Task-switching Costs Without Task-switching

Xiangqian Li

Submitted in fulfillment of the requirements for the Degree of Ph.D

School of Psychology
College of Science and Engineering
University of Glasgow

November 2017

Abstract

It has been suggested that task-switching costs can be eliminated if participants memorise all stimulus-response mappings thereby avoiding task-switching altogether (Dreisbach, Goschke & Haider, 2006, 2007; Dreisbach & Haider, 2008). This has been labelled the "Look-Up Table" (LUT) approach. It has also been suggested that the LUT approach could potentially explain why animals such as monkeys (Stoet & Snyder, 2003; Avdagic et al., 2013) and pigeons (Castro & Wasserman, 2016; Meier, Lea & McLaren 2016) were able to perform task-switching without showing any task-switching costs (Dreisbach, et al., 2006, 2007; Dreisbach & Haider, 2008; Forrest, Monsell & Mclaren, 2014). In a series of eight experiments the following two questions were addressed: (1) Why do some participants show significant task-switching costs even when they do not switch between tasks (e.g., Forrest, Monsell & Mclaren, 2014)? (2) Can the LUT approach explain the absence of task-switching costs? In an attempt to answer both questions different sources of human task-switching costs are investigated in eight behavioural experiments.

Chapter 1 provides an overview of different task-switching paradigms and accounts to explain task-switching costs. Chapter 2 summarises previous attempts to remove human task-switching costs. Evidence for the absence of task-switching costs in animals is also introduced. Following up on previous studies that suggested the LUT approach can explain the absence of task-switching costs, I conducted two task-switching experiments using visual tasks (i.e., colour task and shape task) with bivalent stimuli in an attempt to re-examine the conclusions of previous LUT studies (i.e., Dreisbach, et al., 2006, 2007; Dreisbach & Haider, 2008; Forrest, Monsell & Mclaren, 2014). The results in Chapter 2 indicate that human participants cannot always eliminate task-switching costs and do not always apply the LUT approach when the task-switching strategy is controlled.

Therefore, the experiments in Chapter 3 and 4 sought to ascertain the requirements for eliminating task-switching costs when using the LUT approach. The experiments in

Chapter 3 applied visual tasks where each task had a different stimulus-set. Experiments in Chapter 4 applied two classical mathematical tasks (i.e., big/small task, odd/even task) and used Chinese numbers as stimuli. The results of the experiments in Chapters 3 and 4 suggest that human participants must be able to give the correct answer without processing task-relevant features from the stimuli in order to eliminate task-switching costs. In the experiment of Chapter 5 the cue-stimulus-response mappings from Experiments 2.1 and 2.2 were rearranged so that switching between conventional tasks and rules became impossible. The results suggest that task-relevant features can trigger interferences thereby causing "task-switching costs" even when participants do not switch between tasks.

In Chapter 7, I compare a modified interference account, introduced in Chapter 5, with the compound retrieval account (e.g., Logan & Schneider, 2010) and associative learning account (Forrest et al., 2014; Meier et al., 2016) in order to explain why human participants show task-switching costs even when they do not switch between tasks. I conclude that the modified interference account provides an alternative explanation. It has been proposed that only humans are affected by strong and long-lasting interference from previous trials during task-switching. As a consequence, this interference may explain why human participants consistently show task-switching costs whereas monkeys and pigeons show no task-switching costs.

Acknowledgements

I would like to acknowledge the contributions of my supervisors Martin Lages and Gijsbert Stoet. I would like to thank Gijsbert for providing the initial conception for the present thesis. His enthusiasm towards cognitive psychology inspired me every day. I would like to thank Martin for his helpful inputs and suggestions along the way and for his dedication to support my research. I have learnt a lot from both my supervisors.

Declaration

I declare that, except where explicit reference is made to the contribution of others, that this dissertation is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution."

Printed Name: _________________________

Signature: _________________________

Table of Contents

Chapter 1: Introduction

Compared with the behaviour of other animals, human behaviour seems flexible and highly adaptable. Depending on the diverse environmental contexts in which we live, we can execute different tasks and switch between them. For example, people who are bilingual can switch between two languages effortlessly. Also, when using smartphones, we can easily switch between different applications such as text messaging and surfing the internet. Because it seems that humans can behave adaptively in almost all types of situations, the 17th century philosopher René Descartes proposed that we had a unique and universal problem-solving "device" embedded in our minds (Descartes, 1637, 2008), which he called the "rational soul". In essence, contemporary cognitive psychologists still accept Descartes' perspective, although the universal problem-solving device or rational soul has been relabelled the "executive control process" (O'Reilly, 2010). Even though humans have a high degree of cognitive flexibility, there is now nearly a century of studies which show the limitations of this flexibility, starting with Jersild's (1927) seminal work on task-switching effects. The present thesis intends to explore the sometimes elusive task-switching effect.

Modern-day task-switching experiments have consistently demonstrated that switching to a new task involves longer reaction times and higher error rates than repeating the previous task. These effects are called "task-switching costs" (Monsell, 2003; Vandierendonck et al., 2010; Kiesel et al., 2010; Grange & Houghton, 2014). Behavioural task-switching experiments provide an important means with which to study cognitive flexibility and control of goal-directed behaviour in humans (i.e., the task-set reconfiguration account; Roger & Monsell, 1995). Furthermore, previous studies have demonstrated how participants can be subject to passive interference from previous actions (i.e., the proactive interference account; Allport, Styles & Hsieh, 1994). Taking these studies a step further, it is worth examining to what extent task-switching costs also reflect task-cue encoding processes.

We know for sure that switching between tasks in terms of task rules triggers task-switching costs, the present thesis was motivated by two more elusive but closely related phenomena: (1) Why do some participants show significant task-switching costs even when they do not switch between tasks (Forrest, Monsell & Mclaren, 2014)? (2) Why are animals, such as monkeys (e.g., Stoet & Snyder, 2003; Avdagic, et al., 2013; but see also Caselli & Chelazzi, 2011) and pigeons (Meier, Lea & McLaren 2016; Castro & Wasserman, 2016), able to switch between tasks without showing any task-switching costs? To address both questions, I will investigate task-switching conditions in which human participants may be able to eliminate task-switching costs. I believe that by addressing these questions, I can provide novel interpretations of the underlying mechanisms that cause task-switching costs.

This thesis may be viewed as a comparative study. The major goal is to investigate under what conditions human participants can mirror pigeons' and monkeys' task-switching behaviours: performing task-switching experiments without indicating any task-switching costs. In Chapter 2 I give more details about these animal studies (i.e., Stoet & Snyder, 2003; Avdagic et al., 2013; Meier et al., 2016; Castro & Wasserman, 2016) and address why differences between animals and humans are important. In Chapter 6 I discuss the implications of the between-species difference in task-switching experiments. In short, human task-switching cost is an exquisite measurement of executive control: human participants with stronger executive control tend to have smaller task-switching costs (Monsell, 2003; Monsell & Mizon, 2006; Mayr & Kliegl, 2000; Rogers & Monsell, 1995; Meiran, 2000; Braver, 2012; Zinke, Einert, Pfennig & Kliegel, 2012; Kray et al., 2012). These animal studies are critical because animals’ surprising performance in task-switching experiments do not agree with the previously postulated implications of executive control. The present thesis seeks to explain monkeys and pigeons’ outstanding performance on switch trials without assuming they have better executive controls than human participants. Before pursuing these questions I will review some important aspects of modern task-switching studies, including the development of different task-switching paradigms and their theoretical frameworks.

1.1 From the mental shift paradigm to the modern task-switching paradigms

The study of 'task-switching' can be traced back to 1927. In his seminal article 'Mental Set and Shift', Jersild (1927) started his argument with an interesting observation from daily life: People respond differently to the same stimulus depending on its context. For example, he suggested that in a mathematics class, a two-digit number 10 may serve as a stimulus for mental addition or multiplication but on the football field, the same number might serve as signals of certain tactical actions; for example, to pass the ball to player number 10. Jersild suggested that the reason for this is that we have different mental-sets and each mental-set gives the same stimulus a different meaning. Consequently, in order to make appropriate responses, people sometimes need to shift between mental-sets. It was reasoned that this kind of shift requires additional time and energy. The task-set reconfiguration account is very similar to Jersild's argument; the reconfiguration account replaced the term "mental-set" by "task-set".

1.1.1 Separate Condition and Shift Condition

To examine the above assumption, Jersild (1927) developed a shift condition paradigm. Firstly, to set up a baseline, he asked the participants to perform two different tasks (Task A and Task B) separately (the separate condition). After that, he asked participants to perform two different tasks alternately (i.e., ABABABAB...; the shift condition). Finally, by comparing the performance difference between the shift condition and the separate condition, he was able to measure the shift cost. For example, in one experiment, he listed 100 two-digit numbers on a sheet of paper, using four columns, each containing 25 numbers. In the first column, the participants were required to sum the two digits together (additive task). In the second column, the participants were required to multiply the two digits together (multiplicative tasks). The first two columns represented the separate condition. In the third and the fourth columns, the participants were asked to perform the additive task and the multiplicative task alternately. These represented the shift condition. Participants were asked to write their answers down on paper. The amount of time they spent on each column was recorded by an experimenter (see Figure 1.1).

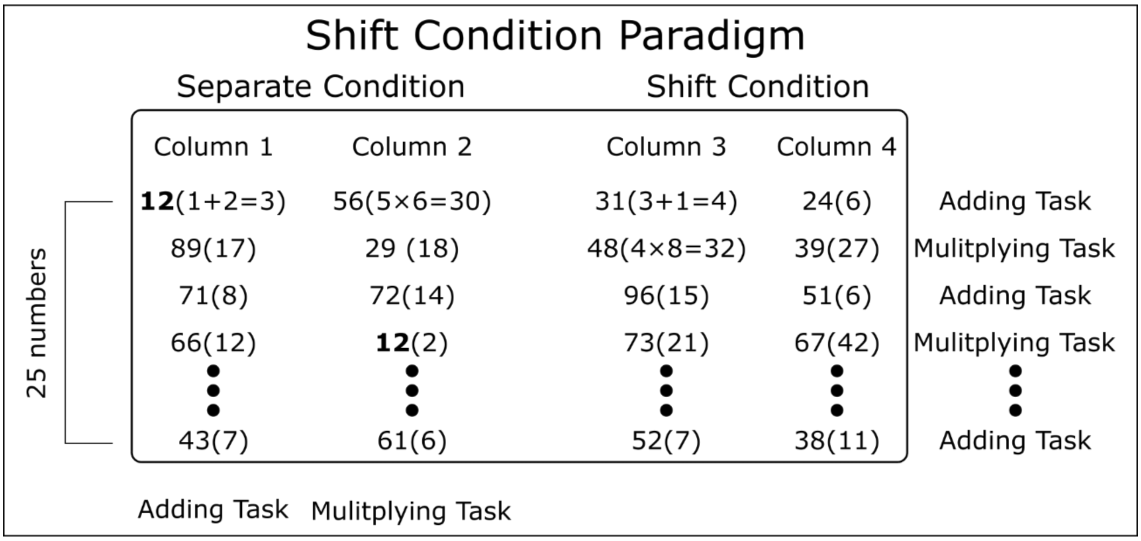

Figure 1.1. An example of the shift condition paradigm. Column 1 and Column 2 show separate conditions. Column 3 and Column 4 describe the shift condition. The numbers between parentheses are the answers that the participants were asked to provide. These numbers are for illustrative purposes only. Jersild (1927) did not include any information about the exact numbers he used.

In this particular experiment, Jersild (1927) reported that the participants spent more time on the shift condition than on the separate condition, which reflects the mental-set shifting process. In a series of experiments, he also tested participants using other types of tasks, including other arithmetic tasks, semantic tasks, and visual tasks. Sometimes, two tasks shared the same stimulus-set. For example, in Figure 1.1, the two-digit number 12 (in bold) can be found in both the additive task and the multiplicative task. However, sometimes both tasks did not share the same stimulus-set. For example, when shifting between a naming-the-opposite task (i.e., if ‘white’ then report ‘black’) and a calculation task, obviously one can either name the opposite of a number mor calculate an English word. As a result, each task must have a different stimulus-set. Jersild found that if both tasks share the same stimulus-set, then a shifting cost can be observed. Conversely, if both tasks do not share the same stimulus-set, there were no stable shifting costs.

Even 70 years later, task-switching studies report similar results: task-switching costs are larger when using bivalent stimuli compared to univalent stimuli (Allport, Styles, & Hsieh, 1994; Allport & Wylie, 2000; Wylie & Allport, 2000; Rogers & Monsell, 1995; Spector & Biederman, 1976). A bivalent stimulus can be used in both tasks whereas a univalent stimulus can be used in only one task.

Although Jersild's (1927) paradigm provided a methodology to explore how people switch or, to use his term, "shift" between tasks and although his results anticipated most of the results of modern task-switching studies, his mental shift paradigm has one major disadvantage: the mental-set shift process is confounded by memory load (Roger & Monsell, 1995). The problem is that in the separate condition (the baseline), participants only need to remember one mental-set as they are only performing one task. In contrast, in the shift condition, participants have to remember two mental-sets. Hence, we do not know if it is the mental-set shifting or the additional memory load that creates the delay in the shift condition.

In fact, from a modern perspective, Jersild's shifting cost (1927) is confounded with the so-called "mixing cost" (MarÍ-Beffa and Kirkham, 2014) as part of the task-switching costs. The measurement in the original paradigm recorded how mixing of two tasks affected response times compared with response times for a single task. Jersild (1927) did not directly measure the cost of switching between tasks, although task-switching certainly contributed to the shifting costs he observed.

1.1.2 The Alternating-runs Paradigm

To separate task-switching effects from memory load, Rogers and Monsell (1995) developed an alternating-runs task-switching paradigm. The idea was that participants would repeatedly perform over several trials in one task (a run) and then switch and perform over several trials in another task (the other run). For example, in their Experiments 1, 2, 3 and 4, the authors required participants to perform one task (say Task A) twice and then switch to a new task (say Task B). As a result, an AA-BB-AA-BB... trial-by-trial sequence was formed. In the even trials (i.e., Trial 2, Trial 4, Trial 6...), participants repeated the tasks of the previous trial. These were labelled “repeat trials”. In all trials with odd indices except for the first trial (i.e., Trial 3, Trial 5) participants had to switch to a new task. Those were labelled “switch trials”. The reason for excluding the first trial is that there was no previous trial to relate it; the first trial was neither a switch trial nor a repeat trial. The task-switching effect was measured by response time and error rate differences between switch and repeat trials.

In Rogers and Monsell’s (1995) experiments, a 2-by-2 table was always visible on the computer screen (see Figure 1.2). In Trial 1, the target stimulus showed up in the top-left position in the table. In every new trial, the target stimulus rotated in a clockwise direction to a new position within the table. In these experiments, a fixed AA-BB-AA-BB… sequence was applied. Therefore, the stimuli from the top two positions always belonged to Task A (letter task), and those from the bottom two positions always belonged to Task B (digits task). As a consequence, participants could identify the task in each trial based on the location of the stimulus. Furthermore, because of the fixed run-by-run sequence, participants could always predict the upcoming task in advance.

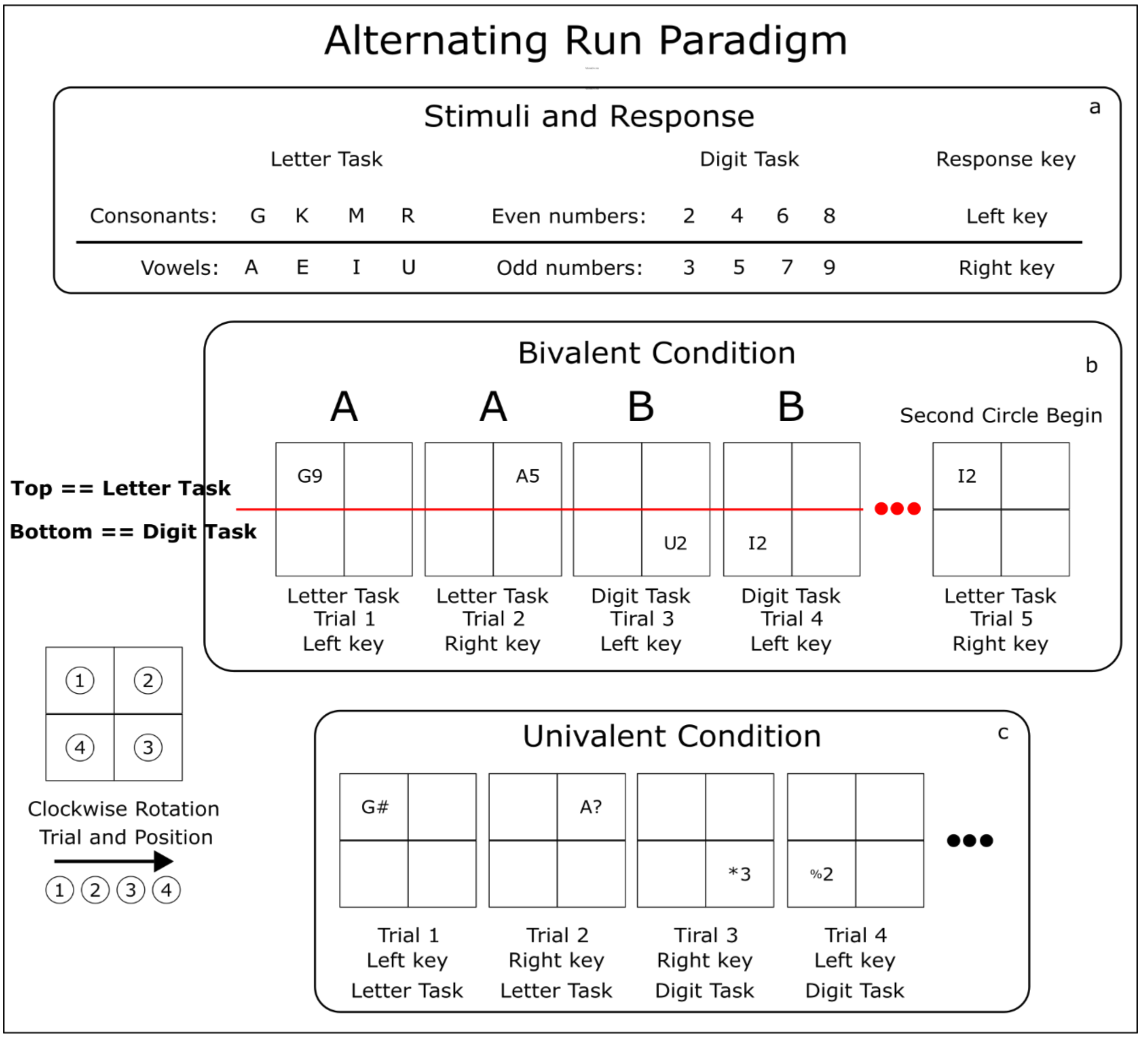

Figure 1.2. Illustration of Rogers and Monsell's paradigm (1995). (a) Stimuli of both letter and digit tasks and their response key. (b) The bivalent condition. In each trial, a letter and a digit appear randomly. Therefore, each stimulus can be used in both tasks. The position of the target stimuli rotates clockwise. Following the AABB sequence, the top row == letter task and the bottom row == digits task. (c) The univalent condition. In each trial, the task-relevant information (letter or number) appears with a neutral symbol. Each stimulus conveys information for only one of the tasks. Hence, each stimulus can only be used in one task.

Variations of the alternating-runs paradigm were introduced by changing the alternating sequence. For example, the alternating-runs paradigm can have the sequence AAAABBBB (e.g., Roger & Monsell, 1995, Experiment 6; Monsell, Sumner & Water, 2003). Nevertheless, the general principle of the alternating-runs paradigm remains the same. It presents both the switch condition and the repeat condition in a single block of trials. Moreover, with a fixed sequence, participants can always predict the upcoming tasks in an alternating-runs paradigm.

1.1.3 The Task-cueing Paradigm

Similarly to the alternating-run paradigm, the task-cueing paradigm presents both task-switch trials and task-repeat trials in the same experimental block. The difference is that the two tasks can alternate randomly. There is no fixed task sequence. In each trial, an explicit cue is presented to indicate the task. Participants do not know which task they will have to perform in the next trial until the task cue appears (e.g., Meiran, 1996; Sudevan & Taylor, 1987). Under the task-cueing paradigm, if *trial n* and *trial n - 1* have the same task cue, then *trial n* is a repeat trial. If the two trials have different task cues, then *trial n* is a switch trial. All the experiments in the present thesis applied the task-cueing paradigm. It is also possible to switch between three different tasks (e.g., Mayr & Keele, 2000). However, this type of paradigm is used in studies investigating n - 2 task repetition costs.

One advantage of the task-cueing paradigm is that it allows better control of task preparation. This is because the task-cueing paradigm can control two important intervals separately: firstly, the inter-trial interval (ITI) which denotes the interval between the response of *trial n - 1* and the cue of *trial n*; secondly, the cue-stimulus interval (CSI), which denotes the interval between the cue and the target stimuli of the trial n. In a task-cueing experiment, each task can appear randomly in a trial. Hence, we can rest assured that the participants cannot start to prepare for the task until the task cue is displayed. Therefore, the ITI reflects the time that has elapsed since the participant performed the task in *trial n - 1*, and the CSI reflects the time that has elapsed since the task preparation process for *trial n* was started (Meiran, 1996; Meiran, Chorev & Sapir, 2000).

In the alternating-runs paradigm, however, researchers can only measure the interval between the response in *trial n - 1* and the stimulus in *trial n* (response-stimulus interval or RSI) to study the task preparation process. The RSI is not as accurate as the CSI. In the alternating-runs paradigm, we do not know the precise moment at which the participants may start to prepare for the upcoming task, because participants can anticipate the task in the next trial. As a consequence, the RSI contains the task preparation process and the elapsed time since the response in the previous task. The RSI reflects a general preparation period whereas the CSI reflects a task-specific preparation period (Meiran et al., 2000). A task-cueing paradigm can have both CSI and RSI (see Figure 1.3).

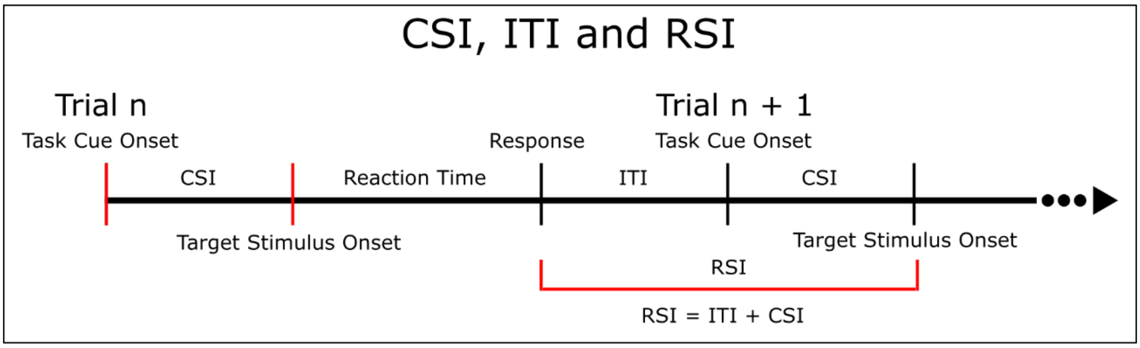

Figure 1.3. Timeline of CSI, ITI and the RSI in a task-cueing paradigm. RSI = ITI + CSI.

A problem with the task-cueing paradigm is that every time a task switch occurs, the cue will switch as well. Hence, the task-switching costs, which are caused by a different cognitive process than cue-switching (for a review, see Jost, De Baene, Koch, & Brass 2013) may be confounded by cue-switching costs. There are two possible solutions to this problem. Firstly, by assigning two task cues for each task, it is possible to separate task-switching costs from cue-switching costs (e.g., Logan & Bundesen, 2003; Stoet & Snyder, 2004). This dual-cue task-switching paradigm is easier to understand within the context of compound retrieval theory (for a review, see Logan & Schneider, 2010), whichwill be introduced in later sections. The other way to separate cue-switching costs from task-switching costs is to apply a voluntary task-switching paradigm (Arrington & Logan, 2004b; for a recent review, see Arrington, Reiman, & Weaver, 2014). This paradigm avoids using any task cues in the first place.

1.1.4 The Voluntary Task-switching Paradigm

In the voluntary task-switching paradigm, no cues are displayed during the experiment. The participants are free to choose which of the two tasks they want to perform. In some studies, they can also choose from four different tasks (e.g., Lien & Ruthruff, 2008). In order to determine in which task the participants actually performed in a trial, each task usually has a different response set. For example, according to some task rules, if a participant decides to perform Task A, they need to press the "D" or "F" key with their left hand. Alternatively, if the participant decides to perform Task B, the task rules prescribe that he or she needs to press the "J" or "K" key with their right hand. The voluntary task-switching paradigm consistently produced task-switching costs (Arrington et al., 2014).

Besides eliminating the task cue, the voluntary task-switching paradigm also provides a method to test task-switching behaviour with more ecological validity. After all, *being forced* to switch between two tasks is less likely to occur during daily life than choosing to do so. However, there is also an important limitation: the task selection process tends to be more complex in voluntary task-switching. It is the participant, rather than the experimenter, who controls which task to perform in a given trial (Arrington et al., 2014). Arguably, the task-switching process may be confounded by this additional task selection process.

1.1.5 Paradigm of Experiments

So far, I have reviewed the three major paradigms that are typically used in modern task-switching studies: the alternating-runs paradigm, the task-cueing paradigm, and the voluntary task-switching paradigm. Each paradigm has its own unique features. Consequently, task-switching costs measured in different paradigms may not be comparable. For example, recent studies have consistently found that the alternating-runs paradigm and the task-cueing paradigm produce different amounts of task-switching costs (e.g., Altmann, 2007a, 2013; Shahar & Meiran, 2014). In the present thesis only the task-cueing paradigm is used, for two reasons. Firstly, in Chapter 3, I want to examine how different preparations can affect task-switching costs. For this, the task-cueing paradigm provides better control of the preparation period. Secondly, I would like to compare my results with animal studies that applied task-cueing paradigms (e.g., Stoet & Snyder, 2003, 2004; Caselli & Chelazzi, 2011).

1.2 Other Effects of the Task-switching Paradigm

1.2.1 The Congruency Effect

There are some "by-products" of the task-switching paradigm. These are effects that occur in addition to the task-switching costs that can be observed in typical task-switching paradigms. Firstly, in many task-switching experiments, the same task stimulus-set is used for both tasks because the stimuli are bivalent. For example, if the two tasks were a colour task (Black, White) and a shape task (Circle, Hexagon), then the left key could be used for a white stimulus and a circle and the right key for a black stimulus and a hexagon. In fact, these are the tasks and the response keys I use in Experiments 2.1and 2.2 of Chapter 2. Therefore, stimuli such as the white circle and the black hexagon, correspond to the same key in both the colour and the shape task:they are congruent stimuli. In contrast, the white hexagon and the black circle require different response keys in different tasks:they are incongruent stimuli. In a typical task-switching paradigm, participants usually react faster to congruent stimuli than they do to incongruent stimuli (e.g., Monsell & Mizon, 2006; Rogers & Monsell, 1995; Meiran & Kessler, 2008; Kessler & Meiran, 2010; Schneider, 2015). This effect is called the "congruency effect". The delay in response times when participants respond to incongruent stimuli may reflect an additional competition that occurs between the two stimulus-response mappings for incongruent stimuli (Meiran & Kessler, 2008; Kessler & Meiran, 2010; Schneider, 2015).

1.2.2 The Mixing Cost

The mixing cost reflects some additional cost that occurs when mixing two tasks in the same block rather than performing in different blocks with a single task (for a recent review, see MarÍ-Beffa & Kirkham, 2014). In a typical study with mixing costs, participants perform in a series of single-task blocks (consisting of only one task) and a series of mixed blocks (consisting of two tasks). The mixing cost is measured by comparing the RT and error-rate between the trials from single blocks and the repeat trials from mixed blocks. The impaired performance in the repeat trials from the mixed blocks is the mixing cost. The mixed blocks can involve the alternating-runs procedure (e.g., Rogers & Monsell, 1995; Kray & Lindenberger, 2000) or the task-cueing procedure (e.g., Rubin & Meiran, 2005; Koch, Prinz & Allport, 2005). However, each procedure might produce slightly different mixing costs (Shahar & Merian, 2015). Finally, task-switching costs and the mixing costs reflect different aspects of executive control. The task-switching cost reflects a participant's ability to switch between different tasks. The mixing cost, on the other hand, reflects a participant's ability to maintain different tasks in the memory load (Braver, Reynolds & Donaldson, 2003; Minear & Shah, 2008; Wylie & Allport, 2000).

1.3 Explaining Task-switching Costs

In the following sections, I review four accounts that can explain task-switching costs: the task-set reconfiguration account, the proactive interference account, the task-set inhibition account and the compound retrieval account. A relatively new account, the associative learning account (e.g. Forrest, et al., 2014; Meier et al., 2016), will be discussed in Chapter 7. Vandierendonck et al. (2010) and Kiesel et al. (2010) provide comprehensive reviews of the task-set reconfiguration account and the task-interference account. Koch et al. (2010) provide reviews of the task-set inhibition account. Logan and Schneider (2010) reviewed the compound retrieval theory. In this chapter, I will focus on aspects of these three theories that are relevant to the results of the experiments in later chapters.

1.4 Task-set Reconfiguration Account

The idea of the task-set reconfiguration process is straightforward; it simply assumes that each task has a task-set. As a consequence, every time participants switch to a new task, the previous task-set becomes irrelevant. Participants need to reconfigure their task-set to a new task before they can give the correct answer. However, when participants repeat an old task, the old task-set is still useful, and a reconfiguration process is not necessary. The task-switching costs, according to this account, reflect the additional task-reconfiguration process that is only required in switch trials and is closely linked to executive control processes (Roger & Monsell, 1995; Monsell & Mizon, 2006; Monsell, 2003; Monsell & Mizon, 2006; Mayr & Kliegl, 2000; Rogers & Monsell, 1995; Meiran, 2000; Braver, 2012). One way to test the reconfiguration theory is to examine whether or not the preparation effect can counteract the task-switching cost. For example, if participants can configure the new task-set before the new trial starts, i.e., prepare in advance, then the task-switching costs should be reduced. If the preparation time is sufficiently long then the task-switching costs may be eliminated completely. This will be discussed in the following section.

1.4.1 Preparation Effect and the Residual Task-switching Costs

Previous studies have clearly demonstrated that with increasing preparation time task-switching costs tend to be reduced. This preparation effect can be observed for both the alternating-runs paradigm (e.g., Roger & Monsell, 1995; Koch, 2003) and the task-cueing paradigm (e.g., Verbruggen, Liefooghe, Vandierendonck & Demanet, 2007, Longman, Lavric, Munteanu, & Monsell, 2014; Poboka, Karayanidis & Heathcote, 2014; Forrest et al., 2014; Schneider, 2016, 2017). As mentioned before, the preparation period in an alternating-runs paradigm can be measured by the RSI, whereas in a task-cueing paradigm, the preparation period is measured by the CSI.

As stated before, the task-set reconfiguration account predicts that task-switching costs can be eliminated if the preparation period is sufficiently long. However, one problem has been identified. In the follow-up studies to Roger and Monsell's (1995) work it was consistently reported that once the RSI or CSI reached a certain length, a further increase in the preparation period did not reduce the task-switching costs. The remaining task-switching costs were called the "residual task-switching costs" (Allport, et al., 1994; De Jong, 2000; Meiran, 1996, 2000; Rogers & Monsell, 1995; Schneider, 2016, 2017; see Figure 1.4). In a number of studies, with CSIs exceeding 1000 ms, the residual task-switching costs remained significant (e.g., Longman et al., 2014; Meiran, 1996; Meiran et al., 2000).

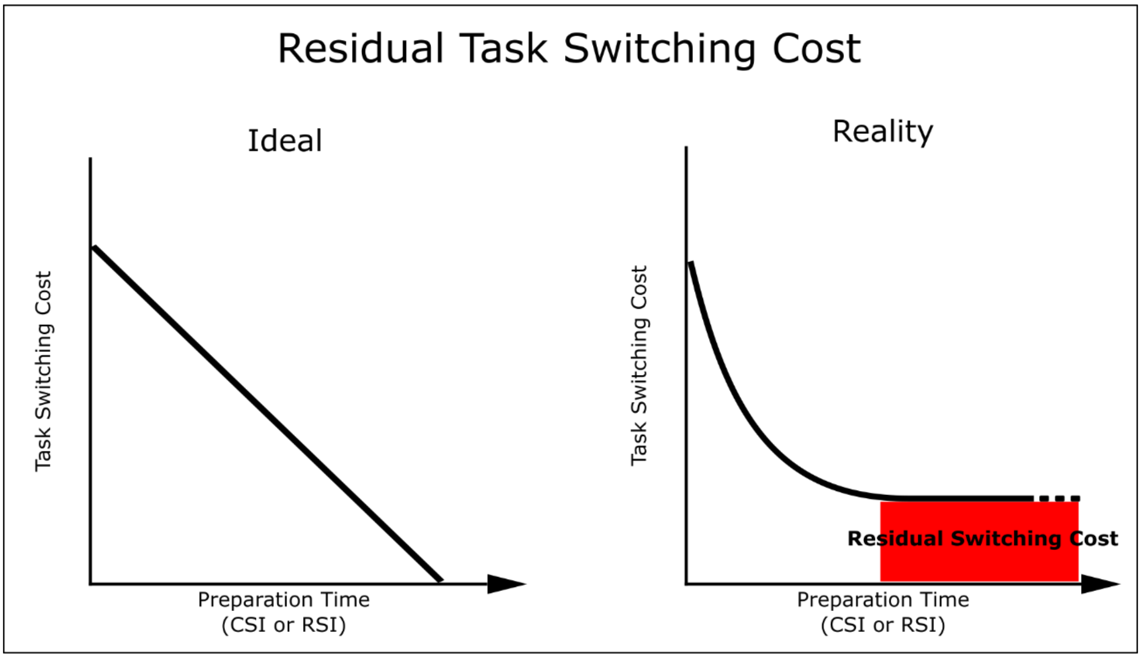

Figure 1.4. Schematic illustrations of residual task-switching costs as a function of preparation time as predicted by task-set reconfiguration and as observed.

A simple task-set reconfiguration process cannot explain residual task-switching costs because they cannot be reduced even after prolonged preparation periods. There may be hidden factors that prevent participants from completing preparation of the upcoming task in advance. To explain the residual task-switching costs, two accounts have been developed that are based on the task-set reconfiguration process.

Two-stage Account

The first account suggests that the task-set reconfiguration process may have two stages. The first stage can be prepared in advance as soon as participants are informed about the task they will have to perform next. Thus, initially, increasing the preparation period can reduce task-switching costs. Nevertheless, the second stage cannot be prepared until participants have received information about the target stimulus. Hence, any residual task-switching costs may reflect the second stage of task-set reconfiguration (Roger & Monsell, 1995; Mayr & Kliegl, 2000; Rubinstein, Meyer & Evans, 2001).

Failure-to-Engage Account

Alternatively, the failure-to-engage account suggests that, although it is *possible* to completely configure the new task-set before target stimulus onset, this is very hard to achieve. In some switch trials, participants can prepare for the task in advance, but in others, participants fail to do so. A longer preparation period may increase the possibility of successful task-set reconfiguration in advance. However, participants will fail to do so in a certain number of switch trials. The residual task-switching costs reflect these failures (De Jong, 2000; Poboka et al., 2014).

Removing Residual Task-switching Costs

The two-stage account always predicts the existence of residual task-switching cost. This is due to the fact that the second stage of the reconfiguration cannot start until the stimulus appears, making residual task-switching costs inevitable. Conversely, the failure-to-engage account allows participants to eliminate the residual task-switching costs completely if certain additional factors facilitate advance preparation and prevent failure-to-engage. As a consequence, the two-stage account would be falsified if a study task-switching costs had been removed completely. Despite this clear hypothesis, studies that tried to reduce or remove residual switch costs had little success (e.g., Nieuwenhuis & Monsell, 2002;

Schneider, 2016, 2017).

In only one studywas it claimed that entirely eliminating the residual task-switching costs is indeed possible (Verbruggen et al., 2007). To encourage participants to successfully prepare in advance for a task switch in all switch trials and to avoid failure-to-engage, Verbruggen et al. (2007) presented the cue at the onset of the CSI for only 96 ms before it disappeared in the remaining CSI. Interestingly, Verbruggen et al. (2007) found no residual task-switching costs at all. One explanation for this result is that participants were strongly motivated to fully prepare for a task switch during the short CSI because the cue was no longer available after the stimulus appeared. Therefore, a failure-to-engage was less likely to occur.

Despite this, a recent study could not replicate their results (Schneider, 2016). In fact, in Experiment 4 of Schneider (2016), contrary to the original finding, the residual task-switching costs actually became larger when the cue was only present briefly during the CSI than in the condition where the cue was fully present throughout a trial. At this point, the evidence is inconclusive and both the failure-to-engage account and the two-stage account provide possible explanations of task-switching costs.

1.4.2 Task-Set

The preparation effect provides strong evidence in support of the task-set reconfiguration account. In addition, including a few additional assumptions (e.g., failure-to-engage or two stages), the reconfiguration account can also explain residual task-switching costs. Nevertheless, one important question remains unanswered: what is a task-set according to the reconfiguration account? In other words, what exactly do participants reconfigure in switch trials?

The problem is that, for now, there is no unified definition of a task-set. Many early definitions remain unclear. For example, Rogers and Monsell (1995) suggested that it is the set of cognitive operations required to effectively perform a task. Elsewhere, Mayr and Keele (2000) defined a task-set as a set of internal control settings. One problem is that such definitions or labels tend to lead us into a dead-end. For instance, what exactly is a “set of internal control settings” in the context of task switching? Knowing that a task-set is a set of internal control settings does not necessarily provide any more insight than knowing that a task-set can be reconfigured. Further meaningful elaborations are necessary.

One approach to solving this problem is to assume that a task-set includes all the parameters or settings that are required for participants to achieve the task goal: to obtain the correct answer according to the task rules (Vandierendonck et al., 2010). Consistent with this view, within a computational model, Logan and Schneider (2010) define a task-set as a set of parameters that is necessary to programme a computational model in order to perform particular task-relevant actions. Hence, to clearly define a task-set, researchers may simply list all its parameters. For example, Schneider and Logan’s (2005) definition of a set of internal control settings is based on two parameters. One is a bias parameter that controls the strength of the tendency toward a response category and makes the computational model more likely to choose that category. The other is a criterion parameter that controls how strong the evidence should be before the model can decide to select a response key. This allows for a trade-off between speed and accuracy. Task-set reconfiguration can be defined as a change of these parameters. Instead of assuming a “metaphysical” task-set reconfiguration process, one can examine which parameters need to be adjusted in order to better support different models of the task-set reconfiguration process.

A potential problem is that in different studies different parameters were proposed. For example, the notion of the task-set can be expressed by different parameters in mathematical models (e.g., Merian, 2000; Meiran, Kessler & Adi-Japha, 2008; Altmann, 2008) and artificial neural network models (Gilbert & Shallice, 2002; Brown, Reynolds & Braver, 2007; Herd et al., 2014). In fact, some models even suggest that task-switching costs may not reflect any cognitive control process like task-set reconfiguration (Logan & Bundesen, 2003; Forrest et al., 2014).

However, the exact nature of the task-set is irrelevant for the purpose of this thesis. I share the basic assumptions of previous studies: the task-set consists of all the parameters participants need before they can achieve the task goal—deduce the correct response based on the task rules (Vandierendonck et al., 2010; Logan & Schneider, 2010). In addition, the task-set reconfiguration process is a goal-oriented activity that requires executive control (Monsell, 2003; Monsell & Mizon, 2006; Mayr & Kliegl, 2000; Rogers & Monsell, 1995; Meiran, 2000). However, these assumptions trigger the following question: what is a task? It is difficult to objectively define different tasks in a task-switching experiment.

1.4.3 Tasks and Strategies

A task is a highly subjective notion. For example, in the context of a task-switching paradigm, instead of assuming two conventional binary tasks (say, the digit task and the letter task), we can assume Task A is the congruent stimulus task and Task B is the incongruent stimulus task. It is also possible that participants perceive a cue and stimulus as a single compound stimulus and respond to a compound stimulus directly, without engaging in task-switching (e.g., Logan & Bundesen, 2003). Participants can, in fact, behave differently depending on the strategy they develop (Dreisbach, Goschke & Haider, 2006, 2007; Dreisbach & Haider, 2008, 2009).

For example, Dreisbach et al. (2006, 2007) conducted task-switching experiments with a group of participants who did not receive an explicit task-switching instruction (the experimental group), and a group of participants who did receive the explicit task-rule based instruction (the control group). Participants in the experimental group had to remember all the stimulus-response mappings. In the control group, the task-rule based instructions required participants to perform a Consonant/Vowel Task and an Animal/Non-Animal Task.

In the Consonant/Vowel Task, the participants had to decide if a word started with a consonant or a vowel. In the Animal/Non-Animal Task the participants had to decide if a word was an animal or not.

Based on an oral report after the experiment, Dreisbach and colleagues (2006, 2007) reported that the participants from the experimental group had memorised all the stimulus-response mappings directly. In the following I call this strategy the “Look-up table” (LUT) approach. In a way participants had to perform only one task in each trial: remember the correct mapping and press the corresponding key accordingly. The participants from the control group, however, applied the conventional task-switching strategy. They had to perform two tasks: the Consonant/Vowel Task and the Animal/Non-Animal Task. The results showed that the participants from the experimental group had no task-switching costs, while the participants from the control group had significant task-switching costs.

In addition, Dreisbach and Haider (2008) introduced three different instructions. They used eight words in their experiment: *bug, polecat, leg, pendular (pendulum), sofa, Ulm (a city), anchor, and ice*. Participants could apply different strategies based on the instruction they received. Firstly, participants could apply the task-switching strategy——switching between the Animal/Non-Animal Task (animal == *left* key; non-animal == *right* key) and the Consonant/Vowel Task (consonant == *left* key; vowel == *right* key). Secondly, participants could memorise all the stimulus-response mappings and apply the LUT approach. The task then was to remember the stimulus-response mapping and to press the corresponding key. Thirdly, participants could also apply a Moving/Non-Moving strategy: if the word referred to something that could move (e.g., *bug, polecat, leg, or pendular*), pressing the *left* key; If the word referred to something that could not move (e.g., *sofa, Ulm, anchor, or ice*), pressing the *right* key. The task then was the Moving/Non-Moving Task (see Figure 1.5).

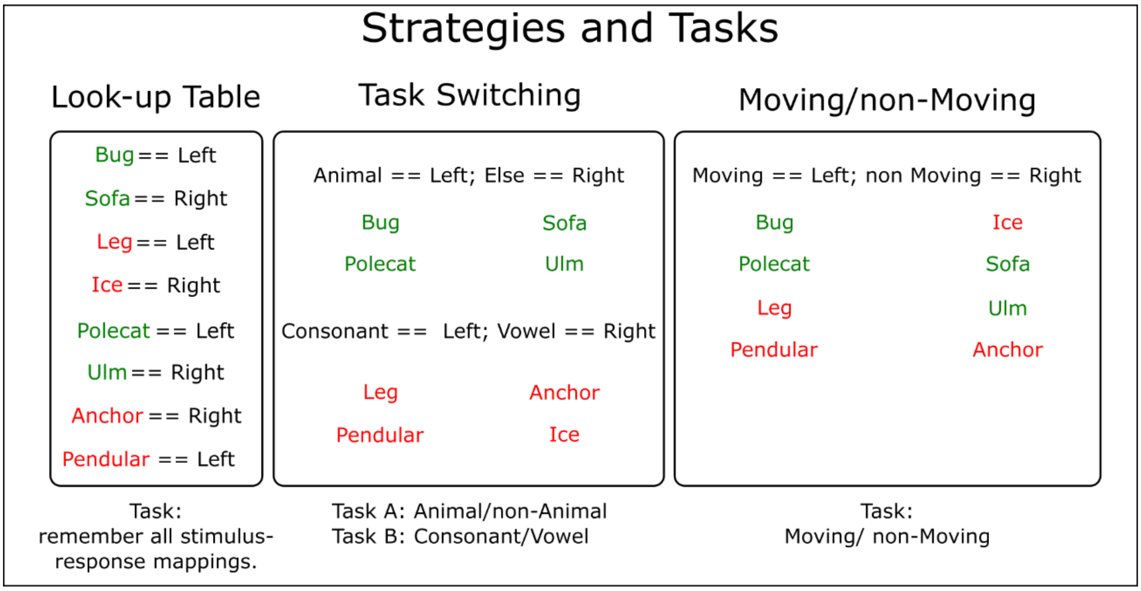

Figure 1.5. Illustration of three different strategies as suggested by Dreisbach & Haider (2008). Participants' strategies depend on the task they applied. For the LUT strategy, the task is to remember the stimulus-response mapping. For the task-switching strategy, two tasks are Consonant/Vowel and Animal/Non-Animal tasks. The ink colour of each word is the task cue. For Moving/non-Moving strategy, there is a Moving/non-Moving task. For the Moving/non-Moving strategy and Look-up Table strategy, the colour was irrelevant.

Dreisbach and Haider (2008) reported that participants who applied different strategies also had different response patterns. They only found task-switching costs among participants who had received the instruction to use the task-switching strategy. Moreover, those who received the instruction to use the LUT approach, showed an interaction between colour-switching and response-switching. More specifically, it was shown that in the response-repeat condition (*trial n - 1* and *trial n* have the same response), responses in colour repeat trials (*trial n - 1* and *trial n* have the same ink colour) were quicker than in colour switch trials (*trial n - 1* and *trial n* have different ink colours). However, in the response-switch condition (*trial n - 1* and *trial n* have different responses), colour responses in the repeat trials were slower than in colour switch trials. Participants who received the instruction to use the Moving/non-Moving strategy indicated neither task-switching costs nor any interaction between stimulus colour and response repetition. When participants applied the Moving/non-Moving strategy or LUT approach, the ink colour was irrelevant.

Participants can therefore give the correct response irrespective of the ink colour. One explanation is that the Moving/non-Moving strategy may protect against irrelevant information (i.e., the ink colour) but the LUT approach cannot. As a consequence, when participants apply the Moving/non-Moving strategy, the interaction between colour-switching effect and response-switching effect disappeared (for more details, see Dreisbach, 2012).

Dreisbach and colleagues' studies (Dreisbach et al., 2006, 2007; Dreisbach & Haider, 2008) showed that, during the task-switching experiments, all participants shared a common goal: to find the correct response in every trial. Nevertheless, there was no fixed strategy to achieve this goal——participants can apply different strategies. Furthermore, different strategies may influence and alter the task and task-sets. I conclude that the notion of a task is not just determined by the paradigm in a task-switching experiment, but is also the result of the strategies participants use to come up with correct responses.

In Dreisbach and colleagues' study (Dreisbach et al., 2006, 2007; Dreisbach & Haider, 2008, 2009), the participants' strategies were induced by the instructions they received. Thus, Schneider and Logan (2014) suggested that the tasks should reflect the instructions provided by the experimenter. These are necessary for the participants to give correct responses. However, as we are about to see in the experiments of Chapter 2 and 4, participants are able to develop and apply novel strategies that are difficult to anticipate and are only revealed when the experimenter collects self-reports form the participants after the experiment. Ultimately, the tasks are the result of the participants' personal understanding of a paradigm and experiment. In the present thesis, I assume that tasks reflect the idiosyncratic strategy a participant applies or develops in order to give correct responses (for more details on the ambiguity of task and task-set see Schneider and Logan, 2014).

