, set-alternation). Error bards represent standard error of the no-alternation and set-alternation trials.

Results and discussion.

Response times. The analyses of response time revealed a significant main effect of Event 1/Event 3 target identity match, $F(1, 31) = 6.21$, $MSE = 19{,}190.49$, $p < .05$. Mean correct Event 3 RTs were 36 msec faster when the Event 1 and Event 3 targets matched than when they were different (1424 vs. 1460 msec). There was also a significant interaction between the target identity match and task sequence conditions, $F(1, 31) = 25.94$, $MSE = 23{,}967.74$, $p < .001$. For the non-match condition, participants were 111 msec slower to respond on set-alternation trials than on no-alternation trials (1515 vs. 1404 msec), $F(1, 31) = 16.07$, $MSE = 12{,}435.40$, $p < .001$. For the match condition, participants responded 50 msec *faster* on set-alternation trials than on no-alternation trials (1399 vs. 1449 msec), $F(1, 31) = 4.66$, $MSE = 8{,}308.79$, $p < .05$.

Errors. Examination of error data revealed a main effect of Event 1/Event 3 target identity match, $F(1, 31) = 5.92$, $MSE = .007$, $p < .05$. Participants were 2% more likely to make errors on non-match trials than on match trials (7% vs. 5%). No other main effects or interactions were significant ($p > .05$, in all instances).

Summary. During set-alternation trials in which the task-set for Event 1 and Event 3 repeat, I proposed that an overlap of information would enhance retrieval of Event 1 processes at the time of the Event 3 task. Since the targets presented and processes required for performing the Event 1 and Event 3 tasks are identical in the target match condition, the memory episode for Event 1 ought to be highly accessible and should represent an appropriate resource for completing the Event 3 task. In consequence, a large set-alternation cost observed when Event 1 and Event 3 targets did not match became a significant benefit on set-alternation trials when the Event 1 and Event 3 targets were identical.

In one of the earliest investigations of set-alternation costs, Mayr and Keele (2000) failed to reveal similar effects of overlap in the Event 1 and Event 3 stimulus displays on the size of costs observed. In their experiment, participants completed one of three perceptual tasks. Their task was to locate a rectangular bar that deviated along either a colour, orientation, or movement dimension. Specifically, if a bar deviated along the colour dimension, it was coloured pink or purple. If it deviated along the orientation dimension, it was rotated 45 degrees left or right. If it deviated along the movement dimension, it oscillated horizontally or vertically. During a task display, there were four rectangular bars in each of the four corners of the screen. Only one of the four bars deviated along the colour dimension, one deviated along the orientation dimension and one deviated along the movement dimension, leaving one rectangular bar in a neutral role. A verbal task cue was presented in the middle of the screen and later replaced by the task display. For example, if the Event 1 task-cue was the word “COLOUR,” this indicated that participants had to locate the rectangular bar that deviated along the colour dimension.

Mayr and Keele found that response times were significantly slower during the set-alternation trials compared to no-alternation trials. Moreover, they found that when the value of a dimension repeated between the Event 1 and Event 3 displays, this overlap had no influence on set-alternation costs. Specifically, if there was a match in colour, orientation, or movement between Event 1 and Event 3 targets, this did not influence the presence or size of the set-alternation cost they observed. From their results, they inferred that repetitions in stimulus features (and by inference, episodic memory processes) had no influence on the occurrence of set alternation costs.

Based on the results of the current study, I suggest an alternative possibility that the relatively primitive displays and simple tasks commonly used in set-alternation studies, including those employed by Mayr and Keele (2000), make it difficult to detect an influence of feature overlap between Event 1 and Event 3 stimulus displays. In the current experiment, I used words as my stimuli (instead of rectangular bars) and the tasks my participants were required to perform were conceptual judgments about the categories that the animals belonged to (instead of merely requiring participants to identify the location of a deviant item on the screen). Adding some complexity to the tasks and stimuli, my results indicated that perfect overlap between the Event 1 and Event 3 stimulus displays could entirely eliminate the sizeable set-alternation costs observed when the Event 1 and Event 3 targets differed. At the very minimum, it is clear that requiring participants to alternate tasks does not necessarily involve processes that operate solely at the task-set level, without contribution of the stimuli that provide the motive for performing those tasks. As seen in my Experiment 2B, the set-alternation effect was modulated by Event 1 and Event 3 target match. It is possible that the slower Event 3 RTs associated with non-match trials occurred because the retrieval of Event 1 processes interfered with Event 3 responding. That is, during set-alternation trials, the overlap of task-sets between Event 1 and Event 3 might cue the retrieval of processes associated with performing the Event 1 task-set. However, if the Event 3 target is different from the Event 1 target, the processing of Event 1 is not entirely appropriate for processing Event 3. The retrieval of Event 1 processes, thus, may act as a source of interference during performance of the Event 3 task-set and result in slower Event 3 RTs. When the Event 1 and Event 3 targets match, the overlap replaces this source of interference with a source of positive transfer.

Experiment 3

Results of Experiment 2B revealed that target match between Event 1 and Event 3 enhanced retrieval of Event 1 processes that were appropriate for completing Event 3 during set-alternation sequences. In consequence, there was faster responding to the Event 3 task-set. I also speculated that the set-alternation cost observed on non-match trials occurred because of a match in task combined with a mismatch in target, in contrast to the inhibitory account that places emphasis on the activation state of mental representations thought to determine engagement of task-sets. Experiment 3 provides another test as to whether task-set representations should be considered the primary source of set-alternation costs in the conceptual variant of the set-alternation procedure that I developed. If task-set representations are the exclusive source of set-alternation costs, it should not be possible to modulate the size of those costs by changing the form of stimuli presented in the three-event sequence. I used stimulus identity to illustrate the importance of the stimuli that appear in this three-event sequence in Experiment 2B.

In the present experiment, target items never matched within the three-event sequences that defined each trial. Instead, I generated two sets of stimuli that permitted overlapping response sets, but that differed in their visual features. This allowed for a test as to whether the alternating task effect depended on an overlap in the stimulus categories from which targets originate. More precisely, unlike the procedure of Experiment 2, the modulation of overlapping target information was at the level of stimulus category (animal names vs. pictures), rather than at the level of stimulus identity (target match vs. target non-match).