1.4.4 Task and Task-set Reconfiguration

Tasks are formed by the participants' subjective understanding of a specific experimental paradigm. In a task-switching experiment, if a participant does not know anything about a task, then performing this task is impossible. For example, in Dreisbach et al. (2006, 2007), participants who applied the LUT approach did not perform the Consonant/Vowel Task and Animal/Non-Animal Task. They did not switch between tasks and therefore did not show task-switching costs. However, this was not because they did not know how to differentiate between consonants and vowels or how to distinguish between animals and non-animals. Instead, they were ignorant about the task rules and genuinely did not realise that these tasks existed. In fact, Dreisbach et al. (2007) demonstrated that, during the experiment, task-switching costs re-emerged as soon as the participants received the task-switching instruction. They started to use the Consonant/Vowel and Animal/Non-Animal Task after they became aware of these tasks.

If a participant cannot perform a task (say, Task A) or a strategy, it is possible that he or she can still figure out the correct response in a trial by employing an alternative task (e.g., remember all the stimulus-response mapping). However, the participant cannot give the correct response based on the rules of Task A. Hence, configuring the task-set for Task A is not possible. Again, based on the idiosyncratic strategy a participant uses, he or she may configure the task-set for other tasks, but not for Task A. Therefore, without knowing Task A, participants can neither perform Task A nor configure the task-set for Task A. This is the assumption many researchers have put forward in previous studies (Dreisbach et al., 2006, 2007; Dreisbach & Haider, 2008, 2009). Forrest et al. (2014) took a slightly different approach. They argued that, when feedback to responses is provided without an instruction based on tasks and task rules, participants must learn to perform the experiment through associative learning. Consequently, any higher-level cognitive control process like task-set reconfigurations is no longer necessary. Either way, it was suggested in these studies that without knowing the tasks, participants cannot execute the appropriate task-set reconfiguration process.

1.4.5 Previous Trials

The task-set reconfiguration account essentially proposes a cognitive control process that allows participants to actively switch between tasks. Such a process creates an additional reconfiguration step in every switch trial, causing task-switching costs. The task interference account and task-set inhibition account provide a different explanation. These accounts suggest that the task-switching costs reflect an effect due to processing in the previous trial. This effect can be an interference or an inhibition. In the following sections, the proactive interference account and the task-set inhibition account will be introduced.

1.5 Proactive Interference

The proactive interference account was originally proposed by Allport and colleagues (1994). In a series of experiments, they found that participants consistently showed task-switching costs. Additionally, they reported that, even with an RSI of 1100 ms, switching costs remained significant. In other words, they found what Roger and Monsell (1995) referred to as residual task-switching costs. Allport (1994) also found an interesting asymmetry in switching-costs. It was more difficult for participants to switch from a hard to a relatively easy task. Surprisingly, when switching from an easy to a relatively hard task, participants actually had smaller task-switching costs. This has been labelled “asymmetric switching costs”.

1.5.1 Asymmetric Switching Costs

In Experiment 5 conducted by Allport et al. (1994), participants had to perform task-switching in a classical Stroop Effect colour-word paradigm: switching between a word-colour task and an ink-colour task. As is well known, the word-colour task is easier than the ink-colour task (for a review, see Macleod, 1991). Allport et al. (1994) found that, when switching from the ink-colour task to the word-colour task, the task-switching costs were relatively large. Conversely, the task-switching costs were small when switching from the word-colour task to the ink-colour task. Similar effects have been observed in many related studies (Monsell, Yeung & Azuma, 2000; Waszak et al., 2003; Schneider & Anderson, 2010; Arbuthnott, 2008; Wong & Leboe, 2009; Barutchu et al, 2013; but also see Yeung & Monsell, 2003).

Suppose that the current trial (*trial n*) is a switch trial. If the task-switching cost truly reflects a task-set reconfiguration process, then the magnitude of the switching costs would be mainly determined by the task-set in the current trial, because it is the task-set of the current trial that needs to be reconfigured. Therefore, an intuitive deduction would be that the more difficult the task is in the current trial, the bigger the task-switching cost. Interestingly, the asymmetric switching costs reported by Allport et al. (1994) suggested the opposite. Their results indicate that switching from a hard task to a relatively easy task causes stronger task-switching effects than the other way round. It appears that the task-set from the previous trial (*trial n - 1*) determines the largeness the task-switching costs. Moreover, Allport et al (1994) observed that task-switching costs remained significant even after a very long preparation period (RSI). They speculated whether the task-set reconfiguration process truly causes task-switching costs.

Task-set Inertia

In order to explain task-switching costs, Allport et al. (1994) suggested that the task-switching cost is triggered by the interference of the previous trial. They termed this interference “task-set inertia”. Allport and Wylie (1999) further proposed that this interference is due to a continued priming of the previous task and a suppression of the current task. According to this explanation, the task-set inertia is small when *trial n - 1* and *trial n* share the same task (*trial n* is a repeat trial) because the previous task-set is still relevant. Conversely, the task-set inertia is strong for two possible reasons when *trial n - 1* and *trial n* have different tasks (*trial n* is a switch trial). Firstly, the task-set of *trial n* is suppressed as it is irrelevant for *trial n* - 1 (negative priming) and needs to be reactivated. Secondly, it is also possible that the task-set from *trial n - 1* remained active, but is irrelevant for *trial n*. Therefore, it competes with the currently relevant task-set (competitor priming).

Task-set inertia can also explain the asymmetric switching costs. The harder a task is in *trial n - 1*, the larger the task-switching cost in trial *n* due to a stronger task-set inertia in *trial n - 1*. In contrast, if the task in *trial n - 1* is simple, the task-set inertia will be relatively small creating small task-switching costs. Furthermore, task-set inertia also provides an explanation for residual task-switching costs: since task-set inertia is an effect of passive priming that only fades away gradually, it cannot be eliminated by an 1100 ms RSI. The residual task-switching costs may simply reflect a long-lasting interference. Allport and colleagues demonstrated that task-set inertia could carry over and continually impact participants' behaviour across several experimental blocks (Allport & Wylie, 1999; Wylie & Allport, 2000).

1.5.2 What is Interference?

Early studies held the view that proactive interference (i.e., task-set inertia) can be derived directly from the stimulus-response mappings of the previous trial (Allport et al., 1994; Allport & Wylie, 2000). Later studies suggested that the interference might come from stimulus-task-set associations (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). In particular, they claimed that interference was formed between all the encoded components of the previous action-event, not only between the immediate stimulus and its response, but also in relation to the goal of the action, the task, and any task-specific processing operations. For example, it was found that interference can even occur when the stimulus is congruent (Waszak et al., 2003; Koch & Allport, 2006). Waszak and Hommel (2007) further reported that congruent and incongruent stimuli show the same interference effects. Congruent stimuli always have by definition the same stimulus-response mapping in both tasks and only incongruent stimulus can be exposed to direct competition from stimulus-response mappings. As a consequence, stimulus-response mappings alone cannot completely explain interference; some interference must be attributed to the task context, such as task-sets.

1.5.3 Decay of Task-sets

By manipulating the ITI, a few studies have found evidence in support of proactive interference. In a task-cueing paradigm, Meiran et al. (2000) demonstrated that, as the ITI increased, the task-switching costs decreased. Since the CSI of 117 ms was fixed and short, they suggested that the task-preparation effect could not explain their results. They concluded that reduced task-switching costs reflected a decay of the task-set of the previous trial. This conclusion is consistent with predictions from the proactive interference account. Allport et al. (1994) suggested that interference during task-switching is caused by the previous performance in a different task, and thatthis interference decays over time. A number of studies have replicated the decay of task-sets (e.g., Meiran, Levine, et al. 2000; Koch & Allport, 2006; but also see Horoufchin, Philipp & Koch, 2011).

1.5.4 Limitations

The proactive interference account has certain limitations. Firstly, the asymmetric switching cost is subject to controversy (e.g., Monsell et al., 2000; Yeung & Monsell, 2003; Schneider & Anderson, 2010; Barutchu et al., 2013). In different studies contradicting observations were made on the phenomenon of asymmetric switching costs and different explanations were provided. For example, Yeung and Monsell (2003) demonstrated that asymmetries in switching costs can be decreased or even reversed by reducing the level of interference between tasks. In addition, Schneider and Anderson (2010) have proposed a 'sequential difficulty' account. This account suggests that sequential changes in task difficulty can reduce executive control and working memory resources. As a result, fewer resources are available to carry out an easier task that follows a difficult task, causing a longer recovery time and delays in response. In other words, it was suggested in this study that changes in task difficulty alone can trigger asymmetrical “switching” costs and that asymmetric switching costs might not be a product of task-switching. A recent study has also demonstrated that even when both tasks are equivalent in difficulty, asymmetric switching costs can be created by manipulating task-related symbols, e.g., assigning the same task cue to the opposite task (Barutchu et al., 2013). In these studies it was suggested that the asymmetric cost effect and task-switching effect are independent and can be manipulated separately.

Secondly, the evidence for task-set decay is also inconclusive with inconsistent empirical data. In several studies researchers were unable to replicate the result that long ITIs reduce task-switching costs (Altmann, 2005; Horoufchin et al., 2011). Furthermore, in recent studies it was proposed that it was not the absolute time of the ITI which caused a decrease of task-switching costs. Instead, it was the ratio of the current ITI (interval between *trial n - 1* and *trial n*) previous ITI (interval between *trial n - 2* and *trial n - 1*) that seemed to cause a decrease in task-switching costs. Particularly, task-switching costs were only reduced for long current ITIs following short previous ITIs. However, when the previous ITI was also long, task-switching costs were not reduced (e.g., Horoufchin et al., 2011; Grange, 2016). These results can be explained by an account of temporal distinctiveness of task-set retrieval (c.f. Horoufchin et al., 2011).

1.6 Task-set Inhibition

Although the proactive interference account can explain the effect of asymmetric task-switching costs and the effect of decay of task-sets better than the task reconfiguration account, it nevertheless still has certain limitations. Studies could not consistently reproduce the effects. Moreover, there is room for alternative explanations. Also focusing on the impact of previous trials, the task-set inhibition account provides an alternative explanation of the task-switching effect.

1.6.1 Switching Between Three Tasks

Mayr and Keele (2000) created a paradigm that required participants to switch between three different tasks. To achieve this, they asked participants in every trial to identify an "odd-one-out" object from a group of four objects. In each trial, there were three different ways (or three dimensions) in which one object could be different from the other three objects: it could have a different colour, a different orientation, or a different motion (e.g., one object was moved to the right a little while the others remained stationary). Therefore, the three tasks were a colour task, an orientation task, and a motion task. There was an explicit cue in every trial before the stimuli appeared. The researchers also manipulatedprobability to make sure *trial n* and *trial n - 1* never shared the same task. There were no conventional repeat trials. Instead, they compared a current trial (*trial n*) with *trial n - 2*. If *trial n* and *trial n - 2* shared the same task, *trial n* was an n - 2 task repeat trial. If *trial n* and *trial n - 2* had different tasks, *trial n* was an n - 2 switch trial (see Figure 1.6).

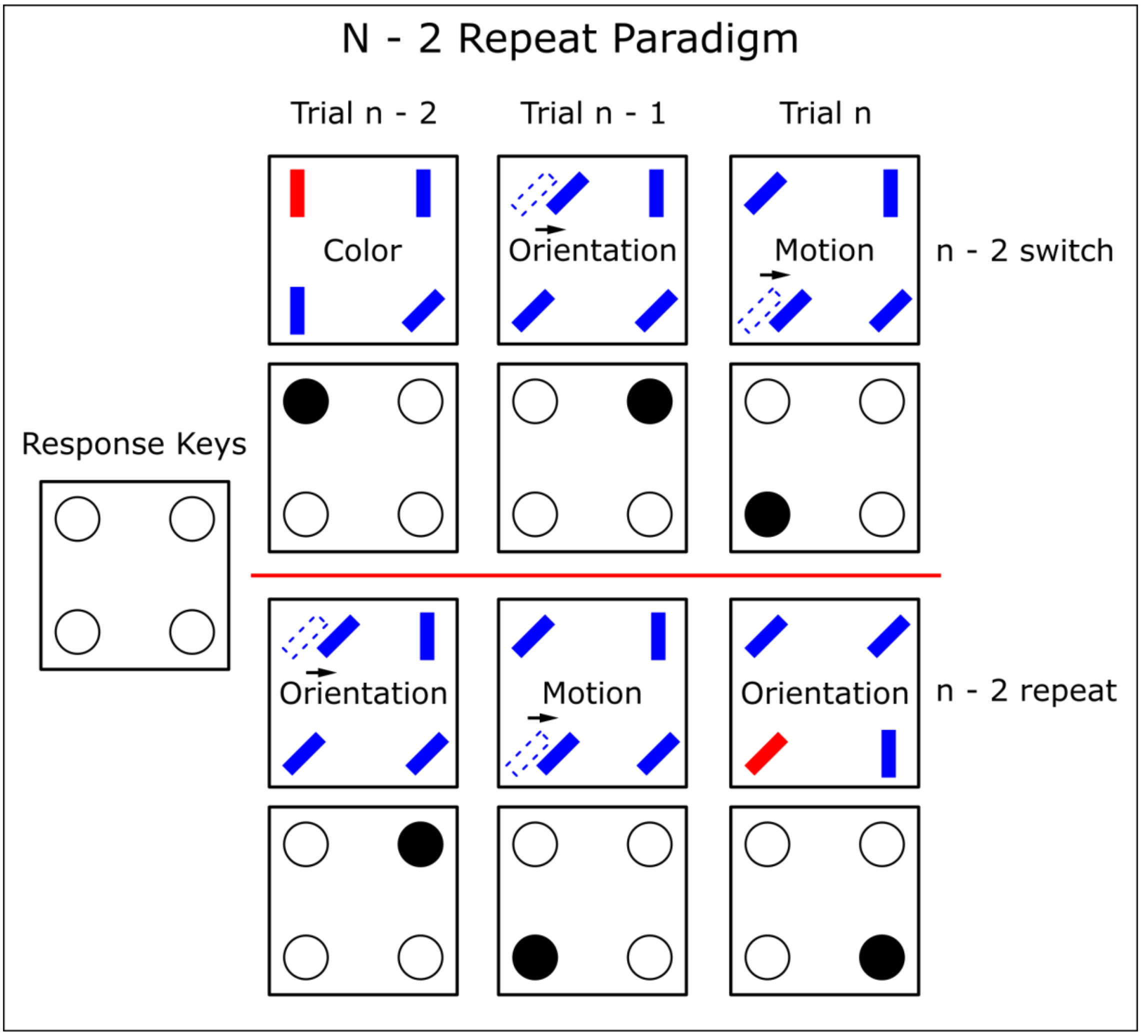

Figure 1.6. Illustration of Mayr and Keele's (2000) *n - 2* task-switching paradigm that required participants to switch between three tasks. There is a stimulus in each of the four corners of the square. The four response keys correspond to four locations (e.g., the top right key should be pressed if the top right stimulus is the odd-one-out object).

Surprisingly, Mayr and Keele (2000) observed that the participants' performance was worse in the *n - 2* repeat trial than in the *n - 2* switch trial. This effect is called the "*n - 2* repetition cost". It was suggested that this cost might reflect an inhibitory control process (Gade, Schuch, Druey, & Koch, 2014; Koch et al., 2010). When Task A was performed in trial *n - 2*, in order to perform Task B in trial *n - 1*, Task-set A had to be inhibited. Thus, if participants had to perform Task A in *trial n* again (*n - 2* repeat), they had to make an extra effort to overcome inhibition. In contrast, if participants had to perform Task C in trial *n* (*n - 2* switch), participants could respond quickly because Task-set C had not been inhibited recently. The *n - 2* repetition cost provides strong evidence in support of the idea that the task from the previous trial can affect the response times of the current trial.

Importantly, the task-set reconfiguration account cannot explain the *n - 2* repetition cost because it would predict the opposite pattern. Since Task-set A has been reconfigured recently (in trial *n - 2*) and Task-set C has not, performing Task A again in trial *n* (i.e., *n - 2* repeat) should be relatively effortless compared with performing Task C in trial *n* (i.e., *n - 2* switch).

1.6.2 Alternative Explanations of the *n – 2* Repetition Costs

Cue-encoding Process

As explained in the previous section, the *n - 2* repetition cost may reflect a task-set inhibition process. However, there are some alternative explanations as well. Firstly, in a typical *n - 2* task-switching paradigm, the tasks are always indicated by specific task cues. Therefore, the *n - 2* task repetition is confounded by an *n - 2* cue repetition. It is possible that the so called “task inhibition” is, created by a task-cue inhibition. In other words, in *n - 2* repeat trials, participants need to make an extra effort to encode the task-cue, because it has been inhibited previously. Therefore, it is the cue-re-encoding process, rather than the task-set inhibition that may produce the *n - 2* repetition cost.

The idea that task-cue encoding is responsible for most of the effects related to task-switching will be revisited in later sections when I discuss Logan and colleagues' compound retrieval theory (for a review, see Logan & Schneider, 2010). However, based on the results of previous *n - 2* repetition studies (e.g., Mayr and Kliegl, 2003; Altmann, 2007b; Gade & Koch, 2008; Lien & Ruthruff, 2008), researchers concluded that the cue-encoding process is not a better explanation for the *n - 2* repetition cost. The simplest way to separate the cue-encoding process from the task-set inhibition is to assign two different cues to each task (e.g., Mayr and Kliegl, 2003; Altmann, 2007b; Gade & Koch, 2008). As a consequence, the cue sometimes changes but the task remains the same (i.e., the *n - 2* cue-switch trials).

To measure the potential cue-encoding effect, researchers compared the difference between *n - 2* cue-repeat trials with *n - 2* cue-switch trials. To measure the task-set inhibition without confounding potential cue-encoding, researchers compared the *n - 2* cue-switch trials with the *n - 2* task-switching trials. By doing so, previous studies consistently reported a substantial effect of task-set inhibition but no clear effect of cue-encoding (Mayr and Kliegl, 2003; Altmann, 2007b; Gade & Koch, 2008). Conclusive evidence against a cue-encoding account was provided by Lien and Ruthruff (2008). They tested the *n - 2* repetition costs in a voluntary task-switching paradigm. Since no cues appeared in this voluntary task-switching paradigm, there was no cue-encoding process. However, they still found *n - 2* repetition costs. A cue-encoding process can therefore not explain the *n - 2* repetition cost.

Episodic Retrieval Account

Mayr (2002) suggested that an episodic retrieval account (Neill, 1997) can also explain the *n - 2* repetition cost. This account suggests that when participants perform Task A in trial *n - 2*, an episodic trace of trial parameters (i.e., the cue, stimulus features, and the response key) is stored in memory. When the same task rule needs to be applied again in trial *n*, participants retrieve the most recent episodic trace.

In a typical *n - 2* repetition experiment (Mayr & Keele, 2000), stimuli varied randomly in three dimensions and the response key had four levels. As a result, the parameters of a trial would normally differ between both trial *n - 2* and trial *n*, even when both trials require the same task. If the parameters of the current trial differ from the retrieved information (e.g., a different response is required), a cost occurs because of the mismatch between the memorised parameters from trial *n - 2* and the current parameters for trial *n*. According to this account, the *n - 2* task repetition cost is the results of mismatching rather than inhibition.

One way to directly test this account is to observe whether or not the *n - 2* repetition cost still occurs once the parameters for trial *n - 2* and *trial n* are precisely matched (i.e., the same cue, the same stimulus, and the same response). Mayr (2002) found that the difference between the matched condition and the unmatched condition was not statistically significant. However, a recent replication study suggested that after controlling the mismatch effect, the *n - 2* repetition cost was significantly reduced (Grange, Kowalczyk & O'Loughlin, 2017). Thus, it is possible that both the task-set inhibition and mismatching of parameters contributed to *n - 2* repetition costs.

1.6.3 Task-switching Costs and *n - 2* Repetition Costs

There is sufficient evidence to support the idea that task-set inhibition occurs during task-switching. However, there is also evidence that the *n - 2* repetition cost and the task-switching cost may not reflect the same cognitive process. For example, Arbuthnott and Woodward (2002) reported that the strength of cue-task association can only affect the task-switching cost but not the *n - 2* repetition cost. Secondly, the preparation effect can largely impact the task-switching cost, but it had no significant impact on the *n - 2* repetition cost in a number of studies (Mayr & Keele, 2000; Schuch & Koch, 2003; Gade & Koch, 2008). A preparation effect is only observed when the participants can prepare both the response and the task in advance (e.g., Koch et al., 2004) or when the paradigm requires participants to switch between three different languages (Philipp, Gade & Koch, 2007).

1.7 Task-switching costs and Cue-switching costs

In a typical task-switching study, researchers assume that participants follow the instructions, understand that there are two tasks and two task-sets, and realise that they need to switch between them. In other words, participants are supposed to apply a task-switching strategy. The task-set reconfiguration account, the proactive interference accounts and the inhibition account were developed based on these assumptions. However, there is also a theoretical account that suggests that even when participants receive clear task-switching instructions, they may not follow the task-switching instruction and switch between tasks. Instead, they may form a cue-stimulus compound and retrieve the corresponding response of each compound directly from memory. This is named the "compound retrieval account" (Logan & Bundesen, 2003; Logan, Schneider & Bundesen, 2007; Arrington & Logan, 2004a; Schneider & Logan, 2005, 2007; Logan & Schneider, 2006a, b; Logan & Schneider 2010). This account suggests that, in a task-cueing paradigm, the task-switching cost is caused by the cue-encoding process rather than by the task-set reconfiguration.

1.7.1 Dual-cue Paradigm and Cue-switching Costs

Logan and Bundesen (2003; Experiments 1 and 2) manipulated the length of the cue-stimulus interval (0, 100, 200, 300, 400, 500, 600, 700, 800, and 900 ms) and observed how the RTs in repeat trials and the RTs in switch trials vary with CSI. They compared two models: firstly, an endogenous control model that includes a task-set reconfiguration process; secondly, a cue encoding benefit model that includes no task-set reconfiguration process. The second model appeared to provide the best account of the data suggesting that the task-switching costs do not reflect any task-set reconfiguration process at all.

Moreover, Logan and Bundesen (2003) also realised that a typical task-cueing paradigm only assigns one cue for each task. This means that every time a task switches, the cue will switch as well. The cue-switching and the task-switching might confound each other. The researchers therefore questioned whether task-set reconfiguration as many previous studies have suggested (e.g., Roger & Monsell, 1995; Mayr & Kliegl, 2000; Meiran, 1996) or cue-switching produces the task-switching cost.

Logan and Bundesen (2003) tested this hypothesis in their Experiments 3, 4 and 5. They assigned two different cues to each task. Thus, cues were mapped at a ratio of 2:1 to tasks. In the following I call this paradigm the "dual-cue paradigm". In the Magnitude Task (decide if a number is bigger than five or smaller than five), there were two cues: a name cue of thetext *Magnitude*; and a mapping cue of the text *high – low*. For the parity task, the two cues were the text *Parity* (name cue) and the text *odd – even* (the mapping cue). By assigning these cues, they could separate task-switching from cue-switching (see Figure 1.7). Their results indicated strong cue-switching costs, while the task-switching costs were very small. In addition to this, their results were consistently fit best by a model that assumes "switching costs" were the result of cue-switching and not the product of an endogenous control process.

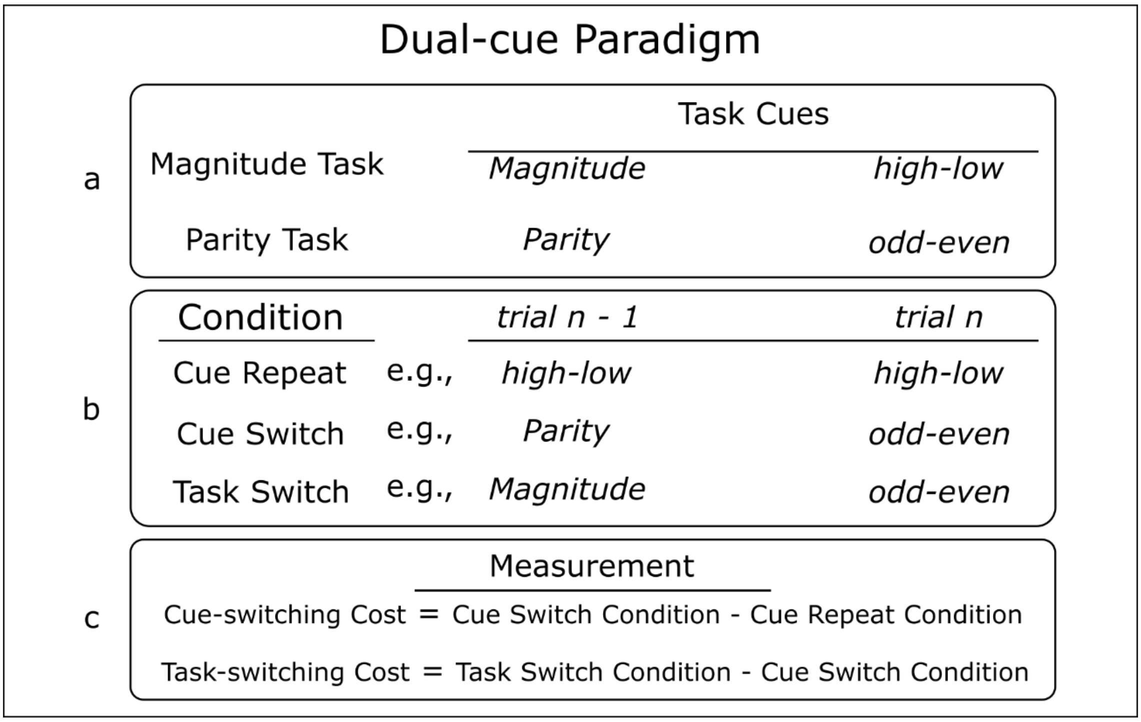

Figure 1.7. (a) In a dual-cue paradigm, each task has two possible cues. (b) Cue repeat condition: trial *n - 1* and *trial n* have the same cue. Cue switch condition: trial *n - 1* and trial *n* have different cues, but both cues represent the same task. Task switch condition: trial *n - 1* and trial *n* have different cues, and each cue represents a different task. (c) In order to measure the cue-switching cost, compare the cue switch condition with the cue repeat condition. In order to measure the task-switching cost, compare the task switch condition with cue switch condition.

Based on these results, Logan and Bundesen (2003) proposed that, in a task-switching paradigm, people do not always apply task-switching strategy; rather, in a given trial, people encode both the task cue and the target and form a combination——a compound cue of response retrieval. Therefore, every time the task cue switches, participants need to encode the cue again. It is this additional cue-encoding process that may cause the delay in switch trials and triggers the task-switching cost in the task-cueing paradigm rather than the task-set reconfiguration process.

1.7.2 Episodic or Semantic Compound Retrieval

Arrington and Logan (2004a) further addressed two different compound retrieval strategies: episodic compound retrieval and semantic compound retrieval. They proposed that participants can remember each cue-stimulus combination and retrieve each response directly when the number of stimuli is small. For example, Logan and Bundesen (2003) only used four cues and eight numbers in their experiments. When the cue *Magnitude* and the number *8* appeared in a given trial, participants formed a compound: *Magnitude 8*. The participants then would activate their memory and retrieve the correct response associated with this compound (i.e., press the *right* key on the keyboard). This has been called “episodic compound retrieval”.

The problem with an episodic compound retrieval strategy is that it requires prior knowledge of all compound-response mappings. It cannot explain why people can give the correct answer as soon as they have received the task rule instructions and without having seen the cue-stimulus combination before. In order to solve this issue, Arrington and Logan (2004a) proposed a semantic compound retrieval strategy. If, for example, in a trial, a novel number *88* appears after the cue *odd–even* then the response of the compound *Parity 88* is not available to the participants because the number *88* has never been presented before. In this case, the participants may process the combination at the semantic level and retrieve the associated response for the compound: *Parity + Even* (88 = even number at a semantic level) and then retrieve the associated response from memory (i.e., press the *left* key on the keyboard).

To examine the semantic compound retrieval strategy, Arrington and Logan (2004a) applied a dual-cue task-switching paradigm with 640 target stimuli that were never repeated during the experiment. Therefore, the participants were unable to remember the response of each cue-stimulus compound episodically. Arrington and Logan reasoned that if they replicated the results of Logan and Bundesen (2003; large cue-switching cost but very small task-switching cost), their hypothesis about semantic compound retrieval would be confirmed. Indeed, their results suggested that the task-switching costs were very small, but the cue-switching costs were large and statistically significant.

1.7.3 Task Cue Priming

In later studies, Logan and colleagues further proposed that different cues assigned to the same task may prime each other associatively or semantically, so that performance in task repetitions can be faster than in task alternations (Schneider & Logan, 2005; Logan & Schneider, 2006). This idea is very similar to the associative learning account proposed by Forrest et al. (2014), that I will address in Chapter 7.

1.7.4 Disadvantages of the Compound Retrieval Account

One obvious problem with the above findings is that the cue-switching cost triggered by a compound memory retrieval process cannot completely explain the delay in switch trials. Early studies consistently indicated that although the cue-switching costs were large, there were always some small task-switching costs as well (e.g., Mayr & Kliegl, 2003; Monsell & Mizon 2006). In fact, Logan and colleagues themselves noticed the remaining task-switching costs (Logan & Bundesen, 2003; Arrington & Logan 2004a). Furthermore, in a series of experiments, Monsell and Mizon (2006) showed that the cue-switching costs were not always bigger than the task-switching costs. Sometimes the cue-switching cost were small and the task-switching costs were substantial (see their Experiments 2 and 3). Monsell and Mizon suggested that the cue type significantly affected results. For example, in their Experiment 2, the task cues were locational: the same circle appearing in four different locations represented four different cues. Also in their Experiment 3, the cues were all visual whereas Logan and colleagues only used written text cues (e.g., Logan & Bundesen, 2003; Arrington & Logan, 2004).

Task-switching Rates and Task-switching Cost

As Monsell and Mizon (2006) suggested, the probability of cue switching and task-switching plays an important role in a dual-cue paradigm. In the original study by Logan and Bundesen (2003), 25% were cue-repeating trials, 25% were cue-switching trials (but with task repeat), and 50% were task-switching trials. In other words, the probability of task-switching was .5, (*p* (task-switching) = .5) and the probability of task-switching given a cue-switch was .67 (*p* (task-switching | cue-switching) = .67). The results of Logan and Bundesen (2003) indicated large cue-switching costs but the task-switching costs were very small.

In their Experiment 6, Monsell and Mizon (2006) replicated the experiment of Logan and Bundesen (2003). The only difference was that they intentionally manipulated the probabilities: there were 25% cue-repeating trials, 50% cue switching trials, and 25% task-switching trials (*p* (task-switching) = 0.25 and *p* (task-switching | cue-switching) = .33). Monsell and Mizon found statistically significant task-switching as well as cue-switching costs.

Monsell and Mizon (2006) therefore proposed that, when the probability of task-switching is high, task-switching trials are favoured, as participants assume task-switching as a default. The problem is that by anticipating task-switching trials a delay occurs when a task-repeating trial comes up because the participant has prepared for the wrong task. As a consequence, the benefits of task-repeating are reduced, hence the task-switching cost. In agreement with Monsell and Mizon (2006), later studies consistently reported that experiments with a higher task-switching rate would have smaller task-switching costs than experiments with a lower task-switching rate (Bonnin, Gaonac'h & Bouquet, 2011; Duthoo, De Baene, Wühr, & Notebaert, 2012; but see Logan et al. 2007 for an alternative interpretation of the relationship between the task-switching rate and the task-switching cost).

Paired Trials

Altmann (2006) re-examined the idea of cue-switching cost and compound retrieval theory by taking a different approach. He organised pairs of trials so that the cue only appeared before the first trial and disappeared after the first trial response. In the second trial, only the target stimuli appeared

Since the cue did not appear in the second trial of each pair and the cue encoding should be completed during the first trial, no cue-switching cost and no task-switching cost were expected in the second trial according to the compound retrieval account. However, Altmann (2006) found significant task-switching costs, but no cue-switching cost, in the second trials. This result suggested that there must be more processes involved in task-switching than just a compound retrieval process. The author further suggested that the cue-switching cost can only explain a subset of the variance associated with the task-switching cost..

1.7.5 Cue-switching costs and the Compound Retrieval Account

I agree with the idea that a pure task-set reconfiguration account does not completely explain the task-switching cost in the paradigm with a single and explicit cue. However, their argument that the compound retrieval process alone produces the task-switching cost may have been too optimistic, because the remaining task switching cost after controlling the cue-switching effect (Monsell & Mizon, 2006) and the results of Altmann (2006) cannot be explained by a compound retrieval process alone.

In fact, many dual-cue task-switching studies have indicated that the cue-switching costs come from active control processes, but not from the perceptual priming of the task cues as Logan and colleagues originally proposed (Mayr & Kliegl, 2003; Grange &

Houghton, 2009; Horoufchin et al., 2011; Schmitz & Voss, 2012, 2014). In other words, the task-switching costs and the cue-switching costs may reflect two independent cognitive processes. For example, a study found that the preparation effect and the practice effect impact only the cue-switching cost but not the task-switching cost (Mayr & Kliegl, 2003). Furthermore, ITI and CSI variation also had a differential effect on the cue-switching cost and the task-switching cost (Horoufchin, et al., 2011; Schmitz & Voss, 2012; for a review, see Jost et al., 2013).

1.8 Summary of Chapter 1 and Preview of Chapter 2

I have introduced paradigms of task-switching studies and visited four major accounts of task-switching: the task-set reconfiguration account, proactive interference account, task-set inhibition account and compound retrieval account. Each of these accounts provides to some degree a valid explanation of task-switching costs, but some researchers have suggested that integrating positions can better explain the task-switching effect (e.g., Meiran 2000; Koch & Allport 2006; Altmann, 2008). For example, Meiran (2000) proposed that switch costs have three components. Firstly, there is a waiting component which is related to proactive interference. Secondly, there is a preparatory component which is related to the task-set reconfiguration process. Thirdly, there is a residual component which may cause the residual switching cost. Various computational models have implemented both proactive interference and task-set reconfiguration to model task-switching costs (e.g., Gilbert & Shallice, 2002; Brown, Reynolds & Braver, 2007). Moreover, recent studies have even suggested that all three accounts may play a role in task-switching experiments and jointly contribute to task-switching costs (Schmitz & Voss, 2012, 2014). In other words, the task-switching cost in the task-cueing paradigm may reflect a heterogeneous cognitive effect triggered by multiple factors such as the task-set reconfiguration process, the interference, and the cue-encoding process.

In the present thesis, I try to explain task-switching costs that arise when participants do not apply a task-switching strategy and therefore do not switch between two tasks. A major theme of this thesis is the investigation of whether it is possible to observe the same absence of task-switching costs in humans that other researchers have observed in monkeys (Stoet & Snyder, 2003; Avdagic, et al., 2013) and pigeons (Meier, Lea & McLaren 2016; Castro & Wasserman, 2016). In Chapter 2, I will introduce studies that have shown that animals can switch between tasks without showing any switching costs (e.g., Stoet & Snyder, 2003; Avdagic et al., 2013; Meier et al., 2016; Castro & Wasserman, 2016) whereas for human participants, it is extremely hard to eliminate task-switching costs.

Chapter 2: The Look-up Table Approach and Bivalent Visual Stimuli

2.1 How to Reduce the Task-switching Costs?

Over the past two decades, a substantial amount of research has focused on task-switching and almost all studies have reported task-switching costs for bivalent stimuli. These robust results have triggered a search for possibilities of reducing or eliminating task-switching costs. Indeed, successfully decreasing task-switching costs may help researchers to draw conclusions about the origin of the task-switching process (e.g., Logan & Bundesen 2003; Verbruggen, et al., 2007).

2.1.1 Practice Effect

One of the most straightforward methods applied to reduce task-switching costs is practice. In fact, Jerslid (1927) already discussed the effect of practice as early as 1927. He concluded that the initial difference between switch conditions and repeat conditions can be reduced by practice because practice effect is larger in switch conditions compared to separate conditions. Similar practice effects have been confirmed in more recent studies. For example, Rogers and Monsell (1995) reported that, over two days of practice, participants' average task-switching costs were reduced from 262 ms to 186 ms. However, other researchers pointed out that, despite long and extensive practice, a certain amount of significant task-switching costs remain (e.g., Strobach, Liepelt, Schubert & Kiesel, 2011; Stoet & Snyder, 2007).

2.1.2 Preparation Effect

Research has consistently demonstrated that allowing participants to have a longer preparation time can reduce task-switching costs (e.g., Rogers & Monsell, 1995; Koch, 2003; Schneider & Logan, 2007; Longman et al., 2014; Meiran, 1996; Meiran et al., 2000). However, it was noted in these studies that even after long preparation times (over 1000 ms in some studies; e.g., Longman et al., 2014; Meiran, 1996; Meiran et al., 2000), there are still some small but significant task-switching costs, which haves been termed the "residual-switching cost". In one study, it was pointed out that the residual-switching cost can be eliminated if the experiment only presents the cue during the CSI and makes it disappear in the remaining CSI (Verbruggen et al., 2007). Nevertheless, a recent replication manipulation of cue status (disappeared or not) did not consistently eliminate residual-switching cost (Schneider, 2016).

In a very recent study, Schneider (2017) attempted to eliminate the residual-switching cost by increasing the phasic alertness——a segment of attention that reveals rapid but brief changes in sensitivity to external stimulation (Posner, 1978, 2008; Posner & Boies, 1971). However, the results suggested that, though increasing the phasic alertness might reduce the overall RT, it still cannot eliminate the residual-switching cost. To completely eliminate the residual-switching cost by advance preparation is hard.

2.1.3 Working Memory

Liefooghe, Barrouillet, Vandierendonck and Camos, (2008) provided a comprehensive review regarding the working-memory account of task-switching costs. In short, the task-set reconfiguration account implies that working memory must be a factor that relates to the task-set reconfiguration process, and it should also help participants to maintain the task-set once configured (e.g., Mayr & Kliegl, 2003; Rubinstein, Meyer & Evans, 2001). A close relationship between task-switching cost and working memory was demonstrated by Leifooghe et al. (2008). The results of their study suggested that switching between tasks may reduce the working memory load. However, the working memory load would not impact the size of the task-switching cost. The asymmetric relationship between working memory and task-switching cost can be explained by the time-based resource sharing theory (see Barrouillet, Bernardin & Camos, 2004; Leifooghe et al., 2008).

Nevertheless, under some special conditions, manipulating working memory can reduce task-switching costs (Schneider & Logan 2006; Schneider & Logan, 2015). For example, in a task-switching experiment with two tasks (Task A and Task B), the order of tasks can form a fixed sequence (e.g., AABABB). It was suggested that if the same sequence repeatedly occurred (e.g., AABABB, AABABB….), the first trial of the sequence would indicate no task-switching costs, because participants can chunk the sequence in their working memory (Schneider & Logan, 2006). Moreover, if the participants make further sub-chunks inside the sequence, their "chunk point" would also indicate no task-switching costs (Schneider & Login, 2015). For example using the six-trial sequence AABBAB, participants may use a 2-2-2 formation. They would therefore form three sub-chunks (AA, BB, AB) in working memory. As a result, the first trial of each chunk (those are the first, third and fifth trials of the sequence) would carry no task-switching costs.

2.1.4 Other Methods

Physical Exercise

It is believed that physical exercise can increase participants' executive control; therefore, reduce task-switching costs (Barenberg, Berse & Dutke, 2015; Kamijo & Takeda, 2010). For example, Barenberg et al. (2015) asked participants in the experimental group to attend an acute and intense bicycle exercise session before participating in the task-switching experiment. Each participant was required to cycle at a speed of 70 revolutions per minute for 10-14 mins. Conversely, the participants in the control group were instructed to watch a cartoon episode and to relax before participating in the task-switching experiment. Their results suggested the experimental group had less task-switching costs than the control group (but their task-switching costs were still significant).

Incorrect Prediction

Two recent studies have suggested that making incorrect predictions about the upcoming task can reduce task-switching costs (Kleinsorge & Scheil, 2015, 2016). In these study, participants were required to predict the task type of the next trial before they saw the task cue (i.e., during the ITI). The results showed that participants who had an incorrect expectation of the upcoming task had reduced task-switching costs.

In their experiments (Kleinsorge & Scheil, 2015, 2016), participants experienced two different sources of conflict in switch trials. The first source was prompted by the task-set reconfiguration process. During the task-set reconfiguration, there is a conflict between the irrelevant task-set activated in the previous trial and the relevant task-set needed to be performed in the current trial. This source of conflict is inherent to all task-switching experiments (c.f.Vandierendonck et al., 2010; Kiesel et al., 2010).The second source of conflict was prompted by the requirement for participants to predict the upcoming task-set. A wrong prediction would cause conflict between the representations of the predicted task-set and the actually relevant task-set. Kleinsorge and Scheil (2015, 2016) argued that adding the second source of conflict to the first would increase the amount of controlled process applied to the establishment of the actually relevant task-set. This is because application of the actually relevant task-set is the resolution of both types of conflict. Specifically, to solve the conflict of task-set reconfiguration, participants need to apply the relevant task-set. To solve the conflict of wrong prediction, participants also need to apply the relevant task-set. Therefore, if participants make a wrong prediction in a switch trial, the amount of cognitive control is somehow doubled, which would cause a reduction of switch costs.

2.1.5 Summary

There are many interesting manipulations that can reduce task-switching costs. Nevertheless, participants can only eliminate the cost completely under very specific experimental conditions (e.g., Kleinsorge & Scheil, 2015, 2016; Schneider & Login 2006; Schneider & Login, 2015) and some of these results cannot be replicated (e.g., Verbruggen et al., 2007). Indeed, as I introduced in Chapter 1, task-switching costs may reflect additional cognitive processing when participants deal with switch trials——either caused by interference or inhibition from previous trials, or the demand to reconfigure the new task-set of switch trials. If this assumption is accurate, then, as long as participants have to switch between tasks, completely removing the cost of switching would be extremely difficult, if not impossible.

2.2 Animal Studies

Surprisingly, animal studies have indicated that rhesus monkeys can somehow switch between tasks without any indication of task-switching costs (Stoet & Snyder, 2003). In this study, two rhesus monkeys were trained to switch between different tasks. For the first monkey, Task A was to judge the colour of squares (either red or green). Task B required the monkey to determine whether the square was brighter inside or outside. To avoid a task-specific effect, the second monkey needed to switch between a colour task and an orientation task. The orientation task required the monkey to judge whether the target figure was horizontal or vertical. This study used a task-cueing procedure. The screen would display a yellow or black background to indicate different tasks. The monkeys showed a 0.2 ms non-significant difference between switch trials and repeated trials. Similarly, in a recent study in which monkeys had to perform a brightness task and a radius task (Avdagic et al., 2013), monkeys were trained to determine the brightness of a circle, or the size of a circle. In line with Stoet & Snyder (2003), the result of this study indicated that monkeys are somehow unaffected by task-switching (but see Caselli & Chelazzi, 2011).

Furthermore, two recent studies have suggested that pigeons can switch between tasks without any indication of task-switching costs (Castro & Wasserman, 2016; Meier et al., 2016). Both pigeon studies included visual tasks. Meier et al. (2016) trained pigeons to switch between a spatial frequency task and an orientation task. The spatial frequency task required the pigeons to judge the spatial frequency for a stimulus (high or low frequency). The orientation task required the pigeons to judge whether the stimulus was horizontal or vertical.

Castro and Wasserman (2016) trained pigeons to switch between a “numerosity” task and a “variability” task. In the numerosity task, pigeons had to discriminate whether a stimulus contained few (6) items or many (16) items. In the variability task, pigeons had to discriminate whether a stimulus contained low-variability or high-variability arrays. The low-variability arrays contained items that were all the same as one another. The high-variability arrays contained items that were all different from one another.

These animal studies are critical, because animals’ surprising performances in task-switching experiments do not agree with the previously postulated implications of executive control. For human participants, the task-switching cost is an exquisite measurement of executive control (Monsell, 2003; Monsell & Mizon, 2006; Mayr & Kliegl, 2000; Rogers & Monsell, 1995; Meiran, 2000; Braver, 2012; Zinke, Einert, Pfennig & Kliegel, 2012; Kray et al., 2012). For example, studies have consistently documented that ADHD is characterised by executive control deficits (Barkley, 1997; Barkley and Lombroso, 2000; Pennington & Ozonoff, 1996; Sergeant, Geurts & Oosterlaan, 2002). Conversly, in task-switching studies atypical participants who have ADHD tend to have larger task-switching costs than typical participants (Cepeda, Cepeda & Kramer, 2000; Kramer, Cepeda & Cepeda, 2001; Rasmussen & Gillberg, 2001; Kray et al., 2012). Similar evidence was provided by comparsion of task-switching costs among different age groups (e.g., Meiran, Gotler & Perlman, 2001; Kray, Li & Lindenberger 2002; Mayr & Kliegl, 2000). These studies pointed out that young adults had the smallest task-switching costs, because they have the best executive control abilities (but also see Wasylyshyn et al., 2011).

2.2.1 Potential Strategy Differences between Humans and Animals

How can we explain monkeys' and pigeons' outstanding performances on switch trials without assuming they have better executive controls than human participants? Stoet and Snyder (2003) proposed two possible interpretations. Firstly, they suggested that, before the experiment, the monkeys in their study had extensive training that helped them to eliminate task-switching costs. However, a follow-up study discarded this assumption by showing that, even after a huge amount of training under the same experimental design, human participants still could not match the performance of the monkeys (Stoet & Snyder, 2007). Secondly, Stoet and Snyder (2003, 2007 and 2009) proposed that humans and monkeys have different cognitive processing and that distinct task-switching behaviours reflect differences between the two species. I will further discuss this argument in Chapter 6.

Later studies have suspected one more possibility: that instead of applying task-switching strateies, monkeys and pigeons could somehow remember all the cue-stimulus combinations and the responses corresponding to those combinations. Therefore, animals responded to the cue-stimulus combination directly. There were no task-switching costs, because the animals performed the experiment without switching between tasks (e.g., Dreisbach et al., 2006, 2007; Dreisbach & Haider, 2008; Forrest et al., 2014; Meier et al., 2016). However, see also Avdagic et al. (2013) for a different interpretation of monkey task-switching performance. Dreisbach and colleagues called this particular method the "stimulus-response mapping approach" (Dreisbach et al., 2006, 2007; Dreisbach & Haider, 2008, 2009), and Forrest et al. (2014) called this method the "cue-stimulus-response approach". In the pigeon study, it is called the "associative learning approach" (Meier et al., 2016). However, these all reflect the same idea: participants (humans or animals) can figure out a correct response directly based on the stimulus they perceive, without applying any rules-based processes like task-switching strategy. I call this method the "Look-up Table" (LUT) approach.

Studies with human participants demonstrated that, by inducing participants to apply the LUT approach without telling them to switch between tasks, participants' task-switching costs disappeared (Dreisbach, et al, 2006, 2007) or were at least reduced (Forrest, Monsell, & McLaren, 2014). In Forrest et al.'s (2014) study, sometimes the task-switching costs were not significant [F (1, 15) = 3.39, p = .086, η^2_G=.00372; see their Experiment 2] after inducing participants to use a LUT approach. At least, regarding a statistical effect, they managed to eliminate task-switching costs.

In short, previous human studies (Dreisbach et al., 2006, 2007; Forrest et al., 2014) showed that participants could perform task-switching without even realising that there were two different tasks, and therefore there was no delay in switching trials. Based on this observation, researchers proposed that the difference between humans and monkeys may reflect differences in cognitive processing. Nevertheless, different strategies rather than a difference between the species, seem to be responsible.

2.2.2 Disadvantages of the LUT Approach

The LUT approach, however, faces several challenges. Firstly, Dreisbach et al. (2006, 2007) and Forrest et al. (2014) conducted LUT studies using entirely different tasks compared with animal studies. Dreisbach et al. (2006, 2007) required participants to switch between two linguistic tasks: the Consonant/Vowel Task and the Animal/Non-Animal Task. The Consonant/Vowel Task required participants to decide whether a word started with a consonant or a vowel. The Animal/Non-Animal Task required participants to decide whether or not the word represented an animal. Furthermore, Forrest et al. (2014) applied two mathematical tasks: an odd/even task (whether a number is an odd number or an even number) and a low-high task (whether a number is a small number [< 5] or a big number [>5]). Conversely, visual tasks were used in the monkey studies (Stoet & Snyder, 2003; Avdagic et al., 2013) and pigeon studies (Castro & Wasserman, 2016; Meier et al., 2016).

Because we do not know whether humans can replicate a similar task-switching pattern to monkeys and pigeons on the visual tasks, the previous results were in conclusive.

Secondly, Dreisbach et al. (2006, 2007) used univalent stimuli. Each univalent stimulus only provides information for only one task. Conversely, all monkey studies and pigeon studies used bivalent stimuli. Each bivalent stimulus provides information for more than one task (e.g., Stoet & Snyder, 2003; Avdagic et al., 2013; Castro & Wasserman, 2016; Meier et al., 2016). The diversity of bivalent stimuli and univalent stimuli will be fully explained in Chapter 3. However, in short, previous studies have demonstrated that when the stimuli were univalent, the task-switching costs were smaller than if the stimuli were bivalent (e.g., Allport & Wylie, 2000; Jersild, 1927; Rogers & Monsell, 1995; Wylie & Allport, 2000). Although Forrest et al. (2014) used bivalent stimuli, their result is not consistent. As mentioned before, the task-switching costs approached significance (p = .086) in their Experiment 2. Moreover, the task-switching costs remained significant in their Experiment 1 and Experiment 3.

2.3 Goals and Hypotheses

The present study sought to investigate the LUT approach. Firstly, I tested Dreisbach et al.'s (2006, 2007) and Forrest et al.'s (2014) results using two visual tasks. Secondly, this study re-examined whether the same results would consistently emerge when using bivalent stimuli. In short, the main question was whether human participants could assimilate the performance of monkeys and pigeons by employing a similar strategy (i.e., the LUT approach). Thirdly, since Dreisbach and colleagues (Dreisbach et al., 2006, 2007; Dreisbach & Haider., 2008, 2009) and Forrest et al. (2014) demonstrated that human participants could apply different strategies when dealing with task-switching experiments, the present study, investigated whether there are other possible strategies that can help participants to reduce, or even to eliminate, task-switching costs. The advantages and disadvantages of each potential strategy are discussed.

2.4 Experiment 2.1

The intention of Experiment 2.1 was to explore whether human participants could apply the LUT approach and eliminate the task-switching costs in a bivalent task-switching experiment with two visual tasks. Thus, one of the key points of this experiment is to instruct the participants to perform both tasks without mentioning the task-switching strategy. To achieve this, the researcher provided an LUT to help them remember the correct response for all possible situations. I predicted that, without an explicit understanding of task rules, participants would apply the LUT approach as previous studies had suggested (Dreisbach et al., 2006, 2007; Forrest et al., 2014; Meier et al., 2016). In addition, without an explicit understanding of task rules, participants would indicate no task-switching costs.

Moreover, this experiment sought to investigate the different strategies participants might apply. Given the fact that switching between tasks has the obvious disadvantage——task-switching costs, it is interesting to ask whether task switching may benefit participants in other aspects. Therefore, this experiment compared task-switching strategies with other potential strategies. I predicted that participants who applied different strategies (e.g., task-switching strategy and the LUT approach) would behave differently during the experiment in terms of reaction time and error rate.

Finally, because I applied bivalent stimuli in Experiment 2.1, I also examined the congruency effect. For a typical bivalent stimuli task-switching experiment, half the stimuli are congruent (i.e., the stimuli were associated with the same response key for both tasks) and half the stimuli are incongruent (i.e., the stimuli were associated with different keys in different tasks). Previous studies have suggested that participants (both animals and humans) tend to have longer RTs and higher error rates on incongruent trials than congruent trials (e.g., Forrest et al., 2014; Stoet & Snyder, 2003; Caselli & Chelazzi, 2011; Meier et al., 2016;

Meiran & Kessler, 2008; Kessler & Meiran, 2010; Schneider, 2015).

I made three predictions in Experiment 2.1. Firstly, I predicted that, without an explicit understanding of the task rule, participants would apply the LUT approach and show no task-switching costs. Secondly, I predicted that participants who applied different strategies would behave differently during the experiment, in terms of reaction time and error rate. Finally, it was predicted that participants from both the experimental and the control groups would have longer RTs and higher error rates on incongruent trials than congruent trials.

2.4.1 Method

Participants

A total of 40 adult students (female = 26) from the University of Glasgow participated in Experiment 2.1. They were 20-37 years of age (mean age = 24.7, *SD* = 3.86). Each participant received £3 for their participation.

Stimuli and Apparatus

There were four target stimuli: a black circle, a white circle, a black hexagon and white hexagon. The size of all figures was 60 × 60 pixels or 16.93 mm × 16.93 mm. There were two task cues (which were also 16.93 mm × 16.93 mm; see Figure 2.1). Stimuli were presented centrally on a BenQ computer monitor (24 inches). A Black Box Toolkit response pad was used to record participants' responses. Participants also used keys on a QWERTY keyboard to start the experiment or to start a new block. The viewing distance was about 50 - 70 cm. All experiments in the present thesis were actualised by Psytoolkit Linux version (http://www.psytoolkit.org).

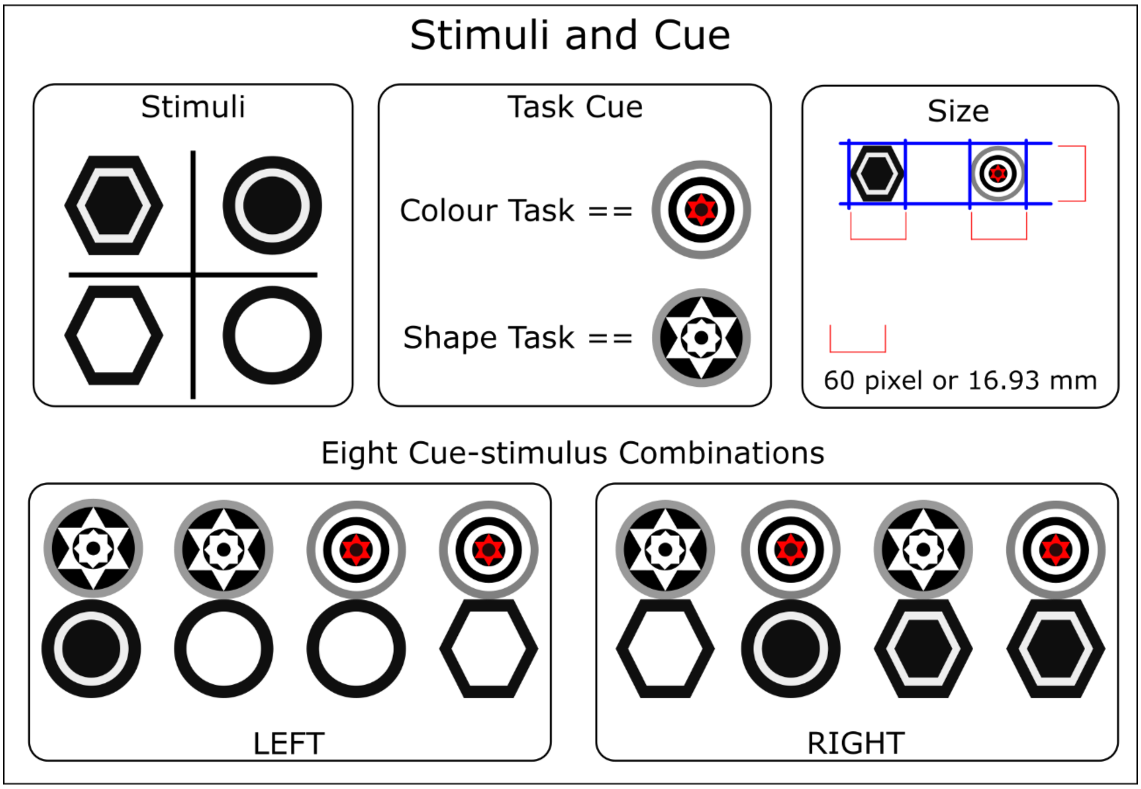

Figure 2.1. The four target stimuli and two task cues of Experiment 2.1. For the colour task, if the colour was white, participants should press a *left* key; and if the stimulus was black, participants should press a *right* key. For the shape task, if the stimulus was a circle, participants should press a *left* key; and if the stimulus was a hexagon, participants should press a *right* key. Participants can also obtain the correct response by memorising the association between Cue-Stimulus combination and response key directly.

Tasks and Timeline

Participants performed two tasks: the colour task and the shape task. For the colour tasks, participants had to determine whether the colour of a stimulus was black or white. If the colour was white, participants should press the *left* key. If the stimulus was black, participants should press the *right* key. For the shape task, participants had to determine whether the shape of a stimulus was a circle or hexagon. If the stimulus was a circle, participants should press the *left* key. If the stimulus was a hexagon, participants should press the *right* key (see Figure 2.1). The experiment used a composite task-cueing design. In each trial the task cue and task stimulus appeared and disappeared simultaneously, and there was no interval between cue and stimulus onset. This setting has two advantages: firstly, without a cue-stimulus interval (CSI), there was no time for preparation. This makes it easy to detect task-switching costs, because the task-switching costs will be much larger. Secondly, the task cue did not disappear, and participants did not need to put the cue into their working memory. As a result, they could easily compare the cue and task stimulus, or identify cue and stimulus as a combination. This induces participants to apply novel strategies. For instance, this composite design should certainly facilitate the LUT approach because the idea is to treat each cue-stimulus combination as one compound stimulus. During the experiment, one of the eight combinations showed up at the centre of the screen on each trial. The inter-trial interval (ITI) was 300 ms (see Figure 2.2).

Feedback

On each trial, participants had five seconds to make a response. If their response was correct, the next trial would started after a 300 ms ITI (see Figure 2.2). If participants did not make a response or made a wrong response, a mistake message or a timeout message was displayed for five seconds (Figure 2.2a). A long feedback for mistake and timeout would encourage a correct response. After the feedback, the next trial began.

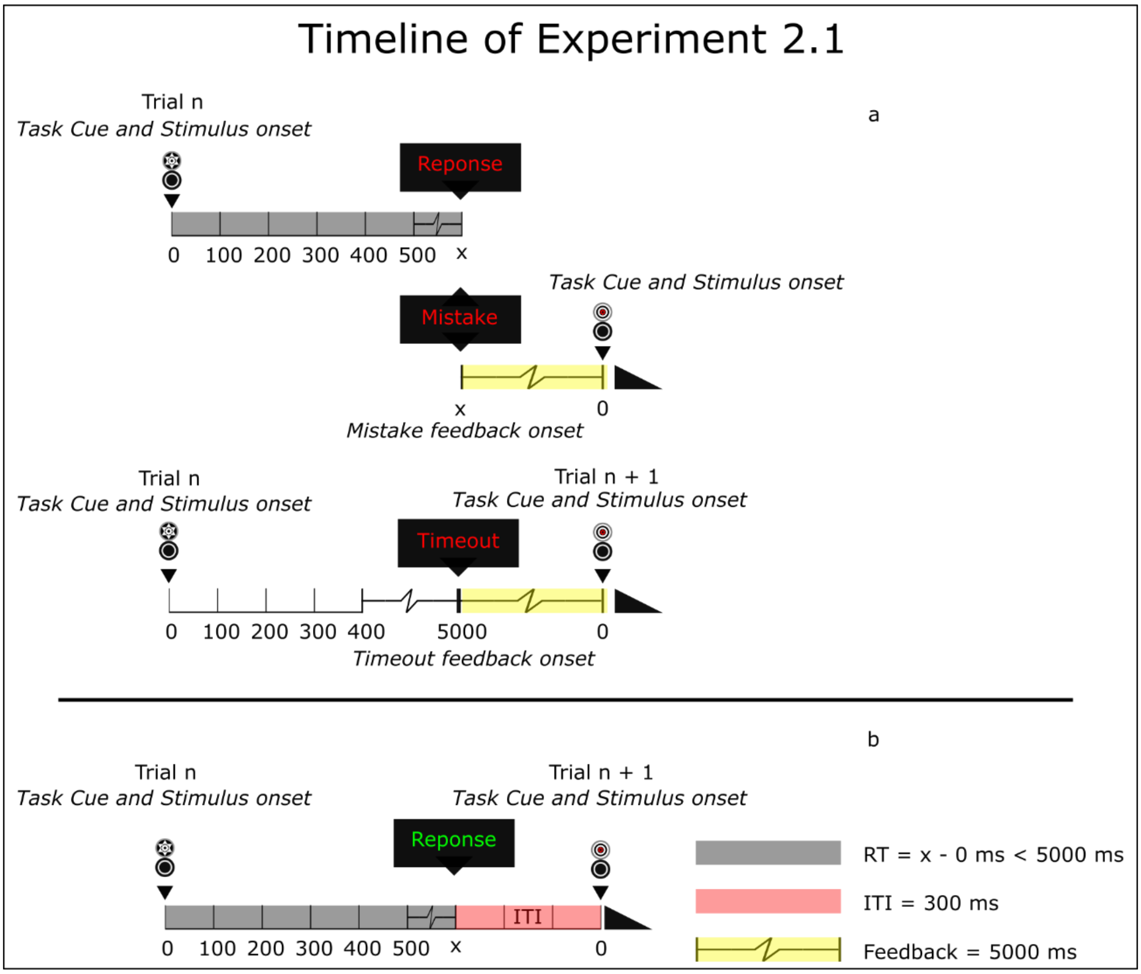

Figure 2.2. The timeline of Experiment 2.1. (a) Demonstrates the consequences of mistake and timeout. The feedback information onset is for 5000ms. (b) After a correct response, the next trial starts after a 300 ms ITI.

Procedure

Experiment 2.1 took place in a quiet and darkened room. Participants were randomly assigned to two groups: an experimental group (N = 20) and a control group (N = 20).

Experimental group

At the beginning of this experiment, participants received written instructions. On a sheet of paper, all eight possible cue-stimuli combinations (2 cues × 4 target stimuli) were printed on the left and right-hand sides of the paper. Participants were instructed that the four combinations on the left-hand side corresponded to pressing the *left* key, and the four combinations on the right-hand side corresponded to pressing the *right* key (See Figure 2.3). They had one minute to remember the combinations and the corresponding keys.

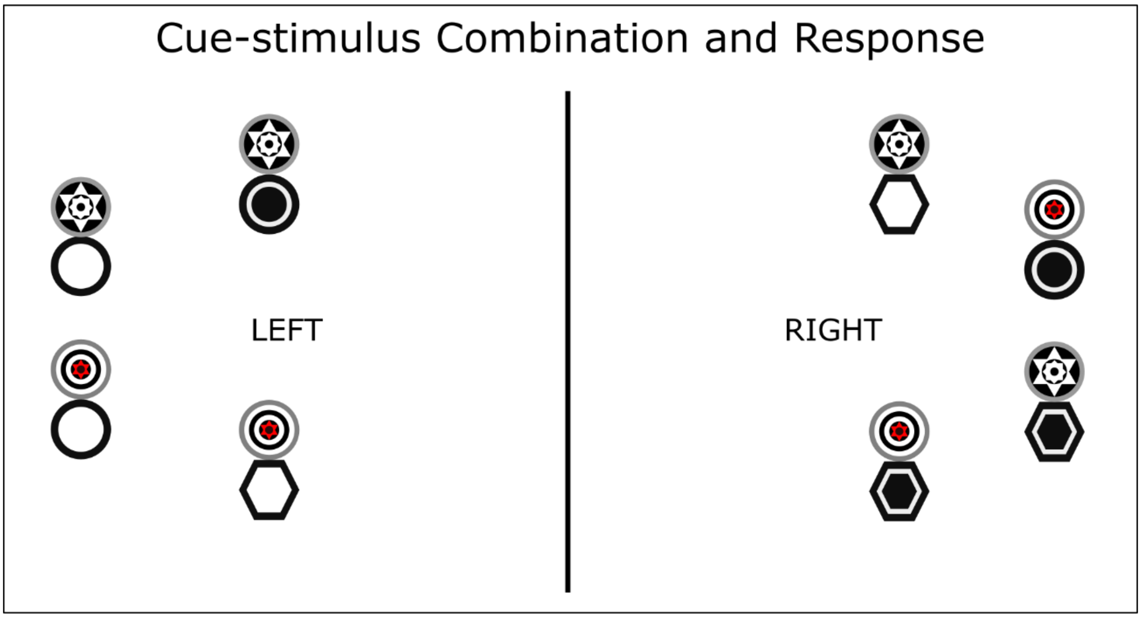

Figure 2.3. The response mappings in the written instructions. Four cue-stimulus combinations were displayed on the left-hand side and four combinations on the right-hand side, corresponding to pressing the *left* and *light* keys respectively.

Before the experiment, participants completed two 20-trial training blocks that were designed to help them remember the cue-stimulus combinations and the corresponding response keys. After the two training blocks, participants completed two experimental blocks of 100 trials. Participants were asked to press the corresponding key as quickly and accurately as possible. They were allowed to take a rest after they had finished the first block and start the second block when they were ready. Finally, after participants finished the experimental blocks, they were required to report their strategy during the experiment. They also confirmed whether they realised that there were two tasks (the colour task and the shape task) or not.

Control group

The experiment conditions were almost identical with those of the experiment group. The only difference was in the instructions the participants received. A task-switching instruction was provided, which explained the task rules, target stimuli, and task cues for making a correct response. Participants in the control group also performed the same two training and experiment sections. After the experiment, they also had to report the strategy they had applied.

2.4.2 Results

Experiment 2.1 had three predictions. Firstly, it was predicted that participants in the experimental group would all apply the LUT approach, and it was predicted that they would not show any task-switching costs. Conversely, participants in the control group would show significant task-switching costs. Secondly, it was predicted that participants in the experimental group and participants in the control group would behave differently regarding RTs and error rates because of their strategy differences. Thirdly, it was predicted that participants from both the experimental and control groups would have longer RTs and higher error rates in incongruent trials than in congruent trials.

Before the final data analysis, I took several factors into account. Firstly, any trial following an incorrect trial would be excluded. If participants made a mistake on *trial n - 1* it is possible that they did not apply the task rule or even did not perceive the stimulus at all. Therefore, *trial n* may have no meaningful reference; it is neither a switch trial nor a repeat trial. Secondly, any trial fully repeating the previous trial would be excluded. In other words, if *trial n - 1* and *trial n* have the same cue-stimuli combination, *trial n* would be removed,ecause on *trial n* participants can simply execute the previous response without any meaningful cognitive process. Again, it is neither a switch trial nor a repeat trial. Unless mentioned otherwise, the same process applied to all the experiments in this dissertation. All results in the present thesis were analysed with the default statistical functions in the R programming language.

Experimental Group and Control Group

Table 2.1

Mean (SD) of RT and Error Rate for Each Trial Condition and Participant Group

	Experimental Group		Control Group	
Trial /Group	RT ms (*SD*)	Error Rate (*SD*)	RT ms (*SD*)	Error Rate (*SD*)
Repeat Congruent	1166 (345)	5.48% (7.1)	1288 (294)	2.01% (3.8)
Repeat Incongruent	1292 (370)	20.2% (9.2)	1376 (314)	7.96% (13)
Switch Congruent	1300 (405)	7.12% (7.8)	1528 (335)	3.32% (4.5)
Switch Incongruent	1428 (484)	17.6% (9.8)	1582 (369)	7.97% (11)
Repeat	1218 (339)	12.7% (7.1)	1331 (298)	4.87% (7.8)
Switch	1362 (435)	12.3% (7.5)	1558 (345)	5.72% (11)
Congruent	1239 (372)	6.44% (7.2)	1424 (300)	2.78% (3.8)
Incongruent	1368 (422)	18.7% (8.9)	1497 (330)	7.83% (12)
Total	1298 (390)	12.5% (7.1)	1461 (311)	5.43% (7.6)

The descriptive data of each condition is listed in Table 2.1 and illustrated in Figure 2.5. A 2 × 2 × 2 ANOVA with mixed effects was conducted to analyse the RT difference between and within conditions. Two within-subjects factors were: the trial transition (repeat, switch) and the congruency effect (congruent, incongruent). The between-subjects factor was the participant group (experimental group, control group). The two within-subjects factors, the trial transition [F (1, 38) = 39.201, p =.0001, η^2_p = .51] and the congruency effect [F (1, 38) = 22.439, p = .0001, η^2_p = .37] were significant. The between-subjects factor of participant group was not significant [F (1, 38) = 1.786, p = .189]. There was no significant interaction.

An equivalent ANOVA with the same design was conducted on the error rates. The within-subjects factor of congruency effect [F (1, 38) = 41.872, p = .0001, η^2_p = .52] and between-subjects factor of participant group [F (1, 38) = 10.21, p =.00281, η^2_p = .16] were significant. However, the within-subjects factor of trial transition was not significant [F (1, 38) = .103, p =.749]. The interaction between congruency effect and participants group [F (1, 38) = 7.404, p=.0097, η^2 = .52] was significant. Post-hoc pairwise comparisons with the Bonferroni correction suggested that the congruency effect was statistically significant in the experimental group (p = .0001), but was not significant in the control group (p = .058). This was because, in the experimental group, the incongruent trials had a very high mean error rate (18.74%; see Figure 2.5). The interaction between congruency effect and trial transition was slightly significant, F (1, 38) = 4.6, p = .038, η^2_p =.06. The congruency effect was lager in repeat conditions (mean $_{[incongruent\ trial]}$ - mean $_{[congruent\ trial]}$ = 10.2 %) than in switch conditions (6.9 %). Nevertheless, post-hoc pairwise comparisons with the Bonferroni correction suggested that the congruency effect remained significant in both the repeat (p =.0001) and the switch conditions (p = .0037). There were no other significant interactions (p >. 05).

Oral Reports and Strategies

All participants orally reported their strategies after the experiment. In the control group, all participants orally reported that they were using the task-switching strategy. In the experimental group, I picked up four strategies. Firstly, six participants had figured out and applied the task-switching strategy. Furthermore, with the exceptions of these six participants the remaining 14 (20 - 6 = 14) participants reported that they did not realise there was a colour task and a shape task, and none of them had figured out that they could switch between these two tasks.

Secondly, a novel strategy some participants reported was to remember only two task cue combinations: the shape cue + black circle is to the *left* and the shape cue + white hexagon is to the *right*. The other six combinations were simple, because all white figures correspond to the *left* key, and all black figures correspond to the *right* key. I called this method the "black and white" (BW) strategy (N = 9; see Figure 2.4). Thirdly, similarly to the second strategy, some participants decided to remember only the colour cue + white hexagon and colour cue + black circle. For the other six combinations, the circle was to the *left* and hexagon was to the right. I called this the "circle and hexagon" (CH) strategy (N = 4; see Figure 2.4). Finally, one participant remembered four combination on the left side (N = 1). No participant in the experimental group used the LUT approach.

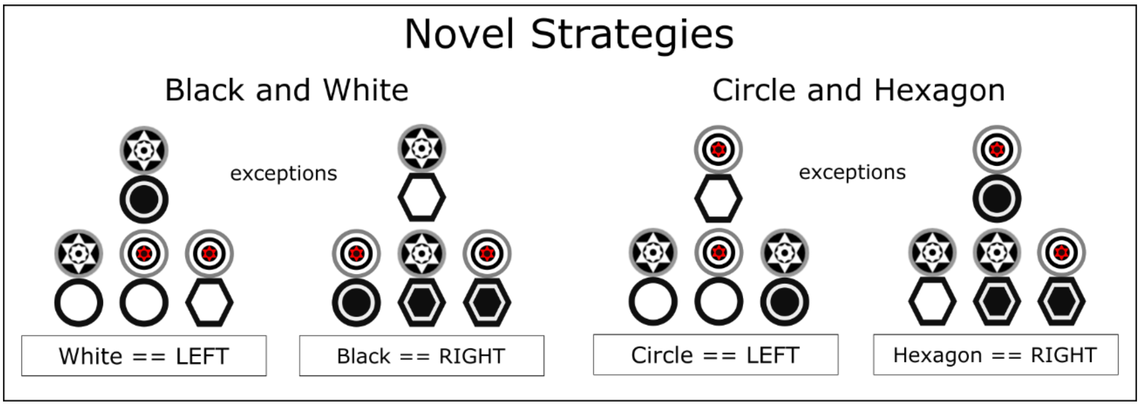

Figure 2.4. For the BW strategy, three white stimuli correspond to the *left* key. Additionally, three black stimuli correspond to the *right* key. Only two exceptions need to be remembered. For these two exceptions, the colour rule is revised: black = *left*; white = *right*. Similarly, in the CH Strategy, three circles correspond to the *left* key, while three hexagons correspond to the *right* key. Only two exceptions need to be remember. For these two exceptions, the shape rule is revised: hexagon = *left*; circle = *right*. Using these two strategies, participants can perform the experiment without even realising there are two tasks (colour and shape).

The Non-switching Strategies

To analyse participants' performances, two $2 \times 2 \times 2$ ANOVAs with mixed design were conducted. This time, to compare performance between the task-switching strategy and other non-switching strategies, I excluded the six participants who employed the task-switching strategy in the experimental group. The factorial design was identical to the previous ANOVAs. The two within-subjects factors were the trial transition (repeat, switch) and congruency (congruent, incongruent). The between-subjects factor was the participant group (experimental group, control group). Descriptive statistics for experimental control after removing the six participants are listed in Table 2.2 and illustrated in Figure 2.5.

Table 2.2

Mean (SD) of RT and Error Rate for Each Trial Condition of Non-Switch Participants

Trial Condition	RT ms (*SD*)	Error Rate (*SD*)
Repeat Congruent	1176 (352)	6.82% (8.0)
Repeat Incongruent	1294 (312)	23.5% (7.5)
Switch Congruent	1285 (443)	8.57% (8.7)
Switch Incongruent	1390 (467)	20.4% (9.2)
Repeat	1226 (321)	14.26 % (7.4)
Switch	1336 (467)	15.17% (6.5)
Congruent	1238 (391)	7.85 % (8.1)
Incongruent	1342 (293)	21.79% (7.5)
Total	1286 (389)	14.72% (6.7)

The results of the RT ANOVA indicated that the two within-subjects factors were significant: trial transition [F (1, 32) = 29.09, p = .0001, η^2_p = .48] and congruency [F (1, 32) = 17.47, p = .0002, η^2_p = .35]. The between-subjects factor of participants' groups was not significant [F (1, 32) = 1.791, p = .19]. There was no significant interaction ($p > .05$).

The results of the error rate ANOVA indicated that the within-subjects factor of congruency [F (1, 32) = 34.102, p = .0001, η^2_p = .51] and the between-subjects factor of participants' groups [F (1, 32) = 14.71, p = .00281, η^2_p = .31] were significant. However, the within-subjects factor of trial transition was not significant [F (1, 32) = .101, p = .753]. The interaction between congruency effect and participants groups [F (1, 32) = 8.737, p = .00581, η^2_p = .32] was significant. Post-hoc pairwise comparisons with the Bonferroni correction suggested that the congruent effect was significant in the experimental group (p = .0001), but not in the control group (p = .058). This is because in the experimental group, the incongruent trials have a very high mean error rate (21.79%). There were no other meaningful interactions (p >. 05; see Figure 2.5).

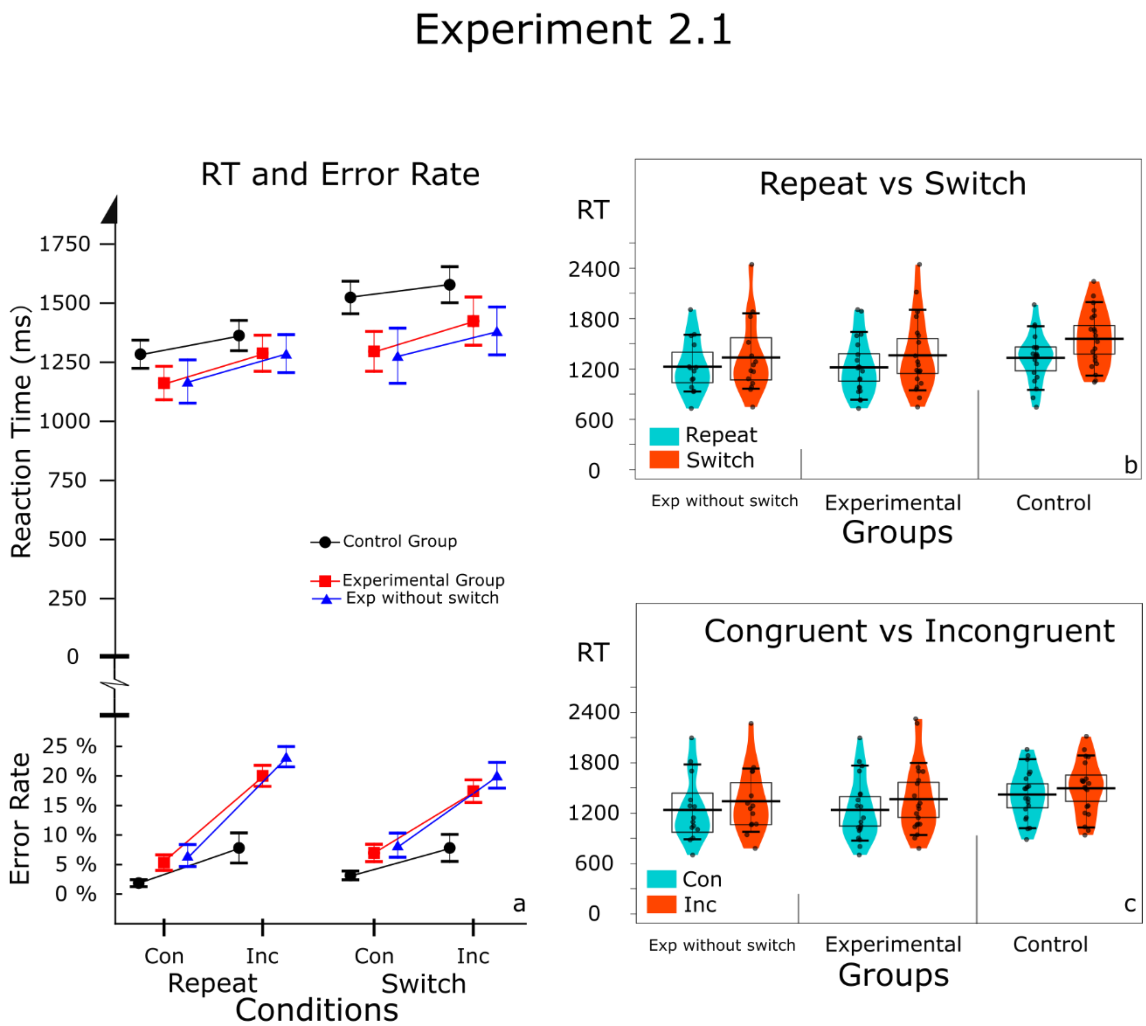

Figure 2.5. (a) The line graph displays the RT (top) and Error Rate (bottom) of each condition (switch, repeat; congruent [Con], incongruent [Inc]; Experimental group, Control group). The blue lines demonstrate the RT and Error Rate of the experimental group after removing the participants who applied the task-switching strategy. The error bar indicates ±1 *SEM*. (b) The violin plots demonstrate RT distributions from the repeat and the switch conditions. A jittered dot represents the average RT for a participant under that trial condition. The black horizontal bar and the box around it represent the mean and the 95% CI of the mean in each condition, respectively. The sesponses slower or quicker than 95% are represented by dots above and below the error bars. (c) Similar violin plots demonstrate RT distributions from the congruent and the incongruent conditions.

2.4.3 Discussion

There were three major findings from Experiment 2.1. Firstly, no participants in the experimental group used the LUT approach (N = 0). In Forrest et al.'s (2014) study, some participants reported that they had understood the task rules and some had not. No further discussion was given, because Forrest et al. (2014) believed that post-hoc oral reports were unreliable: "the ability to articulate a propositional rule does not mean that performance is being driven by it" (p 1021). Thus, they assumed that there were only two strategies: the task-switching strategy and the LUT approach, and participants who did not understand the task rules applied the second strategy. Similarly, Dreisbach et al. (2006, 2007) did not report any other strategy besides the task-switching strategy and the LUT approach. In the present experiment, however, when participants did not figure out the task rules, instead of applying the LUT approach, they assigned the eight cue-stimulus combinations to different groups and remembered them with different novel strategies. As I demonstrated in the results section, I picked up two meaningful novel strategies from the oral reports: the BW strategy and the CH strategy. Moreover, unlike Dreisbach et al. (2006, 2007) and Forrest et al. (2014), in this experiment participants who did not apply the task-switching strategy somehow still showed highly significant task-switching costs. Self-reports after the experiment suggested that these participants never realised that there were two different tasks. This is contrary to my previous prediction.

Secondly, regarding the strategy difference, the results of Experiment 2.1 suggested that participants in the experimental group who received LUT instruction had higher error rates than participants in the control group who received regular task-switching instructions. In particular, participants in the experimental group tended to have much higher error rates on incongruent trials (Mean = 18.7%) than participants in the control group (Mean = 7.83%).

However, notice that their error rates were still better than pure random guessing. This pattern remained significant after I removed participants who applied task-switching strategies. In short, I found that participants who applied the task-switching strategy had lower error rates than participants who applied novel strategies. One disadvantage of Experiment 2.1 is that the strategies I identified in the experimental group are all based on self-reports, which may not reflect the real strategies participants used. The existence of these newly identified strategies needs further confirmation in further experiments.

Thirdly, in line with my previous prediction, there were significant congruence effects in both the experimental and control groups. In the absence of task rules, humans (Forrest et al., 2014), monkeys (Stoet & Snyder, 2003; Caselli & Chelazzi, 2011) and pigeons (Meier et al., 2016) consistently showed congruency effects. The results of Experiment 2.1 agree with these studies.

2.5 Experiment 2.2

In contrast to Dreisbach et al., (2006, 2007) and Forrest et al. (2014), Experiment 2.1 found some unexpected task-switching costs when the participants reported that they were using novel strategies. Particularly, it is suggested that even when participants never realise there is a colour task and a shape task, and never know that they have to switch between two tasks, the task-switching costs remain significant. However, because Experiment 2.1 picked up participants' strategy only based on their oral reports, it is reasonable to suspect that these verbal reports cannot reflect the actual strategies they used, and they may still have used the standard task-switching strategy without realising it. Thus, the primary purpose of this experiment was to detect any behavioural differences between participants who were using novel strategies and participants who were using the task-switching strategy, and prove that different strategies do indeed exist.

Experiment 2.2 focuses on the BW strategy, because it is the most popular novel strategy in Experiment 2.1 and because the second-most popular strategy, the CH strategy, is very similar. One of the important features of the BW strategy is that participants who have applied this strategy can only react to the colour rule but not the shape rule. Instead, when a shape task trial showed up, they had to either merge it into the colour task or remember the exceptions. This strategy worked fine when there were only eight cue-stimulus combinations, but I hypothesised that if a novel shape-task stimulus suddenly showed up, the BW strategy would not provide the correct answer. However, as long as the novel shape-task stimulus follows the task rules, participants using the task-switching strategy can figure out the correct response based on their strategy. At the same time, because both strategies include a colour rule, both strategies can deal with a novel colour-task stimulus correctly (see Figure 2.6).

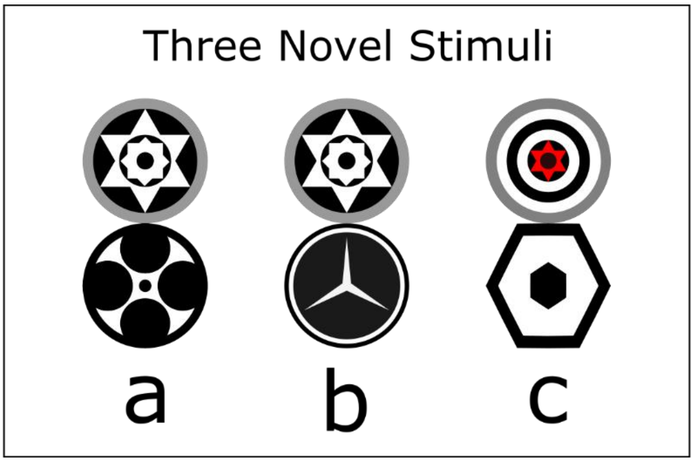

Figure 2.6. There are three novel stimuli. Figures (a) and (b) are novel shape task stimuli. If a participant used the BW strategy, I predicted that the participant would not find the correct response to (a) and (b), because the participant could apply only the colour task rule (white == *left*; black == right), and not the shape rule. Figure (c), on the other hand, is a novel colour task stimulus, and it follows the colour task rule (it is mainly white; so press left). Therefore, both the task-switching strategy and the BW strategy can provide the correct response.

To sum up, I made three predictions in Experiment 2.2. Firstly, I intended to replicate the results of Experiment 2.1. Therefore, I predicted that, without an explicit understanding of the task rule, participants would *still* indicate significant task-switching costs and congruency effects. Secondly, Experiment 2.1 has provided initiatory evidence to indicate that those novel strategies might cause higher error rates than the task-switching strategy. Therefore, I also predicted that novel strategies would cause higher error rates than the task-switching strategy. Thirdly, I predicted that, if the BW strategy did indeed exist, when I suddenly introduced novel shape-task stimuli, participants who believed that they were using such a strategy should have higher error rates than participants who were using the task-switching strategy, but both strategies should deal with novel colour-task stimuli equally accurately.

2.5.1 Method

Participants

A total of 26 adult students from the University of Glasgow participated in Experiment 2.2, aged between 20 - 30 (mean age = 23.5, SD = 2.27, female = 20). Each participant received £3 pounds for their participation.

Stimuli Apparatus, Tasks and Timeline

This experiment included two parts. For the first part, the stimuli and apparatus and tasks were identical with Experiment 2.1. However, the second part included 20 novel stimuli: 10 for the shape task and 10 for the colour task (see Figure 2.7). The task rules were identical for both the first and the second part.

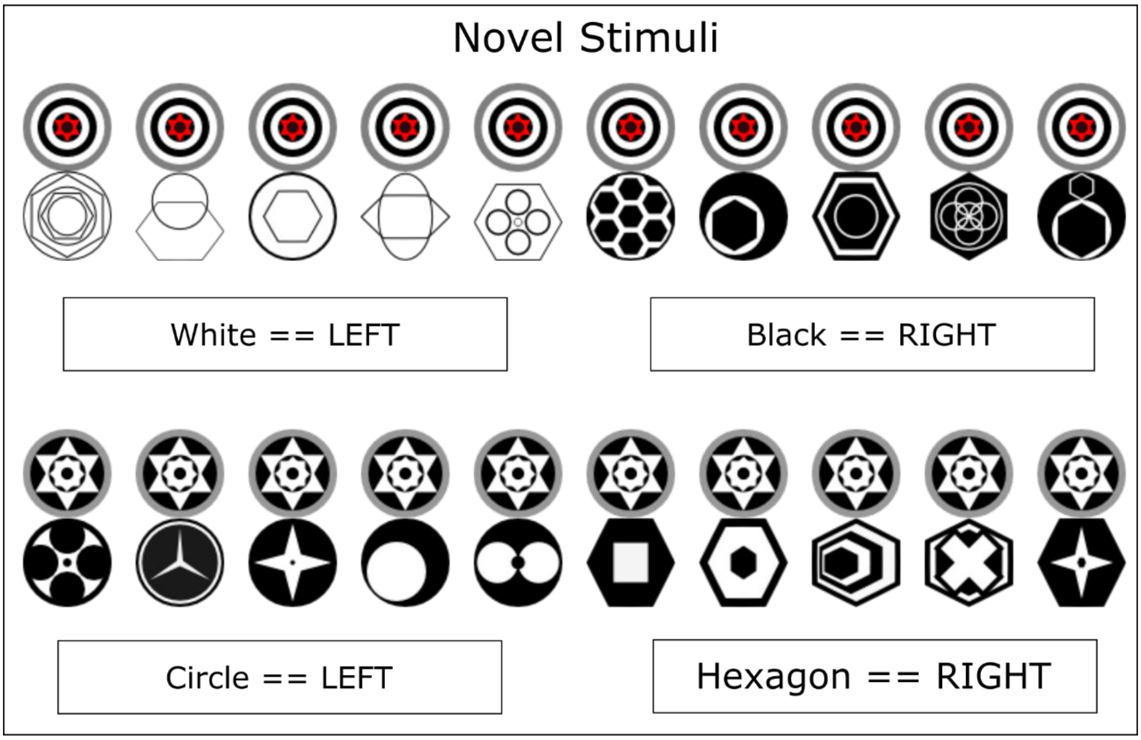

Figure 2.7. There are 20 novel stimuli (cue-stimulus combinations) from the second part of the Experiment 2.2. The ten figures in the top row belong to the colour task, and the then figures in the bottom row belong to the shape task.

In line with Experiment 2.1, Experiment 2.2 also used a composite task-cueing design. In each trial, the task cue and task stimulus appeared up and disappeared together, and there was no cue-stimulus interval. The ITI was 300 ms. The only difference is that, in the first part of this experiment, participants only had 2.5 seconds to make a response. Based on the RT results of Experiment 2.1, I found that a 5 second reaction time window is not necessary. Participants can make a decision within 2.5 seconds. In the second part of the experiment, participants still had 5 seconds to make a response. I gave participants some extra time in the second part of the experiment, because they were dealing with novel stimuli.

Feedback

The feedback in the first part of the experiment was almost identical with Experiment 2.1. If a participant could not make any response within 2.5 seconds, a timeout sign would show up and stay on the screen for 3 seconds. If the participant made a wrong response, a mistake sign would appear and remain on the screen for 3 seconds.

Procedure

Experiment 2.2 took place in a quiet and darkened psychology lab at the University of Glasgow. Participants were randomly assigned into two groups: the experimental group (N= 13) and the control group (N = 13).

Experimental group

At the beginning of this experiment, participants received paper-based instructions. On paper, all eight possible cue-stimuli combinations (2 cues × 4 target stimuli) were printed on either the left side or the right side. Participants were instructed that the four combinations on the left side corresponded to the *left* key, and the four combinations on the right side corresponded to the *right* key. Also, for the purpose of this experiment, the position of these eight combinations was specially arranged. Thus, it was also a direct illustration of the BW strategy. Participants just needed to remember two exception combinations and the remaining six combinations followed the simple colour rule (see Figure 2.8). The instructions induced participants to apply the BW strategy. They had one minute to remember these combinations and their corresponding keys. The instruction also informed participants that the experiment had two parts, and that, in the second part, all the stimuli would be novel. Each participant needed to finish the second part based on the strategy they used in the first part (i.e., the BW strategy).

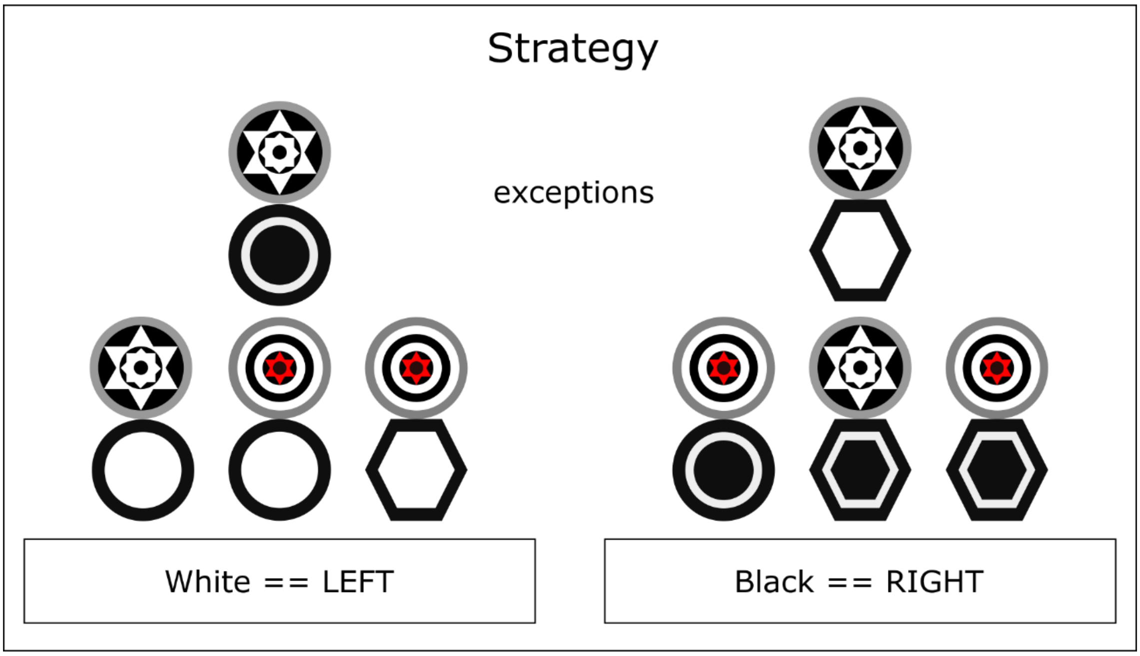

Figure 2.8. For participants in the experimental group, all eight combinations were listed in the paper instructions. This special arrangement is for the illustration of the BW strategy.

Firstly, participants needed to complete two 20-trial training blocks designed to help them remember the corresponding keys for all the combinations (the same as Experiment 2.1). After two training blocks, the participants needed to complete the first part of the experiment: four 75-trial experiment blocks. They were allowed to take a rest after each block and start the next block when they were ready. After that, they needed to complete the second part of the experiment: a 20-trial experiment block including ten colour task novel stimuli and ten shape task novel stimuli. The stimuli showed up randomly, but each stimulus only showed up once. After each participant finished the experiment, they were required to orally report whether they had used any strategy besides the BW strategy and to confirm whether they realised that there were two tasks (the colour task and the shape task) or not.

Control group

The experiment for the control group was almost identical with the experimental group. The only difference was the instructions they received. A task-switching instruction was provided, that explained the task rules, target stimuli, and task cues for making a correct response.. The instruction for the control group also mentioned that, in the second part of the experiment, all the stimuli would be novel. Each participant had to finish the second part based on the strategy they used in the first part (i.e., the task-switching strategy).

2.5.2 Results

Firstly, for the first part of the experiment, it was predicted that participants in both the experimental and the control groups would have significant task-switching costs and congruency effects. Secondly, it was predicted that the experimental group would have higher error rates than the control group. Thirdly, for the second part of the experiment it was predicted that the experimental group would have higher error rates than the control group on the novel shape-task stimuli, but not on the novel colour-task stimuli. The descriptive data is listed in Table 2.3 and illustrated in Figure 2.9.

Experimental Group and Control Group

Table 2.3

Mean (SD) of RT and Error Rate for Each Trial Condition and Participant Group

	Experimental Group		Control Group	
Trial/Group	RT ms (*SD*)	Error Rate (*SD*)	RT ms (*SD*)	Error Rate (*SD*)
Repeat Congruent	948 (170)	6.71% (5.1)	911 (182)	2.50% (2.6)
Repeat Incongruent	1045 (188)	12.8% (5.9)	955 (208)	8.11% (5.8)
Switch Congruent	1056 (156)	9.41% (7.5)	1029 (188)	3.89% (3.9)
Switch Incongruent	1080 (167)	16.4% (8.5)	1072 (192)	10.4% (8.0)
Repeat	996 (163)	9.88% (4.5)	932 (192)	5.40% (4.1)
Switch	1069 (153)	12.78% (7.3)	1074 (169)	7.14% (5.3)
Congruent	1011 (158)	5.69% (6.2)	977 (165)	3.30% (3.1)
Incongruent	1062 (173)	13.5% (6.5)	1018 (188)	9.35% (5.9)
Total	1035 (154)	11.5% (.057)	996 (174)	6.35% (.042)

A 2 × 2 × 2 ANOVA with mixed effects was conducted to analyse the RT difference between and within conditions. Two within-subjects factors were the trial transition (repeat, switch) and the congruency effect (congruent, incongruent). The between-subjects factor was the participant group (experimental group, control group). Two within-subject factors were significant: the trial transition [F (1, 24) = 42.22, p = .0001, η^2_p = .63] and the congruency effect [F (1, 24) = 8.32, p = .00812, η^2_p = .26]. The between-subject factor of participant group was nonsignificant [F (1, 24) = 0.384, p = .541].There were no significant interactions (p > .05).