Participants. Students enrolled in an Introduction to Psychology course at the University of Manitoba were recruited for participation in exchange for course credit. It was required that all participants were under the age of 40, have normal vision (or corrected-to-normal), normal colour vision, and English as a first language. There were 32 participants. However, data for participants who failed to respond accurately for at least one trial for each condition of the design (i.e., they incorrectly responded to one event in the three-event sequence that defined each trial) were omitted from subsequent analysis. This policy resulted in the omission of 1 participant, constraining the analysis to the remaining 31 participants.

Apparatus and stimuli. The stimuli consisted of the same 8 well-known animal names used in Experiments 1 and 2. In addition to the animal names, participants were presented with 8 outlined pictures of animals corresponding to the same animals that were used to construct the set of animal names. On each trial, target names and the outline of target pictures appeared in one of three colours. The colour was either red, blue, or green. The colour indicated which task to complete. Pictures outlined in red colour indicated that a mammal judgment was to be made. A picture outlined in blue colour indicated that a size of animal judgment was required. Lastly, pictures outlined in green colour indicated a habitat judgment. The words subtended approximately 13.58° of visual angle horizontally and 3.41° of visual angle vertically while pictures subtended 13.58° horizontally and 16.90° of visual angle vertically.

Design and procedures. Participants were required to complete 1 practice block consisting of 16 three-event sequences and 8 test blocks comprised of 24 three-event trials for each of the 4 experimental conditions (stimulus category match condition x task sequence condition), totaling 192 experimental trials.

The design and procedure was identical to Experiment 2 except that participants categorized either animal names or outlined pictures of animals according to habitat (Land: Yes vs. No), size (Big: Yes vs. No), and type (Mammal: Yes vs. No). On each trial, participants were required to perform some combination of these tasks in a 3-event sequence. For all sequences, the targets for Tasks 1, 2, and 3 differed. Every combination of these three tasks and the two types of stimuli contributed equally to the set-alternation and no-alternation trials. On half of both set-alternation and no-alternation trials, the stimulus category of all three events matched. In this stimulus category match condition, all three stimuli consisted of pictures or they all consisted of words. On the other half of set-alternation and no-alternation trials, the category of Event 2 and Event 3 stimuli matched, but the Event 1 stimulus category differed. Sequences of the stimulus category non-match condition consisted either of a word followed by two pictures or a picture followed by two words. Figure 7 displays some example trial procedures for the Experiment 3 stimulus category match and non-match conditions.

Figure 7. Details of Experiment 3 Procedure

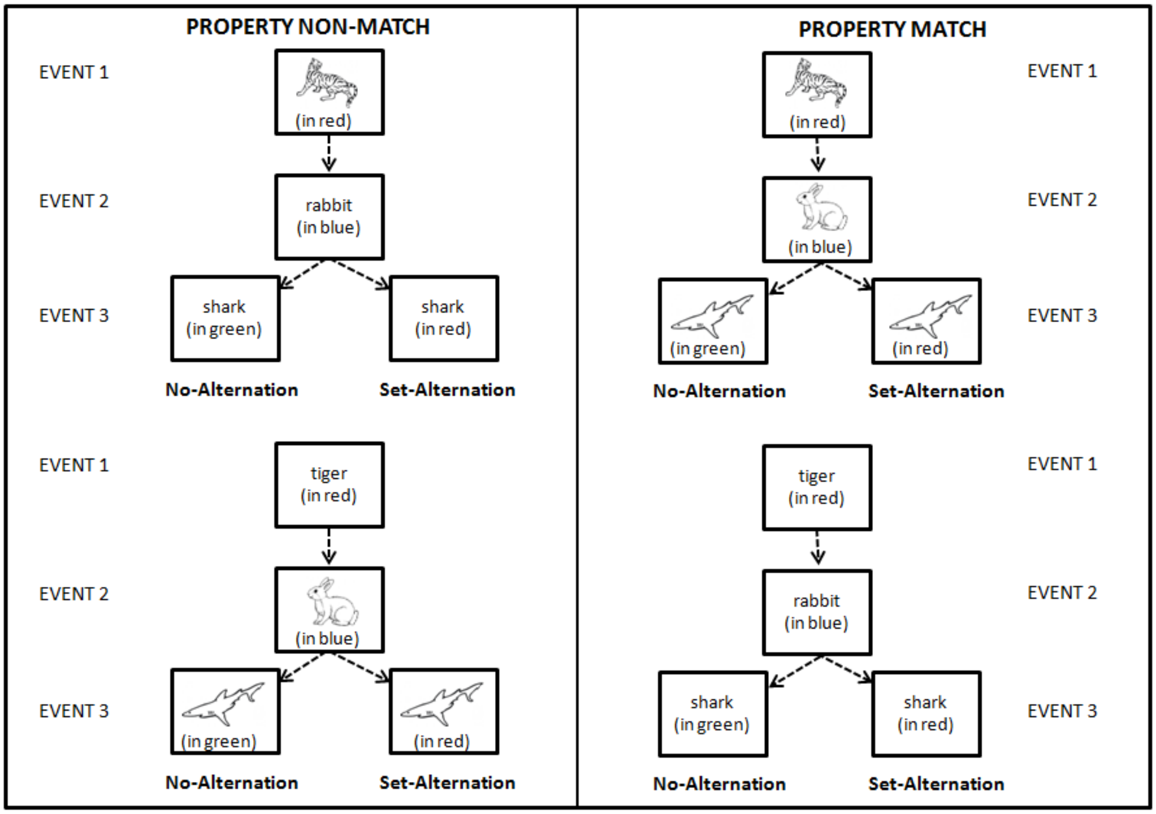

Figure 7. Example of no-alternation and set-alternation sequence when the stimulus category of Event 1 and Event 3 do not match and an example of when they match. The colour of the target acted as a task cue (red = mammal task, blue = size task, green = habitat task). The figure does not represent exact scaling.

Analysis. Within each of the combinations of Stimulus Category Match condition (non-match vs. match) X Task Sequence condition (no-alternation vs. set-alternation), trials in which participants made an error in response to the target of Event 1, 2 or 3 were not included in the analysis of RTs. Specifically, an error was made on 7% of Event 1 trials and on 9% of Event 2 trials, and an error was made on 7% of Event 3 trials. Elimination of trials in which participants made an incorrect response resulted in the overall exclusion of 22% of trials from the RT analysis. After constraining these trials, for each of the conditions of interest, mean correct Event 3 RTs and proportion of errors in responding to the target for Event 3 were computed for each participant. Set-alternation costs were computed as the difference between participants' average correct response times on set-alternation trials and no-alternation trials. The data was then submitted to a 2 X 2 repeated measures ANOVA, treating Stimulus Category Match condition (non-match vs. match) and Task Sequence condition (no-alternation vs. set-alternation) as within-participant factors. Table 4 and Figure 8 displays mean correct Event 3 RTs and the proportion of incorrect responses obtained in Experiment 3.

Table 4

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 3, as a function of Event 1/Event 3 stimulus category match (non-match vs. match), and task sequence (no-alternation vs. set-alternation)

	Non-match		Match	
Task sequence condition	RT	ERR	RT	ERR
No-alternation	1313	.055	1342	.059
Set-alternation	1416	.061	1400	.084
AT effects	-103	-.006	-58	-.025
SE	15	.009	17	.011

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = represents the within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Figure 8. Experiment 3 Response Times and Proportion of Errors

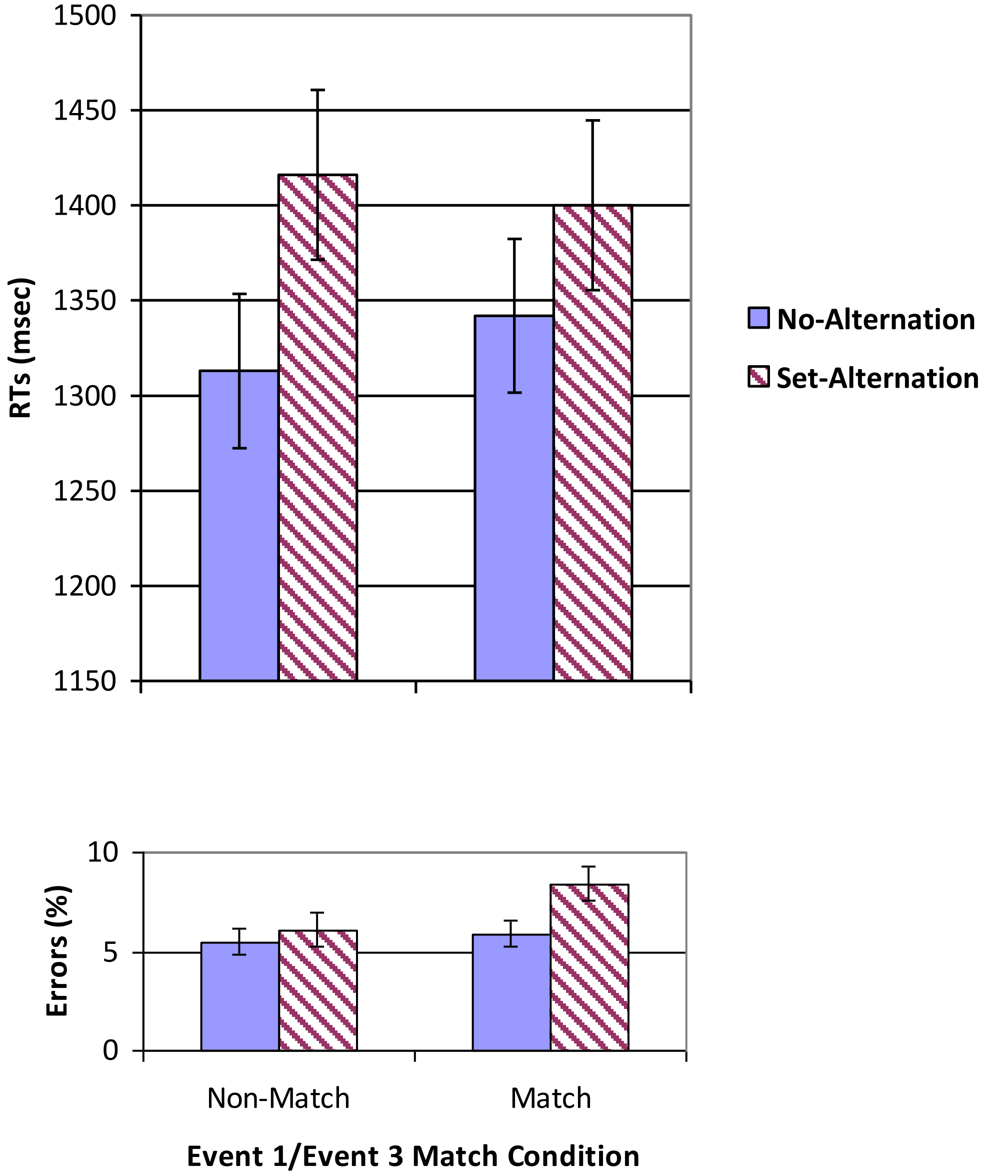

Figure 8. Mean correct response times (RTs) of the Event 3 tasks as a function of Event 1/Event 3 stimulus category match (non-match, match) and the task sequence (no-alternation, set-alternation). Error bars represent standard error of the no-alternation and set-alternation trials.

Results and discussion.

Response times. The analysis of mean correct RTs yielded a significant main effect of switch condition, $F(1, 30) = 44.99$, $MSE = 26{,}487.91$, $p < .001$. In particular, mean Event

3 RTs were 80 msec faster during no-alternation trials compared to set-alternation trials (1328 vs. 1408 msec). No other main effect was significant. That is, there was no main effect of stimulus category match, $F(1, 30) = .39$, $MSE = 10{,}216.42$, $p = .538$. The analysis also revealed a significant interaction between the stimulus category match condition and the task sequence condition, $F(1, 30) = 4.65$, $MSE = 20{,}860.08$, $p < .05$. Within the non-match condition, participants were 103 msec slower to respond on set-alternation trials than on no-alternation trials (1416 vs. 1313 msec), $F(1, 30) = 46.83$, $MSE = 3{,}503.42$, $p < 001$. Within the match condition, participants were only 58 msec faster to generate an Event 3 response on set-alternation trials than on no-alternation trials (1400 vs. 1342 msec), $F(1, 30) = 11.56$, $MSE = 4{,}388.00$, $p < .005$.

Errors**.** Examining proportion of errors, there was no significant main effects. That is, the effect of switch condition fell just short of statistical significance, $F(1, 30) = 4.18$, $MSE = .005$, $p = .05$. [Participants made 1.6% more Event 3 errors on set-alternation trials than on no-alternation trials (7.3% vs. 5.7%)]. Similarly, there was no significant main effect of match condition, $F(1, 30) = 2.17$, $MSE = .009$, $p = .151$. Participants made 1.2% more Event 3 errors during match trials than on non-match trials (.072 vs. 058). There was no significant interaction ($p > .05$).