An equivalent 2 × 2 × 2 ANOVA with mixed effects was conducted to analyse the error rate difference between and within conditions. Two within-subjects factors were significant: the trial transition [F (1, 24) = 7.845, p = .00991, η^2_p = .23] and the congruency effect [F (1, 24) = 47.00, p =.0001, η^2_p = .66]. The between subject factor of participant group was also significant [F (1, 24) = 6.71, p = .016, η^2_p = .25]. There were no significant interactions (p > .05; see Figure 2.9).

Figure 2.9. (a) The line graph displays the RT (top) and Error Rate (bottom) of each condition (switch, repeat; congruent [Con], incongruent [Inc]; Experimental group, Control group). The error bar indicates ±1 *SEM*. (b) The violin plots demonstrate RT distributions from the repeat and the switch conditions. The jittered dots inside each bean represent the average RTs of each participant. The black horizontal bar and the box around it represent the mean and the 95% CI of the mean in each condition, respectively. The responses slower or quicker than 95% are represented by dots above and below the error bars. (c) Similar violin plots demonstrate the RT distributions from the congruent and the incongruent conditions.

Novel Stimuli

To examine the error rate between experimental and control groups under the novel stimuli circumstance, a 2 × 2 ANOVA with mixed effects was conducted. The within-subjects factor was the task type (colour, shape), and the between-subjects factor was the participant group (control, experimental). See Table 2.4 and Figure 2.10 for the mean error rate of each condition).

Table 2.4

Mean (SD) of Error Rates for Tach task and Participant Group

Group/Task	Colour Task	Shape Task	Total
Error Rate	% (*SD*)	% (*SD*)	% (*SD*)
Experiment Group	13.7 (15.5)	42.1 (13.1)	27.6 (8.1)
Control Group	3.07 (6.3)	7.69 (9.2)	5.38 (5.2)

The within-subjects factor of task types [F (1, 24) = 19.55, p = .0002, η^2_p = .45] and the between-subjects factor of participant group were significant [F (1, 24) = 73.7, p = .0001, η^2_p =.75]. The interaction between these two factors was also significant [F (1, 24) = 10.*13*, p =.004, η^2_p = .29].

Post-hoc pairwise comparisons with the Bonferroni correction suggested that the experimental group had a significantly higher mean error rate than the control group in the shape task (p = .0001). However, in the colour task, the two participant groups had no significant differences (p = .14; see Figure 2.10).

Moreover, in the control group, the mean error rate on the shape task was not significantly better than chance [χ^2 (1, N = 130) = 3.39, p = .065]. However, the mean error rate on the colour task was better than chance [χ^2 (1, N = 130) = 66.5, p = .0001].

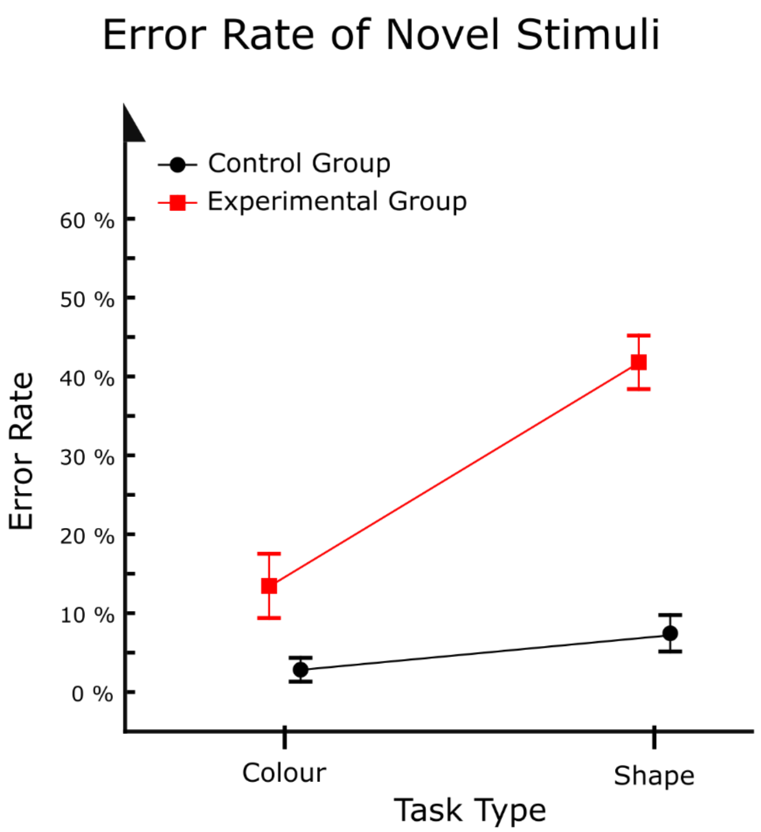

Figure 2.10. The line graph displays the Error Rate of novel colour and shape stimuli in the experimental and the control groups. The error bar indicates ±1 *SEM*.

2.5.3 Discussion

In line with the prior predictions, the results showed that participants in both the experimental and the control groups had significant task-switching costs and congruency effects. Moreover, the experimental group had a higher error rate than the control group. Finally, participants from the experimental group had higher error rates than the control group on the novel shape-task trials. However, participants from both groups had similar error rates on the novel colour-task trials.

In fact, on average, participants from the experimental group could not even perform significantly better than the chance level in those novel shape-task trials. This observation leads to two highly possible deductions. Firstly, they did not understand the shape rule. Furthermore, for them, applying the task-switching strategy was impossible. The error rate results of the novel stimuli trials also confirm that the novel BW strategy I picked up from Experiment 2.1 truly exists. I had not tested the CH strategy. However, both novel strategies share a similar principle: the remembering of only two exceptions, while the remaining six cue-stimulus combinations follow a simple rule. I suggest that the CH strategy was also employed but this is something that needs to be tested in future experiments. In agreement with Experiment 2.1 the present experiment confirms that even when participants did not realise that they had to switch between two tasks, task-switching costs remained significant.

The control group had a lower mean error rate than the experimental group. Forrest et al. (2014) reported a similar error-rate pattern. In their study, participants who were using the task-switching strategy had lower error rates than participants who had not realised the task-switching strategy. The difference is that Forrest et al. (2014) did not discussed any specific novel strategies that the participants applied during the experiment. This is because they believed that the oral report was not completely reliable. They assumed their participants applied the LUT approach. The present experiment compares task-switching strategy with a particular novel strategy (i.e., the BW strategy).

2.6 General Discussion

Previous studies have proposed that it is possible for human subjects to apply the LUT approach and eliminate task-switching costs (Dreisbach et al., 2006, 2007) or at least reduce them to a nonsignificant level (Forrest et al., 2014; see their Experiment 2). The results of Experiments 2.1 and 2.2, however, reject this explanation on two different accounts. Firstly, in Experiments 2.1 and 2.2, participants used strategies that were different from the task-switching strategy, but not a single participant used the LUT approach. Of course, at this point, we cannot fully conclude that it is impossible for human participants to apply the LUT approach in visual tasks. However, it is safe to suggest that the LUT approach is at least not the prioritised strategy for us. I will further discuss the LUT approach in the next chapter.

Secondly, in Experiments 2.1 and 2.2, amongst those participants who were using novel strategies such as the BW strategy, task-switching costs were still highly significant. Importantly, in line with Dreisbach et al. (2006, 2007) and Forrest et al. (2014), the oral reports from Experiments 2.1 and 2.2 also suggested that participants who were using novel strategies never realised that they were switching between tasks. Furthermore, in Experiment 2.2, the error rate results of the novel stimuli block further indicate that applying the task-switching strategy is very unlikely for participants in the experimental group.

2.6.1 Strategies and Functions in Task-switching Experiments

Since the results of Experiment 2.2 provided clear evidence that the novel strategy I found in Experiment 2.1 was more than a subjective oral report, I can be certain that the comparison between novel strategy and task-switching strategy is valid. In line with the findings of Forrest et al. (2014), I found that in comparison to other strategies, the task-switching strategy allows participants to react more accurately and allows participants to response to novel stimuli better than chance. Therefore, the task-switching strategy is better than other strategies when participants are presented with bivalent stimuli in a task-cueing paradigm.

However, Dreisbach et al. (2006, 2007) drew a different conclusion. Using univalent stimuli, they found that participants who were using the LUT approach performed as accurate as participants who were using the task-switching strategy. In their study, they found no evidence that the task-switching strategy was better than the LUT approach. In contrast, they found that the reaction time was slightly quicker if participants used the LUT approach. Does the stimulus type (univalent or bivalent) cause this difference? I will address this question in the next Chapter.

2.6.2 Animal Task-switching

Previous animal studies have suggested that monkeys and pigeons can perform the task-switching experiment without showing any significant task-switching costs (Stoet & Snyder, 2003; Avdagic, et al., 2013; Castro & Wasserman, 2016; Meier et al., 2016). One possible explanation is that, because they never received any oral or paper based task-switching instructions, the animals applied unique strategies that differ from the human task-switching strategy. Those strategies helped them to eliminate task-switching costs, because they did not need to switch between tasks. Additionally one potential alternative strategy is the LUT approach (Dreisbach et al., 2006, 2007; Forrest et al., 2014; Meier et al., 2016).

However, Experiments 2.1 and 2.2 challenges this explanation. My results showed that firstly although other novel strategies exist, they cannot help human participants to eliminate the task-switching costs. Moreover, no participant applied the LUT approach despite it being simple and straightforward. Dreisbach et al. (2006, 2007) and Forrest et al. (2014) used linguistic tasks and mathematical tasks. In contrast, the present study applied two visual tasks which were close to the previous monkey studies. My experiments are more similar to previous animal studies (i.e., Stoet & Snyder, 2003; Avdagic, et al., 2013; Castro & Wasserman, 2016; Meier et al., 2016). Therefore, the results of Experiments 2.1 and 2.2 can make a more valuable comparison between the task-switching costs of humans and animals. I suggest that novel strategy (the LUT approach in particular) is not a perfect explanation for animal behaviours in task-switching experiments. In Chapter 6, the present dissertation will continue to explore how animals can eliminate task-switching costs.

2.6.3Unexpected Task-switching Costs

Why do participants indicate task-switching costs without realising that they have to switch between tasks? In Chapter 1, I have introduced modern studies which have suggested that it is the process of task-set reconfiguration or the proactive interference effect (or both) which lead to task-switching costs. Based on these information, the present study raises two possible hypotheses.

Switching and Task-sets

Firstly, it is possible that even if participants are unaware that the tasks are based on the task-switching strategy, they still identify two different novel tasks. Take the BW strategy as an example (see Figure 2.4). The intention of this strategy is to remember the response for only two cue-stimulus combinations (the exceptions), while the other six combinations can be quickly determined by their colour. Since all combinations belong to the colour task, they can still be determined by the colour rule (i.e., white = *left*; black = *right*). Perhaps the participants still treat those combination as the colour task, with or without realising it. Importantly, both exceptional combinations belong to the shape task. Thus, once the participants perceive a shape cue, they have to decide whether the combination is one of the exceptions first. If it is,, then they should retrieve the correct response from memory, or apply a "reversed" colour rule (black = *left*; white = *right*). If the combination is not an exception, then they would apply the "normal" colour rule (white = *left*; black = *right*; see Figure 2.4). In short, although the participants never realised that there is a colour task, and never performed a shape task, the strategy they used nevertheless treats the four cue-stimulus combinations with the colour cue and the other four cue-stimulus combinations with the shape cue as two different tasks. As a result, the novel strategy can trigger a novel task-set reconfiguration process and create switching costs. A similar argument can explain the CH strategy as well.

Switching and Interference

Secondly, I hypothesise that it is possible that, when participants applied a novel strategy, the conventional task-set reconfiguration process based on task-switching strategy was eliminated. However, I hypothesise that the proactive interference still exists after we rule out the task-set reconfiguration process. Furthermore, the task-switching costs are inescapable as long as the proactive interference exists. In other words, I hypothesise that the proactive interference and task-switching can be two independent events. Therefore, after controlling the task-switching strategy, it is the interference from the previous trial, not the switching between tasks that causes the switching cost.

Allport et al., (1994) have already suggested that the interference is a result of the direct competition between the stimulus-response mapping from the previous trial (*n - 1*) and the current trial (*n*). In spite of this, in their experiments, such competition coexisted with the notion of task-sets, because their participants received a task-rule based instruction. In a task-switching experiment, we are not sure whether the stimulus alone, without actual switching between tasks, would trigger the interference. The following Chapters intend to examine this very question.

2.6.4 A Preview of the Following Chapters

I sought to explain the task-switching costs when participants do not apply the task switching strategy. In the following Chapters (Chapter 3, Chapter 4 and Chapter 5) I will test two potential explanations from the above section. There are other possible explanations based on the compound retrieval account (for a review see Logan & Schneider 2010) and the associative learning account (e.g., Forrest et al., 2014; Meier et al., 2016). Those explanations will be discussed in Chapter 7. I also suspect that the disagreement between the present chapter and previous studies (i.e., Dreisbach et al., 2006, 2007; Forrest et al., 2014) was caused by the difference between bivalent and univalent stimuli or/and the difference between semantic and perceptual tasks. I will examine these assumptions in Chapter 3 and Chapter 4.

Chapter 3: Bivalent Stimuli and Task-switching Costs

The present study continues to explore task-switching effects: the costs when humans alternate between tasks. Dreisbach et al. (2006, 2007) suggested that participants are able to apply a "Look-up Table" (LUT) approach by remembering all the stimulus-response mappings without realising the existence of two competing tasks. As a consequence, they performed a task-switching experiment without any task-switching costs because they never switched between tasks. It was suggested that the LUT approach might also explain animals' outstanding performances in task-switching experiments. Though Experiments 2.1 and 2.2 were intended to replicate Dreisbach et al. (2006, 2007), the results, however, indicate two strong disagreements.

Firstly, in Experiments 2.1 and 2.2, no participant applied the LUT approach. Instead, participants developed novel rule-based strategies. Secondly, Experiments 2.1 and 2.2 suggested that even without applying the task-switching strategy, participants still demonstrated significant task-switching costs. One possibility is that different stimulus types caused the disagreements between Experiments 2.1 and 2.2 and previous studies: Experiments 2.1 and 2.2 included bivalent stimuli, whereas Dreisbach et al. (2006, 2007) used univalent stimuli. The present study first focused on the difference between the bivalent stimuli and the univalent stimuli. There is also a difference between task types: Experiments 2.1 and 2.2 applied visual tasks, whereas previous studies (Dreisbach et al., 2006, 2007) used semantic tasks. The difference between task types was addressed in section 3.6.3.

Although Forrest et al. (2014) in a recent study demonstrated that eliminating task-switching costs in an experiment with bivalent stimuli is possible, their results were inconclusive for two reasons. Firstly, the RT difference between switching trials and repeating trials was almost significant (F (1, 15) = 3.39, p = .086, η^2_G=.00372; see their Experiment 2). Secondly, Forrest et al. (2014) successfully removed the task-switching costs in their Experiment 2. However, in Experiments 1 and 3, they reported significant task-switching costs when participants believed they were using the LUT approach.

The present study sought to explore whether univalent stimuli are truly the minimum requirement for eliminating task-switching costs by using the LUT approach. In other words, I tested under which conditions we could replicate the result from the previous study——eliminating task-switching costs by ruling out the task-switching strategy and remembering all the stimulus-response mappings (e.g., Dreisbach et al., 2006, 2007).

3.1 Univalent Stimuli and Bivalent Stimuli

In a task-switching experiment, bivalent stimuli are stimuli that provide information for more than one tasks. Conversely, univalent stimuli are stimuli that provide information for only one task. All stimuli in Experiments 2.1 and 2.2 were bivalent. For example, a black circle conveys *colour information* for the colour task (black) and also *shape information* for the shape task (circle). However, imagine if a black character triplet were presented in a colour task trial during Experiments 2.1 or 2.2 (e.g., "£££"). This would be a univalent stimulus because, it provides colour information (the triplet is written in black), but no shape information.

Moreover, Meiran (2014) suggested that it is important not to confuse univalent/bivalent stimuli with "dimensions". It is possible to design univalent multidimensional stimuli. For example, it is possible to present univalent coloured shapes (say a red triangle) in experiments involving shape and colour tasks like Experiments 2.1 and 2.2. However, in Experiments 2.1 and 2.2, the shape task included only a circle and a hexagon, and a triangle was not among the shapes in this shape task. Therefore, in Experiments 2.1 and 2.2 if a red triangle was onset, it would still be a univalent stimulus. In fact, previous literature has suggested that, while bivalent stimuli delay participants' reaction times, the presence of irrelevant target dimensions does not increase switching challenges (Meiran et al., 2012; Rubin & Meiran, 2005). All stimuli in the present chapter are authentic bivalent stimuli.

The stimuli in Dreisbach et al. (2006, 2007) are somewhat ambiguous. In their study, the two tasks were Vowel/Consonant discrimination (decide whether the first letter is a vowel or a consonant), and Animal/Non-animal discrimination (decide whether a word means an animal or non-animal). Some might argue that those stimuli were bivalent, because it is possible for an animal word to start with a vowel; so, for example, the word “owl” can be included in both tasks. However, the problem is that, when the animal word “owl” appears in an Animal/Non-animal task trial, it is the semantic representation of the bird, which conveys no syllabic information. When the same word “owl” onsets in a Vowel/Consonant task trial, it is merely a syllable, which conveys no animal information. In other words, we cannot determine whether an animal (say “owl”) is a vowel “owl” or a consonant “owl”, or decide whether a vowel (say “c”) is an animal “c” or non-animal “c”. In contrast, in Experiments 2.1 and 2.2 because, both tasks were visual tasks (colour task, shape task), participants were able to decide whether a black figure was a circle, and/or whether a hexagon was white. For the sake of simplicity, I therefore follow previous suggestions (Forrest et al. 2014; Meier et al., 2016) that the stimuli in Dreisbach et al. (2006, 2007) are univalent.

3.1.1 Stimulus-set and Stimulus-response Mapping

The other factor that may be confounded with bivalent-univalent difference is the number of stimulus-sets. For univalent stimuli, because each stimulus can only convey the information of one task, each task needs to have a unique stimulus-set. In other words, a univalent stimulus can only show up in one task. Thus, a univalent stimulus can only lead to one correct response and has only one stimulus - response mapping. For example, in the colour task, one might have a black triplet “$$$” as a stimulus, but it cannot show up in a shape task trial, as it has no overt geometric form. Its response is unique during the experiment.

However, a bivalent stimulus can be included in two tasks. As a result, two tasks can share the same stimulus-set. For example, in Experiments 2.1 and 2.2, the colour task and the shape task shared the same stimulus-set, which included four stimuli: a black circle, a white circle, a black hexagon and a white hexagon. Furthermore, sometimes one bivalent stimulus can lead to different responses in different tasks (i.e., the incongruent stimulus). For instance, in Experiments 2.1 and 2.2 a black circle is “black” in the colour task, and the correct response for it is the *right* key. It is also a “circle” in the shape task, and the correct response is the *left* key. The same black circle can lead to distinct responses according to the tasks. Hence, it has two stimulus-response mappings.

In short, a task-switching study using univalent stimuli must have two stimulus-sets, because each task will have one unique stimulus-set; whilst using bivalent stimuli can (but not obligatorily) include only one stimulus-set, which means both tasks share the same stimulus-set. Moreover, univalent stimuli can have only one stimulus-response mapping, but bivalent stimuli may potentially have two stimulus-response mappings.

Conventional bivalent task-switching studies usually include only one stimulus-set. Therefore, the incongruent stimuli would have two stimulus-response mappings and the congruent stimuli would have only one stimulus-response mapping. This set-up also allows us to examine the congruency effect in the task-switching paradigm. Incongruent stimuli tend to cause a longer reaction time and a higher error rate than congruent stimuli (e.g., Monsell & Mizon, 2006; Rogers & Monsell, 1995; Meiran & Kessler, 2008; Kessler & Meiran, 2010; Schneider, 2015). Although very unusual, it is still possible to include two bivalent stimulus-sets in a task-switching study.

To sum up, the difference between univalent and bivalent stimuli is the information: univalent stimuli have information of one task, bivalent stimuli have information of two tasks. However, the number of stimulus-sets may be confounded with the difference between univalent and bivalent stimuli because bivalent task-switching experiments usually employ a shared stimulus-set.

3.1.2 Bivalent Stimuli and the Task-switching Effect

Apart from task-switching costs, the bivalent effect itself is also an interesting cognitive effect. Participants tend to react more slowly on bivalent stimuli than to univalent stimuli. In fact, performances on univalent stimuli become worse when a few bivalent stimuli very infrequently show up amongst them, which is called the “bivalent effect” (Woodward, Meier, Tipper, & Graf, 2003; Meier, Woodward, Rey-Mermet & Grafm 2009; Metzak, Meier, Graf & Woodward, 2013).

The present study, however, focused on how bivalent stimuli make an impact upon task-switching cost. Previous studies have demonstrated that when the stimuli were univalent, the task-switching costs were small (e.g., Allport & Wylie, 2000; Jersild, 1927; Rogers & Monsell, 1995; Wylie & Allport, 2000). However, if the stimuli were bivalent, the task switching costs were relatively large. Both the task-reconfiguration account (e.g., Monsell, Yeung & Azume, 2000; Monsell & Mizon, 2006; Roger & Monsell, 1995) and the proactive interference task account (Allport and Wylie 1999; Wylie & Allport, 2000; Allport et al., 1994; Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006) can explain the difference between bivalent task-switching effect and univalent task-switching effect.

Task-set Reconfiguration

Woodward et al. (2003) proposed an interesting difference between univalent and bivalent stimuli. Theoretically, in a task-switching experiment with univalent stimuli, participants only need two pairs of IF-THEN arguments to figure out the correct response. For example, (1) if colour == white then press *left*, if colour == black then press *right*; (2) if shape == circle then press *left*, if shape == hexagon then press *right*. However, when a bivalent stimulus such as a black circle shows up, participants do not know which pair of arguments is needed, to solve this problem; participants need an additional pair of IF-THEN arguments. For example, if cue == colour then applies the first pair of arguments and if cue == shape, then applies the second pair of arguments. According to the task-reconfiguration perspective, the additional pair of IF-THEN arguments in bivalent stimuli makes the task-set more complicated than the task-set for univalent stimuli. Therefore, during task-switching, bivalent stimuli require more cognitive resources and causes more processing time for task-reconfiguration.

Proactive Interference

Proactive interference account can also explain the difference between univalent and bivalent stimuli. Firstly, according to previous studies, using bivalent stimuli, proactive interference might have two sources. 1) It is possible that proactive interference derives directly from the competition of stimulus-response mappings of the previous trial (Allport et al., 1994; Allport & Wylie, 2000). 2) The interference may also come from stimulus-task-set associations in the immediately preceding trial (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). However, univalent stimulus always has the same stimulus-response mapping, and this stimulus-response mapping never has any direct competition. Hence, it is possible that, for univalent stimuli, the interference can only derive from stimulus-task-set associations. Consequently, the task-switching costs from univalent stimuli are smaller than the costs from bivalent stimuli.

Feature-Response Mapping

Also based on the proactive interference account, Woodward et al. (2003) proposed that a univalent stimulus elicits only one feature-response mapping that is unique to that stimulus, whereas, with bivalent stimuli, each elicits two feature-response mappings that may overlap the mapping of another stimulus. When working with bivalent stimuli, the task performed on *trial n - 1* requires activating one mapping (e.g., the one for naming the colour) while at the same time inhibiting or suppressing another mapping (e.g., the one for making shape decisions). If the inhibited or negatively primed mapping is relevant on *trial n*, however, additional time is required to reactivate it, and this results in a switching cost. Woodward et al. (2003) suggested that such a negative priming notion is the core of the proactive interference account. Additionally for univalent stimuli, this type of negative priming is less likely to occur. Consequently, the bivalent stimuli produce larger task-switching costs. It is not a novelty to suggest that a feature-response mapping may impact participants' reaction times. Hommel (1998), for example, has already indicated that, if the feature-response mappings between *trial n - 1* and the *trial n* are mismatching, the reaction time of the current trial can be delayed.

3.2 Goals and Hypotheses

The present study explored whether univalent stimuli are truly the minimum requirement for bypassing task-switching costs with the LUT approach. In fact, the difference between univalent stimuli and bivalent stimuli is an important aspect of task-switching studies. Thus, the result of the present study helps us to draw further conclusions about the mechanism of task-switching. In particular, at the end of Chapter 2, I hypothesised that task-switching costs may have at least two independent origins: task-set reconfiguration and proactive interference. In addition, task-switching costs are inescapable as long as the interference exists. In the present study, I sought to test those hypotheses.

3.3 Experiment 3.1

In the present experiment, I sought to test whether and under which condition the LUT approach can successfully eliminate task-switching costs. In other words, I investigated the minimum requirement to eliminate task-switching costs. Biased on the divergence between the previous studies (Dreisbach et al., 2006, 2007) and Experiments 2.1 and 2.2, I proposed two potential factors that might stop participants from bypassing the task-switching costs. The first factor is the stimulus type (univalent or bivalent). The second factor is the number of stimulus-sets: a task switching experiment might include two separate stimulus-sets or one shared stimulus-set. This is sometimes confounded with the first factor (see Figure 3.1).

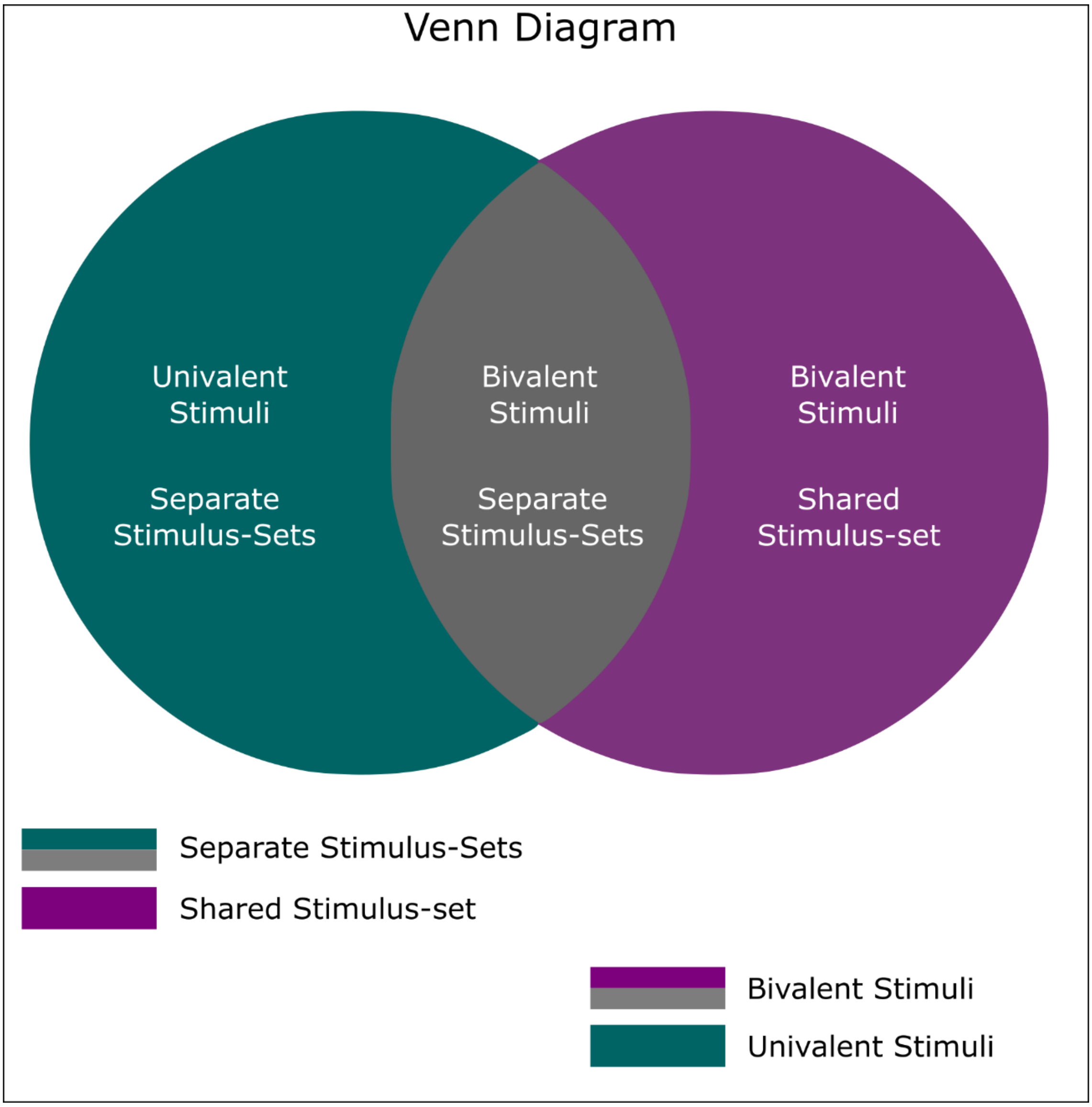

Figure 3.1. All task-switching studies with univalent stimuli must have two separate stimulus-sets (one set for each task). Studies with bivalent stimuli, however, can use one stimulus-set which is shared by two tasks. Nevertheless, it is still possible for studies with bivalent stimuli to include two stimulus-sets. Thus, studies with univalent stimuli is one sub-aggregate of studies with separate stimuli-sets.

As illustrated in Figure 3.1, the univalent stimuli is a sub-aggregate of the separate stimulus-sets. To ascertain the minimum requirement to bypass task-switching costs, we need to test the entire aggregate first. The present experiment, therefore, examined whether the number of stimulus-sets could impact task-switching costs when participants were using the LUT approach. In particular, I applied the same pair of visual tasks from Experiments 2.1 and 2.2: the colour task (whether a stimulus is *mainly* black or white) and the shape task (whether a stimulus is *mainly* a circle or a hexagon), and I induced participants to apply the LUT approach. However, this time, the experiment included two separate bivalent stimulus-sets. In other words, each task had a unique bivalent stimulus-set and, for each stimulus, there was only one stimulus-response mapping. I predicted that, in the present experiment, if participants applied the LUT approach, they would not indicate significant task-switching costs.

The results of Experiments 2.1 and 2.2 suggest that the task-switching strategy allows participants to react more accurately than other strategies. In addition, the results of Dreisbach and colleagues' (2006, 2007) studies show that participants who applied the task-switching strategy responded slower than participants who applied the LUT approach. These results imply that participants who apply different strategies may have different response patterns. Thus, in Experiment 3.1, I sought to explore the functional differences between different strategies.

To sum up, experiment 3.1 had two predictions. Firstly, I predicted that, if participants applied the LUT approach in the present experiment, they would not indicate any significant task-switching costs. Secondly, I predicted that either participants' overall reaction time or error rate would reflect significant differences when they applied different strategies.

3.3.1 Method

Participants

Students from the University of Glasgow participated in this experiment (N = 15, female

= 11; mean age = 24, *SD* = 3.7, range = 21-37). Each student received £3 for their participation.

Apparatus and Stimuli

All stimuli were presented centrally on a BenQ computer monitor (24 inches). A Black Box Toolkit response pad was used to record participants' responses. Participants also used a QWERTY keyboard to go through the instructions and to start the experiment. There were eight target stimuli: each task had four stimuli, and all stimuli were bivalent (see Figure 3.2). The size of all stimuli was 16.93 mm × 16.93 mm. There were two task cues (sized 33.86mm × 33.86 mm; see Figure 3.2). In order to reduce eye strain, the screen background was light green (RGB: 200, 255, 200).

Notice that, in this experiment, the colour task stimuli were only displayed in a colour trial and the shape task stimuli were only displayed in a shape trial. However, the two stimulus-sets were interchangeable. Because the stimuli were bivalent, the colour task stimuli could theoretically show up on a shape task trial, and vice versa. Participants could always obtain the correct response by following the task rules correctly.

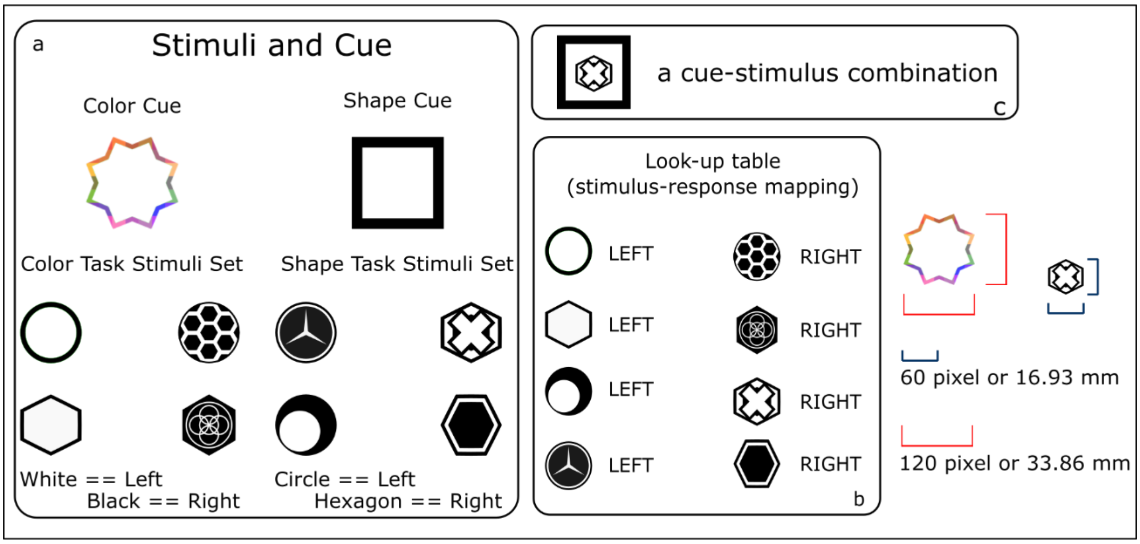

Figure 3.2. Stimuli, Cue and task rules of Experiment 3.1 are summarized here. (a) The four colour stimuli are only presented in the colour task, and the four shape stimuli are only presented in the shape task. (b) The LUT shows eight stimulus-response mappings. Four stimuli correspond to a *left* key, and four to a *right* key response. One can obtain the correct answer either by applying the task rules or remembering the LUT. (c) An example of a cue-

stimulus combination.

Task and Timeline

In this experiment, participants learned to perform two tasks: the colour task and the shape task. For the colour tasks, participants needed to determine whether the colour of a stimulus was mainly black or mainly white (white == press the left key; black == press the right key). For the shape task, participants needed to determine whether the shape of a stimulus was mainly a circle or mainly a hexagon (circle == press the left key; hexagon == press the right key; see Figure 3.2). The present experiment used a composite design. On each trial, the task cue and task stimulus appeared and disappeared together in the centre of the screen as a cue-stimulus combination (e.g., see Figure 3.2c). Once the stimulus was visible, a response had to be made within 2.5 seconds, or a timeout occurred. The cue-stimulus combination would disappear immediately after a response was made; otherwise, it would stay on the screen for 2.5 seconds (maximum reaction time). The inter-trial interval (ITI) was 300 ms.

Feedback

On each trial, if a correct response was made, the next trial would start automatically after the inter-trial interval. If an incorrect response was made, the text message "Mistake" would show up and stay on the screen for 3 seconds before disappearing. After that, the next trial would start immediately. If no response was made within 2.5 seconds, the text message "Timeout" would be displayed and stay on the screen for 3 seconds before disappearing. After that, the next trial would start immediately.

Different Stages

In this experiment, I tried to compare the functional difference between different strategies in a within-subjects design. Therefore, it included three experiment stages and each stage had a different instruction to induce participants to apply different strategies.

Stage 1 (training stage)

In this stage, participants were instructed that there were eight figures (the target stimuli). The instructions stated that the four figures on the *left* side of the screen corresponded to *left* key, and the four figures on the right side corresponded to the *right* key (see Figure 3.2b). Participants had to remember the figures and their corresponding keys. When the experiment started, on each trial, one of the target stimuli would randomly show up in the centre of the screen. In Stage 1, participants needed to finish a 64-trial training block (Figure 3.3a).

Notice that, in Stage 1, the task cues did not show up. Stimuli in this experiment all have a unique stimulus-response mapping. Thus, a cue was not necessary, because the participant should be applying the LUT approach. The data from Stage 1 was not included in the analysis.

Stage 2

In this stage, participants were instructed that every time a figure was shown, an “interference frame” would be shown at the same time. Importantly, the instruction stated that these interference frames were completely meaningless, and had nothing to do with providing a correct response. However, these interference frames were the task cues. Therefore, the design in Stage2 was the same as in a typical task-cueing experiment. The only difference was that the participants did not receive any rule-based instructions. Participants had to complete two 100 - trial blocks (see Figure 3.3b).

Stage 3

Just as in Stage2, in the Stage3, the target stimulus and the task cue were displayed together. This time, however, each participant was instructed that the "interference frames" were actually task cues. Also, the task rules were introduced and explained. As a consequence, participants could either use a task-switching or an LUT approach. Participants completed two 100 - trial blocks (Figure 3.3b).

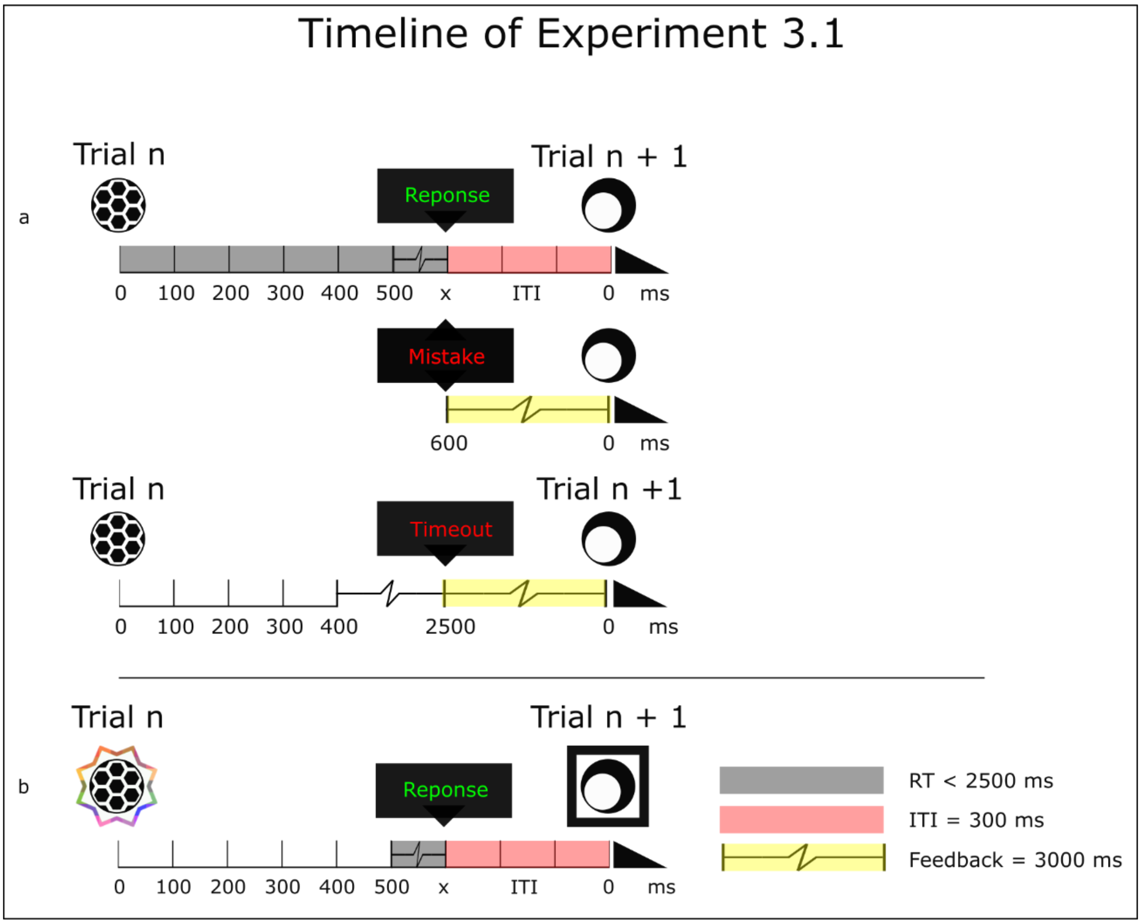

Figure 3.3. The timelines of each stage. (a) The timeline of training stage (Stage 1), the stimuli disappears once a response has been made. A wrong response triggers feedback. If no response has been made within 2.5 seconds, a timeout feedback onsets. (b) The timelines of Stage 2 and Stage 3 are almost identical with the training stage (Stage 1). However, on each trial, a task cue onsets simultaneously with the stimulus.

Procedure

The participants were asked to sign the consent form and sit in front of the computer screen (viewing distance around 40-60 cm). Before they started, they needed to read a general instruction about Experiment 3.1 on the screen. After that, they needed to complete the three experiment stages in order. An instruction showed up on the screen before each stage. After reading the instruction, participants needed to press the QWERTY keyboard to start the stage. After they had accomplished all three stages, they needed to orally report the strategy they had applied during the experiment. After that, they received £ 3.

3.3.2 Results

On the one hand, it was predicted that participants in Stage 2 would not indicate significant task-switching costs, but participants in Stage 3 would indicate significant task-switching costs. On the other hand, it was predicted that participants' reaction times or error rates would reflect a significant difference between Stage 2 and Stage 3. The summary of behavioural data for each condition is listed in Table 3.1 and illustrated in Figure 3.4. Stage 1 is a training stage, which was not included in the data analysis.

Table 3.1

Mean (SD) of RT and Error Rate for Each Trial Condition and Stage

Conditions	RT ms (*SD*)	Error Rate (*SD*)
Stage2 Repeat	539 (63)	2.42% (3.6)
Stage2 Switch	550 (69)	2.50% (3.3)
Stage3 Repeat	725 (225)	2.23% (2.7)
Stage3 Switch	839 (275)	3.48% (3.5)
Repeat	632 (188)	2.33% (3.4)
Switch	695 (246)	3.00% (3.1)
Stage2	545 (66)	2.48% (3.3)
Stage3	792 (252)	2.97% (2.9)

Stage 2 and Stage 3

A 2 × 2 ANOVA with repeated-measurements was conducted to compare the mean RTs within conditions. The two factors were the trial transition (switch, repeat) and the instruction (Stage 2: look-up table instruction; Stage 3: rule-based instruction). The factor of trial transition [*F* (1, 14) = 19.78, *p* = .0006, η^2_p = .59] and the factor of instruction [*F* (1, 14) = 19.52, *p* = .0006, η^2_p = .58] were both significant.

The interaction between trial transition and instruction was significant [*F* (1, 14) = 16.45, *p* = .0012, η^2_p = .54]. Post-hoc pairwise comparisons with the Bonferroni correction suggested that the trial transition effect of 113 ms was significant in Stage 3 (*p* = .0041). However, the trial transition effect of 11 ms in Stage 2 was not significant (*p* = .256).

A corresponding 2 × 2 ANOVA with repeated-measurements was conducted to compare the mean error rates within conditions. The two factors were the trial transition (switch, repeat) and the instruction (Stage 2: look-up table instruction; Stage 3: rule-based instruction). There were no statistically significant results although the factor of instruction was approaching significant [*F* (1, 14) = 4.06, *p* =.064]. The factor of trial transition [*F* (1, 14) = .48, *p* = .497] and the interaction between trial transition and instruction [*F* (1, 14) = 2.25, *p* = .156] were far from significant.

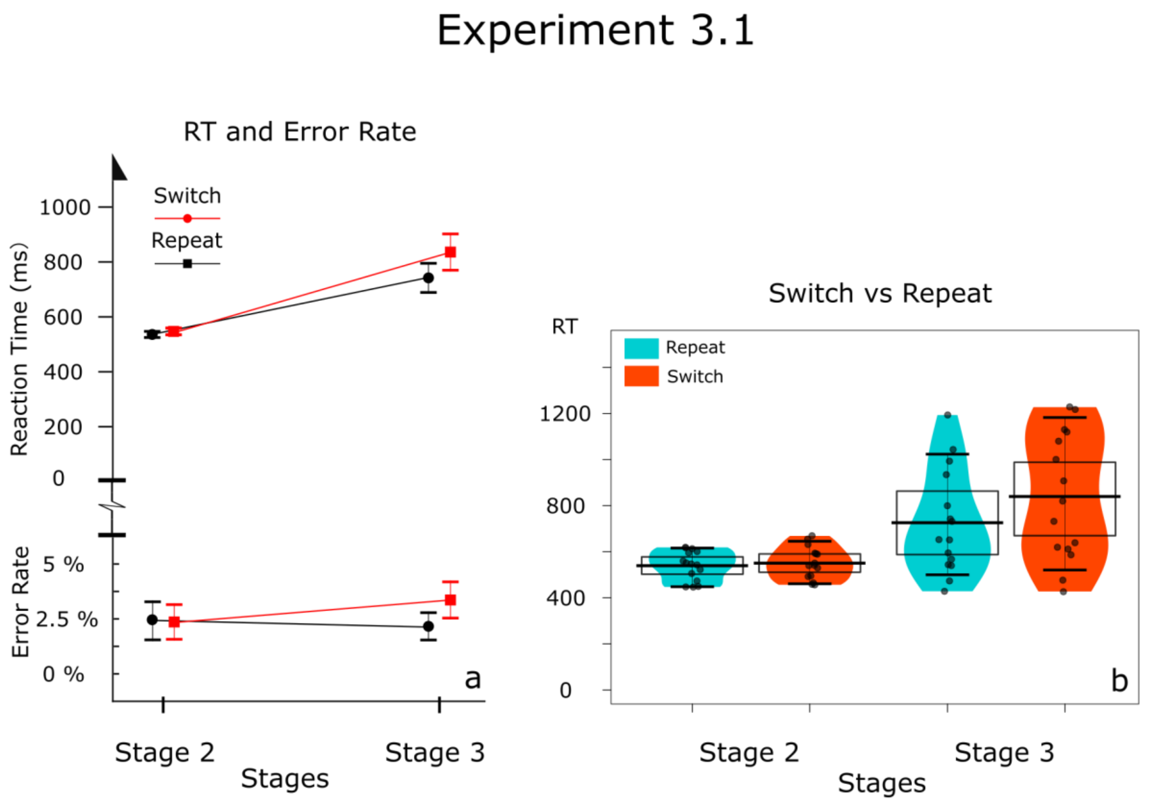

Figure 3.4. (a) The line graph shows mean RTs (top) and Error Rates (bottom) for each trial

condition (switch, repeat) and stage (Stage 2: look-up table instruction; Stage 3: rule-based instruction). The error bar indicates ±1 *SEM*. Data from Stage 1 (training stage) is not included. (b) The violin plots show the RT distributions in the repeat and switch conditions. The jittered dots inside each bean represent the average RTs of each participant. The black horizontal bar and the box around it represent the mean and 95% CI of the mean in each condition, respectively. The responses slower or quicker than 95% are represented by dots above and below the error bars.