Summary. During set-alternation trials in which the task-set for Event 1 and Event 3 repeat, an overlap of stimulus category significantly reduced the cost of set-alternation compared to a condition in which Event 1 and Event 3 targets represented different stimulus categories. This factor was independent of the nominal tasks participants were required to perform in response to stimuli that comprised the three-event sequence. Consequently, Experiment 3 provides another demonstration that the size of set-alternation costs can depend on factors other than the presence of a task alternation. Recall my proposal above in which I suggested that set-alternation costs might arise from a partial overlap between the episodic representation for Event 1 and the features of the Event 3 task and stimulus, at least in this variant of the procedure in which participants must perform conceptual judgments. By that view, retrieval of the Event 1 episode during efforts to respond to Event 3 might contribute a source of interference because there is a match on the task dimension, but a mismatch in the target that the task must be performed upon.

Analogous sources of mismatch have been revealed to impair performance in other studies that rely on the more conventional 2-event priming procedure (Hommel, 1998; Leboe, Whittlesea, & Milliken, 2005; MacDonald & Joordens, 2000). In this case, on set-alternation trials, the match in task impaired performance more when the Event 1 and Event 3 stimuli came from different categories (pictures vs. words). Thus, adopting a partial match framework for interpreting priming effects, these results suggest that the degree of difference that is present on the mismatching dimension increases the magnitude of impairment observed on set-alternation trials. Of course, the difference in magnitude of set-alternation costs is not easily explained merely by reference to a task-set inhibition account of such costs. These inhibitory processes are considered to operate on task-set representations that exist independently of the objects on which people perform those tasks. At the very least, I suggest that maintaining an inhibitory account of set-alternation costs requires that similarities in stimuli within the 3-event sequence must contribute an independent influence to the observed size of set-alternation costs.

Experiment 4

Having established that overlap in stimulus identity (Experiment 2B) and stimulus category (Experiment 3) within the three-event sequence can influence set-alternation costs, I was interested in testing whether the nature of Event 2 might also play a role in the size and/or observation of those costs. I expected that if a partial match between Events 1 and 3 are most critical in generating set-alternation costs I should find little to no modulation of costs when there was a partial match between Events 2 and 3. Presenting identical vs. different stimuli in Event 2 and Event 3 did not contribute significantly to set-alternation costs in Experiment 2A, suggesting that it is the relation between features of Event 1 and Event 3 that is most critical in the generation of my version of the set-alteration procedure. As a result, I suspected that, upon onset of Event 3, Event 2 would be important to the extent that it supported or obstructed access to the episodic representation for Event 1. In contrast to the procedure of the preceding experiments, I manipulated the form of Event 2 task such that generating a response to that event required a conceptual judgment on half of trials and a perceptual judgment on the other half of trials. My expectation was that requiring performance of a relatively involving conceptual task during Event 2 would make integrating Event 1 and Event 3 more difficult than when a relatively easy perceptual task separates those two events. In other contexts, whether a priming procedure promotes treating events as integrated versus separate and distinct can determine whether priming effects are positive or negative (Milliken & Lupianez, 2007). Perhaps the set-alternation procedure would generate a similar result by encouraging integration with a relatively easy intervening task or discouraging it with a relatively elaborate intervening task. During set-alternation trials, the overlap of Event 1 and Event 3 task-sets cues retrieval of the Event 1 memory episode. However, if the efficiency of retrieving the Event 1 memory episode is poor and there is a mismatch in Events 1 and 3 target information, the combination of obstructed retrieval of appropriate task-set and the retrieval of inappropriate target processes produces interference and results in large costs.

Participants. Students enrolled in an Introduction to Psychology course at the University of Manitoba were recruited for participation in exchange for course credit. Thirty-five participants were placed in Condition 1 (conceptual Event 2 task condition) and thirty-seven were placed in Condition 2 (perceptual Event 2 task condition). It was required that all participants were under the age of 40, have normal vision (or corrected-to-normal), normal colour vision, and English as a first language. There were 72 participants. Data from all 72 participants were used for the analysis.

Apparatus and stimuli. The same 8 well-known animal names used in Experiments 1, 2, and 3 were used for Experiment 4. However, for Experiment 4, the font in which those words appeared was either red or green. The colour of font manipulation was a way to investigate the effect of switching between conceptual tasks and an easier perceptual task.

Design and procedure. The design and procedure were identical to Experiment 2, except that participants were required to complete 1 practice block comprised of 8 three-event trials and 4 test blocks comprised of 38 three-event trials for each of the experimental conditions (Event 2 Task Elaborateness x Task Sequence) totaling 152 experimental trials. Participants were either placed in the conceptual Event 2 task condition or the perceptual Event 2 task condition.

The elaborateness of processing the Event 2 task was manipulated by task difficulty. Using animal names, participants placed in the Conceptual Event 2 Task condition completed three conceptual tasks, requiring judgments of relative animal size (Big: Yes vs. No), typical habitat (Land: Yes vs. No), and a mammal judgment (Mammal: Yes vs. No). In that condition, participants always performed a mammal judgment in response to the Event 2 target, whereas both Event 1 and Event 3 required either performing the animal habitat or animal size task. Those in the Perceptual Event 2 Task condition completed two of the conceptual tasks (Big: Yes vs. No) and typical habitat (Land: Yes vs. No). As well, they were required to complete one perceptual task involving a colour judgment (Red: Yes vs. No). For all participants, half of the trials involved set-alternation sequences and the other half involved no-alternation sequences. Figure 9 displays examples of the trial procedure for the different conditions of Experiment 4.

Figure 9. Details of Experiment 4 Procedure

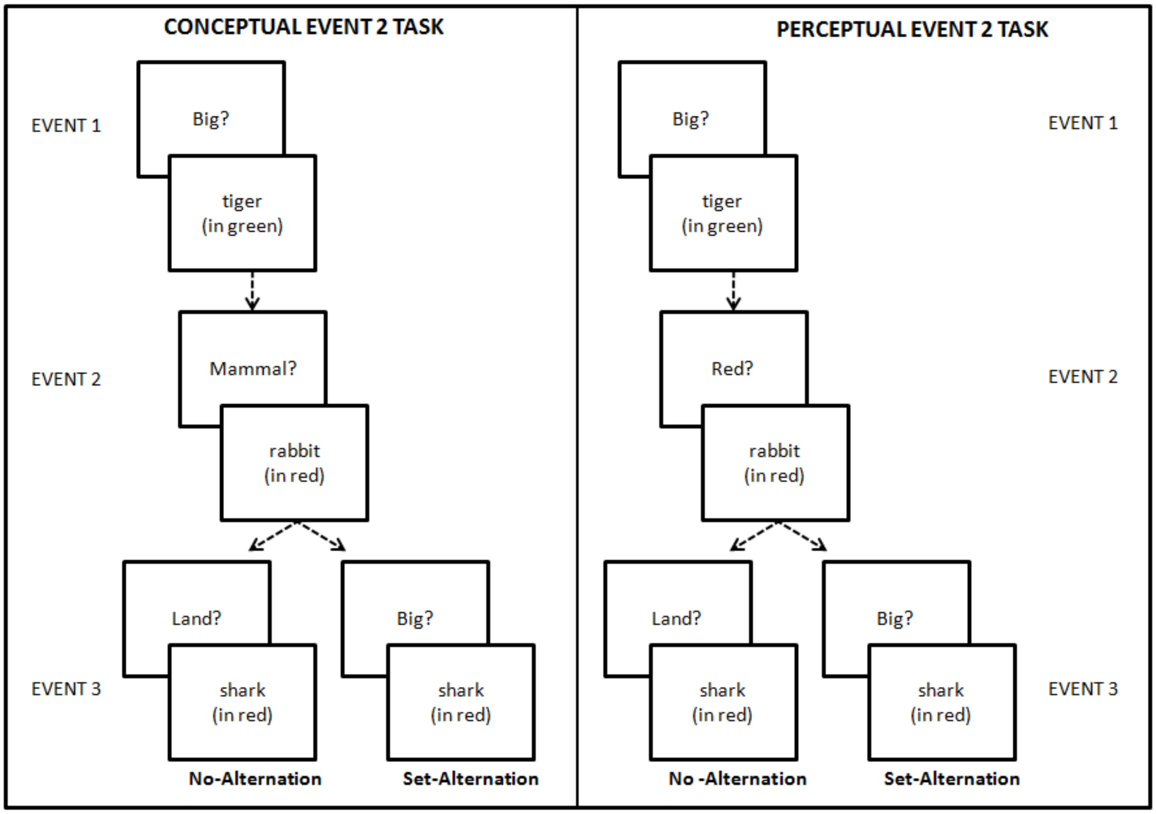

Figure 9. Example of no-alternation and set-alternation sequence when Event 2 is a conceptual task involving a mammal judgment and an example of when Event 2 is a perceptual colour-naming task. The figure does not represent exact scaling.

For all sequences, the target words for Events 1, 2, and 3 differed. A verbal task cue (Land? vs. Big? vs. Mammal? vs. Red?) was presented visually on the screen alone for 1000 msec. The target was presented along with the cue until a response was made. Since varying the cue stimulus interval (CSI) up to 900 msec does not modulate set-alternation costs (Mayr & Keele, 2000), presenting the task cue prior to the target was not a major concern. The response stimulus interval (RSI) was set to 100 msec, whereas a delay of 500 msec separated each 3-event sequence, followed by presentation of the request, “Press b to Begin”. Participants initiated each three-event sequence by pressing the ‘b’ button on the keyboard.

Analysis. The font colour of items during Event 1, 2, and 3 were counterbalanced independently of the type of task. Before each trial, the font colour was randomly selected without replacement so that both the red and green font colours were presented equally often across the experimental session.

In particular, within each of the combinations of the Event 2 Task (conceptual vs. perceptual) x Task Sequence (set-alternation vs. no-alternation) conditions, trial sequences in which an error was made in response to the target item during Event 1, 2, or 3 were not included. Specifically, those in the conceptual Event 2 task condition made an error on 8% of Event 1 trials and on 10% of Event 2 trials, and on 6% of Event 3 trials. Elimination of trials in which an error was made resulted in the overall exclusion of 20% of trials. Participants in the perceptual Event 2 task condition made an error on 5% of Event 1 trials, 2% of Event 2 trials and 4% of Event 3 trials. Elimination of trials in which an error was made resulted in the overall exclusion of 10% of trials from RT analysis. After constraining these trials, for each of the conditions of interest, mean correct RTs and the proportion of Event 3 errors were computed for each participant. For both Event 2 task conditions, the RT and error rate data were submitted to a 2 X 2 mixed-design ANOVA, treating switch condition as a within-participants factor and Event 2 task elaborateness as a between participants factor. Table 5 and Figure 10 displays the mean correct Event 3 RTs and error rates for Experiment 4.

Table 5

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 4, as a function of Event 2 task elaborateness (conceptual vs. perceptual) and task sequence (no-alternation vs. set-alternation)

	Conceptual		Perceptual	
Task sequence condition	RT	ERR	RT	ERR
No-alternation	963	.060	883	.037
Set-alternation	991	.048	864	.036
AT effects	-28	.012	19	.001
SE	12	.008	11	.004

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = represents the within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Figure 10. Experiment 4 Response Times and Proportion of Errors