3.3.3 Discussion

In an attempt to replicate the results of previous studies (Dreisbach et al., 2006, 2007), in this experiment, participants were instructed to apply both the LUT approach (Stage 2) and the task-switching strategy (Stage 3). The results suggest that, after receiving the rule-based instruction, the participants produced significantly more task-switching costs in Stage 3 than in Stage 2 (113 ms vs 11 ms). In line with the previous study, the task-switching costs were non-significant in Stage 2, when the task rules had not yet been introduced.

Dreisbach and colleagues (2006, 2007) indicated that, even when participants mastered the LUT approach and were able to practice the experiment without any task-switching costs, the task-switching costs returned immediately after they received the rule-based instruction. Similarly, in the present experiment, after participants received rule-based task instructions in Stage 3, the task-switching costs reappeared. Participants preferred the task-switching strategy.

The functional differences between Stage 2 and Stage 3 are also interesting. In Experiments 2.1 and 2.2, participants performed more accurately with a task-rule based strategy than with novel strategies, and the RT difference between strategies was not significant. In the present experiment, however, compared with the LUT approach, participants reacted significantly more slowly after they received the task-switching instruction (Stage 3), and this time the difference in error rate was negligible. A similar pattern of results was also reported by Dreisbach et al. (2006, 2007). This experiment confirms their results. However, please note that in the present experiment, the error rate difference between Stage 2 and Stage 3 was approaching the significant level (p = .064).

3.4 Experiment 3.2

So far, the results have shown that an LUT approach can eliminate task-switching costs as long as each task has an independent stimulus-set. Moreover, Dreisbach et al. (2006, 2007) and Experiment 3.1 have demonstrated that the LUT approach can be better than the task-switching strategy because it allows participants to react more quickly. It may be argued that, by applying task rules or the task-switching strategy, participants can respond to a stimulus immediately, irrespective of how many stimuli are used. The LUT approach requires participants to remember the stimulus and to establish response mapping. If the number of stimuli is increased (e.g., 100 stimuli for each task), or if novel stimuli are used (e.g., see Experiment 2.2) then such a strategy would become impractical. Nevertheless, based on the evidence we have so far, it may be concluded that, if a task-switching experiment only includes a few stimuli, and each task has a separate stimulus-set then the LUT approach is more suitable than the task-switching strategy, because it reduces reaction times.

However, drawing this conclusion may be too premature. One problem is that many previous studies on the LUT approach have applied a composite design where, on each trial, cue and stimulus onset coincide (e.g., Dreisbach et al., 2006，2007; Dreisbach & Haider, 2008; Experiment 3.1). In such a design, participants cannot prepare the task in advance. Therefore, the task-switching strategy may not reach its full potential. However, when each task has an independent stimulus-set and participants apply the LUT approach, the cue becomes irrelevant. As a result, whether or not the cue and stimulus are displayed together is not important for the LUT approach. I hypothesised that the composite design is not conducive for the task-switching strategy. Therefore, I sought to re-examine the advantages of the LUT approach over the task-switching strategy in a task-switching experiment with a cue-stimulus interval (CSI) in which the cue is displayed before the stimulus so that the participant has the opportunity to prepare the task in advance.

To sum up, experiment 3.2 had three predictions. First, I hypothesised that, in the condition with CSI, the task-switching strategy and the LUT approach should perform equally well. Secondly, based on the results of Experiment 3.1, I hypothesised that, in the composite condition, the LUT approach should perform better than the rule-based strategy in terms of reaction time. Thirdly, I predicted that task-switching costs should be significant when participants applied the task-switching strategy, but that when participants applied the LUT approach, they would not indicate any task-switching costs.

3.4.1 Method

Participants

Students from the University of Glasgow volunteered to participate in this experiment (N = 21, female = 13; mean age = 25.2, *SD* = 2.9, age range = 21-30 years).

Apparatus and Stimuli

The apparatus and stimuli remained the same as in Experiment 3.1. The only difference was that in Stage 3, the two task cues were displayed simultaneously, forming an uninformative or No-cue signal (see Figure 3.5c). Although the present experiment also had three stages, their timelines were slightly different from each other (Figure. 3.5).

Procedure

The procedure for Experiment 3.2 was equivalent to the procedure of Experiment 3.1: there was a general instruction at the beginning of the experiment, and there were further instructions at the beginning of each stage. All participants completed the three stages in the same temporal order (Stage 1, followed by Stage 2, followed by Stage 3).

Stage 1 (the composite condition)

Participants received a task-switching instruction. They were informed of task rules, the task cues and they were informed that they needed to switch between tasks. After a 20-trial training block, they completed two experimental blocks with composite stimuli, in which the cue and the stimulus appeared and disappeared simultaneously. This time each experimental block had 75 trials.

Stage 2 (the CSI condition)

In Stage 2, participants performed in the CSI condition. The task rules and stimuli were equivalent to Stage 1, with the only difference being there was a 500 ms interval between the onset of the cue and the stimulus. On any given trial, the task cue appeared at the centre of the screen 500 ms before the stimulus onset. After a short instruction, participants completed two 75-trials blocks.

Stage 3 (the No-cue condition)

In Stage 3, participants were instructed that switching between tasks would be unnecessary and that they should deduce the correct answer by applying stimulus-response mapping directly. In Stage 3, a No-cue signal appeared and stayed on the screen for 500 ms. Without a task cue, participants should not be able to apply the task-switching strategy (see Figure 3.5c). After 500 ms, the target stimulus was displayed at the centre of the screen. After receiving a short instruction, participants completed two experimental blocks, each block have 75 trials.

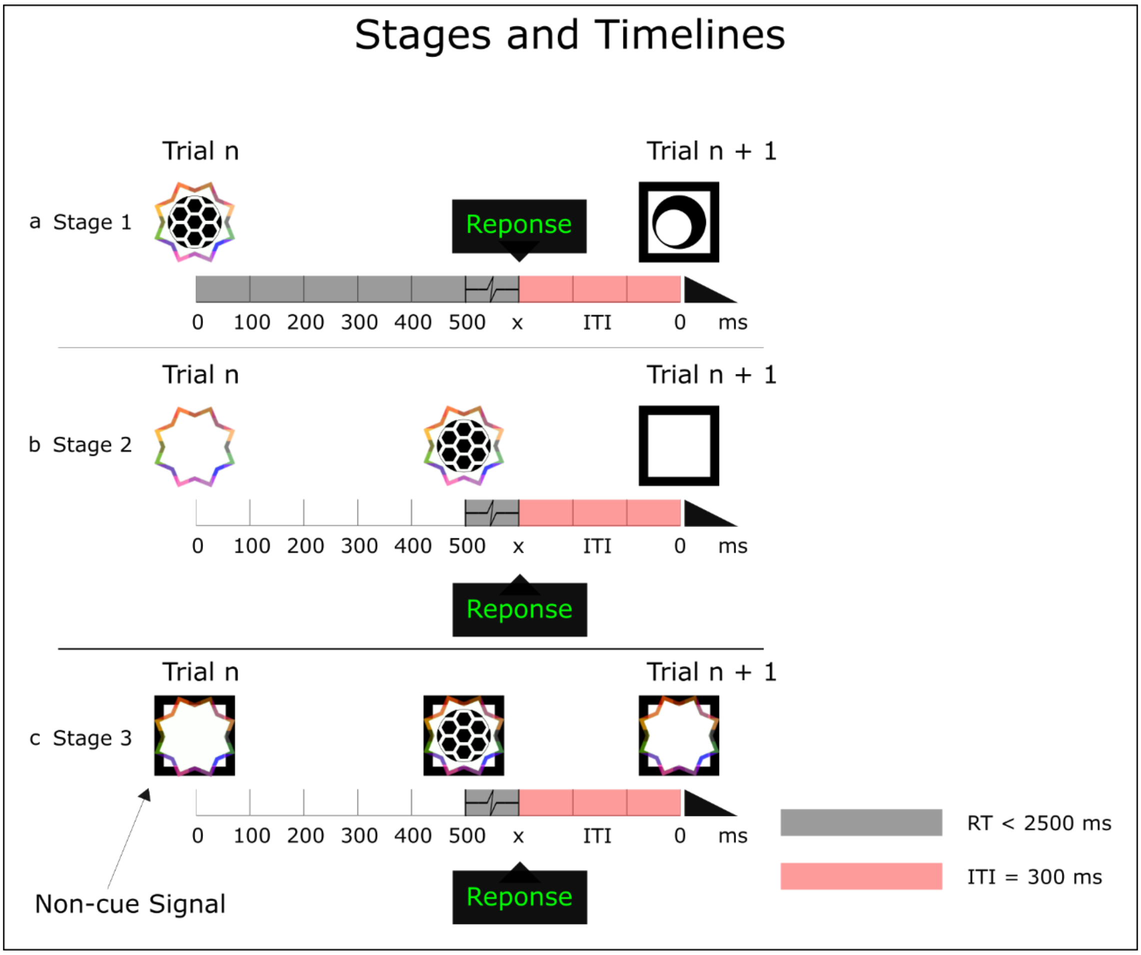

Figure 3.5. (a) The timeline of Stage 1 for a composite condition. (b) The timeline of Stage 2 for a 500 ms CSI condition (c) The timeline of Stage 3 for a No-cue signal onset 500 ms before the stimulus onset.

In summary, participants were first asked to perform in two task-switching rule based stages (Stage 1 and Stage 2). After that, participants received instructions about the LUT approach and then. They then performed using a LUT approach in Stage 3.

3.4.2 Results

I made three predictions for the present experiment. Firstly, in the composite condition, the LUT approach should perform better than the rule-based strategy. Hence the difference between Stage 1 and Stage 3 should be significant. Secondly, in the condition with CSI, the task rule-based strategy (task-switching strategy), and the LUT approach should perform equally well. Thus there should be no difference between Stage 2 and Stage

3. Thirdly, participants would show task-switching costs in Stage 1 and Stage 2 (the task-switching stages), but not in Stage 3 (the LUT stage). The mean (SD) RTs and ERs for each condition are listed in Table 3.2 and illustrated in Figure 3.6. In Stage 3, the task cue was not informative, but each task had a unique stimulus-set. Thus, if the stimulus in *trial n - 1* and the stimulus in *trial n* were from the same stimulus-set, *trial n* was categorised as a repeat trial, and otherwise as a switch trial.

Table 3.2

Mean (SD) of RT and Error Rate for Each Trial Condition and Stage

Condition	RT ms (*SD*)	Error Rate (*SD*)
Stage1 Repeat	923 (227)	2.86% (4.3)
Stage1 Switch	1063 (279)	4.88% (5.2)
Stage2 Repeat	588 (131)	3.57% (4.3)
Stage2 Switch	629 (107)	4.21% (5.6)
Stage3 Repeat	633 (122)	3.90% (3.6)
Stage3 Switch	651 (134)	5.11% (3.0)
Repeat	715 (218)	3.45% (4.1)
Switch	781 (279)	4.73% (4.7)
Stage1	1003 (250)	4.16% (4.8)
Stage2	612 (123)	3.91% (4.7)
Stage3	645 (128)	4.73% (3.1)

Task-switching and Stages

A 2 × 3 ANOVA with repeated-measurements was conducted to compare the mean RTs within conditions. The two factors were the trial transition (switch, repeat) and the stage (Stage 1, Stage 2, and Stage 3). The two factors, the trial transition [*F* (1, 20) = 32.38, *p* = .0001, η^2_p=.62] and the stage [*F* (2, 40) = 58.36, *p* = .0001, η^2_p=.74) were both statistically significant. For the stage, the results of post-hoc pairwise comparisons with the Bonferroni correction suggested that the difference between Stage 1 and Stage 2 (*p* =.0001) and the difference between Stage 1 and Stage 3 (*p* = .0001) were both significant. However, there was no significant difference between Stage 2 and Stage 3 (*p* = .89).

The interaction between trial transition and stage was significant. *F* (2, 40) = 13.37, *p* = .0001, η^2_p = .40. Post-hoc pairwise comparisons with the Holm-Bonferroni correction were conducted to examine the trial transition in each stage. The Bonferroni correction can be too conservative when comparing more than five different conditions. The Holm-Bonferroni correction can largely increase the statistical power (Holm, 1979).

The result of the post-hoc test suggested that the trial transition effect of 140 ms was significant in Stage 1 (*p* = .0001). Although the difference between switch trial and repeat trial was reduced to 41 ms in Stage 2, it remained statistically significant (*p* = .048). However in Stage 3, the trial transition effect of 18 ms was no longer statistically significant (*p* = .37). In other words, the task-switching costs disappeared in Stage 3. The *p*-values of the pairwise comparisons are listed in Table 3.3.

A 2 × 3 ANOVA with repeated-measurements was conducted on the error rates. The factor of trial transition was significant [*F* (1, 20) = 11.53, *p* =.0028, η^2_p = .36]. The factor of stage was not statistically significant [*F* (2, 40) = .256, *p* =.776]. The interaction between trial transition and stage was not significant [*F* (2, 40) = .849, *p* =. 435]. A further paired t-test suggested that the difference between switch trials and repeat trials was not statistically significant in Stage 3, *t* (21) = 1.88, *p* = .075 (see Figure 3.6).

Table 3.3.
The p-value of a Post-hoc Pairwise Comparisons

	Stage1 Repeat	Stage1 Switch	Stage2 Repeat	Stage2 Switch	Stage3 Repeat
Stage1 Switch	.00023	-			
Stage2 Repeat	.00001	.00001	-		
Stage2 Switch	.00001	.00001	.048	-	
Stage3 Repeat	.00001	.00001	.182	.848	-
Stage3 Switch	.00001	.00001	.0048	.701	0.372

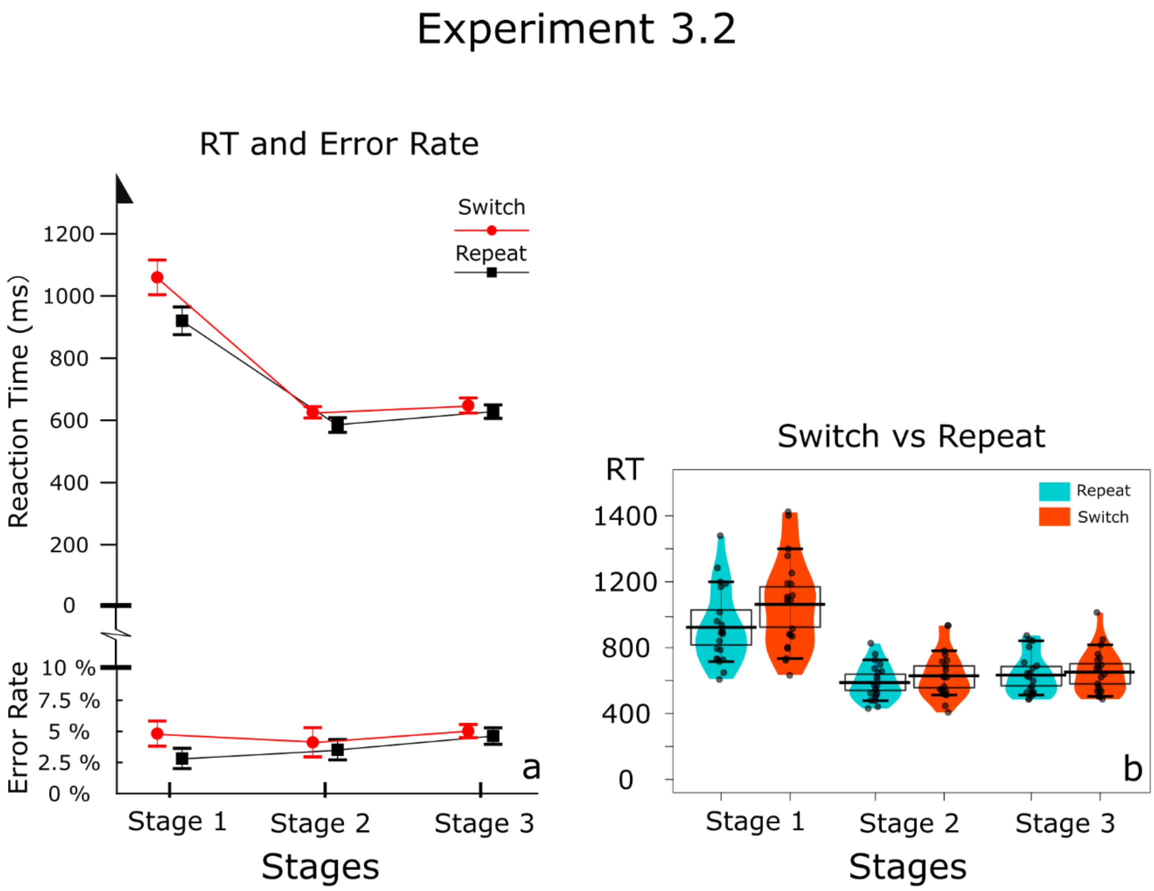

Figure 3.6. (a) The line graphs illustrate the mean RTs (top) and mean ERs (bottom) in each condition (switch, repeat; Stage 1, Stage 2, Stage 3). The error bars denote ±1 *SEM*. (b) The violin plots illustrate the RT distributions in the repeat and switch conditions for each stage. The jittered dots inside each bean represent the average RTs of each participant. The black horizontal bar and the box around it represent the mean and 95% CI of the mean in each condition, respectively. The responses slower or quicker than 95% are represented by dots above and below the error bars.

3.4.3 Discussion

The results of Experiment 3.2 confirm the preceding prediction. I found that participants show significant task-switching costs in Stage 1 and Stage 2 (the task-switching stages), but not in Stage 3 (the LUT stage). In Stage 3, there was no significant task-switching cost. Since there was no task cue, participants were less likely to apply the task-switching strategy in Stage 3, and the LUT approach was the default strategy. However, please remember that, in Stage 3, the task-switching costs in terms of error rates approached significance (p = .075). One initial hypothesis is that, even when participants applied the LUT approach, it was still possible for them to receive interference from task-relevant features (see the general discussion section).

In addition, I found that, in a composite design (Stage 1), the task-switching strategy lead to slower RTs than the LUT approach. In fact, in the composite condition, even the repeat trials were slower than the switch trials in the LUT approach condition (see Table 3.3). However, in Stage 2, when there was a sufficiently long preparation period (500 ms CSI), the task switching strategy was as fast as the LUT approach. In line with previous studies (e.g., Schneider & Logan, 2007; Verbruggen et al., 2007; Forrest et al., 2014; Schneider, 2016), the present experiment also indicates a relationship between preparation period and task-switching costs. With sufficient preparation, the RT task-switching costs were reduced from 140 ms to 41 ms as indicated by the results in Stage1 and Stage 2.

The results of Experiment 3.2 suggest that, if participants have sufficient CSI, then participants can perform as well with the task-switching strategy as with the LUT approach. However, the present study only had a fixed CSI of 500 ms. In a future replication study with different CSIs, one will be able to determine exactly how much preparation time is required for the task-switching strategy to match the LUT approach. This may help to determine how long it takes to apply task rules and/or to reconfigure the task-set.

3.5 Experiment 3.3

Participants in Experiments 3.1 and 3.2 applied two different strategies: the task-switching strategy and the LUT approach. The results of Stage 3 in Experiment 3.1 suggested that, when the task cue was available, participants tended to apply the task-switching strategy. In contrast, the results of Stage 3 in Experiment 3.2 suggested that when the task cue was unavailable, participants tended to apply the LUT approach. These preferences between strategies I found in Experiment 3.1 and 3.2 provide an opportunity to observe participants' behaviour patterns when they switch between different strategies/approaches. One previous study pointed out that switching between strategies can trigger a strategy-switching cost (Luwel et al., 2009).

In summary, in Experiment 3.3, I investigated three predictions. Firstly, I predicted that task-switching costs should be significant when participants applied a task-switching strategy. Secondly, this experiment sought to replicate the strategy-switching cost. I predicted that, when participants were switching between strategies, they would be subject to strategy-switching costs. Finally, I attempted to replicate the results of Experiment 3.2. I predicted that when there is a sufficiently long preparation period (e.g., 500 ms CSI), the rule-based strategy, and the LUT approach should perform equally well.

3.5.1 Method

Participants

Students from the University of Glasgow participated in this experiment (N = 16, female = 12; mean age = 23, *SD* = 1.71, range = 20-26 years). Each student received £3.

Apparatus, Stimuli, and Timelines for Each Stage

The apparatus and stimuli were the same as in Experiment 3.2. Experiment 3.3 also had three stages, and each stage had a slightly different timeline.

Stage 1 (the composite condition)

In Stage 1, on any given trial, a task cue and a target stimulus would be displayed together (composite condition). Participants received task-rule based instructions at the beginning. First, participants completed 20 training trials. After that, they completed two blocks of 100 trials each. The participants were given the same feedbacks as in Experiments 3.1 and 3.2.

Stage 2 (the training stage)

In this stage, participants were instructed that the task rule was not necessary, because they could obtain the correct response based on stimulus-response mapping (the LUT approach). This stage was the same as Stage 1 in Experiment 3.2. On any given trial, the target stimulus was displayed without a task cue. First, participants needed to complete a 20-trial training block first before they performed in an additional 64 trial training block. The intention of Stage 2 was to help participants remember all stimulus-response mappings. The data from Stage 2 was not included in the data analysis.

Stage 3

This was the main stage of the present experiment. At the beginning, participants were instructed that, in any given trial during Stage 3,two different situations would occur randomly. In the first situation (the No-cue condition), a No-cue signal would show up and stay on the screen for 500 ms, and after that the target stimulus would onset at the centre of the screen. Participants could then start to make a response. I believed that participants would be more likely to apply the LUT approach in this situation. In the second situation (the Cued condition), a real task cue would show up and stay at the centre of the screen for

500ms, and after that the stimulus would show up on the centre of the task cue (see Figure 3.7 for both situations). Participants needed to accomplish two 100-trial blocks on Stage 3. It was believed that participants would be more likely to apply the rule-based strategy in this situation. Therefore, if *trial n - 1* and *trial n* have the same situation, then *trial n* is a strategy-repeat trial. Otherwise, *trial n* is a strategy-switch trial.

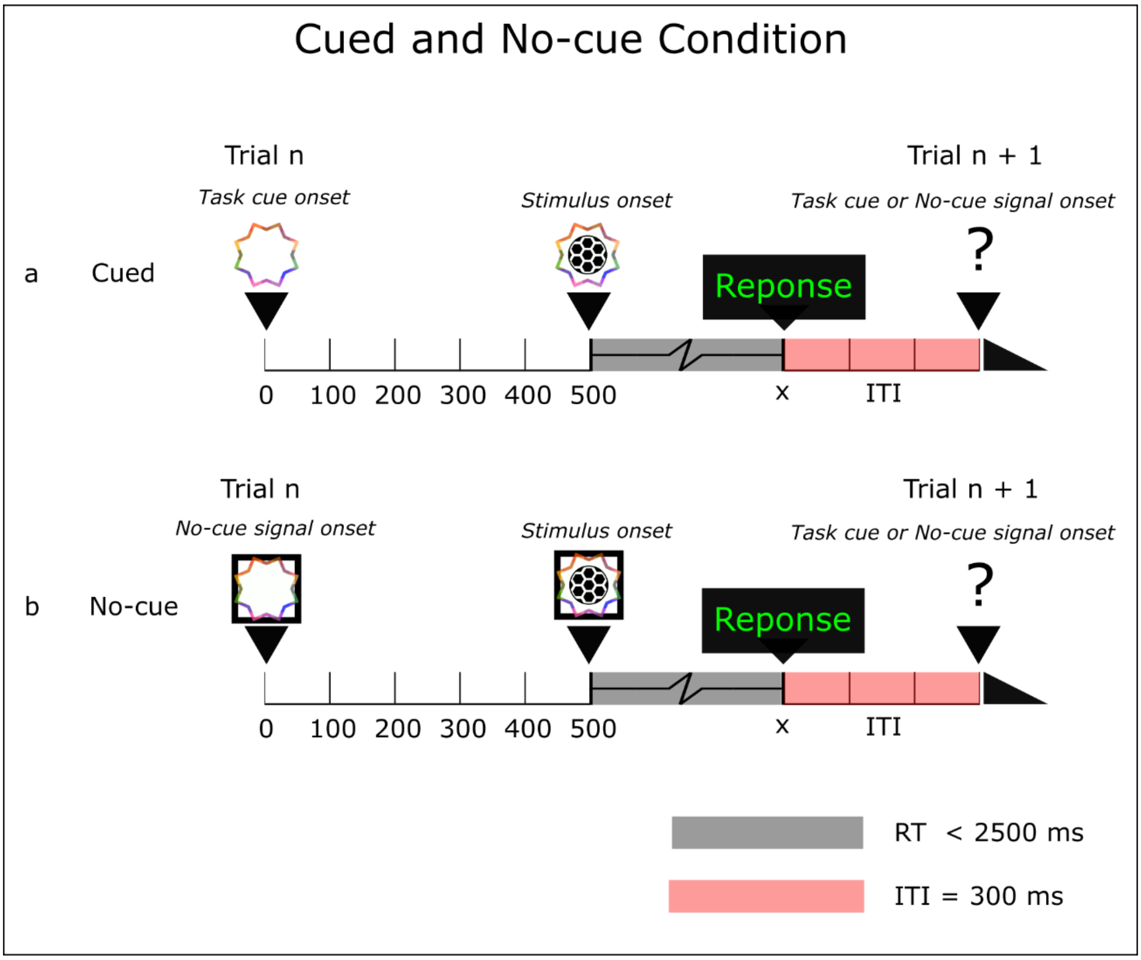

Figure 3.7. (a) In the Cued condition, a real task cue onset 500 ms before the stimulus onset. (b) In the No-cue condition a No-cue signal onset 500 ms before the stimulus onset.

Procedure

The procedure of Experiment 3.3 was identical to the procedure of Experiment 1. There was a general instruction at the beginning of the experiment, and then there were further instructions at the beginning of each stage. Participants were required to complete all three stages in the same temporal order.

3.5.2 Results

It was predicted that, in Stage 1, participants would show clear task-switching costs. In Stage 3, it was predicted that alternating between the Cued situation and No-cue situation would cause a longer RT or/and higher error rate than repeating the same situation. Moreover, it was predicted that participants would perform as well in the No-cue situation as in the Cued situation. The second block was not included in the data analyses because it was mainly a practice stage to help participants remember the stimulus-response mapping. See Table 3.4 for the mean RTs and ERs for each condition.

Table 3.4

Mean (SD) of RT and Error Rate for Each Trial Condition and Stage

Condition	RT ms (*SD*)	Error Rate (*SD*)
Stage1 Repeat	702 (168)	1.15% (1.2)
Stage1 Switch	804 (223)	1.20% (2.4)
Stage3 No-Cue	514 (52)	1.76% (1.9)
Stage3 Cued	502 (71)	1.52% (1.1)
Stage3 Straegy Repeat	503 (57)	1.27% (1.1)
Stage3 Straregy Switch	512 (62)	1.87% (2.1)

Stage 1: Task-switching Costs

A paired t-test showed that on average in Stage 1, the switch trials were significantly slower than the repeat trials, (t (15) = 4.56, p =.0004; see Figure 3.8).The equivalent t-test on error rates was not statistically significant [t (15) = 1.37, p = .19].

No-cue Situation, Cued Situation and Stage 1

In addition, I conducted a one-way ANOVA with repeated measurments on RTs to compare differences amongst the overall RTs in Stage 1 (i.e., the rule-based strategy under composite condition), the RTs of the No-cue situation in Stage 3 (i.e., the LUT condition) and the reaction times of the Cued situation from Stage 3 (i.e., the rule-based strategy under CSI condition). The result suggested that there is a significant difference [F (2, 30) = 37.15, p =.0001, $\eta^2 p$=.49]. The results of post-hoc pairwise comparisons with the Bonferroni correction suggested that the difference between Stage1 and the No-cue situation was significant (p = .001; see Figure 3.8), and the difference between Stage1 and the Cued situation was significant (p = .001). However, the difference between the No-cue situation and the Cued situation was non-significant (p = 0.89; see figure 3.8). An equivalent ANOVA was applied to examine differences between error rates. No statistically significant differences were found (see Figure 3.8).

Stage 3: Strategy-switching Costs

Paired t-tests showed that, in Stage 3, the strategy-switch trials and the strategy-repeat trials have no significant difference: RT: (t (15) = 1.72, p =.11); Error Rate: (t (15) = 1.63, p =.12).

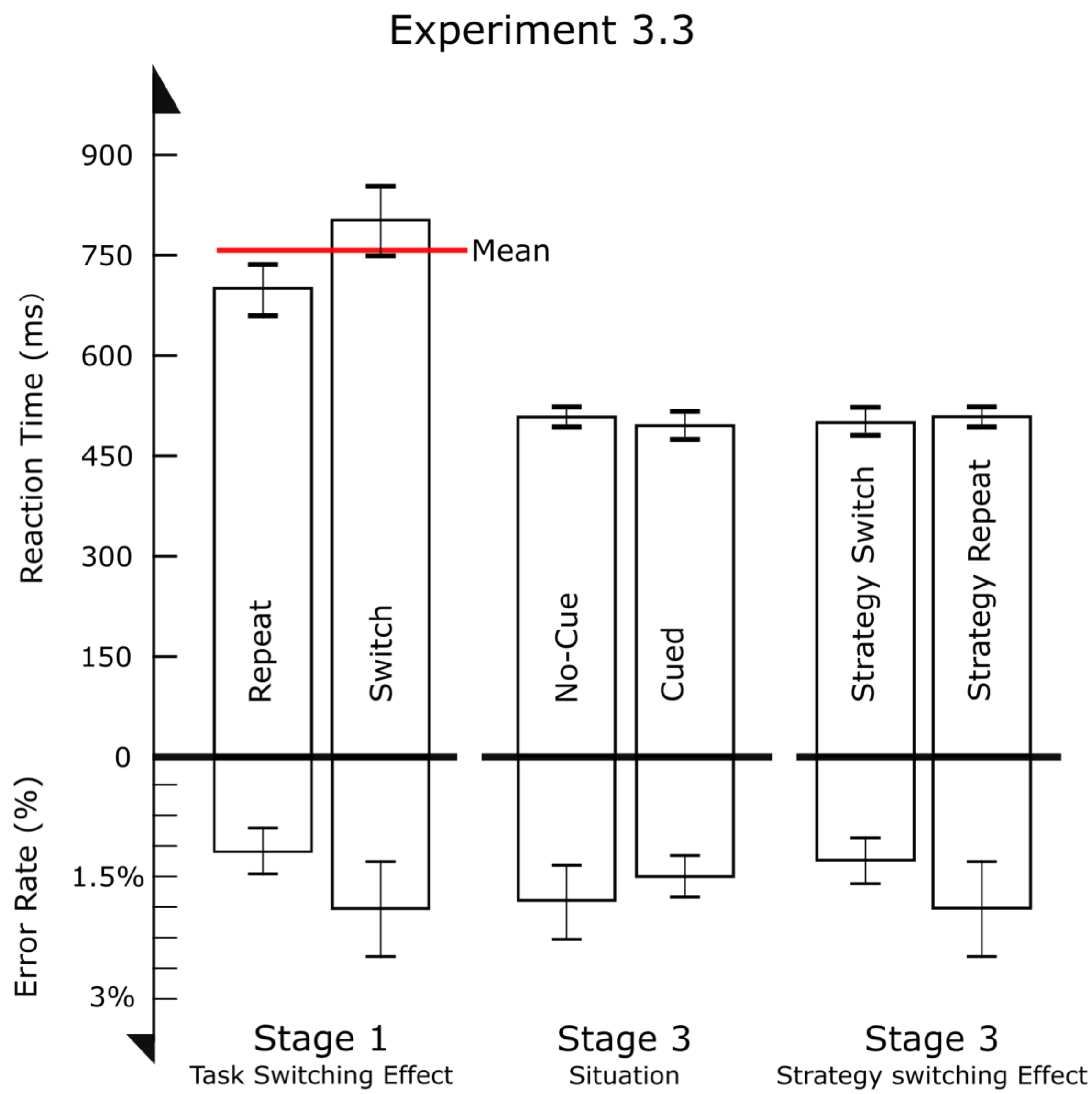

Figure 3.8. The bar graph indicates the mean reaction time for each condition (switch, repeat; No-Cue, Cued; strategy switch, strategy repeat). Error bars denote ±1 SEM. The red line shows the mean RT for Stage 1.

3.5.3 Discussion

Similarly to Experiments 3.1 and 3.2 the results of the present experiment indicated significant task-switching costs when participants received a rule-based instruction. In addition, in Stage 3, there was no statistically significant difference between the Cued-situation and the No-cue situation. The results of Stage 3 replicated the results of Experiment 3.2: when there was a sufficiently long preparation period, the rule-based strategy was as good as the LUT approach.

The present experiment cannot replicate the strategy-switching costs from Luwel et al. (2009). In their study, participants were required to determine the number of coloured blocks in a 10 × 10 grid. Participants applied two main strategies. Firstly, when there were few coloured blocks and many empty squares, participants typically used the addition strategy, in which the coloured blocks were added to determine the total numerosity. Secondly, when there were many coloured blocks and few empty squares, participants applied the subtraction strategy: the number of empty squares is subtracted from the total number of squares in the grid. As a consequence, in Luwel et al. (2009), participants could not prepare their strategy before the stimuli onset. In the present experiment, because the task cues and No-cue signal were presented 500 ms before the stimulus, participants may have selected a strategy in advance. Perhaps it is the preparation period that eliminates strategy-switching costs.

3.6 General Discussion

With only a few minor changes, the present study successfully replicates Dreisbach et al.'s (2006, 2007) major discovery. Using the LUT approach, participants can eliminate task-switching costs. Nevertheless, please remember that the error rate for task-switching costs in Experiment 3.2 Stage 3 approached statistical significance t (21) = 1.88, p = .075. In Experiments 3.1 and 3.2, I have generalised Dreisbach and colleagues' (2006, 2007) results using two visual tasks instead of two linguistic tasks. Furthermore, the results of the present study suggest that regardless of the stimulus type (bivalent or univalent), as long as each task has a different stimulus-set, the LUT approach will eliminate task-switching costs in a task-switching paradigm.

Moreover, in line with Dreisbach et al. (2006, 2007), my experiments show that participants can use the LUT approach to eliminate task-switching costs. Immediately after they have received the instructions for task rules, however, task-switching costs return, as was the case Experiment 3.1 Stage 3. In Stage 3 of Experiment 3.1, participants only received information about the task rules, without any explicit requirement to use a rule-based strategy. In fact, the instructions specifically stated that participants were free to choose any strategy they wanted to use. Dreisbach et al. (2007) believed that their results confirmed that applying task rules was an automatic process. Here, I propose that it is also possible that participants just preferred to apply a rule-based strategy over the LUT approach. Given the fact that, in real life, each task might include an infinite number of stimuli, always using a rule-based strategy by default may be a straightforward way to solve any problem.

3.6.1 The Composite and CSI Conditions

Dreisbach et al. (2006) indicated that in a composite design experiment, switching between tasks requires longer reaction times than simply memorising all eight stimulus-response mappings. I fully agree with their observation, but from the present study I conclude, that with sufficient preparation time, participants can perform task-switching as quickly as using the LUT approach.

In the composite condition (e.g., Stage 1 of Experiment 3.2), even the repeat trials were slower than the LUT approach. I suspect that the delay in RT for the task-switching strategy reflects additional rule processing. Participants need to process the relevant task rules before they can determine the correct response. However, the LUT approach associates the stimulus and the response directly. Therefore, it can be a simpler and quicker strategy (Dreisbach et al., 2006, 2007). Nevertheless, such an additional rule processing is not a disadvantage under conditions with increased CSI. The results of the present study provide evidence that providing a sufficiently long CSI, the RTs for both strategies are equally quick.

3.6.2 Task-switching Costs and Strategies

One of the most important features of the Experiments from Chapters 2 and 3 is that when the instructions were manipulated, participants performed the task-switching experiments without having an explicit understanding of the task rules. The results of Experiments 2.1 and 2.2 and the present study reveal two important differences. Firstly, in Experiments 2.1 and 2.2, none of the participants applied the LUT approach: participants either applied the task-switching strategy, or developed novel rule-based strategies. However, in the present study, no meaningful novel strategy was reported by the participants after each experiment. All participants applied the LUT approach when the task-switching strategy was unavailable as for example in Experiment 3.1 Stages 1 and 2 and Experiment 3.2 Stage 3.

Secondly, even without explicit understanding of the tasks and task-sets, significant task-switching costs were observed in Experiments 2.1 and 2.2. However, in Experiments 3.1 and 3.2, the task-switching costs disappeared when the participants reported that they did not have an explicit understanding of the tasks.

Features and Strategies

In order to explain the difference mentioned above, we need to interpret the task-switching paradigm from a slightly different angle. No matter what strategy participants applied during the experiment, they had to identify the stimulus and recall the relevant response key assigned to it. In this section, I will explain how the features of the stimulus may affect stimulus-response identification.

Usually, studies only discuss the features of the stimuli that are related to tasks: the univalent stimuli only included the information (feature) for one task, but bivalent stimuli convey information for two tasks. However, a stimulus may also contain "irrelevant" information if the task-switching experiments apply separate stimulus-sets. For example, in the present study, the typical perspective would be that each stimulus carries both colour and shape information because the stimuli are bivalent. Nevertheless, as illustrated in Figure 3.2, it is noticeable that each stimulus may also include information beyond both tasks. For example, the number of geometrical shapes in a stimulus and the orientation of a stimulus are features that are independent of the task-relevant information. This gives each stimulus a unique feature that is unrelated to any task: non-task-relevant feature. In a task-switching experiment with separate stimulus-sets, there must be at least one additional feature in each stimulus. Otherwise, the stimuli in the colour task and the stimuli in the shape task would be identical, and the experiments would become shared stimulus-set experiments, as in Experiments 2.1 and 2.2.

Since each non-task-relevant feature was unique in this study, the task-relevant features could be easily covered up by that non-task-relevant feature. For example, for the stimulus in Figure 3.9, participants could quickly apply the non-task-relevant feature and identify the stimulus: Mercedes-Benz == *left*. The colour feature of the stimulus (*mainly* black) and the shape feature of the stimulus (*mainly* a circle) is no longer important. From another perspective, in a separate stimulus-sets experiment, it is possible to associate the stimulus with the response with a single IF-THEN argument. As a consequence, the LUT approach is easy to apply: simply establish eight single IF-THEN arguments to form associations between non-task-relevant features and the correct response.

Similarly, we can also explain why all participants applied the LUT approach in the univalent stimuli study (Dreisbach et al., 2006, 2007) in which eight German words were applied: four words for Task 1 and four for Task 2. Participants could simply associate the words with the response key. For example, one can apply the IF-THEN rule: if “bett” (bed) then press *left*, without even thinking about whether the word "bed" is an animal or not. The task-relevant feature is not important, and a single IF-THEN argument can solve the problem.

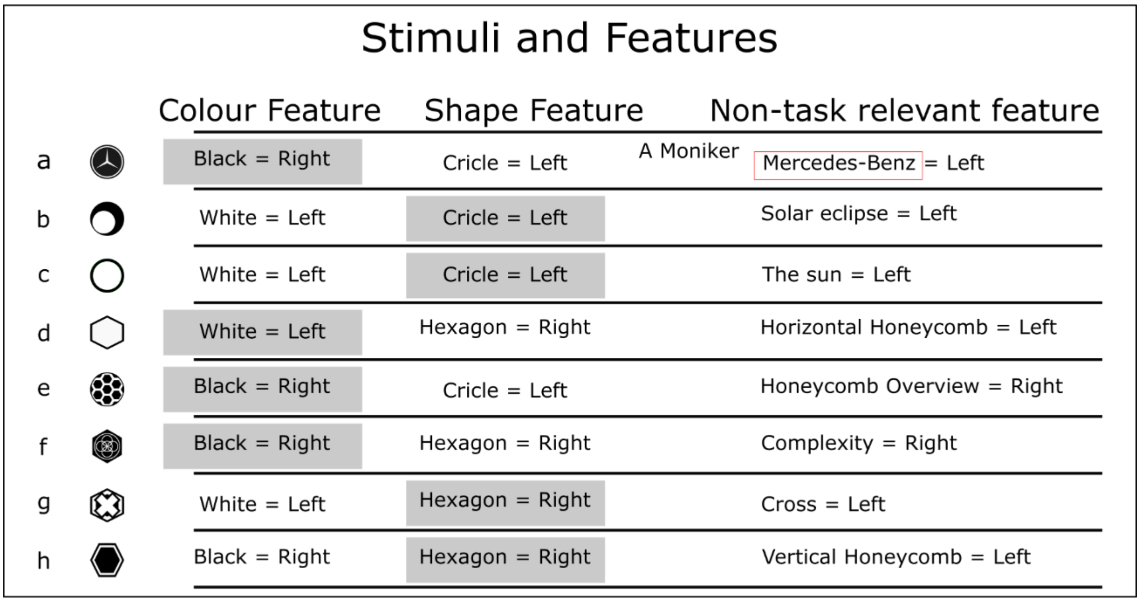

Figure 3.9. The eight stimuli and their task-relevant feature-response mappings (colour and shape features). Since the present study applied separate task stimulus-sets, only one task-relevant feature-response mapping was shown during the experiment (highlighted in grey in the two columns on the left). The other associations are implied only. The column on the right lists possible non-task-relevant features with responses. Participants can make a correct response by employing the non-task-relevant features directly. The monikers lists in the right columns, like Mercedes and Solar Eclipse were picked up from post experiment oral reports. They are for illustrative purposes only, as each participant created unique monikers.

In contrast, in Experiments 2.1 and 2.2 two tasks shared one stimulus-set. Therefore, non-task-relevant features were not available. In Experiments 2.1 and 2.2, each stimulus is a combination of the task cue and the target stimulus. Hence, to identify each cue-stimulus combination, participants *had to* process both the task-relevant features (i.e., the colour and the shape of a target stimuli) and the task cues. In other words, they needed to process three layers of information before they were able to deduce the correct response from a cue-stimulus combination. This is always true no matter which strategy the participants applied.

Moreover, in Experiments 2.1 and 2.2, the same task-relevant feature could be associated with different responses. For example, a white target stimulus could be associated with the *left* key or the *right* key. In addition, a hexagon could be associated with the *right* key or the *left* key. Also, even the same task cues can be associated with the *left* key or the *right* key. As a consequence, in Experiments 2.1 and 2.2, obtaing the correct response with a single IF-THEN argument is impossible. Additional conjunction rules were required (see Figure 3.10). Therefore, the LUT approach is not practical in Experiments 2.1 and 2.2. In this view, the different strategies I found in Experiments 2.1 and 2.2 reflect the different sets of conjunctive rules that participants used to identify each cue-stimulus in order to give the correct response.

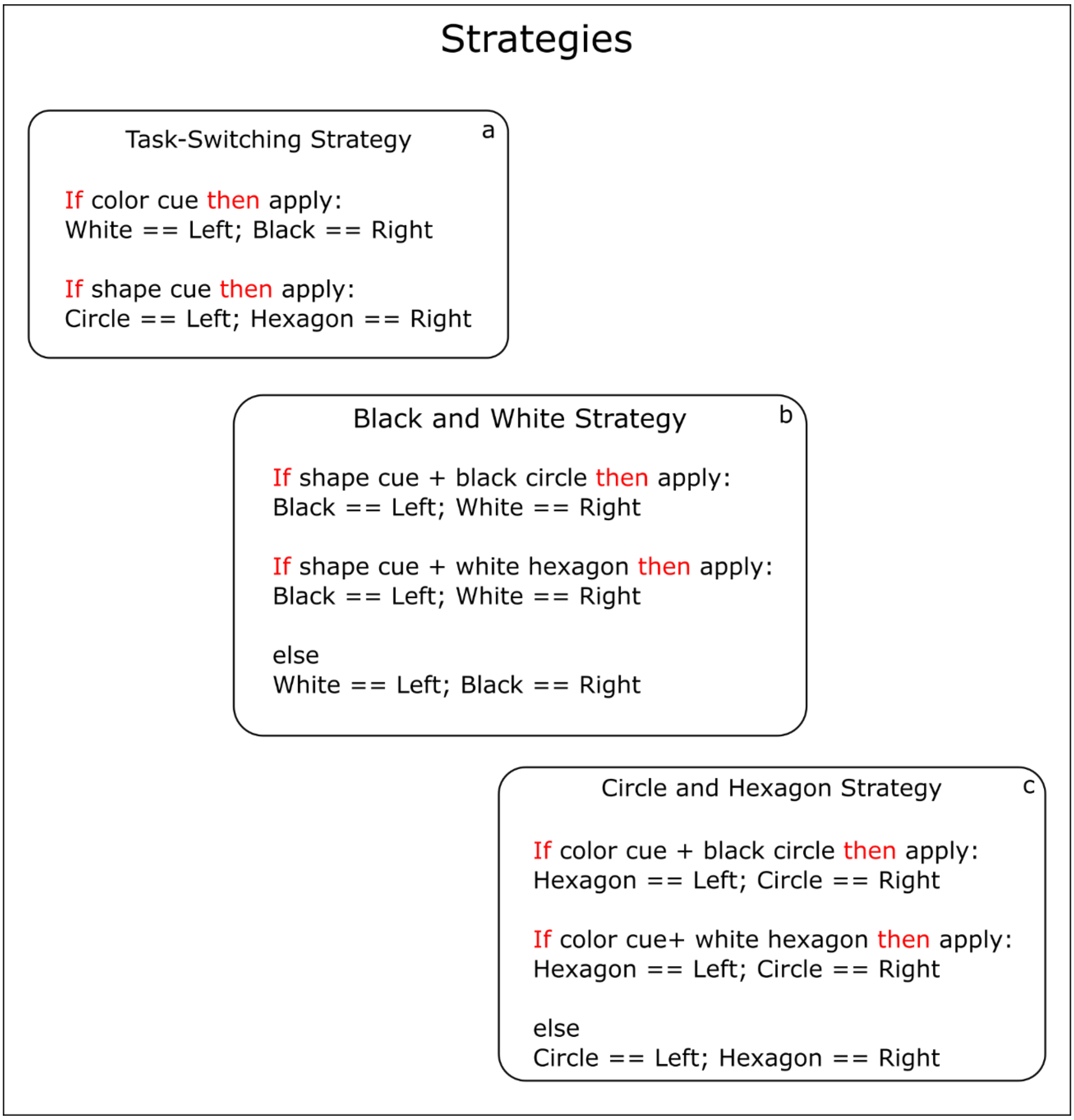

Figure 3.10. The three boxes summarise three possible strategies identified in Experiment 2.1. (a) The task-switching strategy. (b) The "Black and White" strategy. (c) The "Circle and Hexagon" strategy. All strategies require more than one IF-THEN argument in order to deduce the correct response key.

In short, I propose that, in the present study, the non-task-relevant feature of the stimuli allowed participants to deduce the correct response to a stimulus using a single IF-THEN argument. In this context, the LUT approach is simpler and more straightforward to use. However, in Experiments 2.1 and 2.2, participants had to apply additional conjunction rules before they could associate the stimulus with the correct answer. Thus, applying a simple LUT approach would have been very difficult, if not impossible.

Interference between Features

In the previous section, I concluded that, in Experiments 2.1 and 2.2, participants have to process both task-relevant features before they can deduce the correct response. In this section, based on this conclusion, I attempt to explain the unexpected task-switching costs in Experiments 2.1 and 2.2 and the absence of task-switching costs in Experiments 3.1 and 3.2.

Although rarely mentioned explicitly by Allport and colleagues (Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000; Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006), Woodward et al. (2003) proposed that the proactive interference account is based on task-relevant features. According to Woodward and colleagues (2003), the task performed on *trial n - 1* (e.g., colour task) requires activating one feature-response mapping (e.g., white == *left*) while at the same time inhibiting or negatively priming another feature-response mapping (e.g., hexagon == *right*). If, however the inhibited or negatively primed mapping is useful on *trial n*, additional time is required to reactivate it, and this results in a switching cost. This interpretation implies that without processing both task-relevant features, there would be no interference.

This could explain why participants in Experiments 2.1 and 2.2 always showed task-switching costs, even when they were not applying the task-switching strategy. In these experiments participants always process the task-relevant features. Therefore, it is unavoidable that they will receive proactive interference from the previous trial. In the present chapter, when participants apply the LUT approach, the task-relevant features can be concealed by non-task-relevant features. Consequently, if participants do not apply the task-switching strategy, they may not experience interference either. Therefore, they can avoid both the task-set reconfiguration process and interference from the previous trial, so that task-switching costs are eliminated. The task-switching costs in terms of error rates in Experiment 3.2 Stage 3 approached statistical significance, t (21) = 1.88, p = .075. Perhaps, even with the non-task-relevant features, sometimes participants still process the task-relevant features.

This explanation assumes that the interference from the previou trial produces task-switching costs, without any task-set or reconfiguration process. However, some proactive interference studies considered interference as a consequence of task-set (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). In order to confirm the hypothesis that interference can be independent of the task-set, I will demonstrate in Experiment 5.1 that, even when tasks based on task-switching strategy no longer exist, the interference from stimuli alone can still produce task-switching costs. I propose a modified interference account in Chapter 5.

3.6.3 Semantic Stimuli Can Hide Task-relevant Features

Forrest et al. (2014) eliminated task-switching costs in their study. They also used a shared stimulus-set. In other words, as in Experiments 2.1 and 2.2, both tasks shared the same task-sets. Based on my hypothesis from the previous section, even if Forrest et al. (2014) had managed to eliminate the task-set reconfiguration process, the interference would have still produced task-switching costs. However, this is not what happened in their Experiment 2. To fully explain Forrest et al.'s (2014) results, we need to consider another factor, the nature of semantic stimuli.

Forrest et al. (2014) applied two numerical tasks: the odd/even task and the big/small task, with numbers as stimuli. Therefore, the task-relevant information was semantic in nature: the parity and the magnitude of a number. However, in Experiments 2.1 and 2.2, the task-relevant features were visual colour and the shape of the stimulus. One possibility is that semantic information is less prominent/immediate than visual features. Therefore, not every participant in Forrest et al. (2014) may have processed the task-relevant information. Some participants may have identified the number as a single unit and obscured the task-relevant information in the stimuli. Thus, those participants do not experienced interference from task-relevant features. This may explain why some participants in Forrest et al., (2014) had no task-switching costs. Other participants may have experienced interference from task-relevant information. Hence, their result are not consistent. I will examine this explanation further in Chapter 4.

3.6.4 Summary and Preview of Chapter 4

In summary, based on the differences between Chapter 2 and the present study, I propose that eliminating the task-switching strategy alone does not rule out task-switching costs. Unless participants also avoid the interference that is triggered by task-relevant features, task-switching costs remain. Finally, I suggest that there are two ways to avoid the processing of task-relevant features and interference. Firstly, for separate stimulus-sets experiments such as Experiments 3.1 and 3.2, it is possible to use non-task-relevant features to form the stimulus-response mapping directly. Thus, no task-relevant features are involved. The results of Experiments 3.1 and 3.2 support this explanation. Secondly, for shared stimulus-set experiments, I hypothesised that, if the tasks were semantic, participants might be able to obscure the task-relevant features in stimuli. I will provide direct evidence in support of the second hypothesis in the next chapter.

Chapter 4: Do Semantic Task-Relevant Features In Remove Task-Switching?

Task-switching costs can be elusive. Dreisbach (2012) in her review article, questioned whether participants even need to apply a task-switching strategy in the first place. Why not just directly memorise all stimulus-response mappings (or, if the stimuli are bivalent, cue-stimulus-response mappings) thereby avoiding task-switching altogether? By doing so, task-switching costs should be eliminated. Given the fact that a conventional task-switching paradigm only includes a very limited number of stimuli, this "Look-up table" (LUT) approach seems to be a likely contender. In fact, Dreisbach et al. (2006, 2007) demonstrated that, when the stimuli were univalent, participants who applied the LUT strategy were able to respond even more quickly than participants who applied a task-switching strategy.

Nevertheless, previous findings have suggested that such an approach does not work consistently if both tasks share the same stimulus-sets (Forrest et al., 2014; see also Experiments 2.1 and 2.2). Using a shared stimulus-set design, even without explicitly realising the task rules and without switching between tasks, participants can still show reliable task-switching costs. However, Forrest et al., (2014) in their second experiment found no statistically significant task-switching costs, [F (1, 15) = 3.39, p = .086, η^2_G=.00372]. In Chapter 3, I hypothesised that, without task-switching, the corresponding task-set reconfiguration process can be eliminated. Nevertheless, participants cannot completely eliminate task-switching costs, because interference between task-relevant features can still produce task-switching costs.

In Chapter 3, I suggested that there are two possible ways to eliminate the interference from task-relevant features. Firstly, if non-task-relevant features are available, one can avoid identifying the task-relevant features by corresponding the non-task-relevant features with the response directly. The results in Experiments 3.1 and 3.2 provide clear evidence for this explanation. When participants in Experiments 3.1 and 3.2 used the non-task-relevant features instead of the task-relevant features their task-switching costs disappeared.

However, this method only worked for the separate stimulus-sets experiments. This is because, when both tasks share the same stimulus-set, participants will always need conjunctive rules to obtain the correct responses. Participants have to identify all task-relevant features in order to apply those conjunctive rules in a shared stimulus-set design as in Experiments 2.1 and 2.2: participants cannot apply the LUT approach in a shared stimulus-set design, and therefore task-switching costs appear to be unavoidable.

In Chapter 3, I hypothesised, that in a shared stimulus-set design, a possible exception in which task-switching costs may be avoidable is an experiment with semantic tasks (e.g., Forrest et al., 2014). This is because, when dealing with semantic tasks, the task-relevant features are all semantic (e.g., the meaning of a word or the parity of a number). When the task-relevant features are semantic, it may be possible to obscure the task-relevant features in the stimuli. For example, if the stimulus following a task cue is the digit *8*, then participants may associate a response key with this cue-number combination. Consequently, they may not realise the semantic task-relevant feature of the number (e.g., *8* is an even number or *8* is a number larger than 5). Thus, they should be immune to interference from task-relevant features. This suggests an alternative method with which to bypass task-relevant features. In the following study, I sought to provide evidence for this strategy.

4.1 Simplified and Traditional Chinese Numbers

In the following two experiments, I invited Chinese and non-Chinese speakers and used numbers written in simplified and traditional Chinese characters as stimuli. *Simplified Chinese characters* is the standardised simplification of *Traditional Chinese characters* promulgated in the 1950s by the Chinese government. These contain the existing *Simplified Chinese characters* that are in use today, while *Traditional Chinese characters*, are used less often nowadays. In the following experiments, “Chinese participants” are participants who can read and speak Chinese fluently and “non-Chinese participants” are participants who cannot read or speak Chinese at all. In the following sections I call these participants “Chinese” and “non-Chinese” participants, for short.

4.2. Experiment 4.1

The challenge in the present experiment was to create a shared stimulus-set in which some participants easily identified the task-relevant features, whereas for other participants identifying the task-relevant features was impossible. I used language proficiency to manipulate the identification of task-relevant features. The present experiment applied two standard numerical tasks: an odd/even task and a big/small task. Forrest et al. (2014) applied the same two tasks in their experiments. In the present experiment, the task stimuli (i.e., four numbers) and two task cues were written in simplified Chinese. Hence, it was only possible for participants who knew and understood written Chinese to identify the task-relevant features. I predicted that, similarly to participants in Experiments 2.1 and 2.2, Chinese participants would show significant task-switching costs with or without the task-switching rules being made explicit.

More importantly, for participants who do not understand Chinese, the task stimuli and task cues should be meaningless symbols. Consequently, non-Chinese participants should not be able to identify the task-relevant features in these stimuli (i.e., the magnitude or parity of a Chinese number). Unsurprisingly, rather than applying the task-switching strategy, they should use the LUT approach instead. In other words, they should match the Chinese symbols entirely by shape and respond according to the LUT approach. They should not indicate any task-switching costs.

Apart from task-switching costs, previous studies have suggested that, in the absence of task rules, humans (Forrest et al., 2014), monkeys (Stoet & Snyder, 2003; Caselli &

Chelazzi, 2011) and pigeons (Meier et al., 2016) still consistently show congruency effects. Therefore, Experiment 4.1 also examined whether or not human participants exhibited congruency effects after controlling the task-switching strategy.

In summary, Experiment 4.1 examined three hypotheses. Firstly, I predicted that Chinese participants would have significant task-switching costs, while non-Chinese participants would not. Secondly, I predicted that both Chinese and non-Chinese participants would show a congruency effect. Finally, I predicted that all non-Chinese participants should apply the LUT approach, but Chinese participants might apply both the LUT approach and the task switching strategy.

4.2.1 Method

Participants

Fifteen Chinese and eighteen non-Chinese (N = 33, female = 24; mean age = 23, SD = 4.48) students from the University of Glasgow participated in Experiment 1. Each participant received £3 for their participation. The Chinese participants were all Chinese international students.

Apparatus and Stimuli

All stimuli were presented at the centre of a BenQ computer monitor (24 inches). A Black Box Toolkit response pad was used to record participants' responses and reaction times. Participants also used a QWERTY keyboard to go through the instructions and to start the experiment. The four target stimuli were the numbers *4, 5, 6, 7* written in simplified Chinese characters: 四， 五， 六 and 七 respectively. Both tasks shared the same stimulus-set. The two task cues were also simplified Chinese characters: 质 (quality) == the cue for the odd/even task; 量 (quantity) == the cue for the big/small task. The size of each Chinese character was 16.93 mm × 16.93 mm. All stimuli were green (RGB: 0, 255, 0) on a black screen (RGB: 0, 0, 0) to avoid eye strain.

Task and Timeline

In Experiment 4.1, each participant performed the odd/even task and the big/small task in a single trial. For the odd/even task, the participants needed to decide whether a number was an odd number or an even number (odd == press *left* key on the response pad; even == press *right* key on the response pad). For the big/small task, a participant needed to decide whether a number was smaller than 5.5 or bigger than 5.5 (small == press *left* key; big == press *right* key). Experiment 4.1 used a composite design. In each trial, the task cue and task stimuli appeared simultaneously on the screen. The number was presented at the bottom, and the task cue was presented at the top (see Figure 4.1b). Once the stimulus appeared, a response had to be made within 2.5 seconds or a timeout error occurred. The cue-stimulus combination disappeared immediately after a response was made. Otherwise, the cue and stimulus would stay on the screen until the maximum reaction time of 2.5 seconds was reached. The inter-trial interval (ITI) was 300 ms.

Feedback

If a correct response was given, the next trial would be initiated after the ITI. If an incorrect response was given, the text message "Mistake" would show up and stay on the screen for 3 seconds before disappearing. After that, the next trial would start immediately. If no response was given within 2.5 seconds, the text message "Timeout" would appear and stay on the screen for 3 seconds before disappearing. After that, the next trial would start immediately (see Figure 4.1).

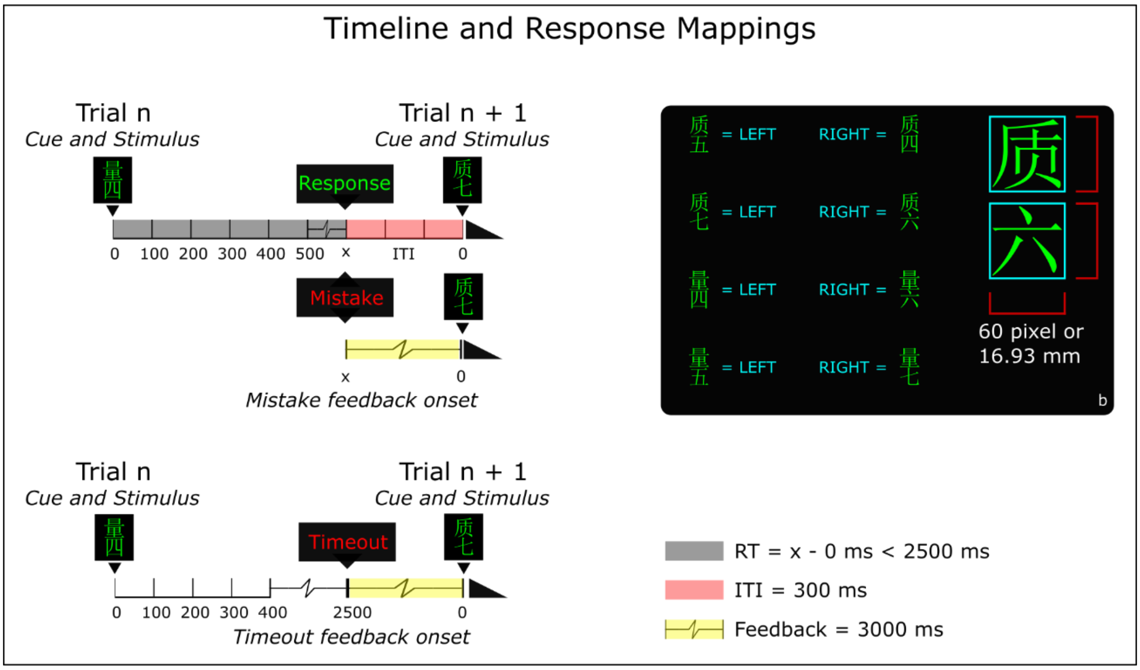

Figure 4.1. On the left, the illustration of the timelines of Experiment 4.1. (b) All eight cue-stimulus combinations in green and their correct response keys in cyan. The top part of each combination is the task cue, and the bottom part is the stimulus (numbers).

Procedure

The participants were asked to sign a consent form and sit in front of the computer screen (viewing distance 40-60 cm). Before the experiment, both Chinese and non-Chinese participants received non-task rule-based instructions. These instructions listed all associations between the cue-stimulus combinations and response keys, and required participants to memorise these. For Chinese participants, the instructions were in Chinese, and for non-Chinese participants, the instructions were in English. The content of the instructions was exactly the same for all participants. The experiment consisted of one block with 20 training trials, followed by four experimental blocks with 75 trials, giving a total of 300 experimental trials. Finally, each participant was asked to report the strategy they applied during the experiment. After the experiment, each participant received a payment of £3.

4.2.2 Results

There were five non-Chinese participants who had extremely high error rates (ranging from 39% to 49%) in the incongruence condition (this is not significantly different from pure random guessing). Their data was therefore excluded from the analyses (N non-Chinese speaker = 18 – 5 = 13). The mean RTs and ERs with SDs for each condition and group are provided in Table 4.1 and illustrated in Figure 4.2. It was predicted that, for non-Chinese participants, the task-switching costs should not be significant and the mean RTs of 954 ms for repeat and 950 ms for switch trials confirm this (Table 4.1). However, for Chinese participants, the task-switching costs were substantial, with a difference of 96 ms (1038 ms - 942 ms) between the switch and repeat trials. It was predicted that both Chinese and non-Chinese participants would show significant congruency effects.

Table 4.1

Mean (SD) of RT and Error Rate for Each Trial Condition and Language Group

	Chinese		Non-Chinese	
Condition/Group	RT ms (*SD*)	Error Rate (*SD*)	RT ms (*SD*)	Error Rate (*SD*)
Repeat Congruent	927 (143)	4.53% (5.2)	823 (132)	6.6% (5.7)
Repeat Incongruent	955 (169)	8.06% (6.8)	1109 (192)	28.7% (13.1)
Switch Congruent	1029 (147)	5.34% (5.6)	838 (185)	6.9% (7.1)
Switch Incongruent	1048 (120)	8.76% (5.7)	1076 (179)	25.7% (13.6)
Repeat	942 (139)	6.32% (5.2)	954 (141)	18.6% (8.8)
Switch	1038 (139)	7.03% (5.4)	950 (173)	16.3% (9.0)
Congruent	990 (139)	4.94% (5.4)	832 (153)	6.9% (6.3)
Incongruent	1006 (134)	8.36% (5.1)	1091 (182)	27.0% (12.6)
Total	998(123)	6.66% (.052)	953 (159)	13.31% (6.5)

Task-switching, Congruency and Language Group

A 2 × 2 × 2 ANOVA with mixed effects was conducted to compare the mean RTs between and within conditions. The two within-subjects factors were trial transition (switch, repeat) and congruency (congruent, incongruent). The between-subjects factor was language group (Chinese, Non-Chinese). The two within-subjects factors of trial transition [F (1, 26) = 10.49, p = .0033, η^2_p = .29] and congruency [F (1, 26) = 35.87, p = .0001, η^2_p = .58] were both statistically significant. However. the between-subjects factor of language group was not significant [F (1, 26) = .28, p = .598].

The interaction between the trial transition and language group was significant [F (1,

26) = 12.8, *p* = .0013, η^2_p = .33]. Post-hoc pairwise comparisons with the Bonferroni correction suggested that that the task-switching effect amongst Chinese participants was significant (*p* = .0001). However, it was not significant amongst non-Chinese participants (*p* = .83). The interaction between the congruency effect and language group was also significant [*F* (1, 26) = 27.91, *p* = .0001, η^2_p = .52] (see next section for further analysis of this interaction). The interaction between switching and congruency [*F* (1, 26) = 1.37, *p* = .251] and the interaction amongst all three factors was not significant [*F* (1, 26) = .748, *p* = .395].

An equivalent 2 × 2 × 2 ANOVA with mixed effects was conducted to compare the mean error rates amongst different conditions. Overall, the within-subjects factor of congruency was significant [*F* (1, 26) = 102.87, *p* = .0001, η^2_p = .80], and the between-subjects factor of language group was also significant [*F* (1, 26) = 9.25, *p* = .00542, η^2_p = .26]. However, the within-subjects factor of trial transition was not significant [*F* (1, 26) =.428, *p* =.519].

There was a significant interaction between congruency and language group [*F* (1, 26) = 43.95, *p* = .0001, η^2_p = .62]. Other interactions were not significant (*p* > .05). The main results of the ANOVAs on RT and ER are illustrated in Figure 4.2 and 4.3.

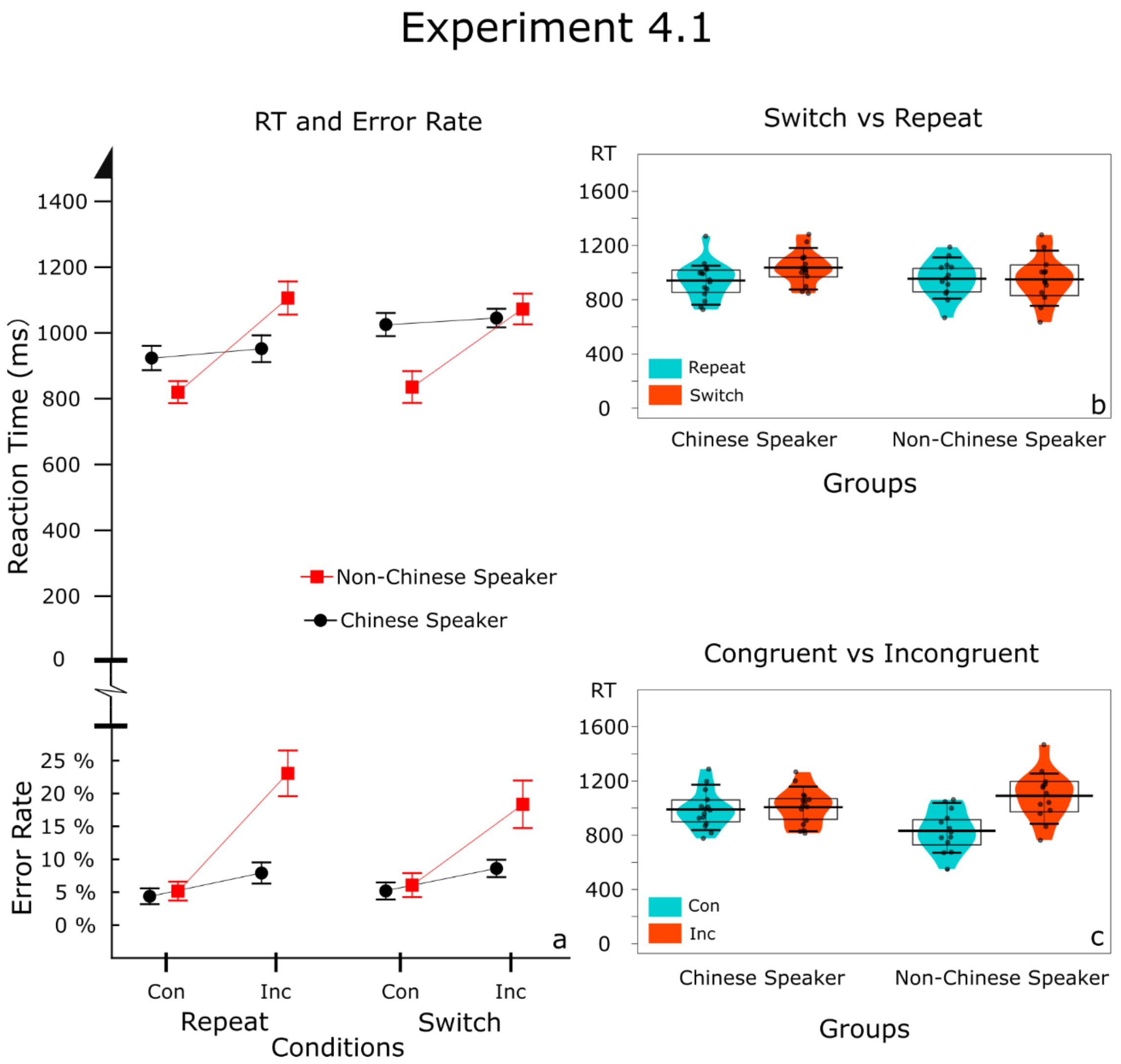

Figure 4.2. (a) The line graph shows the mean RTs (top) and error rates (bottom) for each condition (switch, repeat; congruent [Con], incongruent [Inc]; Chinese speaker, non-Chinese speaker). The error bar indicates ±1 *SEM*. (b) The violin plots illustrate the RT distributions for the repeat and switch conditions. The jittered dots inside each bean represent the average RTs for each participant. The black box and horizontal bar at the centre represent the mean and 95% CI of the mean, respectively. (c) Violin plots illustrate the RT distributions in the congruent and incongruent conditions for each group.

Congruency Effect and Language Group

To examine the interaction between congruency and language group in detail, post-hoc pairwise comparisons (Bonferroni corrected) were applied. The results suggest that, for Chinese participants, the RT congruency effect was not statistically significant (p = .59) whereas for non-Chinese participants, the RT congruency effect was significant (p = .005).

Similarly, the ER congruency effect was not statistically significant for Chinese participants (p = .89). In contrast, for non-Chinese participants, the ER congruency effect was significant (p = .0001).

The error-rates also indicated that the difference between Chinese and non-Chinese participants was not significant in the congruent condition (p = .94), but highly significant in the incongruent condition (p = .0001). The interactions are shown in Figure 4.3.

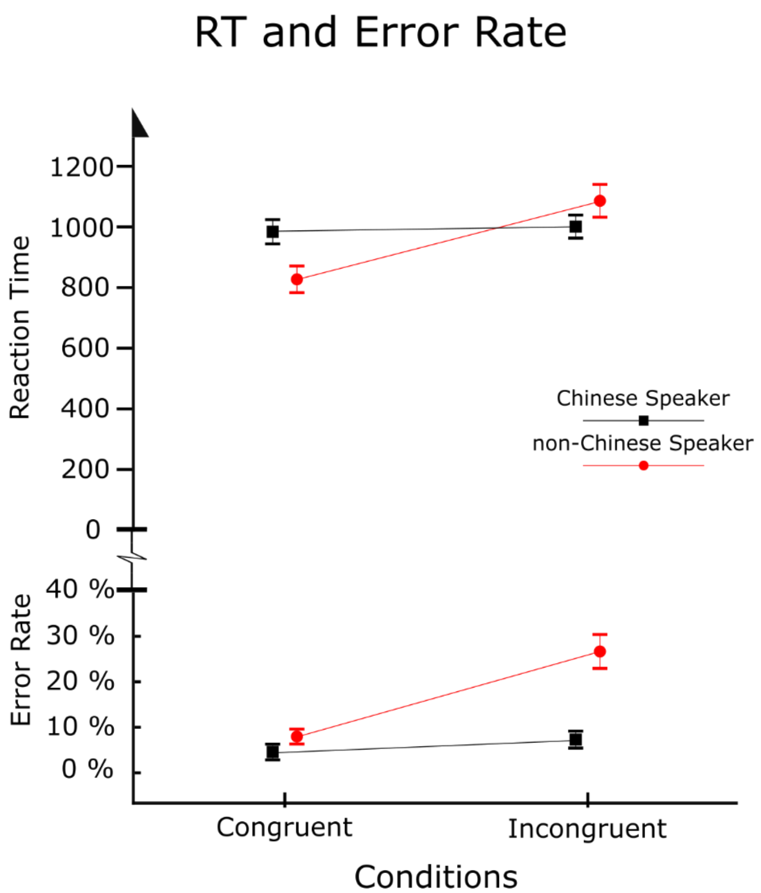

Figure 4.3. The line graph shows the RT and error-rate interaction between congruency effects and language groups. The error bar indicates ±1 *SEM.*

Participants' Self-reports

Contrary to the prior prediction, nobody applied the LUT approach. All Chinese participants reported that they applied a task-switching strategy in the experiment. In contrast, all non-Chinese participants reported that they applied a "bottom first" (BF) strategy. In any given trial, non-Chinese participants looked at the bottom part of the cue-stimulus combination first (i.e., the Chinese numbers). If the bottom was 五 (5), they pressed the *left* key immediately. If the bottom was 六 (6), they pressed the *right* key immediately. For numbers 五 and 六, the top part of the combination (i.e., the task cue) was irrelevant because they were congruent stimuli and shared the same response key in both tasks. If the bottom was 四 or 七, the correct answer was determined by the top of the cue-stimulus combination: 四 + 量 = *left*, 四 + 质 = *right*; 七 + 量 = *right*, 七 + 质 = *left* (see Figure 4.4b).

4.2.3 Discussion

In line with the first hypothesis, using language proficiency as a task feature filter led to task-switching costs being eliminated entirely amongst non-Chinese participants, while the task-switching costs amongst Chinese participants remained significant. In disagreement with the initial hypothesis, none of the participants applied the LUT strategy. All Chinese participant applied a task-switching strategy. Furthermore, all non-Chinese participants applied a novel strategy called the BF strategy. Forrest et al. (2014) also reported a similar strategy. Nevertheless, they believed that participants' self-reports were not reliable: "The ability to articulate a propositional rule does not mean that performance is being driven by it (p 1021)." Instead, they suggested that despite the self-reports, their participants had learned to perform the experiment based on associative learning without applying any rule-based strategy.

Interestingly, only non-Chinese participants showed a significant congruency effect, whereas Chinese participants showed a much reduced congruency effect. Previous studies have consistently found statistically significant congruency effects (e.g., Stoet & Snyder, 2003; Forrest et al., 2014; Meier et al., 2016). It is unclear whether the results of the present experiment reflect a Type II error or whether they reflect a unique behavioural pattern that only occurs when using Chinese numbers as stimuli. To the best of my knowledge Chinese numbers have not been used as stimuli in task-switching studies. The absence of a congruency effect is investigated further in the next experiment.

Strategy and Congruency Effect

Based on the self-reports of the participants, non-Chinese participants tended to identify the bottom of the Chinese phrase first (i.e., the target stimulus number). This is called the BF strategy (Figure 4.4b). Besides the self-reports, the behavioural data of Experiment 4.1 also provides preliminary evidence in favour of the BF strategy.

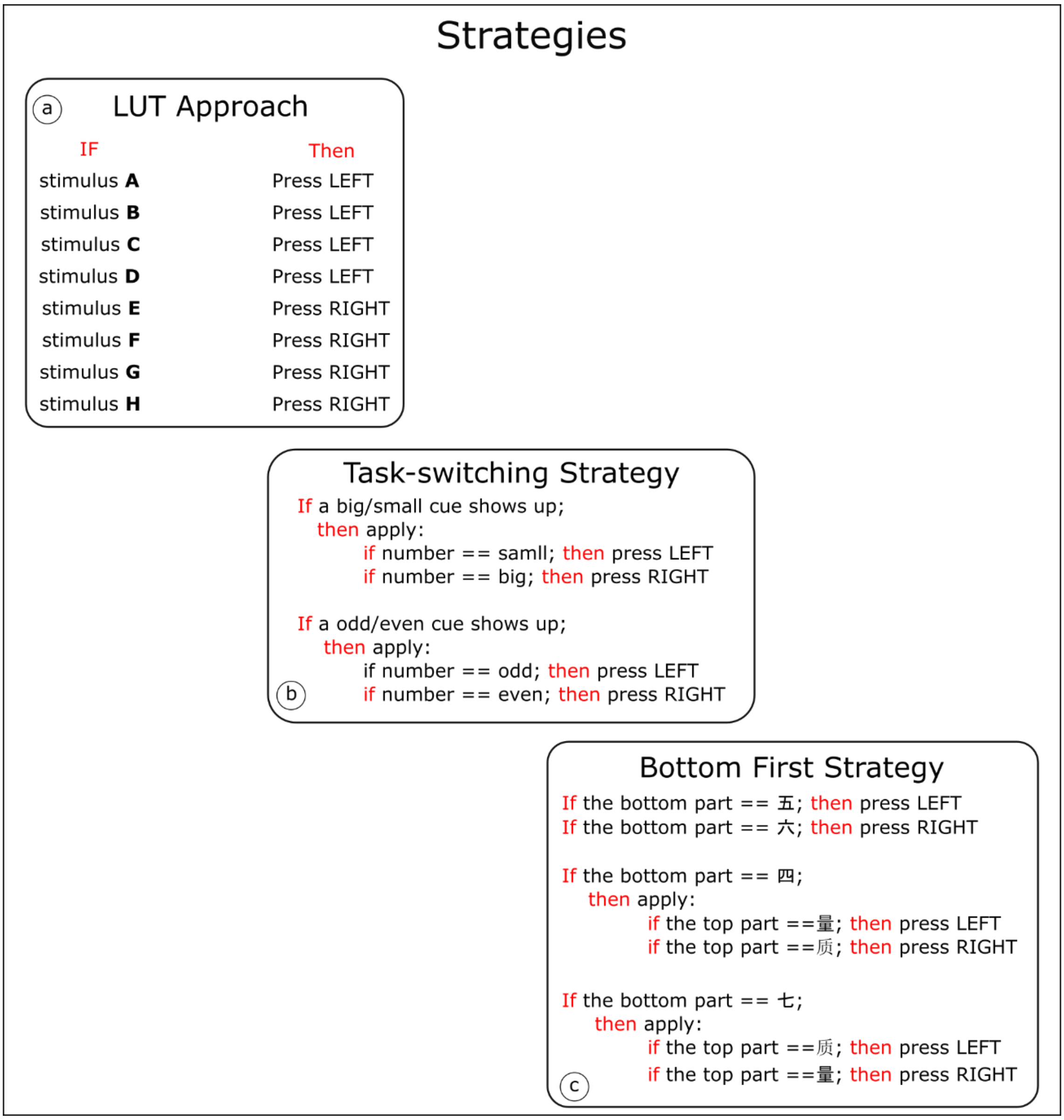

Figure 4.4. (a) An LUT strategy includes eight single IF-THEN arguments. A participant can give a correct response with a single IF-THEN argument. (b) A typical task-switching strategy includes two sets of conjunctive rules. Importantly a participant needs to perform two IF-THEN arguments in sequence to give the correct response. (c) The BF strategy in

Experiment 4.1. If the number 五 (5) or 六 (6) is shown, a participant only needs to execute a single IF-THEN argument as in the LUT approach. However, if the number 四 (4) or 七 (7) is shown the participant needs to execute two nested conjunctive rules similar to a task-switching strategy.

The results suggest that non-Chinese participants reacted significantly quicker in the congruent condition than in the incongruent condition (mean difference = 259 ms). However, for the Chinese participants, the difference between the congruent and incongruent conditions was not significant (mean difference = 17 ms). In fact, the non-Chineses participants' reaction times in the congruent condition were actually 158 ms quicker than those of the Chinese participants, although this difference was not significant after the Bonferroni correction.

Perhaps the BF strategy that the non-Chinese participants applied requires only one IF-THEN argument in the congruent condition. Once the non-Chinese participants had identified the bottom number (五 or 七), they could immediately figure out the correct response. The cue-first task-switching strategy requires participants to execute two nested IF-THEN arguments, because there is an additional conjunction rule. After identifying the task cue at the top, the Chinese participants (who applied the task-switching strategy) also needed to identify the number at the bottom. See Figure 4.4 for an illustration of both strategies. Indeed, the non-Chinese participant performed quicker than the Chinese participant in congruent trials because, they simply ignored the cue at the top, as suggested by participants'sself-report.

Error Rate

The results for error rate reveal a slightly different picture. In the congruent condition, the non-Chinese participant had a 6.9% error rate. When compared with the Chinese participants' error rate in the same condition (4.9%), the difference was not statistically significant. However, in the incongruent condition, the non-Chinese participants had a significantly higher error rate (27%) than the Chinese participants (8.3%). This is most likely due to the fact that the non-Chinese participants never tried to remember the Chinese characters. As many participants reported after the experiment, it is quite difficult for non-Chinese participants to identify or discriminate between Chinese characters. However, all the non-Chinese participants performed significantly better than a chance level in both the repeat-incongruent and the switch-incongruent conditions. However, perhaps the BF strategy allowed non-Chinese participants to identify only one Chinese character (the bottom one; the number), in the congruent condition, so that familiarity with Chinese characters had a relatively small impact for non-Chinese participants.

Limitations

Experiment 4.1 had two possible disadvantages. Firstly, all the Chinese participants reported that they had applied the task-switching strategy. All Chinese participant reported that they were naive about task-switching effects before the experiment, but I suspect some of them had previously participated in another task-switching study conducted at the same institution. Therefore, it is difficult to replicate the results of Experiments 2.1 and 2.2 and in Forrest et al. (2014): without an explicit understanding of the task rules, participants can still produce task-switching costs, as long as they can identify the task features.

Secondly, the only evidence to support the application of the BF strategy is the congruency effect: participants who applied the BF strategy performed better in congruent trials than those who applied the task-switching strategy. A potential problem is that similarly designed studies consistently found a congruency effect, whereas in the present experiment, the Chinese participant (who applied the task-switching strategy) did not show a statistically significant congruency effect. Hence, the difference between the two language groups in terms of congruency effect may not support the BF strategy supposedly observed in the performance of the non-Chinese participants. In Experiment 4.2, I tried to avoid these two limitations.

4.3 Experiment 4.2

Since all the Chinese participants from Experiment 4.1 reported that they applied the task-switching strategy, it is difficult to replicate the results from Experiments 2.1 and 2.2. That is, the task-switching costs remained significant when participants reported that they did not have an explicit understanding of the task-switching rule. To prevent the Chinese participants from applying the task-switching strategy, in Experiment 4.2, I applied traditional Chinese numbers as stimuli. Traditional Chinese numbers are mainly used in commercial or financial contexts (e.g., on neon lights for brands; on bank notes) and are used less frequently than the simplified Chinese numbers for everyday writing.

4.3.1 The Preparation Effect and BF Strategy

The behavioural results of Experiment 4.1 provide some evidence of the existence of the BF strategy. The non-Chinese participants who applied this strategy had a shorter RT in the congruent condition than the Chinese particpants who applied the task-switching strategy. I propose that this difference might reflect the fact that participants who apply the BF strategy take advantage of congruency. However, the results of Experiment 4.1 were inconclusive.

In Experiment 4.2, I proposed a new method to test the existence of the BF strategy. I hypothesised that if the bottom part (the number) appeared first and if the top part (the cue) appeared after a short delay (i.e., if there was a Stimulus-Cue Interval [SCI]), participants' performances in the congruent trials would be further enhanced. This is because, when participants are using the BF strategy, the congruent number itself reveals the correct response, and participants can therefore give the correct response even before the task cue appears at the top.

One problem is that, if participants consider the bottom part to be the “cue” and the top part to be the "target stimuli", then inserting an SCI also increases the RSI. Thus, it would be inappropriate to make a comparison between the composite condition (RSI = ITI) and the SCI condition (RSI = ITI + SCI). The RSI difference might confound the strategy advantage. In order to avoid this problem, I also tested the participants' performances under a CSI condition (i.e., the cue appeared first and the target stimulus appeared after a delay; RSI = ITI + CSI). The idea was to set an SCI equal to CSI so that both conditions would have the same RSI.

I predicted that applying the BF strategy would reduce the RT in congruent trials under the SCI condition, because the bottom part suggests the correct response as soon as the stimulus is displayed. In contrast, under the CSI condition, participants cannot give the correct response before the stimulus is presented. Hence, if the BF strategy is applied, participants should have shorter RTs in the SCI congruent condition than in the CSI congruent condition. For the incongruent condition, participants have to identify both the top and bottom part before they can give a correct response. Therefore, identifying the bottom part first may have little effect on reaction times in both the SCI and the CSI incongruent conditions.

In short, for Experiment 4.2, I made three predictions. Firstly, if the BF strategy was applied, participants should have shorter RTs in the SCI congruent condition than in the CSI congruent condition. Secondly, I predicted that both the Chinese participants and the non-Chinese participants would show a significant congruency effect. Thirdly, I predicted that the Chinese participants would have significant task-switching costs regardless of their awareness of the task-switching rules, while the non-Chinese participants would show no task-switching costs at all.

4.3.2 Method

Participants

Fifteen Chinese participants (female = 10; mean age = 24, SD = 2.06) and fifteen non-Chinese participants (female = 10; mean age = 23, SD = 3.79) from the University of Glasgow participated in Experiment 2. All the Chinese particpants were international students from China. This time, each participant received a payment of £4 for their participation.

Apparatus and Stimuli

All stimuli were presented at the centre of a 21 inch Dell computer monitor. A Black Box Toolkit Response Pad linked to a Dell computer with Linux operating system was used to record the participants' responses and reaction times. The participants also used a QWERTY keyboard to go through the instructions and to start the experiment. The four target stimuli were the numbers *1, 2, 8, 9* written in Traditional Chinese characters as 壹，贰，捌 and 玖 respectively. The two task cues were also Chinese characters: 质 (quality) == the cue for the odd/even task; and 量 (quantity) == the cue for the big/small task. The size of each Chinese character was 60 × 60 pixels. In order to reduce eye strain, all stimuli were displayed in green (RGB: 0, 255, 0) on a black background (RGB: 0, 0, 0).

Task and Timeline

In the present experiment, each participant performed two tasks that were similar to those in Experiment 4.1. The only difference was that, this time, 壹 (one) and 贰 (two) were the small numbers (press *left* key) while 捌 (eight) and 玖 (nine) were the big numbers (press *right* key); and 贰, 捌 were the even numbers (press *left* key) while 壹, 玖 were the odd numbers (press *right* key). The response mappings are listed in Figure 4.5b. Experiment 4.2 had three experimental conditions. In each experimental condition the cue-stimulus sequence (timeline) was slightly changed. However, the task rules and feedback remained the same and the feedback was given as in Experiment 4.1 was given.

Composite Condition

The timeline of the composite condition was equivalent to Experiment 4.1 (see Figure 4.5). This time, each participant was asked to complete one 36-trial training session and three 68-trial experimental sessions in the composite condition.

CSI Condition

In this condition, the CSI was applied. In each trial, the cue appeared first and remained on the screen. After a 500-ms delay, the stimulus appeared and formed a cue-stimulus combination. After the stimulus appeared participants had to derive the correct response (see Figure 4.5 for the timeline). In this condition, each participant was asked to complete a 20-trial training block followed by three 68-trial experimental blocks.

SCI Condition

In this condition, the bottom part of the cue-stimulus combination (i.e., the Chinese number) was displayed first and stayed on the screen. After a 500 ms delay, the task cue appeared and formed a cue-stimulus combination. The participants had to derive the correct response after the task cue appeared (see Figure 4.5 for the timeline). In this condition, each participant was asked to complete a 20-trial training block followed by three 68-trial experimental blocks

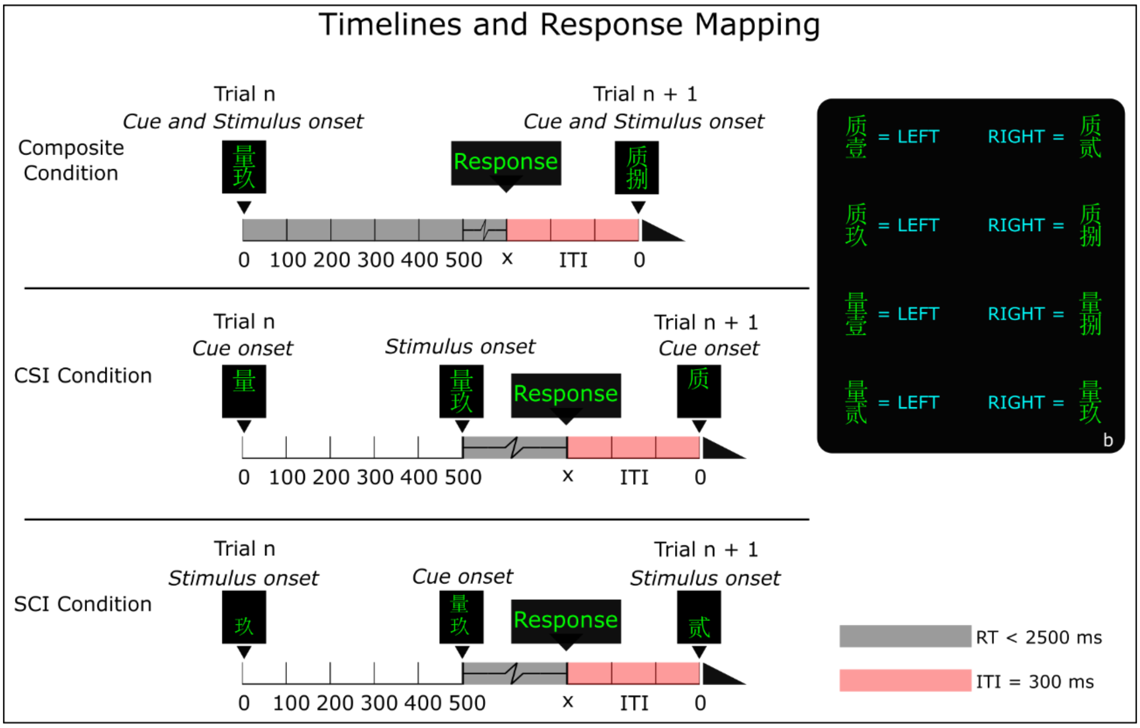

Figure 4.5. The cue-stimulus sequences (timelines) of the three experimental conditions. (b) All eight cue-stimulus combinations in green and their correct response keys in cyan. The top part of each combination is the task cue, and the bottom part is the stimulus (numbers).

Procedure

The experiment took place in a quiet and dark laboratory. The participants were asked to sign a consent form and sit in front of the computer screen at a viewing distance of between 40 - 60 cm. Before the experiment, both the Chinese participants and the non-Chinese participants received LUT instructions, listing all cue-stimulus combinations and the corresponding response keys. Participants were asked to memorise these mappings. After that, the participants were asked to complete all three experimental conditions. Each participant completed the composite condition first. However, the order of the other two conditions was counterbalanced. After participants finished all three experimental conditions of the experiment, they were asked to report the strategy they applied in each condition. Each participant received a payment of £4.

4.3.3 Results

Three predictions concerning the outcome of the experiment were made. Firstly, if the BF strategy was applied, participants should have shorter RTs in the SCI congruent condition than in the CSI congruent condition. Secondly, I predicted that both the Chinese and the non-Chinese participants should show a significant congruency effect. Thirdly, I predicted that the Chinese participants would exhibit significant task-switching costs regardless of their awareness of the task-switching rules, while the non-Chinese participants would show no task-switching costs at all. Before entering the data into an ANOVA, I ran a binomial test to check the error rate of each participant. The results suggested that many of the non-Chinese participants were unable to perform significantly better than chance in this experiment. Only nine of the 15 non-Chinese participants performed better than chance in both the CSI and the SCI conditions. Furthermore, only six non-Chinese participants performed better than random guessing in the composite condition. Therefore, as I had only a few observations in the unbalanced language groups, I tested the Chinese and non-Chinese participants separately.

Non-Chinese Participants

Composite Condition

Two 2 × 2 ANOVAs with repeated measurements were conducted on the mean RTs and Error Rates within conditions. The two factors were the trial transition (switch, repeat) and the congruency effect (congruent, incongruent). Only six non-Chinese participants were included in the analysis, because they performed above chance level in the composite design (see Table 4.2 and Figure 4.6).

Table 4.2.
RT and Error Rate of Each Observation and Its Mean

Observations	Repeat				Switch			
	Congruent		Incongruent		Congruent		Incongruent	
	ms	%	ms	%	ms	%	ms	%
1	578	3.25	978	14.3	588	2.04	1161	12.5
2	1063	0	1319	10.8	1125	0	1256	3.92
3	1132	5.26	1356	18.4	1162	2.38	1573	8.16
4	571	0	997	31.4	555	0	997	28.5
5	746	6.06	1602	17.5	649	2.63	1406	21.6
6	731	3.03	920	29.6	692	27.0	964	31.4
Mean	799	2.93	1195	20.4	795	1.62	1226	17.7

	Mean	
Condition	RT (ms)	Error Rate %
Repeat	989	11.57
Switch	1005	10.34
Congruent	797	2.22
Incongruent	1215	20.8

For RT, the results of the ANOVA suggested that the factor of trial transition was not significant [F (1, 5) = .121, p =. 742]. However, the factor of congruency effect was highly significant [F (1, 5) = 20.19, p = .0064, η^2_p = .80]. The interaction between trial transition and congruency effect was not significant [F (1, 5) = .45, p =.532].

For error rate, the factor of trial transition was again not significant [F (1, 5) = 3.524, p =.131] whereas the factor of congruency effect was highly significant [F (1, 5) = 17.75, p =. 008, η^2_p = .78]. The interaction between trial transition and congruency effect was not significant [F (1, 5) = .35, p =.582].

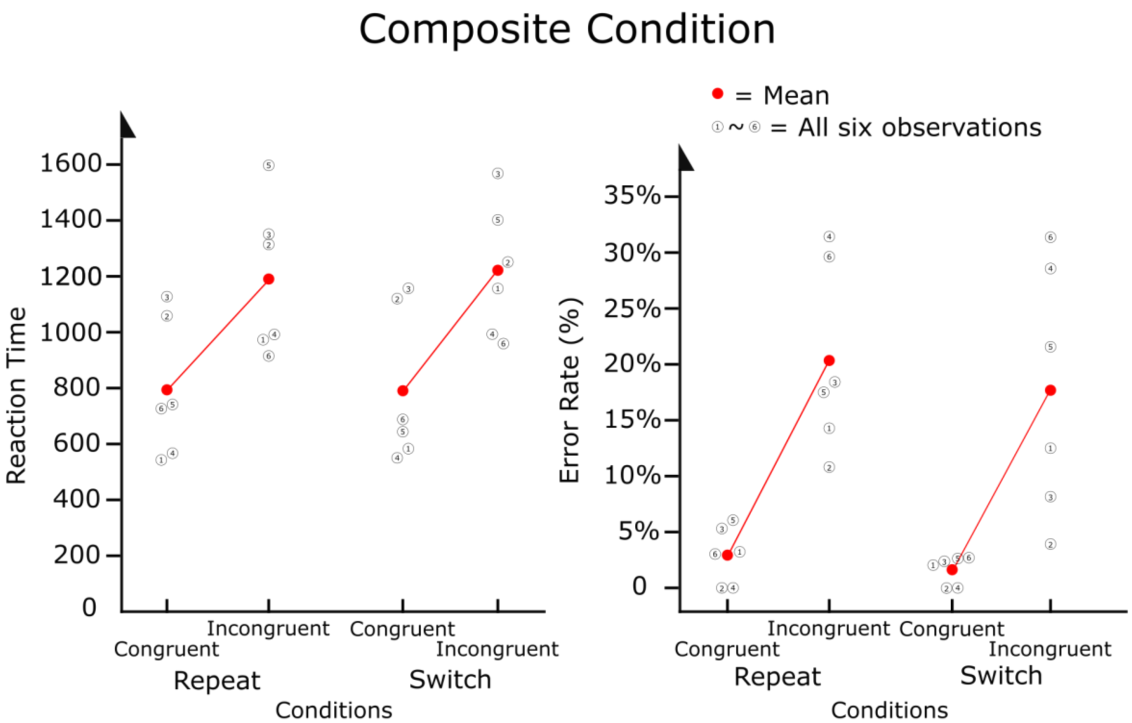

Figure 4.6. The line graphs illustrate the mean RTs (top) and ERs (bottom) in each trial condition (switch, repeat; congruent, incongruent). The open circles with numbers indicate each of the six observations.