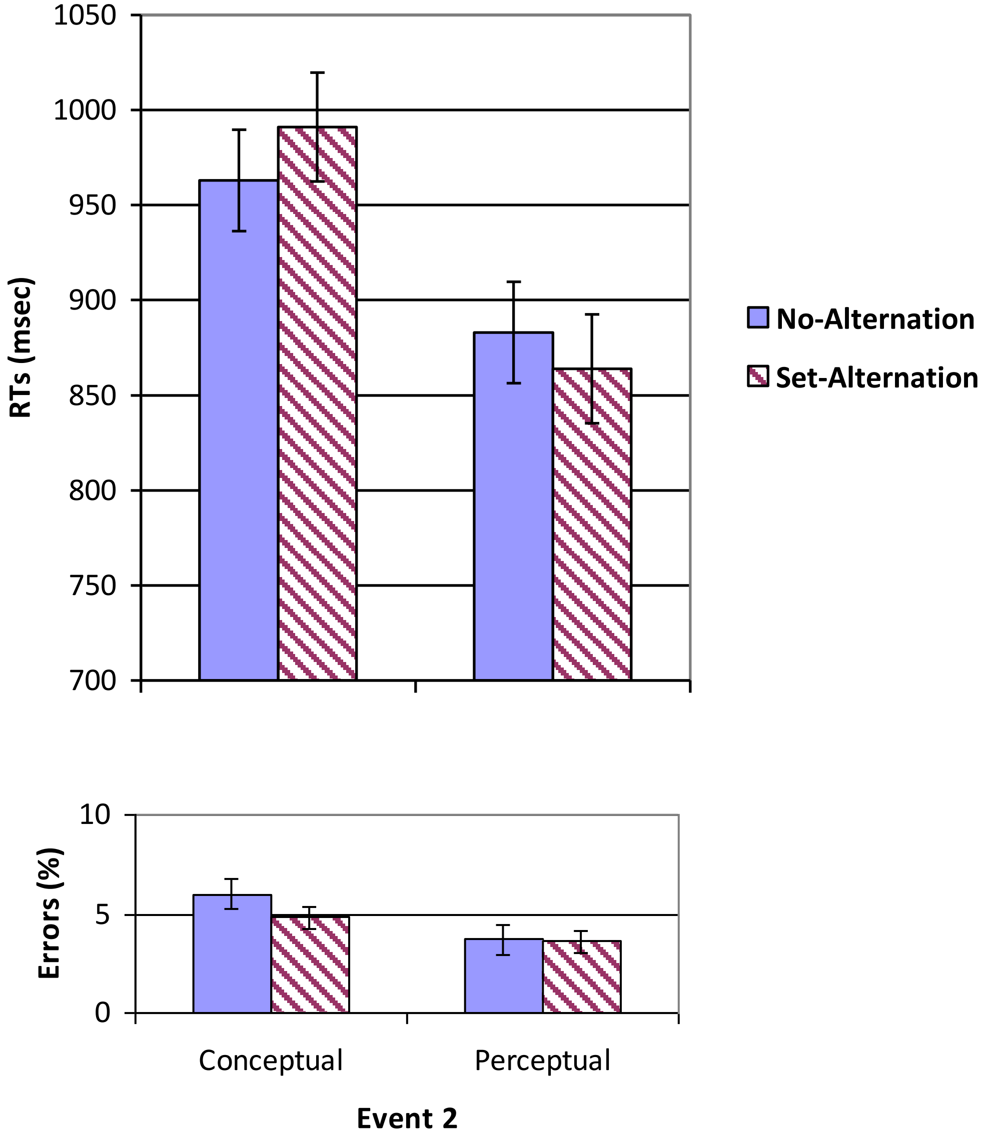

Figure 10. Response times (RTs) for Event 3 tasks as a function of Event 2 task elaborateness (conceptual, perceptual) and the type of task transition (no-alternation, set-alternation). Error bars represent standard error of the no-alternation and set-alternation trials.

Results and discussion

Response times.

Analysis of response times revealed a significant main effect of Event 2 task condition, *F*(1, 70) = 5.03, *MSE* = 37,906.36, *p* < .05. Event 3 response times were 104 msec faster when Event 2 involved the perceptual colour-naming task (873 vs. 977 msec). Further analysis revealed no main effect of switch condition, $F(1, 70) = .34$, $MSE = 5{,}076.71$, $p = .564$. Additional analysis of response time data revealed a significant Event 2 Task x Task Sequence interaction, $F(1, 70) = 7.86$, $MSE = 5{,}076.71$, $p < .05$. Within the conceptual Event 2 task condition, participants were 28 msec slower at responding on set-alternation trials than on no-alternation trials (991 vs. 963 msec), $F(1, 34) = 5.35$, $MSE = 2{,}642.52$, $p < .05$. Within the perceptual Event 2 task condition, Event 3 response times were not significantly different for set-alternation and no-alternation trials, $F(1, 36) = 2.65$, $MSE = 2{,}439.94$, $p = .112$.

***Errors*.** Examination of error data revealed no significant main effects or interaction ($p > .05$, in all instances).

Summary. In Experiment 4, I tested the idea that the elaborateness of the Event 2 task would influence set-alternation costs, inspired by other research suggesting that the nature of priming effects can often depend on whether conditions encourage integration vs. separation of the two critical events. In other contexts, inserting a longer time delay between two successive events can change a positive priming effect into a negative priming effect (Leboe et al., 2010). In this case, I presumed that a more elaborate conceptual judgment during Event 2 would minimize participants’ ability to integrate their prior experience of Event 1 as they performed the Event 3 task. In consequence, the utility of having just performed the same task would be lessened in favour of the contribution to performance arising from a partial match between the Event 1 and Event 3 tasks and stimuli. Indeed, when a conceptual task separated Event 1 and Event 3, I observed a significant set-alternation cost, as I had observed in my preceding experiments in which a conceptual task separated Event 1 and Event 3. By contrast, when a simpler perceptual task separated Events 1 and 3, I observed no significant set-alternation cost. I propose that when participants were more able to integrate Event 1 with Event 3, the advantage of an overlap in task took greater precedence and minimized the cost of a partial match that consisted of a match in task and a mismatch in target identity.

An inhibitory perspective does not come readily equipped to explain the difference in set alternation costs observed in the Event 2 conceptual vs. perceptual task conditions of my Experiment 4. That approach emphasizes the role of task abandonment during Event 1 in causing suppression of a corresponding task-set representation, which is thought to obstruct engaging in that same task during Event 3. Perhaps there is less of a requirement to inhibit the task-set representation for the Event 1 task when the Event 2 task is relatively easy (Arbuthnott, 2008). If so, that would account for the elimination of a set-alternation cost when the Event 2 task required a simple colour judgment.

In any case, the results of my Experiment 4 provide another demonstration that set-alternation costs can depend on influences that are separate from the presence of a task alternation.

Chapter 4: General Discussion

Most often, the set-alternation effect is explained by an inhibitory account (Koch, Gade, Schuch & Philipp, 2010). From this view, completion of Event 2’s Task-Set B requires inhibiting the competing irrelevant Event 1 Task-Set A. When this same task-set becomes relevant during Event 3, additional activation is required to overcome the inhibition. The time necessary for applying additional activation reflects the set-alternation costs.

An alternative explanation of set-alternation costs is the episodic retrieval approach. Based on an episodic retrieval approach, factors such as recency (MacLeod et al., 2003; Roediger & Crowder, 1976), similarity (Eysenck, 1978; Morris et al., 1977; Tenpenny, 1995; Tulving & Osler, 1968; Tulving & Thompson, 1973), and elaborateness of processing (Craik & Lockhart, 1972; Hunt, 1995; Hunt & Smith, 1996; Underwood & Freund, 1968) will enhance retrieval of prior processes. When there is enhanced retrieval but the targets of Events 1 and 3 mismatched, I expected that there would be interference. The time required to resolve the interference stemming from the retrieval of the same task-set applied to a different target during Event 3 would be the source of set-alternation costs. To examine whether alternating task effects are influenced by memory retrieval principles, I conducted four experiments.

Synopsis of Results

The purpose of Experiment 1 was to demonstrate that the magnitude of set-alternation costs depends on the location-based manipulation of the proportion of set-alternation versus no-alternation sequences – in other words, that set-alternation cost originates from retrieval of memory representations. Results indicated that responding to the Event 3 task was more impaired when the location of the Event 3 target was predictive of the requirement to complete a set-alternation sequence rather than a no-alternation sequence. The expectation of having to complete a set-alternation sequence might encourage the retrieval of the Event 1 memory episode because it should assist performance during Event 3. Supposing that the Event 2 memory episode includes inhibition of the task-set that was just activated for Event 1, this enhanced retrieval of the Event 2 memory episode should act as a greater source of interference and result in large costs to performance. To examine this possibility, Experiment 2A was designed to test whether a target match between Event 2 and Event 3 would increase costs.

The results of Experiment 2A indicated that costs were not modulated by a target match between Events 2 and 3. I suspect that the absence of a task-set overlap between Event 2 and Event 3 lessened the amount of retrieval to the extent that the target match between the events failed to modulate costs. Alternatively, set-alternation trials involve a task-set overlap between Event 1 and Event 3 which might enhance retrieval of Event 1 processes. The larger costs associated with trials in which one expects to complete a set-alternation sequence rather than a no-alternation sequence might occur because the requirement to respond to an Event 3 target that is different from the Event 1 target is a source of conflict and impairs responding to the Event 3 task set. From this, I expected that when the event targets match, the overlap replaces this source of interference with a source of positive transfer. The purpose of Experiment 2B was to examine whether a target match between Events 1 and 3 would modulate costs. Results indicated that it did modulate costs. When the Events 1 and 3 targets matched, set-alternation costs were eliminated.

A target match between Event 1 and Event 3 should enhance the retrieval of the Event 1 memory episode. The retrieval of processes associated with the Event 1 memory episode would be appropriate for responding to the same task-set and target of Event 3 during set-alternation trials, thus the retrieval should speed Event 3 responding to the extent that there is a facilitatory alternating task effect. In support of my prediction, I found faster RTs when the Event 1 and Event 3 targets matched.

Expanding on the findings of Experiment 2B, my Experiment 3 was designed to test the role of overlapping stimulus category information between Event 1 and Event 3 in determining the magnitude of set-alternation costs. Similar to Experiment 2B, the task-set and stimulus category overlap between Event 1 and Event 3 facilitated responding. These results provide further support that episodic retrieval principles are involved in the costs generated during task switches.

The key observation of Experiments 2 and 3 is that the overlap between Event 1 and Event 3 can modulate costs. In Experiment 4, the task-set elaborateness of the intervening Event 2 task was manipulated so that it was easy or relatively difficult. Assuming that the intervening Event 2 is a greater source of interference when it involves elaborate processing compared to less elaborate processing, performance of the Event 3 task should be more impaired when the Event 2 task involves elaborate processing. In particular, the retrieval of the Event 1 memory episode during performance of the Event 3 task-set is likely to occur when the two events involve similar processes. The overlap of information between Event 1 and Event 3 would cue the retrieval processes but the extent to which the Event 1 memory episode is retrieved may depend partly on how much Event 2 interferes with the retrieval. If the Event 3 task involves conceptual processing, the elaborateness of processing may obstruct the retrieval of the Event 1 memory episode. The extent that Event 2 interferes would be reflected in the amount of set-alternation costs. Results of Experiment 4 supported my prediction of larger costs when the Event 2 task was difficult compared to when it was relatively easy.

Limitations

Trials in which participants made an error were omitted from analysis. This was done to examine Event 3 RTs that were influenced by retrieval of processes involved with successful completion of an earlier task. However, using this elimination procedure reduced the number of trials that were included in my analysis. Thus, interpretation of the results should be made with caution. Future studies might consider reducing error rates by providing a larger set of practice trials.

Another limitation of the current study was the type of tasks that were used. The use of three different tasks for each experimental session was necessary for the examination of set-alternation costs. However, the RTs and error rates differed across the type of tasks used in my study suggesting that set-alternation costs could have been influenced by task type. Future studies might control the influence of task by increasing the amount of practice trials with each task. Greater exposure to the task-sets and targets might reduce any differences in RTs and error rates between the tasks.

Theoretical Implications

Both the inhibitory and episodic-retrieval accounts of the alternating task effect discuss set-alternation costs as being the result of interference. The inhibitory approach discusses the alternating task effect as reflecting the time it takes to resolve interference caused by persisting inhibition (Arbuthnott & Frank, 2000). Whereas, an episodic retrieval view explains the alternating task effect as a reflection of the time it takes to resolve interference caused by the retrieval of inappropriate processes. An explanation of the alternating task effect that involves episodic retrieval principles is more parsimonious than an inhibitory approach. Unlike an inhibitory account, an episodic retrieval perspective can explain both the costs and benefits generated by task switching as a result of retrieving inappropriate or appropriate processes respectively. In contrast, inhibition of an irrelevant task-set does not readily explain why there can be faster RTs on set-alternation trials compared to no-alternation trials without some mention of task-set activation processes associated with Event 1 that persist and facilitate Event 3 responding. This study was designed to examine whether factors that are known to enhance retrieval processes can modulate the alternating task effect.

Conclusions

The central finding of this study is that the alternating task effect was modulated by factors known to enhance retrieval of earlier memory episodes. For example, by increasing the overlap of information between events through target repetitions during set alternation trials, response times were faster compared to when there was a target non-match. I proposed that enhancing retrieval of prior processes would decrease the costs when the retrieved memory episode is appropriate for responding to the current task and increase the costs when the retrieved memory episode is inappropriate for the performance of the Event 3 task. In support of my hypotheses, I found that the alternating task effect was influenced by the amount of overlap of information between events. Nevertheless, this does not exclude inhibition from influencing the alternating task effect. Rather, my findings provide evidence of the stimulus-triggered retrieval of memory episodes as one of the underlying mechanisms of task switching.