Comparing the CSI condition with the SCI condition

Nine of the non-Chinese participants performed above chance level in both the CSI and the SCI condition. Their RT and error rates are shown in Table 4.3.

Table 4.3

Mean (SD) of RT and Error Rate for Each Trial Condition in the CSI and SCI condition

	CSI		SCI	
Condition/Group	RT ms (*SD*)	Error Rate (*SD*)	RT ms (*SD*)	Error Rate (*SD*)
Repeat Congruent	590 (60)	2.12% (4.1)	229 (123)	.594% (1.1)
Repeat Incongruent	865 (248)	10.3% (5.7)	742 (222)	12.0% (10.8)
Switch Congruent	592 (65)	.570% (1.7)	241 (110)	.483% (.9)
Switch Incongruent	846 (261)	9.74% (7.2)	723 (211)	12.7% (8.7)
Repeat	729 (164)	6.22% (4.3)	478 (145)	6.21% (5.1)
Switch	722 (161)	5.27% (4.0)	485 (149)	6.53% (4.6)
Congruent	591 (63)	1.25% (2.9)	235 (114)	.523% (.6)
Incongruent	855 (255)	10.3% (5.9)	732 (214)	12.5% (9.7)
Total	724 (162)	5.68% (.4.0)	484 (143)	6.45% (4.6)

A 2 × 2 × 2 ANOVA with repeated-measurements was conducted to examine the mean RTs. The three factors were the trial transition (switch, repeat), the congruency effect (congruent, incongruent), and the cue-stimulus sequence (CSI, SCI). The factor of congruency effect [F (1, 8) = 37.88, p = .0002, η^2_p = .83] and the factor of cue-stimulus sequence were significant [F (1, 8) = 43.11, p =.0001, η^2_p = .84]. However, the factor of trial transition was not significant [F (1, 8) = 0.701, p = .427]. In addition, the interaction between trial transition and the congruency [F (1, 8) = 6.08. p =.039, η^2_p = .43] and the interaction between the congruency effect and cue-stimuli sequence were statistically significant [F (1, 8) = 75.28, p = .0001, η^2_p = .90]. No other interactions reached statistical significance ($p > .05$).

An equivalent ANOVA was conducted to analyse the mean error rates (see Figure 4.7). The factors of trial transition [F (1, 8) = 1.36, p =.277] and cue-stimulus sequence (CSI, SCI) [F (1, 8) = 0.21, p =.657] were not significant. The factor of congruency effect was significant [F (1, 8) = 28.07, p = .0007, η^2_p = .15]. No statistically significant interaction was found (p > .05).

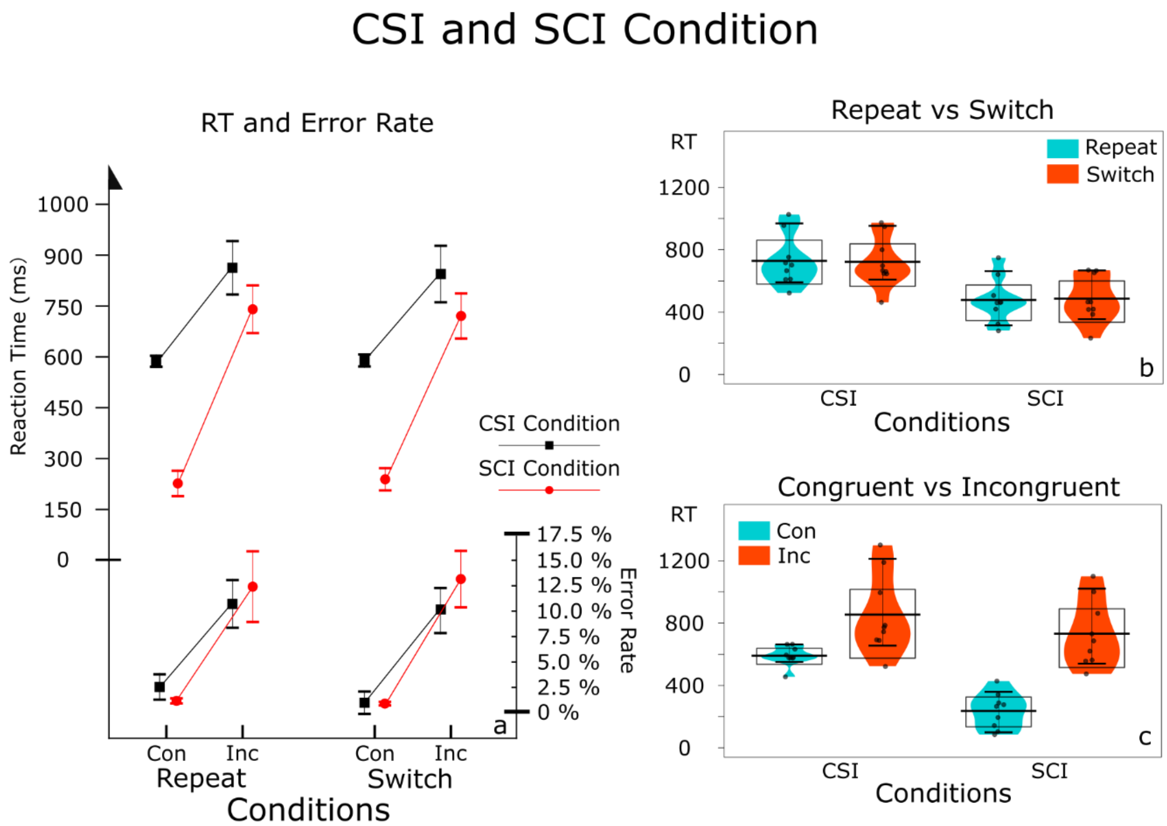

Figure 4.7. (a) The line graph shows the RT (top) and error rate (bottom) of each condition (switch, repeat; congruent [Con], incongruent [Inc]; CSI, SCI). The error bar denotes ±1 *SEM*. b) The violin plots show RT distributions from the repeat and the switch conditions. A jittered dot inside each distribution represents the average RT for each participants. The black horizontal bar and the band in the central area represent the mean and 95% CI of the mean in that condition, respectively. The responses slower than 90% of the total in that condition are reflected by the dots above the top horizontal bar. The responses quicker than 95% of the total in that condition are reflected by the dots below the bottom horizontal bar. c) Similar violin plots show the RT distributions from the congruent and the incongruent conditions.

Congruency and Switching Effect

There was a small but significant interaction between congruency and trial transition for RTs. This is because in the congruence condition, there were small task-switching costs (mean $_{[switch\ trial]}$ - mean $_{[repeat\ trial]}$ = 7 ms). However, the task-switching effect reversed under the incongruent condition (mean $_{[switch\ trial]}$ - mean $_{[repeat\ trial]}$ = -19 ms), meaning that the switch trials were quicker than the repeat trials but the differences were not significant ($p > .05$).

Congruency Effect and Cue-stimulus Sequence

There was a statistically significant RT interaction between congruency and Cue-stimulus sequence. The results of post-hoc pairwise comparisons with the Bonferroni correction suggest that, in the congruent trials, participants reacted significantly quicker in the SCI experimental condition than they did in the CSI experimental condition (mean difference = 355 ms; p = .0011). However, the difference between the CSI and SCI conditions for incongruent trials was not statistically significant (mean difference = 122 ms; p =.261).

Chinese Participants

All the Chinese participants were able to perform above-chance. A summary of their mean RTs and error rates is provided in Table 4.4 and illustrated in Figure 4.8.

Table 4.4

Mean (SD) for RT ms and Error Rate (ER) of Each Trial Condition in the Composite, CSI and SCI Conditions

	Composite		CSI		SCI	
Trial/Condition	RT (*SD*)	ER (*SD*)	RT (*SD*)	ER (*SD*)	RT (SD)	ER (SD)
Repeat Congruent	871 (159)	4.97% (4.4)	567 (72)	.426% (1.1)	314 (190)	.965% (1.7)
Repeat Incongruent	1079 (194)	15.67% (10)	774 (135)	12.3% (6.1)	616 (155)	8.91% (6.8)
Switch Congruent	867 (128)	3.85% (3.6)	579 (75)	.0645% (1.1)	344 (210)	1.07% (1.3)
Switch Incongruent	1099 (155)	18.71 (10)	772 (129)	10.26% (9.8)	610 (177)	11.1% (6.7)
Repeat	976 (155)	10.6% (6.2)	666 (90)	6.58% (3.5)	465 (163)	4.91% (3.6)
Switch	971 (120)	11.31% (6.2)	669 (93)	5.48% (5.4)	470 (186)	5.69% (3.1)
Congruent	869 (128)	4.34% (3.4)	573 (72)	.510% (.6)	323 (201)	1.05% (1.3)
Incongruent	1090 (169)	18.05% (8.6)	771 (128)	11.5% (8.4)	612 (166)	9.14% (5.7)
Total	973 (128)	11.01% (5.6)	667 (89)	5.90% (4.0)	468 (174)	5.35% (3.2)

Task-Switching, Congruency Effect and Cue-stimulus Sequence

A 2 × 2 × 3 ANOVA with repeated measures was conducted on the RTs. The three factors were: trial transition (switch, repeat), congruency effect (congruent, incongruent) and cue-stimulus sequence (composite, CSI and SCI). The factors of congruency effect [*F* (1,

14) = 65.03, p = .0001, η^2_p = .82] and cue-stimulus sequence were statistically significant [F (2, 28) = 124.8, p = 0001, η^2_p = .89]. Post-hoc pairwise comparisons with the Bonferroni correction indicated that the differences amongst composite, CSI and SCI were all statistically significant ($p < .0001$).

However, the factor of trial transition was not significant [F (1, 14) = 1.268, p =.279]. There was a significant interaction between the cue-stimulus sequence and the congruency effect [F (2, 28) = 4.83, p = .016, η^2_p = .34]; see later section for additional analyses. No other interactions were statistically significant ($p > .5$).

An additional ANOVA with the same design was conducted on the error-rates. The factor of cue-stimulus sequence was significant [F (2, 28) = 15.29, p = .0001, η^2_p = .52]. Post-hoc pairwise comparisons with the Bonferroni correction indicated that the difference between the composite condition and the CSI condition was statistically significant (p = .0018). Likewise, the difference between the composite condition and the SCI condition was statistically significant (p = .0002). However, the difference between the CSI condition and the SCI condition was not significant ($p > .05$).

The factor of congruency effect [F (1, 14) = 52.14, p =.0001, η^2_p = .78] was significant. The factor trial transition [F (1, 14) = 0.674, p = .435] was not significant. There were no other significant interactions. Figure 4.8 provides an overview of the RT and error rate.

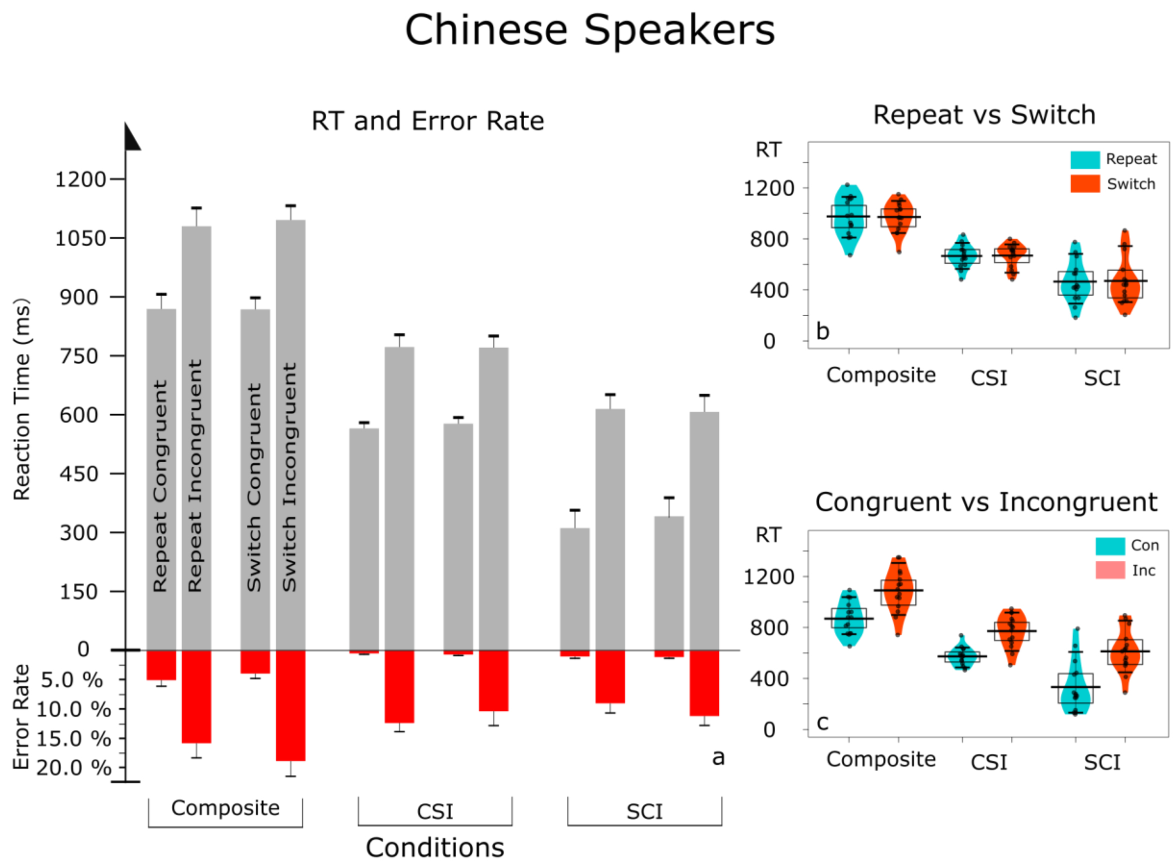

Figure 4.8. (a) The bar charts show the RT (top) and error rate (bottom) in each condition (switch, repeat; congruent [Con], incongruent [Inc]; composite, CSI, SCI). The error bars denotes *SEM*. (b) The violin plots illustrate the RT distributions in the repeat and switch conditions. The jittered dots inside each bean represent the average RTs of each participant. The black horizontal bar and the box around it represent the mean and 95% CI of the mean in each condition, respectively. Responses slower or quicker than 95% are represented by dots above and below the error bars. (c) Similar violin plots illustrate the RT distributions from congruent and incongruent conditions.

Interaction between the Congruency and Cue-stimulus sequence

Post-hoc pairwise comparisons with the Holm-Bonferroni correction were applied to further examine the RT interaction between the congruence effect and the cue-stimulus sequence. The Bonferroni correction is too conservative when comparing more than five different conditions. The Holm-Bonferroni correction can increase the statistical power (Holm, 1979). The results of the post-hoc comparisons are listed in Table 4.5.

Table 4.5.
The p-value of a Post-hoc Pairwise Comparisons

	Composite Cong	Composite Incongruent	CSI Congruent	CSI Incongruent	SCI Congruent
Composite Incongruent	.0005	-			
CSI Congruent	.0001	.0001	-		
CSI Incongruent	.1582	.0001	.002	-	
SCI Congruent	.0001	.0001	.0002	.0001	-
SCI Incongruent	.0001	.0001	.4717	.014	.0001

The results of the post-hoc test suggested that participants had a significantly quicker RT in the SCI condition than in the CSI and the composite conditions. This factor of congruency effect was significant in all three cue-stimulus sequences. In addition, the congruent trials in the SCI condition were extremely fast (mean = 323 ms). As a consequence, the largest congruency effect was found in the SCI condition (mean incongruent trials - mean congruent trials = 289 ms), and the factor of congruency effect was smaller in the CSI condition (198 ms) and in the composite condition (221 ms). This finding explained the interaction between the congruency effect and the experimental condition (see Figure 4.9).

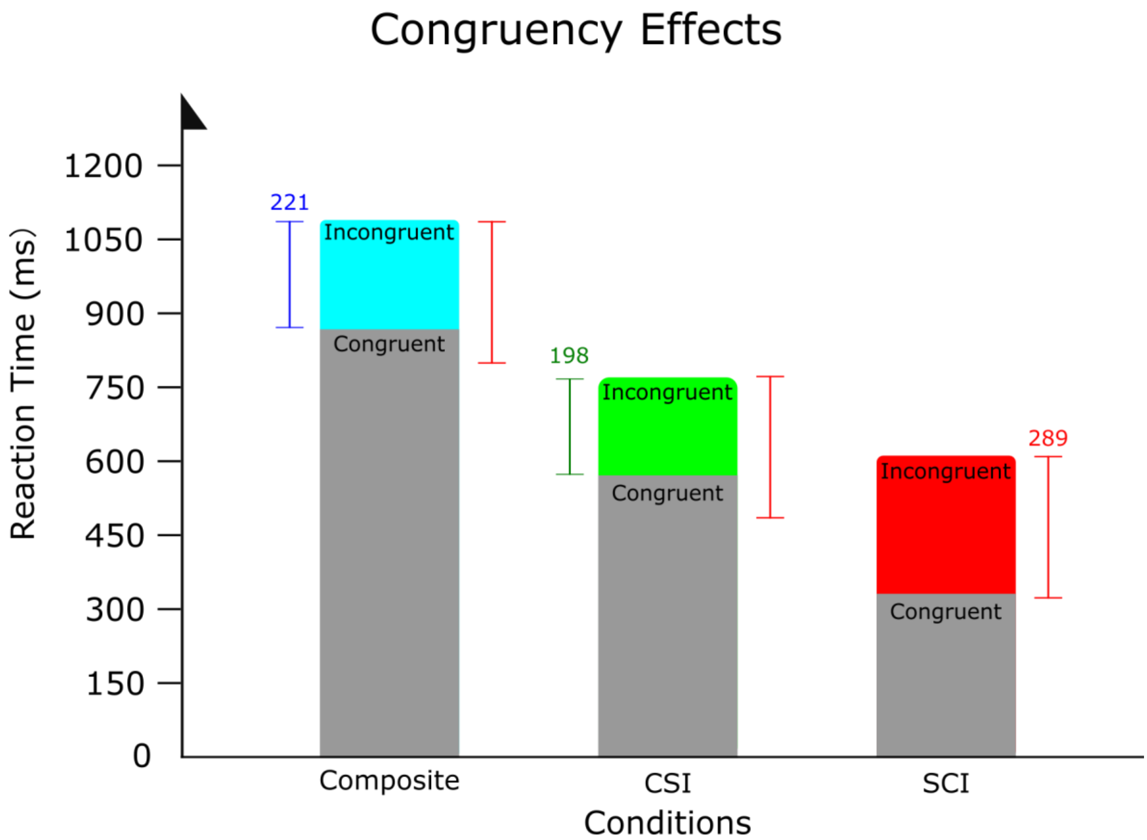

Figure 4.9. The bar graph shows the congruency effects for the composite, CSI and SCI conditions in blue, green and red, respectively. The largest congruency effect was found in the SCI condition.

Self-reports

All participants, Chinese and non-Chinese, reported that they had applied the BF strategy. None of the Chinese participants had noticed the association between the big/small and the odd/even tasks. In other words, none of the participants had applied the task-switching strategy.

4.3.4 Discussion

In agreement with my predictions, participants had shorter RTs in the SCI congruent condition than in the CSI congruent condition when the BF strategy was applied. In line with my predictions, the non-Chinese participants showed no task-switching costs. However, for Chinese participants, Experiment 4.2 was not able to replicate the results of Forrest et al. (2014) and Experiments 2.1 and 2.2. In those experiments, the task-switching costs remained significant although the participants indicated no explicit understanding of the task rules. In this experiment, without explicit understanding of the task rules, there were no task-switching costs for the Chinese participants.

In contrast to Experiment 4.1, the congruency effect was statistically significant for both language groups. Since none of the participants applied the task-switching strategy, we do not know whether or not this particular strategy eliminated the congruency effect amongst the Chinese participants in Experiment 4.1. In other words, one possibility is that the congruency effect is only eliminated when participants apply the task-switching strategy and the stimuli are Chinese numbers. The other possibility is that the results in Experiment 4.1 simply reflect a Type II error. Thus, a larger study that applies Chinese numbers with participants applying the task-switching strategy is required.

Differences between Experimental Conditions (Cue-stimulus Sequences)

Experiment 4.2 also found differences between all three experimental conditions. Firstly, the participants from both language groups performed better in the CSI and the SCI conditions than in the composite condition: they had shorter RTs and lower ERs. This difference may reflect a practice effect since all the participants were required to finish the composite condition first. When piloting the study, I tried to counterbalance all three conditions, but some of the non-Chinese participants failed to understand the CSI or SCI conditions when those two conditions came up first. Participants seemed to gain a better understanding of the experimental conditions if when were required to complete the composite condition first.

One of the most important discoveries in Experiment 4.2 is the performance difference between the CSI and the SCI conditions. Firstly, for the congruent trials, all the participants were significantly quicker in the SCI condition than in the CSI condition. The difference between the CSI and SCI conditions in incongruent trials was different for the two language groups. For non-Chinese participants, the CSI-SCI difference in incongruent trials was not statistically significant (mean difference = 122 ms; p =.261). For the Chinese participants, the difference was statistically significant (mean difference = 159 ms; p = .014). It is important to remember that only nine of the fifteen non-Chinese were able to perform above chance level in the CSI and SCI conditions. As a consequence, the sample size and power of this study was low. In the following section, I propose that the difference in performance in the SCI and CSI conditions may be due to the BF strategy.

The BF Strategy and the Congruency Effect

The idea of the BF strategy is that participants who followed this strategy treated the congruent and incongruent number stimuli differently. On the one hand, if a congruent number appeared, then a single IF-THEN rule was applied and a correct response was given. On the other hand, if an incongruent number appeared, the participant had to apply an additional conjunction rule (IF-THEN) before they could provide the correct response (see Experiment 4.1, Figure 4.4c). For example, if 贰 (*2*) appeared, participants had to apply the following conjunction rules: if 量 then press *left* key; if 质 then press *right* key. For participants who applied this strategy, a stimulus-cue interval was highly beneficial.

The Congruent Trials

Under the SCI condition, the congruent number appeared 500 ms before the task cue, so that participants could work out the correct response and wait. Once the top part appeared, they could then provide the response immediately. They could even start to carry out the response before the top part appeared (Figure 4.10). This may be the reason why some participants have mean RTs that are shorter than 200 ms in congruent trials of the SCI condition. This is illustrated in Figures 4.7c and 4.8c, in the SCI congruence conditions where a few RTs are lower than 200 ms.

For humans, the time from stimulus onset to the initiation of a motor response occurs around 200 ms (e.g., Welford 1980). Usually, research would exclude trials with RTs that are shorter than 200 ms in a reaction-time experiment, as it is beyond the average human response time and suggests that participants might have execute the response before they saw the target stimulus. In other words, the participants performed close to guessing. In the SCI congruent condition, however, it is perfectly reasonable for a participant to obtain and execute the correct response before the stimulus appears, because, in some trials, participants may even anticipate their own response delays and execute a response just before the task cue appears (see Figure 4.10c). Under the CSI congruent condition, the bottom part appears after the top part and participants cannot obtain the response in advance. As a consequence, the observed reaction times are longer (see Figure 4.10).

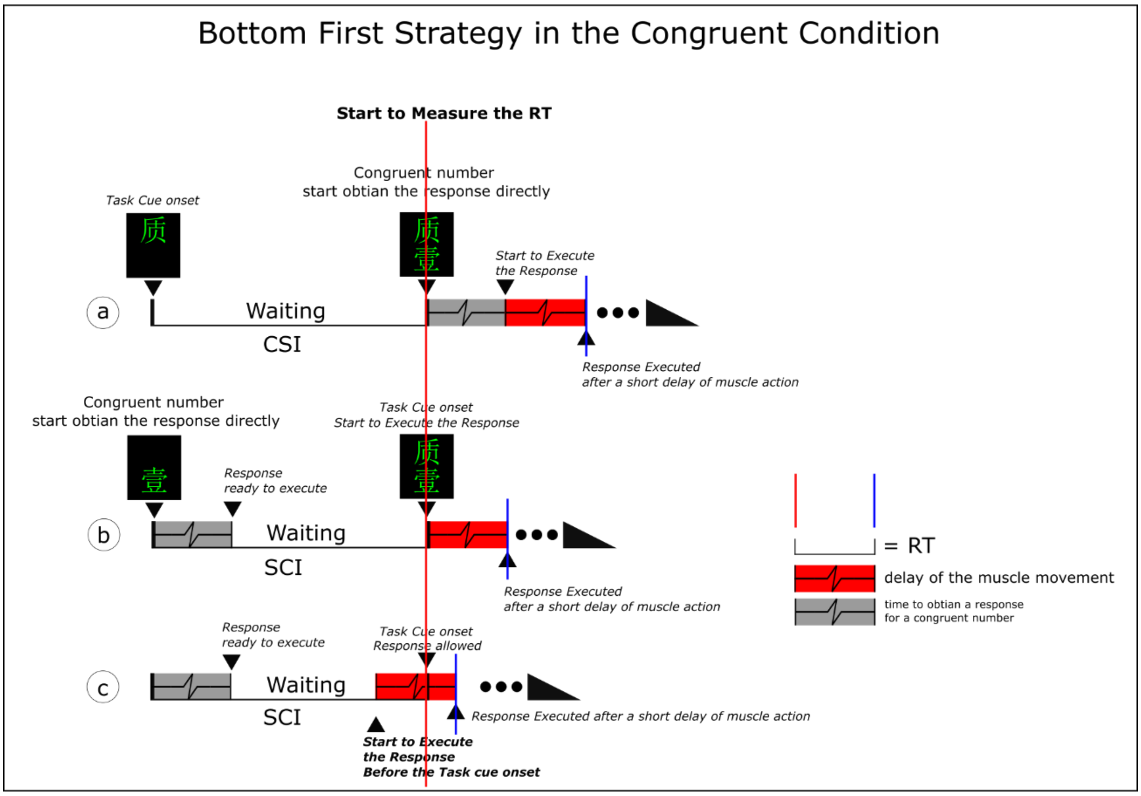

Figure 4.10. Schematic illustration of how the BF strategy can explain the relatively shorter RT in congruent trials of the SCI condition compared with the CSI condition. (a) This shows the response process under the CSI condition. (b) In the SCI condition, the response is prepared before the task cue appears. Therefore, the response is quicker than in the CSI condition. (c) In some trials, participants may even anticipate their own response delays and execute a response just before the task cue appears. This explains why, in some trials, the RT was less than 200 ms.

Incongruent Trials

All participants reported that they applied the BF strategy. The Chinese participants even reacted significantly quicker in the SCI *incongruent* condition than they did in the CSI *incongruent* condition. This is contradicts previous prediction. Unlike in the congruent trials, in an incongruent trial, no matter which part of the cue-stimulus combination appears first, a correct response cannot be obtained or fully prepared until both parts appears and, therefore, participants cannot deduce the correct response in advance. The remaining possibility is that although it is impossible to completely prepare a response in advance under the SCI incongruent condition, participants can still prepare the conjunction rule in advance. For example, in the SCI condition, if the incongruent number 贰 (2) shows up, there are only two possible variations: the top number is 量 or 质. In this case, it is relatively simple to prepare the two rules in advance:

If 量 then press *left*;

If 质 then press *right*.

Instead, in the CSI incongruent condition, if the task cue 质 appears, as the upcoming number remains unknown, participants do not know which conjunction rule to prepare in advance. In fact, they do not even know whether or not it is necessary to apply the conjunction rule, since there is a 50% chance that a congruent number will show up. Thus, they have to wait for the Chinese number to appear. As a result, participants who apply the BF strategy can respond more quickly in the incongruent trials of the SCI condition than the CSI condition (see Figure 4.11).

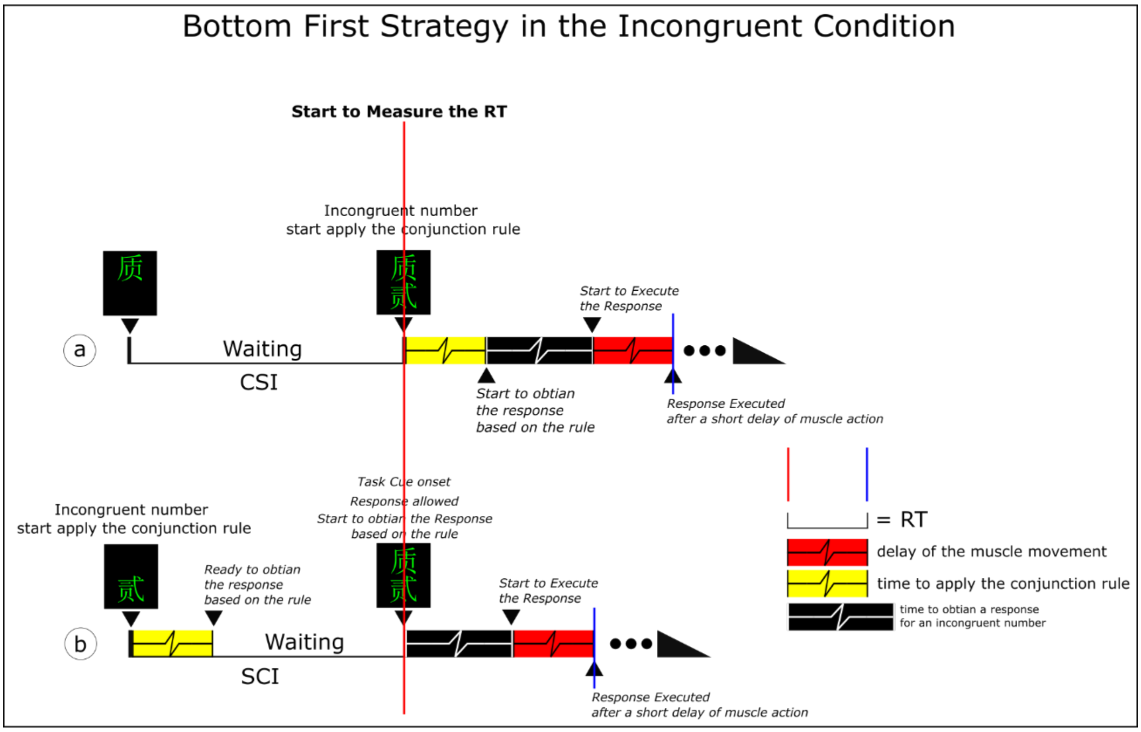

Figure 4.11. Schematic illustration of how the BF strategy can explain the shorter RTs in SCI incongruent trials than in CSI incongruent trials. (a) shows the response process in the CSI condition, (b) shows the response process in the SCI condition.

Task-Switching Strategy and the SCI condition

In a CSI condition, Forrest et al. (2014) reported that participants who applied the task-switching strategy performed better than participants who claimed to have applied the BF strategy. A possible reason for this difference is that participants who applied the task-switching strategy could start to apply the task rules after the task cue appeared, whereas participants who claimed to have applied the BF strategy had to wait until the stimulus number appeared.

Based on the same logic, I propose that the participants who applied the task-switching strategy should perform worse than the participants who applied the BF strategy in the SCI condition——at least, in the incongruent trials. This is because, in incongruent trials, participants who apply the task-switching strategy cannot prepare any task rules until the task cue appears, whereas those who apply the BF strategy can apply the relevant rules in advance. In congruent trials, the difference in strategy may be small, because the participants who apply the task-switching strategy may eventually notice that an additional rule is unnecessary and that they can always deduce the correct response directly after a congruent number appears.

One limitation of the present experiment is that no one applied the task-switching strategy, which means it is difficult to compare different strategies in the SCI condition of Experiment 4.2. An additional study that incorporates such a comparison may confirm or disprove the proposed strategy difference in the SCI condition. This is something that needs to be tested in the future.

Disadvantages of Traditional Chinese Numbers and Suggestions for Future Research

Because Traditional Chinese numbers are not very common in the Chinese language and are applied infrequently in everyday life, I hypothesised that using traditional Chinese numbers would prevent Chinese participants from applying the task-switching strategy. The results of Experiment 4.2 confirmed my hypothesis. Nevertheless, applying traditional Chinese numbers leads to two major disadvantages. Firstly, I underestimated the difficulty of memorising traditional Chinese numbers for non-Chinese participants. Traditional Chinese characters are more complicated than the Simplified Chinese versions. Therefore, many non-Chinese participants were unable to perform the experiment above chance level. Future studies need to include more training sessions before conducting the actual experiment.

Secondly, I suspect that, because traditional Chinese numbers are rarely used in written language in daily life, even Chinese participants had difficulty to recognising them as numbers. Therefore, if participants could not establish the task-switching strategy, they also could not identify the task-relevant mathematical features (i.e., the magnitude or parity of a Chinese number). This may be the reason why that even the Chinese participants showed no task-switching costs at all.

In other words, participants in Experiments 4.1 and 4.2, could not process or identify the task-relevant features when they did not apply the task-switching strategy. In contrast, I suggest that, in Experiments 2.1 and 2.2 and in Forrest et al. (2014), participants were able to process or identify the task-relevant features even without applying the task-switching strategy. In future studies, I recommend using Simplified Chinese numbers as stimuli. However, it should be possible to use traditional Chinese numbers. To ensure Chinese participants can identify the mathematical features that are relevant to the task, researchers may require participants to complete some simple arithmetic problems using the same Traditional Chinese numbers before the experiment. After training, participants should be more aware of the task-relevant features in the stimuli.

Is the BF Strategy a Rule-Based Strategy?

As mentioned in the previous section, the reason why some participants can react within 200 ms in the SCI congruent condition is that they can start to obtain the correct answer before the task cue (top part) is displayed. If they could not identify the bottom part first, such a quick reaction time would be very unlikely if not impossible. Participants undoubtedly applied the BF strategy during experiments. The question is whether the BF strategy is a rule-based strategy that requires certain levels of executive control similar to the task-switching strategy. Forrest et al. (2014) suggested that, after controlling the task-switching strategy, participants can only learn to perform the experiment by means of an associative learning process. In Experiment 4.2, the RTs in the incongruent trials provide evidence against this argument. I suggested that, in the SCI condition, not only do participants identify the bottom part of the cue-stimulus combination first, but they can also prepare the response according to the rules in advance, before the top part (task cue) appears. This is why Chinese participants, when applying the BF strategy, may respond significantly faster in incongruent trials in the SCI condition compared to the CSI condition. It is difficult to explain such an advance preparation effect without considering cognitive functions like applying the task rules in advance. I suggest that the BF strategy is a rule-based strategy that requires cognitive control.

4.4 General Discussion

One of the major contributions of Experiments 4.1 and 4.2 is that I have created a method that may consistently eliminate task-switching costs even for a shared stimulus-set. In my view, this is possible because, once participants have eliminated the task-switching rules, the semantic task features (i.e., the magnitude and the parity of a number) will be obscured in the Chinese number stimuli. As a result, participants can bypass both the task-set reconfiguration process and the interference from task-relevant features, so that task-switching costs should eventually disappear.

So far, the results from Experiments 2.1, 2.2, 3.1, 3.2, 4.1 and 4.2, suggest a consistent story. It is the interference from the previous trial alone that can produce task-switching costs, even when the task-set reconfiguration process based on the task-switching strategy is controlled. The argument that proactive interference can produce task-switching costs is not a novel idea. Many studies have provided detailed demonstrations of this effect (Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000; Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). However, some previous studies have considered the interference to be a product of the task-sets (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). Early studies posited that stimulus-based incompatibility (i.e., reversed stimulus-response mapping) was the source of the interference (Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000). Nevertheless, in these studies, all the participants received rule-based strategy and the stimulus-based inference still coexisted with task-sets. The present experiments propose that, even without participants realising the task-sets based on the task-switching strategy, interference can still impact participants’ responses. In the next chapter, I will further investigate this claim.

4.4.1 Novel Strategies and Task-set Reconfiguration

Before discussing the interference account further in Chapter 5, I need to discuss two potential counterarguments. Firstly, at the end of Chapter 2, I proposed an alternative explanation. I hypothesised that, even if participants were not aware of the two tasks based on the task-switching strategy, due to their application of a novel strategy such as the "black and white" strategy or the "circle and hexagon" strategy, they still treated the four colour-cue combinations and the other four shape-cue combinations as two different tasks. As a result, participants never truly eliminated the task-set reconfiguration process; rather, they simply created a novel task-set reconfiguration process.

However, this argument cannot explain the results found by Forrest et al. (2014). In their study, participants have not reported any novel strategies like the "black and white" strategy. Nevertheless, they reported that, after controlling the task-switching strategy, sometimes the task-switching costs were statistically significant (see their Experiments 1 and 3) and sometimes the task-switching costs were not significant (see their Experiment 2). The participants in Forrest et al. (2014) reported that they adopted a novel strategy that was similar to the BF strategy. Based on the results of Experiments 4.1 and 4.2, we know that the BF strategy does not always produce task-switching costs. Although the novel task-set reconfiguration might explain the results of Experiments 2.1 and 2.2, it cannot consistently explain all the results. So far, the interference account provides a better explanation.

4.4.2 Task-set Reconfiguration: Limitations and Preview of Chapter 5

The second counterargument we need to consider was advanced by Meier et al. (2016). They proposed that the use of the task-switching strategy and the corresponding task-set reconfiguration process can never be entirely discounted when testing humans. The problem is that, although researchers induce their participants to complete the experiment without receiving a task-switching instruction, they can only hope that the participants do not infer the task-switching strategy. They can offer no evidence to verify whether this does in fact occur. In order, to fully examine behaviour patterns without any possible task-set reconfiguration, Meier et al. (2016) employed pigeons, which have no higher executive control (e.g., Lea & Wills, 2008; Lea et al., 2009; Maes et al., 2015; Smith et al., 2011; Smith et al., 2012; Wills et al., 2009). Since rule-based strategies require executive control, applying rules and switching between tasks is impossible for pigeons. They found that, unlike humans, pigeons can perform task-switching experiments without any task-switching costs.

Experiment 2.2 examined participants' strategies with novel stimuli, and the results suggested that, in the experimental group without an explicit understanding of the task rules, the performance (error rate) with novel shape-task stimuli was not above chance level. In contrast, those participants who applied the task-switching strategy were able to respond to the novel shape-task stimuli significantly more accurately than they would have done by pure guesswork. In my view, the results of the novel stimuli trials indicate that, without an explicit understanding of the task rules, applying the task rules is almost impossible. Therefore, I further deduce that participants in the experimental group could not apply the task-switching strategy.

Nevertheless, according to Meier and colleagues' perspective (2016), the behavioural patterns with novel stimuli might only suggest that applying the task-switching strategy is less likely, but not impossible. For example, Meier et al., 2016 can assume that human participants can always apply task rules implicitly. A poor performance in novel stimuli trials might reflect the fact that dealing with novel stimuli requires an explicit understanding of task rules and that an implicit understanding is not enough.

Meier and colleagues would perhaps argue that, in Experiments 2.1 and 2.2, all the participants applied the task-switching strategy either explicitly or implicitly. Therefore, the task-switching costs remained significant. Moreover, they could further argue that in

Experiments 3.1, 3.2, 4.1 and 4.2, since the participants genuinely stopped using the task-switching strategy at times, the task-switching costs eventually disappeared. In particular, perhaps the decisive evidence to support their perspective is in the results of the non-Chinese participants from Experiments 4.1 and 4.2. As applying the task-switching strategy was impossible for those participants, unsurprisingly, the task-switching costs disappeared in Experiments 4.1 and 4.2.

The results of Forrest et al. (2014) can be explained in the same way. Perhaps, in their second experiment, no (or only a few participants) figured out the task-switching strategy, even implicitly. Thus, the task-switching costs were not significant. Conversely, in their Experiments 1 and 3, all the participants figured out the task-switching strategy implicitly. Therefore, the switching effect remained significant. From Meier and colleagues' (2016) perspective, any factor besides task-set reconfiguration is unnecessary.

However sophisticated such a counter argument appears, it is theoretically possible. This is because, in Experiments 2.1 and 2.2, I was unable to fully control the task-set reconfiguration process based on the task-switching strategy. Participants may have applied the task-switching strategy covertly. Moreover, in Experiments 4.1 and 4.2, I could not separate the interference from the task rules——if participants could not figure out the task rules, then it is very likely they could not identify the task-relevant features either. Hence, I could not objectively manipulate the interference. To provide conclusive evidence to prove my argument that——the interference of task features alone can produce task-switching costs, even without any task-set reconfiguration and without switching between tasks, I need to design an experiment that includes no tasks based on the task-switching strategy but the interference still exist. This is one of the intentions of the next chapter.

Chapter 5: Binary Feature-response Mappings and Proactive Interference

Based on the results of Experiments 2.1, 2.2, 3.1, 3.2, 4.1 and 4.2, I propose that, as long as participants need to identify and process both task-relevant features before deducing a correct response, there will be interference between trials that produces task-switching costs. For example, in Experiments 2.1 and 2.2 participants were required to identify and process both colour features and shape features before deducing a correct response.

Moreover, I suggest that, even when participants do not apply a task-switching strategy, interference from the preceding trial can still create task-switching costs. However, as discussed in Chapter 4, the problem, according to Meier et al. (2016), is that there is no objective control of the strategy that participants apply during the experiment. In a typical task-switching experiment, it can be argued that participants may apply the task rules or the task-switching strategy covertly (i.e., implicitly or unconsciously), so it is difficult to eliminate task-set reconfiguration processes.

In the present study, I tried to overcome this problem by removing both tasks entirely. In consequence, participants could not apply the task-switching strategy. In particular, the present study applies the same stimulus-set and task cues as Experiments 2.1 and 2.2, without including the colour and shape tasks. Consequently, unlike in Experiments 2.1 and 2.2, where I was left with no other option than to *induce* the participants not to apply a task-switching strategy, here switching between the colour task and the shape task is not possible. This provides an opportunity to examine whether task-switching costs between the "colour" task and the "shape" task remain when I control the task-set reconfiguration process.

Similarly to Experiments 2.1 and 2.2, there were eight possible cue-stimulus combinations in the present study. Four of these combinations had a "colour cue" while the other four combinations had a "shape cue". However, because I rearranged the mappings between cue-stimulus combinations and responses these cues do not represent binary tasks. I measured "task-switching costs" even though there were no explicit tasks. In this chapter, if *trial n - 1* and *trial n* had different task cues, then *trial n* was defined as a switch trial. Conversely, if *trial n - 1* and *trial n* had the same task cue, then *trial n* was defined as a repeat trial. I measured the RT and error-rate differences between the repeat and the switch trials. I reasoned that, as long as the participants had to identify and process both task-relevant features (i.e., the colour and the shape of a stimulus) before obtaining the correct response, there would be interference between successive switch trials.

The idea that feature-response mappings might affect reaction times is not new (e.g., Hommel, 1998, 2005). Hommel (1998) used a task that required two successive responses (R1 and R2) to two successive stimuli (S1 and S2). S1 and S2 varied randomly in form, colour, and location. In other words, they had multiple features. In addition, each feature was binary with only two levels. The responses were also binary, with only two possible keys: the *left* key or *right* key. At the start, a cue indicated the correct answer to the first response, R1. The participants were required to prepare R1, and to execute it as soon as S1 was presented, regardless of the stimulus included as of S1. One second later S2 was presented; and one feature of S2 (say, colour) determined the second response R2. Hence, R1 was executed in response to the mere onset of S1, whereas R2 was made in response to the relevant feature of S2. Hommel (1998) found that R2 was fastest when both the task-relevant stimuli features and the response were the same between the first and second stimulus-response-events. In contrast, when the two stimulus-response-events matched only partially (e.g., the same response, but a different task-relevant stimulus feature), R2 was slow even compared to a complete mismatch of the two stimulus-response-events (e.g., different responses and different features).

Furthermore, previous task-switching studies have suggested that proactive interference is essentially the competition between feature-response mappings. Woodward et al. (2003) suggested that the task performed on *trial n - 1* (e.g., a colour task) requires participants to activate one feature-response mapping (e.g., white == *left*), while at the same time to inhibit or negatively prime another feature-response mapping (e.g., hexagon == *right*). If, however the inhibited or negatively primed mapping is useful on *trial n*, additional time is required to reactivate it, and this results in a switching cost. Hence, as long as the participant had to identify both task-relevant features in order to make the correct response, there would be interference from the previous trial. This interference would delay the response time and trigger task-switching costs.

5.1 Arrangement and Features

The major challenge of the present study was to eliminate the colour task and the shape task from Experiments 2.1 and 2.2 while still requiring participants to identify the task-relevant features. I believe identifying the task-relevant features is the source of the interference. Therefore, we may look at a typical task-switching experiment from a different perspective. The ultimate goal for a typical task-switching experiment is to deduce the assigned response key for each cue-stimulus combination. For example, in Experiments 2.1 and 2.2 participants had to remember eight cue-stimulus combinations (e.g., c1, c2, c3…c8; see Figure 5.1a) and assign four combinations to the *left* key and the remaining four to the *right* key.

Here is a simple combinatorial question: if we want to randomly assign eight combinations to two groups (i.e., the *left* key group and the *right* key group) so that each group has four combinations, how many different arrangements without repetition are possible? The answer is as follows:

$$N = \frac{8\times7\times6\times5}{4\times3\times2\times1} = 70$$

There are 70 different arrangements, and the arrangement in Experiments 2.1 and 2.2 is just one of them. Furthermore, each cue-stimulus combination from Experiments 2.1 and 2.2 has three layers of information, and each layer has two levels: the cue (colour cue or shape cue), the colour (black or white), and the shape (circle or hexagon; see Figure 5.1b).

These are all binary, so that each cue-stimulus has three binary features.

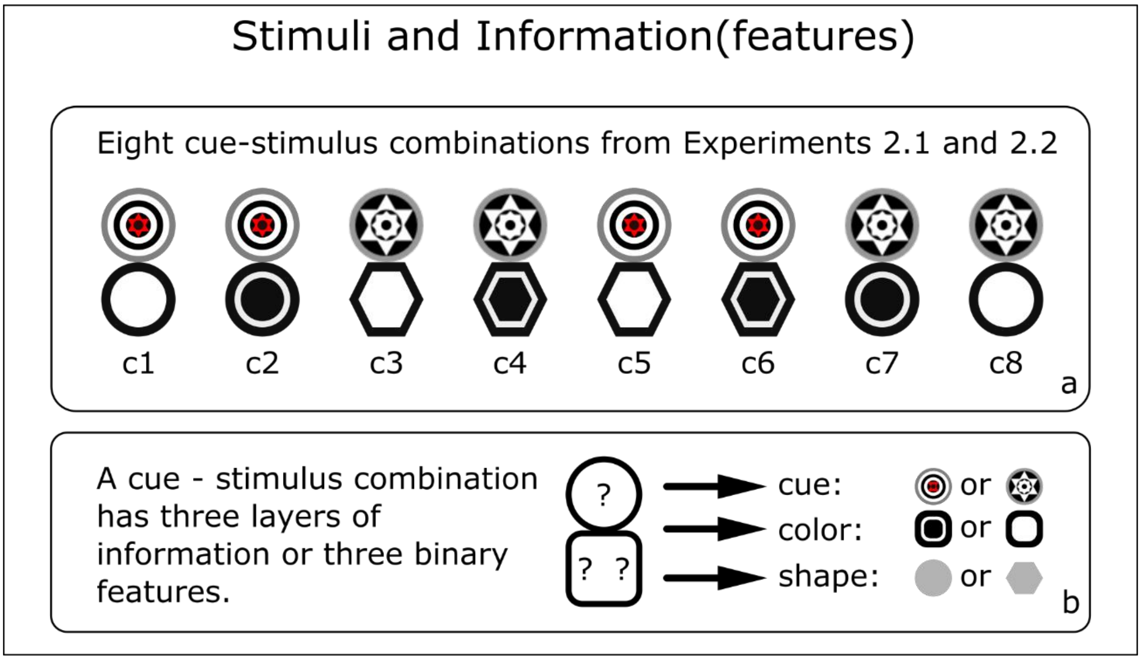

Figure 5.1. (a) All possible cue-stimulus combinations in Experiments 2.1 and 2.2. I assigned an index (c1 to c8) to each combination. (b) Each combination has three layers of information, or three binary features: colour, shape and the task cue.