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Appendix A

Table A1

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 1, as a function of Event 3 task (size vs. habitat vs. mammal), repetition probability (mostly no-alternation vs. mostly set-alternation), and task sequence (no-alternation vs. set-alternation)

	Size		Habitat		Mammal	
Task sequence condition	RT	ERR	RT	ERR	RT	ERR
			Mostly no-alternation			
No-alternation	1301	.021	1361	.072	1352	.095
Set alternation	1256	.041	1323	.090	1338	.046
AT effects	45	-.020	38	-.018	14	.049
SE	33	.016	33	.018	35	.020
			Mostly set-alternation			
No-alternation	1396	.014	1217	.038	1389	.093
Set alternation	1304	.039	1405	.103	1409	.066
AT effects	92	-.025	-188	-.065	-20	.027
SE	44	.013	39	.024	39	.021

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Table A2

Significant effects in Experiment 1 as a function of task condition

Effect	*df*	*n*	*MSE*	*F*
Response time data				
Task X Task Sequence X Set Alternation Probability	2	94	39,188.68	6.04*
Habitat task condition				
Task Sequence X Set Alternation Probability	1	47	34,016.32	17.92**
Mostly set alternation				
Task sequence	1	47	35,699.28	23.60**
Size task condition				
Task sequence	1	47	18,602.20	6.08*
Error data				
Task	2	94	.010	13.46**
Task X Task Sequence	2	94	.009	9.09**
Habitat task condition				
Task sequence	1	47	.006	6.87*
Size task condition	1	47	.002	5.11*
Task sequence				
Mammal task condition				
Task sequence	1	47	.005	6.82*

* = p < .05
** = p < .001

All other task condition effects are non-significant.

Appendix B

Table B3

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 2A, as a function of Event 3 task (size vs. habitat vs. mammal), target identity (non- match vs. target match), and task sequence (no-alternation vs. set-alternation)

	Size		Habitat		Mammal	
Task sequence condition	RT	ERR	RT	ERR	RT	ERR
			Non-match			
No-alternation	1362	.032	1402	.080	1403	.071
Set alternation	1449	.054	1565	.126	1542	.073
AT effects	-87	-.022	-163	-.046	-139	-.002
SE	34	.022	50	.034	50	.020
			Match			
No-alternation	1248	.031	1371	.131	1346	.141
Set alternation	1339	.033	1497	.154	1436	.145
AT effects	-91	-.002	-126	-.023	-90	-.004
SE	37	.020	43	.029	41	.033

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Table B4

Significant effects in Experiment 2A as a function of task condition

Effect	*df*	*n*	*MSE*	*F*
Response time data				
Task	2	54	44,303.64	8.18**
Error data				
Task	2	54	.011	20.80**
Task X Target Match	2	54	.006	7.56**
Habitat task condition				
Target match	1	27	.003	6.43*
Mammal task condition	1	27	.004	16.97**
Target match				

***** = $p < .05$
****** = $p < .001$

All other task condition effects are non-significant.

Appendix C

Table C5

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 2B, as a function of Event 3 task (size vs. habitat vs. mammal), target identity (non- match vs. target match), and task sequence (no-alternation vs. set-alternation)

	Size		Habitat		Mammal	
Task sequence condition	RT	ERR	RT	ERR	RT	ERR
			Non-match			
No-alternation	1335	.034	1466	.085	1411	.056
Set alternation	1455	.021	1615	.111	1476	.097
AT effects	-120	.013	-149	-.026	-65	-.041
SE	32	.018	38	.018	43	.018
			Match			
No-alternation	1402	.047	1536	.114	1408	.075
Set alternation	1338	.012	1464	.017	1396	.014
AT effects	64	.035	72	.097	12	.061
SE	38	.019	39	.021	42	.023

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Table C6

Significant effects in Experiment 2B as a function of task condition

Effect	*df*	*n*	*MSE*	*F*
Response time data				
Task	2	62	50,888.98	12.66**
Error data				
Task	2	62	.006	14.85**
Task X Task Sequence X Target Match	2	62	.005	4.94*
Habitat task condition				
Task Sequence X Target Match	1	31	.005	23.45**
Task sequence				
Target match	1	31	.007	20.57**
Mammal task condition				
Task Sequence X Target Match	1	31	.008	10.00*
Task sequence	1	31	.005	5.54*

***** = *p* < .05
****** = *p* < .001

All other task condition effects are non-significant.

Appendix D

Table D7

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 3, as a function of Event 3 task (size vs. habitat vs. mammal), category (non- match vs. match), and task sequence (no-alternation vs. set-alternation)

	Size		Habitat		Mammal	
Task sequence condition	RT	ERR	RT	ERR	RT	ERR
			Non-match			
No-alternation	1264	.032	1332	.047	1343	.085
Set alternation	1382	.028	1431	.071	1434	.084
AT effects	-118	.004	-99	-.024	-91	.001
SE	25	.010	22	.017	31	.019
			Match			
No-alternation	1285	.030	1406	.059	1336	.090
Set alternation	1378	.037	1437	.096	1384	.119
AT effects	-93	-.007	-31	-.037	-48	-.029
SE	29	.011	27	.020	27	.021

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Table D8

Significant effects in Experiment 3 as a function of task condition

Effect	*df*	*n*	*MSE*	*F*
Response time data				
Task	2	60	56,721.63	6.15*
Error data				
Task	2	60	.017	14.83**

***** = *p* < .05
****** = *p* < .001

All other task condition effects are non-significant.

Appendix E

Table E9

Mean correct response times (RTs, in milliseconds) to Event 3 targets and proportion errors (ERR) in Experiment 4, as a function of Event 3 task (size vs. habitat), Event 2 task elaborateness (conceptual vs. perceptual), and task sequence (no-alternation vs. set-alternation)

	Size		Habitat	
Task sequence condition	RT	ERR	RT	ERR
		Conceptual		
No-alternation	885	.038	1040	.082
Set alternation	886	.013	1095	.083
SA Effects	-1	.025	-55	.001
SE	13	.010	24	.013
		Perceptual		
No-alternation	819	.014	946	.060
Set alternation	806	.022	922	.050
AT effects	13	-.008	24	.010
SE	9	.006	23	.009

AT effects = Alternating task effects were derived by calculating the difference in mean Event 3 response times for no-alternation vs. set-alternation trials, with negative values representing slower responding on set-alternation trials and positive values representing faster responding on set-alternation trials.

SE = within-participants standard error of the mean difference between no-alternation and set-alternation trials.

Table E10

Significant effects in Experiment 4 as a function of task condition

Effect	*df*	*n*	*MSE*	*F*
Response time data				
Task	1	70	12,026.21	138**
Habitat task condition				
Event 2 elaborateness	1	70	49,971.44	6.38*
Error data				
Task	1	70	.002	94.96**

***** = *p* < .05
****** = *p* < .001

All other task condition effects are non-significant.