It is important to mention that, amongst the 70 different arrangements, not all arrangements require the processing of all three features in the cue-stimulus to deduce the assigned response keys. For some arrangements, obtaining a correct response may only require a single feature. For example, if we assign four colour-cue combinations to the *left* key and four shape-cue combinations to the *right* key, then the correct answer solely depends on the cue type and the stimuli become irrelevant. Sometimes, an arrangement may require participants to process more than one feature to deduce the correct response. For example, Figure 5.2 shows an arrangement in which participants need to process two features before they can give the correct response. The correct response is determined by the cue and the shape of the stimuli, whereas the colour of the stimuli (black, white) is irrelevant. Since the circle and hexagon are associated with opposite response keys when the cue switches (see Figure 5.2), the task-set reconfiguration account would predict a task-switching cost. Arguably, every time the cue switches, the task-set needs to be reconfigured, because it has reversed. Previous task-switching studies with a similar design consistently reported significant task-switching costs (e.g., Hsieh & Yu 2003; Hsieh & Liu, 2005; Barber & Carter, 2005).

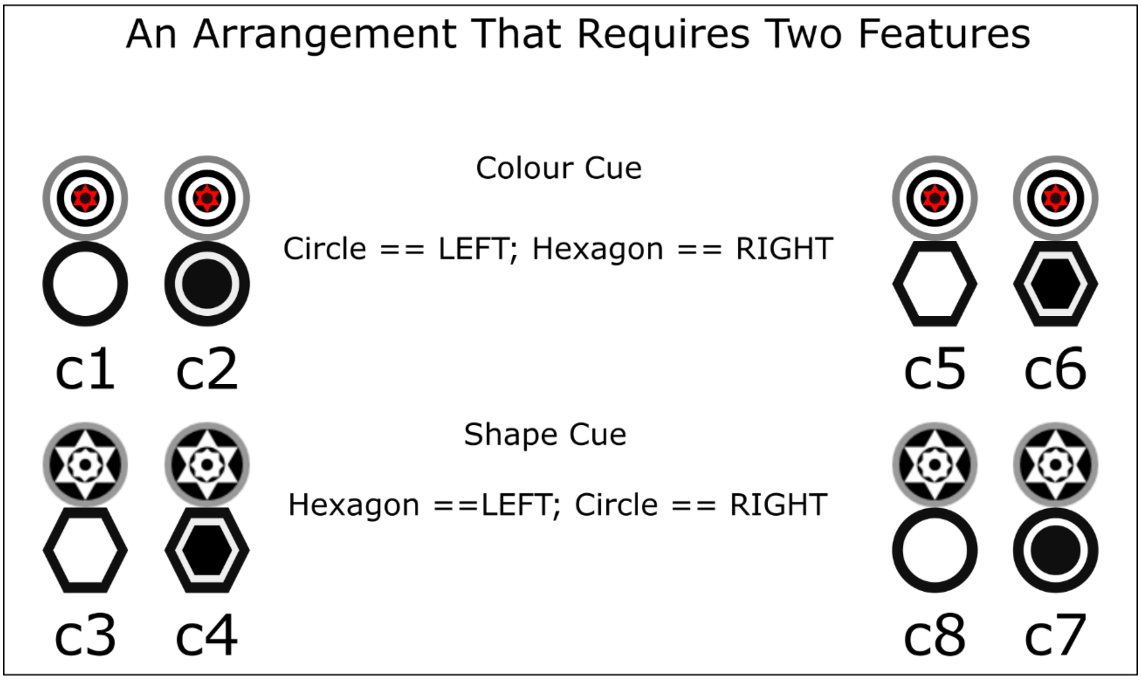

Figure 5.2. For this arrangement, participants have to process two features: shape feature and task cue, before they can deduce the correct response. The colour feature is irrelevant. Arguably, every time the cue switches, the task-set needs to be reconfigured, because it has reversed. Therefore, the task-set reconfiguration account would predict a task-switching cost.

5.1.1 Three Features and the Task-switching Strategy

For some arrangements, participants have to process all three cue-stimulus features before they can deduce the correct response. Figure 5.3 lists three different examples. For these three arrangements, identifying a particular combination of response mapping requires sets of conjunctive rules or IF-THEN statements that use colour, shape and the cue. Therefore, participants have to identify and process all three features to obtain correct responses.

Figure 5.3a illustrates the arrangement applied in Experiment 2.1 and 2.2. Under this particular arrangement, participants can apply the task-switching strategy and switch between the colour task and the shape task. Amongst all 70 possible arrangements, only four arrangements suggest a task-switching strategy (see Figures 5.3a and 5.3b). Meier and colleagues (2016) proposed that in the task-switching experiment participants may implicitly apply the task-switching strategy even when researchers take precautions to eliminate the task-switching strategy. If an experiment incorporates the two arrangements in Figure 5.3a and 5.3b, then both the interference account and the task-set reconfiguration account would predict that there will be significant task-switching costs. However, if we avoid these specific arrangements, then we may be able to eliminate the task-switching strategy completely.

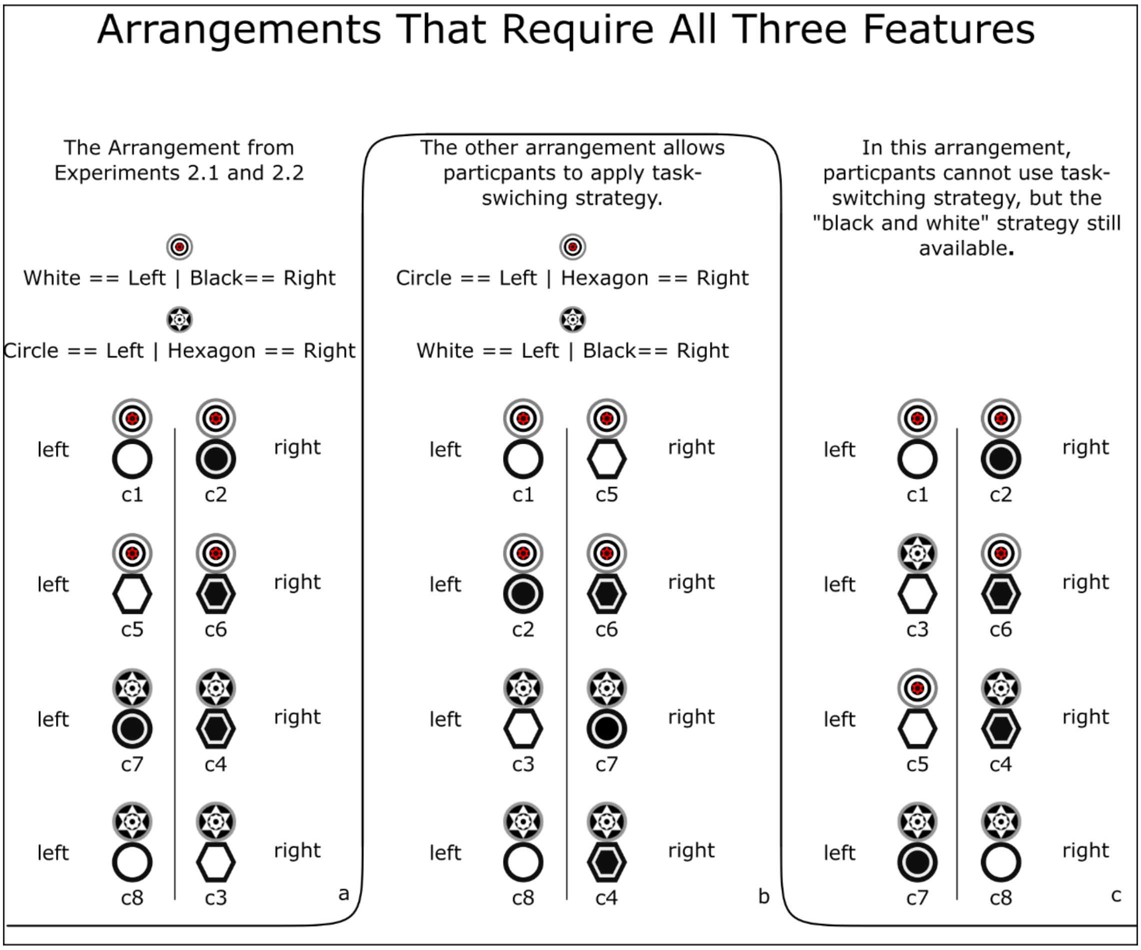

Figure 5.3. Three arrangements that require participants to process all three types of features to deduce the correct response. (a) and (b) allow participants to perform both the task-switching strategy and the “black and white” (BW) strategy; (c), however only allows participants to apply the BW strategy.

5.1.2 Three Types of Features and the BW Strategy

Figure 5.3c depicts another arrangement that requires participants to process all three types of features to deduce the correct response. Although applying the task-switching strategy is not possible, participants can apply a strategy similar to the novel strategies I identified in Experiment 2.2 (see Figures 4a and 4b).

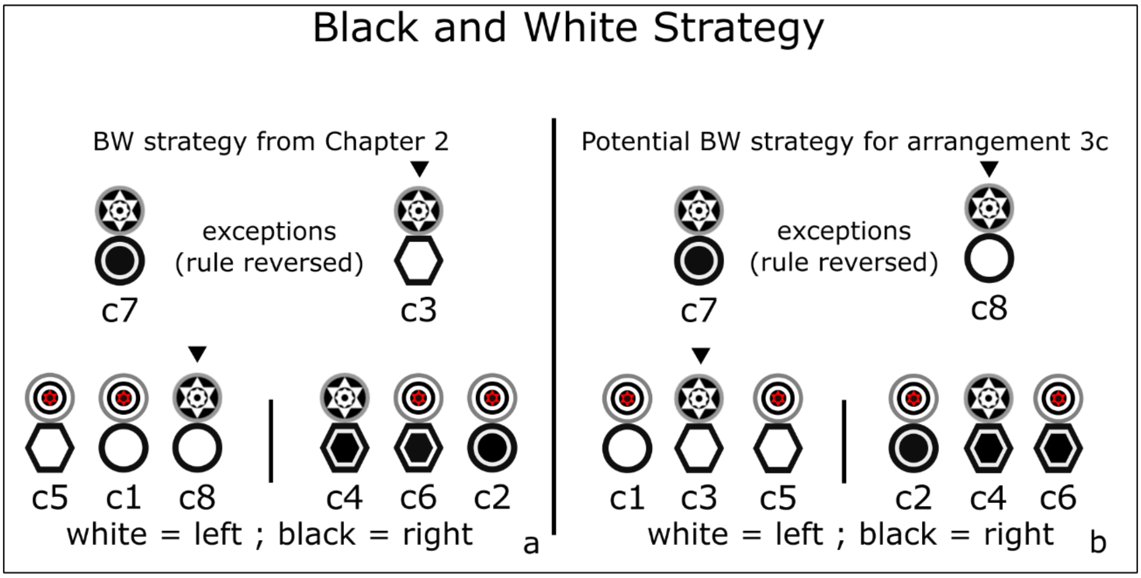

Figure 5.4. The principle of the BW strategy: three white stimuli correspond to the *left* key. Additionally, three black stimuli correspond to the *right* key. Only two exceptions need to be remembered. (a) The actual strategy participants used in Experiments 2.1 and 2.2. (b) The strategy participants would potentially use under the arrangement in Figure 5.3c. The difference between (a) and (b) is highlighted by black triangles above the cue-stimulus.

Arguably, when participants apply the BW strategy, they may still treat the four colour-cue combinations and four shape-cue combinations differently although the colour and the shape task no longer exist. In particular, if participants apply the BW strategy every time a colour cue shows up, they may apply the colour rule: white == *left* | black == *right*. However, because both exceptions involve the shape cue, every time the shape cue is presented, participants need to decide whether this combination is an exception or not. If it is an exception, the colour rule reverses; otherwise, the colour rule applies.

Still, every time the cue switches, there may be an extra reconfiguration process going on. That process might trigger a task-switching cost. I proposed this hypothesis at the end of Chapter 2 but in Chapter 4, I also suggested that this extra task-set reconfiguration process is not a perfect explanation of all the results.

5.1.3 A Special Arrangement

In the last section, I introduced three different arrangements in which participants have to process all three features before they can give the correct response. If an experiment applies these arrangements, we cannot eliminate the possibility of task-set reconfiguration every time a cue switch occurs. However, the arrangement in Figure 5.5a is special. It is likely that participants develop a "Colour-Shape Cue" (CSC) strategy: only remembering two exceptions (see Figure 5.5b), but in general applying the colour cue == *left* and the shape cue == *right*. Because both exceptions involve the white hexagon, participants may treat the white hexagon differently from the other stimuli. Importantly, participants cannot use the BW strategy. One exception is based on a colour cue and another exception employs a shape cue. Consequently, even if the task-set reconfiguration process still exists, the following will be true: on average, a switch trial will not cause any additional task reconfiguration process compared to a repeat trial. As a consequence, task-set reconfiguration does not predict task-switching costs in this arrangement. If there is any task-switching cost, it is very likely to be due to interference.

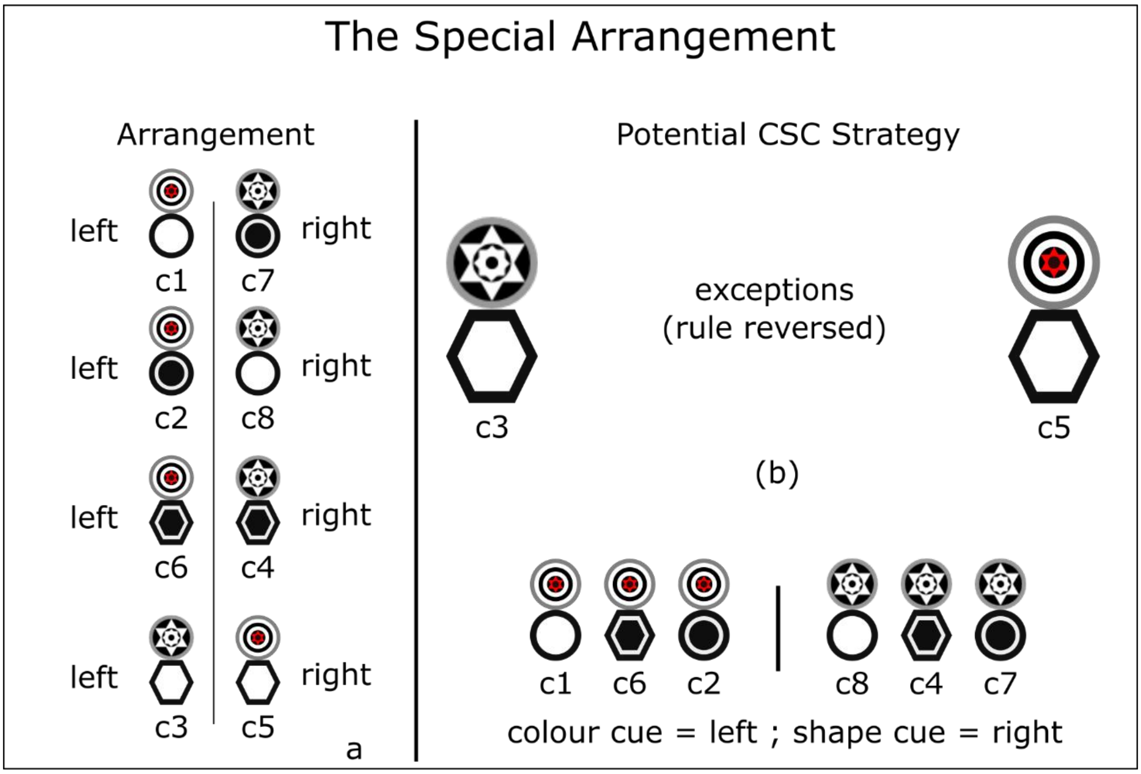

Figure 5.5. (a) The special arrangement in the present study. (b) Illustrates a hypothetical CSC strategy that participants are likely to adopt.

5.2 Experiment 5.1

In summary, in this experiment, I sought to eliminate the colour task and shape task by modifying the cue-stimulus arrangement in Experiments 2.1 and 2.2. This setting allows the testing of whether task-switching costs can arise from interference between trials without invoking the task-set reconfiguration process. Although colour task and shape tasks no longer exist after I modified the cue-stimulus arrangement, for the sake of simplicity, if *trial n - 1* and *n* had different task cues, I would still call *trial n* a switch trial. Also, if *trial n - 1* and *n* had the same task cue, trial n would still be a repeat trial.

To demonstrate the effect of interference from previous trials, I tested the arrangement in Figure 5.5. There is always a task or a strategy to achieve a goal during a psychology experiment. Here, I tried to ensure that interference from the previous trial was not confounded by task-set reconfiguration. The arrangement described in Figure 5.5

suggests that, on average, a switch trial would not cause any more task-set reconfiguration than a repeat trial. Therefore, in the present experiment, any task-switching costs could not be due to the reconfiguration process.

However, it was presumed that as long as participants needed to identify multiple features of the cue-stimulus combination (i.e., the colour, the shape), they would receive enough interference from the previous trial to produce task-switching costs. The arrangement in Figure 5.5 requires participants to identify three binary features (cue type, stimulus colour, and stimulus shape) before they can deduce the correct response. I predicted that participants in Experiment 5.1 would indicate significant task-switching costs. If participants showed any task-switching costs, these were very likely be the product of interference.

Apart from task-switching costs, it was also suspected that participants might apply the CSC strategy (see Figure 5.5). Therefore, participants might treat the two exceptions differently from the other six "normal" cue-stimuli combinations. I predicted that, if participants applied the CSC strategy, they would respond differently regarding reaction time and error rate in the trials with exceptions than in the trials with the other normal cue-stimuli combinations.

5.2.1 Method

Participants

Thirteen (mean age = 25.1, SD = 2.76; female = 9) PhD students from the University of Glasgow participated voluntarily in Experiment 5.1.

Apparatus and Stimuli

All stimuli were presented centrally on a BenQ computer monitor (24 inches). A Black Box Toolkit response pad was used to record participants' responses. Participants also used a QWERTY keyboard during instructions and to start the experiment. Experiment 5.1

included eight cue-stimuli combinations. These stimuli were identical to the stimuli in Experiments 2.1 and 2.2 (see Figure 5.5).

Procedure

The participants were instructed to memorise all eight items (cue-stimulus combinations) and the corresponding response keys. In Figure 5.5a, the four items on the *left* were associated with the *left* key; and the four items on the *right* were associated with the *right* key. Once an item appeared, participants were asked to press the corresponding key. The timeline of Experiment 5.1 was straightforward. In each trial, once an item appeared, the participants had 2.5 seconds to respond. If the participants made a mistake, an error message would be displayed on the screen for three seconds before the next trial started. If a correct response was given, the next trial would start after a 300 ms inter-trial interval (ITI).

The participants were asked to sign a consent form and sit in front of the computer screen (viewing distance 40 - 60 cm). Before the experiment, the participants were given instructions. They viewed a list of all cue-stimulus combinations and response keys and were asked to memorise these. The experiment had one 40-trial training block and five 100-trial experimental blocks. At the end of the experiment, each participant verbally reported the strategy they had used in the experimental blocks.

5.2.2 Results

It was predicted that the participants would indicate significant task-switching costs. It was also predicted that participants would respond differently regarding reaction time and error rate in the trials with two exceptions than in the trials with the normal cue-stimuli combinations. The descriptive data of mean RTs and error rate are listed in Table 5.1 and visualised in Figure 5.6.

Table 5.1.

Mean (SD) for RT and Error Rate of Each Trial and Stimuli condition.

	RT		Error Rate	
	Mean (ms)	*SD*	Mean	*SD*
Normal Repeat	904	230	2.28%	2.5
Normal Switch	975	268	2.95%	2.1
Exception Repeat	978	220	4.37%	4.6
Exception Switch	1140	275	7.03%	5.0
Repeat	923	221	2.77%	2.8
Switch	1014	264	3.89%	2.4
Normal	909	228	2.64%	2.0
Exception	1047	230	5.82%	3.8

A 2 × 2 ANOVA with repeated-measurements was conducted to compare the mean RTs within conditions. The two factors were the trial transition (switching, repeating) and the cue-stimuli condition (normal, exceptional). The factor of trial transition [F (1, 12) = 27.5, p = .0002, η^2_p = .70] and the factor of cue-stimuli condition were both significant [F (1, 12) = 8.17, p = .014, η^2_p = .41]. The interaction between the trial transition and the cue-stimuli condition was also significant [F (1, 12) = 8.02, p = .015; η^2_p = .40]. The factor of trial transition was larger in the exceptional conditions than in the normal cue-stimuli condition. Post-hoc pairwise comparisons with the Bonferroni correction suggested that the factor of trial transition was significant in both the normal (p = .015) and the exceptional (p = .003) cue-stimuli conditions.

An equivalent 2 × 2 ANOVA with repeated-measurements was conducted to compare the error rates. The factor of cue-stimuli condition was significant [F (1, 12) = 14.77, p = .0023 η^2_p = .55]. The factor of trial transition was not significant [F (1, 12) = 3.24, p = .096]. There was no significant interaction. In addition to the behavioural data, all participants reported that they applied a CSC strategy.

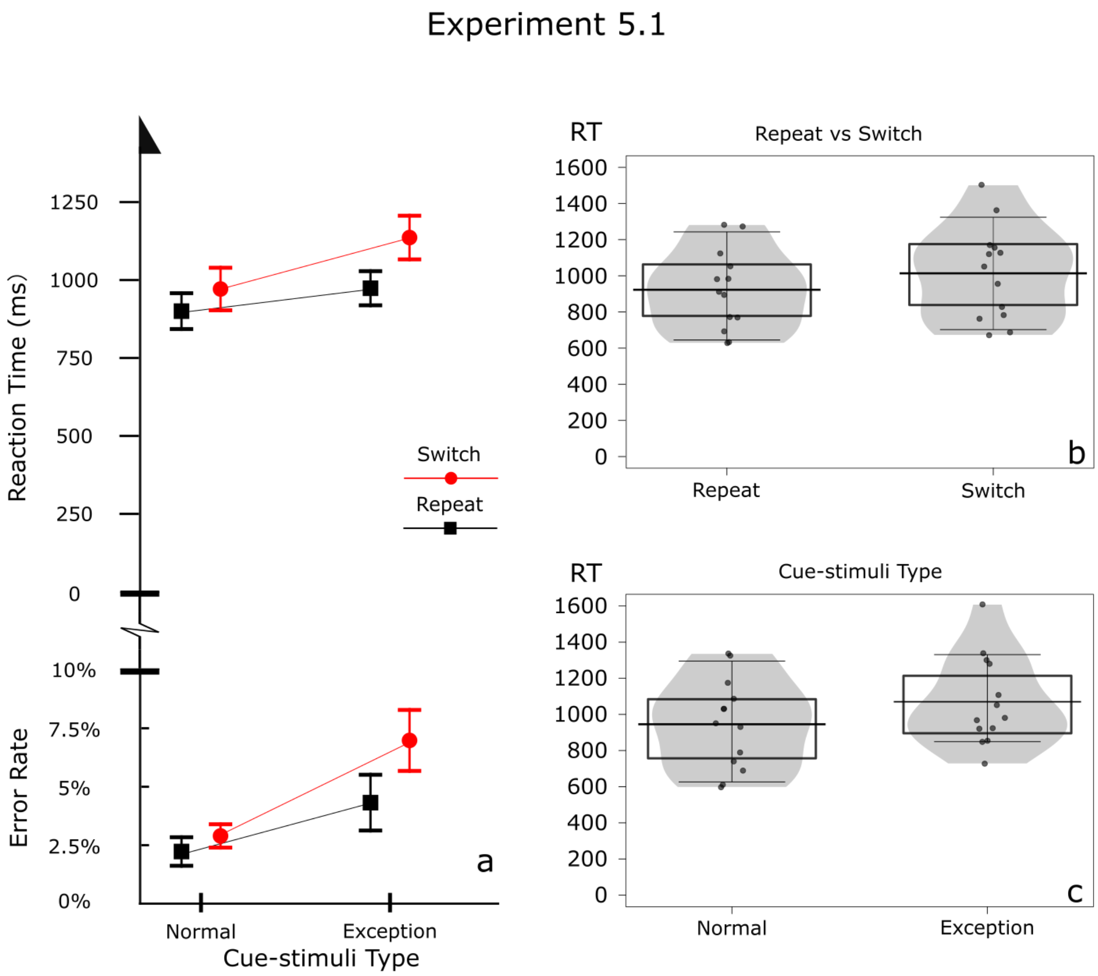

Figure 5.6. (a) The line graph shows the RT (top) and Error Rate (bottom) of each condition (switch, repeat; normal, exception). The error bars indicate ±1 *SEM*. (b) The violin plots illustrate the RT distributions for the repeat and switch trials. The jittered dots inside the shaded area represent the average RTs of each participant. The black horizontal bar and the box around it represent the mean and 95% CI of the mean in each condition, respectively. Responses slower or quicker than 95% are indicated by dots above and below the error bars. (c) Violin plots illustrate the RT distributions for the normal and exceptional cue-stimuli conditions.

5.2.3 Discussion

There were statistically significant task-switching costs, even though the switch trials did not trigger a task-set reconfiguration process and applycation of the task-switching strategy was impossible. In other words, the results suggest that, even without switching between a colour task and a shape task, we can still produce task-switching costs. My hypothesis is that task-switching costs in Experiment 5.1 are due to interferences of the features of the cue-stimulus combinations (colour, shape, and cue type) from the immediately preceding trial.

In addition, participants had longer RTs, higher error rates and greater task-switching costs in the exceptional cue-stimuli condition than in the normal cue-stimuli condition. All participants reported that they applied a CSC strategy. Hence, the differences between the two exceptions and the normal cue-stimuli combinations might reflect the CSC strategy. Since 75% of the trials included the normal cue-stimuli combination, perhaps, when applying the CSC strategy, participants assumed the rules for the normal cue-stimuli as a default (colour cue == *left*; shape cue == *right*). The problem lies in the fact that the two exceptions imply a delay when they eventually occur because the wrong rules have been prepared for.

It is worth mentioning that the task-switching costs I found in Experiment 5.1 may be confounded by the response-switching effect. The arrangement I used in this experiment produced an overlap between task-switching and response-switching. Therefore, 75% of the time when the "task" switches (i.e., the cue switches) the response key also switches (*trial n - 1* and *trial n* have different response keys).

Some previous studies have shown that switching between different responses can leads to response-switching costs (e.g., Bertelson, 1965; Eichelman, 1970; Notebaert & Soetens, 2003; Smith, Chase, & Smith, 1973). However, other studies did not find any response-switching costs (cf., Campbell & Proctor, 1993; Pashler & Baylis, 1991; Rabbitt, 1968). Could it be that the task-switching costs in Experiment 5.1 were actually response-switching costs? I suggest that it is not very possible.

Altmann (2011) suggested, that in task-switching experiments, the statistical support for any overall response-switching costs is very limited. Instead, there is usually only a response-switching by task-switching interaction. In particular, there are response-switching costs in the task-repeating condition, but in the task-switching condition, the response switching costs are somehow reversed——switching to a different response, in fact, reduces the reaction time in task-switching trials. The diverse explanations behind such interaction are beyond the scope of the present thesis (cf., Altmann, 2011 and Druey, 2014). However, the point is that there is not enough evidence to support any overall response-switching costs in task-switching experiments. To further illustrate this point, I conducted two paired t-tests to compare the mean RT of response-switching trials with the mean RT of response-repeating trials in Experiment 2.1 [switch - repeat = 5 ms; t (39) = .31, p = .76] and Experiment 2.2 [switch - repeat = -13 ms; t (25) =1.31, p = .20]. There is no meaningful difference between the response-switching trials and the response-repeating trials at all. Remember, Experiments 2.1, 2.2 and 5.1 applied the same cue-stimuli combinations.

Secondly, if the task-switching costs in Experiment 5.1 were caused by the response-switching effect, then because task-switching trials account for only 75% of all response-switching trials, the mean RT for the response-switching trials in Experiment 5.1 should be longer than the mean RT for the task-switching trials. However, a paired t-test suggested, that in Experiment 5.1, the mean RT of task-switching trials (1014 ms) was actually longer than the mean RT for the response-switching trials (984 ms), t (13) = 4.60, p =.0006). The response-switching effect is not a good alternative explanation for the results of Experiment 5.1.

5.3 Task Relevant Features and Task-switching Costs

In Experiment 5.1, I demonstrated that, without any additional task-set reconfiguration process during switching, we can still produce task-switching costs with the same cue-stimulus combinations as in a typical task-switching experiment. It was potentially the interference between features from the previous trial that triggered the task-switching costs in Experiment 5.1. However, to further confirm this hypothesis, it is necessary to address interference and to highlight that interference in a typical task-switching experiment, such as Experiments 2.1, 2.2 and Experiment 5.1, is caused by the same mechanism.

5.3.1 Proactive Interference and Task-set Inertia

In the following, I will address how one of the original task-switching accounts may explain all the results I have found so far. My explanation adopts the proactive interference account by Allport and colleagues (Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000; Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). Starting with their hypothesis, I will develop a method to estimate the amount of interference each trial received from the previous trial.

Allport and colleagues (1994) originally proposed that when a given *trial n - 1* commences, the task-set $_{(n-1)}$ is activated. If the next *trial n* is a repeat trial, then the new task-set $_{(n)}$ is very similar to the previous task-set $_{(n-1)}$. However, if *trial n* is a switch trial, then a disparity between task-set $_{(n)}$ and task-set $_{(n-1)}$ emerges. Therefore, in a switch trial, the current task-set is more incompatible with the previous task-set than in a repeat trial. Consequently, participants need to make additional effort to deal with priming and suppression (Allport et al., 1994), resulting in task-switching costs. Allport and colleagues called this incompatibility between two task-sets the “task-set inertia”.

Three Layers of Interference

For the present study, the term “task-set inertia” seems counterintuitive as one of this study’s major challenges is to eliminate the tasks and corresponding task-sets based on the task-switching strategy. However, the task-set inertia Allport and colleagues (1994) proposed is a passive cognitive process that requires no executive control. In fact, they originally assumed that stimulus-response mapping triggers task-set inertia. Therefore, task-set inertia is not a by-product of applying task rules or any task-set, rather, it is a direct consequence of processing the cue-stimuli combination. My interpretation here is that inertia is more closely related to “cue-stimulus inertia” than task-set inertia. Nevertheless, some studies suggested that interference might in fact arise from associations between the stimulus and task-set (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006).

In Chapter 3, I discussed how participants need to process all three features (task cue, stimulus colour, and stimulus shape) from the stimulus before they can obtain the correct response. Therefore, I suggest that, in each given trial of Experiments 2.1 and 2.2, each cue-stimulus combination could have triggered three layers of cue-stimulus inertia (or interference): cue interference, colour interference, and shape interference. Woodward and colleagues (2003) already proposed that feature-response mapping is at the core of proactive interference. Here, I suggest that the task cue can also be a feature that causes proactive interference.

Interference and Feature-response Mapping

Previous studies suggested that the mere co-occurrence of a stimulus and a response can create an automatic association between features (of the stimulus) and the response (e.g., Hommel 1998, 2004, 2005; Zmigrod & Hommel, 2013). Based on this idea, I will demonstrate that each layer has binary feature-response mappings. In Figure 5.7a, I use the cue-stimulus combinations from Experiments 2.1 and 2.2 as examples. In Experiments 2.1 and 2.2, the combination c8 was assigned to the *left* key. Therefore, every time c8 was presented, participants had to press the *left* key. The combination c8 included three features: the shape cue, the white colour and the shape of the circle. Based on the idea of feature-response integration (Hommel, 1998, 2005), I propose that, when the participants executed the correct response for c8 (i.e., pressed the *left* key), those features formed three mappings with the response:

shape cue == *left*
white == *left*
circle == *left*

In addition, because in Experiments 2.1 and 2.2 each features had only two levels (they were binary), I further hypothesise that when one level was associated with the *left* key, then the other level was automatically associated with the opposite key. Thus, c8 actually triggered the following three binary feature-response mappings:

shape cue == *left* | colour cue == *right*
white == *left* | black == *right*
circle == *left* | hexagon = *right*

Similarly to the specific combination c8, all cue-stimulus combinations from Experiments 2.1 and 2.2 can trigger three binary feature-response mappings. In general, I propose that any task-cueing paradigm with a shared stimulus-set can trigger the three binary features-response mappings. For visual tasks (e.g., Experiments 2.1 and 2.2), the mappings were all visual, whereas for linguistic tasks (e.g., Experiments 4.1 and 4.2), the mappings involved some semantic representations. In short, building on the idea of task-set inertia, I propose that interference is actually based on three binary feature-response mappings.

5.3.2 The Sum of Interferences

Proactive interference sometimes causes very little and sometimes large interference in subsequent trials. If this was not the case, then there would be no difference between repeat trials and switch trials on average. This section will demonstrate how interference may vary from trial to trial, and how to calculate the interference difference.

Let us assume that in *trial n - 1*, the combination c8 appears, and in *trial n*, one of the eight combinations appears randomly. Since different combinations trigger different feature-response mappings, *trial n - 1* can affect *trial n* differently. As illustrated in Figure 5.7b, in some trials all three feature-response mappings are compatible with the previous trial. In other trials one or two feature-response mappings are incompatible. Yet, in other trials all three pairs are incompatible. Notice that “incompatible” means that the feature-response mappings are reversed between trials. For example, if in *trial n - 1*, white == *left* | black == *right*, but in *trial n*, black == *left* | white == *right*, then we can say the colour-response mappings are incompatible between the two trials. According to previous studies (e.g., Hommel, 1998, 2005), this feature-response incompatibility can delay the response.

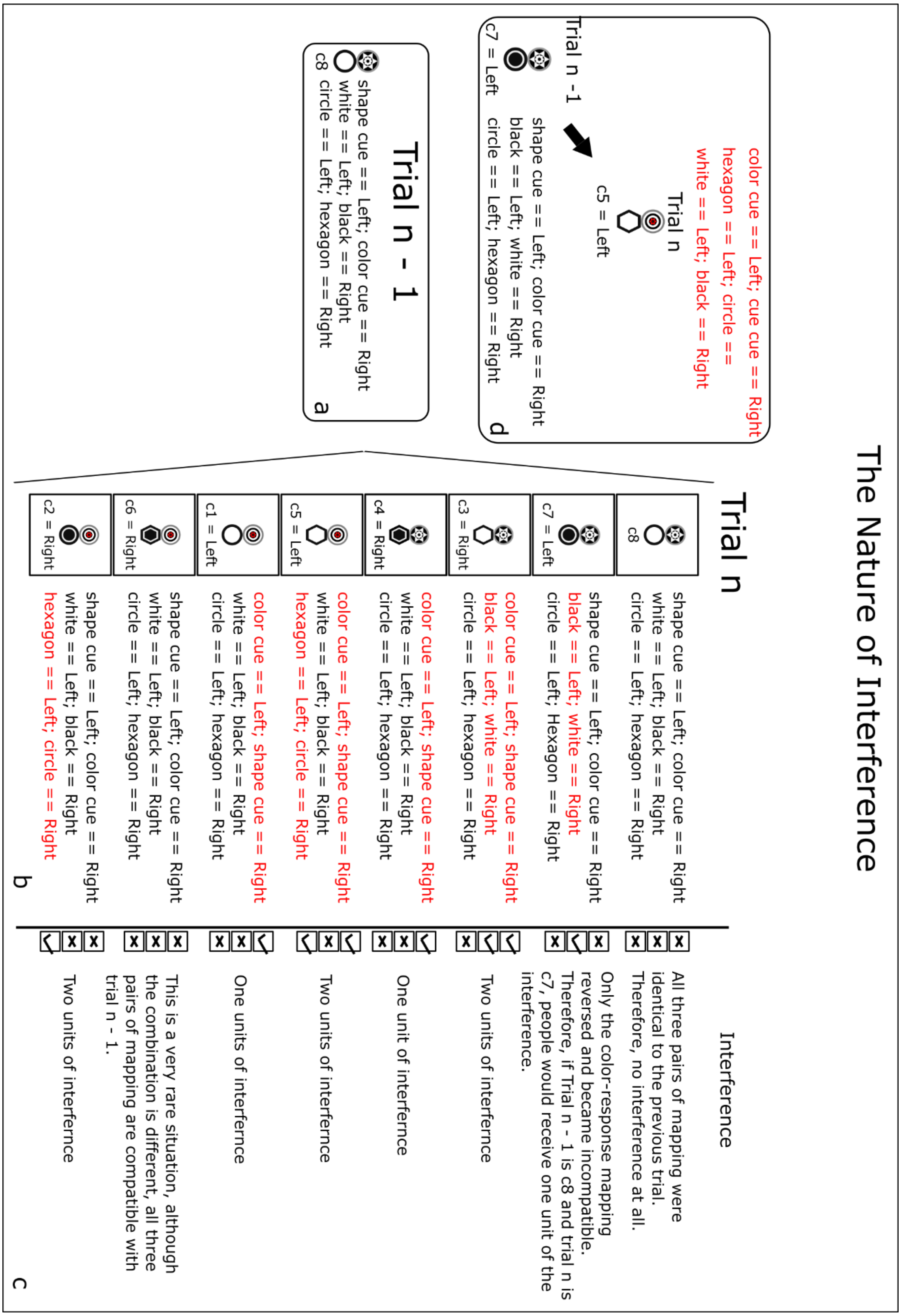

Figure 5.7. Illustration of interference between trials. (a) If c8 is presented in *trial n - 1*, three pairs of feature–response mappings will be activated. (b) In *trial n*, there are eight different possibilities that can activate different feature–response mappings. (c) Depending on which combination appears in *trial n*, the proactive feature–response mappings from *trial n -1* will cause different amounts of interference. (d) Very rarely (6.25%; see Appendix A) participants may experience three different interferences at the same time in *trial n*.

For the sake of simplicity, let us assume that each incompatible feature-response mapping between trials contributes exactly one unit of interference. We can then estimate the amount of interference that participants may experience from the previous trial (ranging from one to three units; see Figure 5.7c and d). As there are eight possible cue-stimulus combinations, progressing from *trial n - 1* to *trial n* will create 64 different possibilities: 32 repeat trials in which *trial n* and *trial n - 1* share the same cue and 32 switch trials in which the cue differs. By calculating the sum of the interference for the repeat and switch trials and dividing each by 32, we get the average interference for repeat trials and switch trials in arbitrary units.

On average, each repeat trial will receive 1 unit of interference from the previous trial. On the other hand each switch trial will have an average of 1.5 units of interference (see Appendix A for details). The .5 difference may explain why interference alone can produce the task-switching costs in Experiments 2.1 and 2.2, even when we rule out the task-switching strategy and the task-set reconfiguration process task-switching strategy.

Units of Interference in Experiment 5.1

Here I applied this logic in order to quantify the interferences and explain switching costs. Firstly, I focused on Experiment 5.1. This experiment applied the same eight cue-stimulus combinations as in Experiment 2.1 and 2.2. The only difference was the arrangement between cue-stimulus and response: the same combination could been assigned to the same or a different response key. Therefore, each cue-stimulus combination could have a binary feature-response mapping that was different to Experiment 2.1 and 2.2. For example, in Experiment 5.1 combination c8 was assigned to a *right* response. Thus, this could result in three alternative feature-response mappings:

colour cue == *left* | shape cue == *right*
black == *left* | white == *right*
hexagon == *left* | circle == *right*

In Experiment 5.1, shifting from *trial n - 1* to *trial n* created 64 different possible interferences in 32 repeat and 32 switch trials. Applying the above logic, I calculated the average interference that participants experienced in the repeat trials and in the switch trials of Experiment 5.1 (see Appendix A for details). On average, participants experienced 1.0625 units of interference in the repeat trials, but 1.68 units of interference in the switch trials. There is a difference of .62 units between the switch trials and the repeat trials. This demonstrates that a modified proactive interference account can explain the task-switching costs in Experiment 5.1.

The Weak Interference in the Chinese Number Paradigm

In Experiments 4.1 and 4.2, the non-Chinese group did not know the semantic task-relevant features of the Chinese numbers. For them the Chinese numbers were meaningless symbols. Consequently, these participants could only identify two features or two layers of information (the cue and the Chinese number) from each cue-stimulus combination. Furthermore, because the Chinese numbers had four levels (e.g., 五,六,七,八 for Experiment 4.1 and 壹,贰,捌,玖 for Experiment 4.2), these numbers could not form any binary feature-response mappings as in Experiment 2.1 and 2.2 and in Experiment 5.1. Hence, the only source of interference must have come from the binary cue-response mappings. In Experiments 4.1 and 4.2, the congruent numbers have the same response key in both tasks; the cue is irrelevant. Therefore, the “bottom first” strategy specially requires participants to identify the number and to ignore the cue, under congruency conditions. In an incongruent trial, both the cue and the stimulus determine the correct response. Hence, the cue-response mapping could only cause interference in very rare situations: when both *trial n - 1* and *trial n* were incongruent trials with different cues. I suggest that there was not enough interference to create task-switching costs. In fact, cue-response mappings would not produce interference even if participants identified them in every trial. Applying the same calculation of interference to Experiments 4.1 and 4.2, I found that, on average, both repeat and switch trials had about the same .5 unit of interference due to the cue-response mapping in the previous trial. The interference alone could not produce task-switching costs in Experiments 4.1 and 4.2.

Units of Interference and Non-task-relevant Features

In some trials in Experiment 3.1 and 3.2, the non-task-relevant feature in each stimulus may have prevented participants from employing the task-relevant feature. When participants applied the LUT strategy, they did not need to identify the cue. Therefore, they may have activated only one feature-response mapping: the non-task-relevant feature-response mapping. As the non-task-relevant feature had eight levels, it is difficult for participant to form a binary feature-response mapping. This may not have produced enough interference to create task-switching costs.

Interference in the Dual-cue Paradigm

Forrest et al. (2014) applied the dual-cue task-switching paradigm in their experiments. Each task had two cues, and there was a total of four task cues. Therefore, in some trials the cue switched while the task remained the same (i.e., cue-switch trials). In this paradigm, task-switching costs were measured as the difference between the task-switching trials and the cue-switching trials. The difference between cue-switching trials and cue-repeating trials is called the cue-switching cost (see Chapter 1, Section 1.8.1**)**.

Experiment 3 in Forrest et al. (2014) was a typical dual-cue task-switching paradigm. The four cues were: circle, triangle, square, and pentagon (see Figure 5.8). The two tasks were: the odd/even task and the big/small task. For the odd/even task, the participants were asked to decide whether a number was odd or even (odd == press *left* key; even == press *right* key). For the big/small task, a participant had to decide whether a number was smaller than 5.5 or bigger than 5.5 (small == press *left* key; big == press *right* key). Their results suggest that significant task-switching costs remained after controlling the task-switching strategy. Therefore, we can examine whether the modified proactive-interference account would also predict task-switching costs in such a dual-cue paradigm.

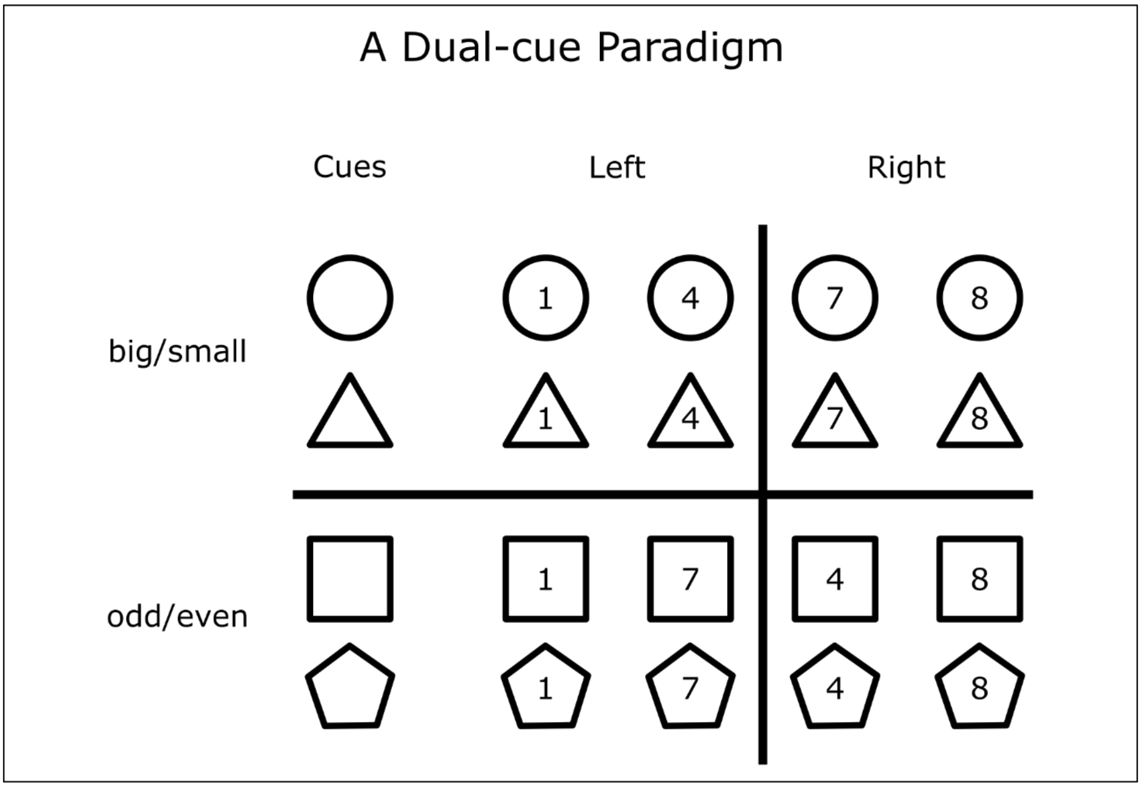

Figure 5.8. Cue-stimulus combination in Experiment 3, Forrest et al. (2014).

Since there were four task cues and four stimuli, there was a total of 16 cue-stimulus combinations. Thus, *trial n - 1* and *trial n* gave 256 different possible permutations between these cue-stimulus combinations. Figure 5.9 lists 16 possible examples. Since the cues have four levels, it is difficult to form binary cue-response mappings. I assume that the interference arose from two semantic binary feature-response mappings: the magnitude-response mapping and the parity-response mapping. According to my calculations, on average, a cue-switch trial had .5 units of interference due to the previous trial. Furthermore, a task-switch trial had an average of 1 unit of interference due to the previous trial (see Appendix A). There was a .5 unit difference between the task-switching trials and the cue-switching trials. Therefore, my modified interference account can explain the remaining task-switching costs in Forrest et al. (2014).

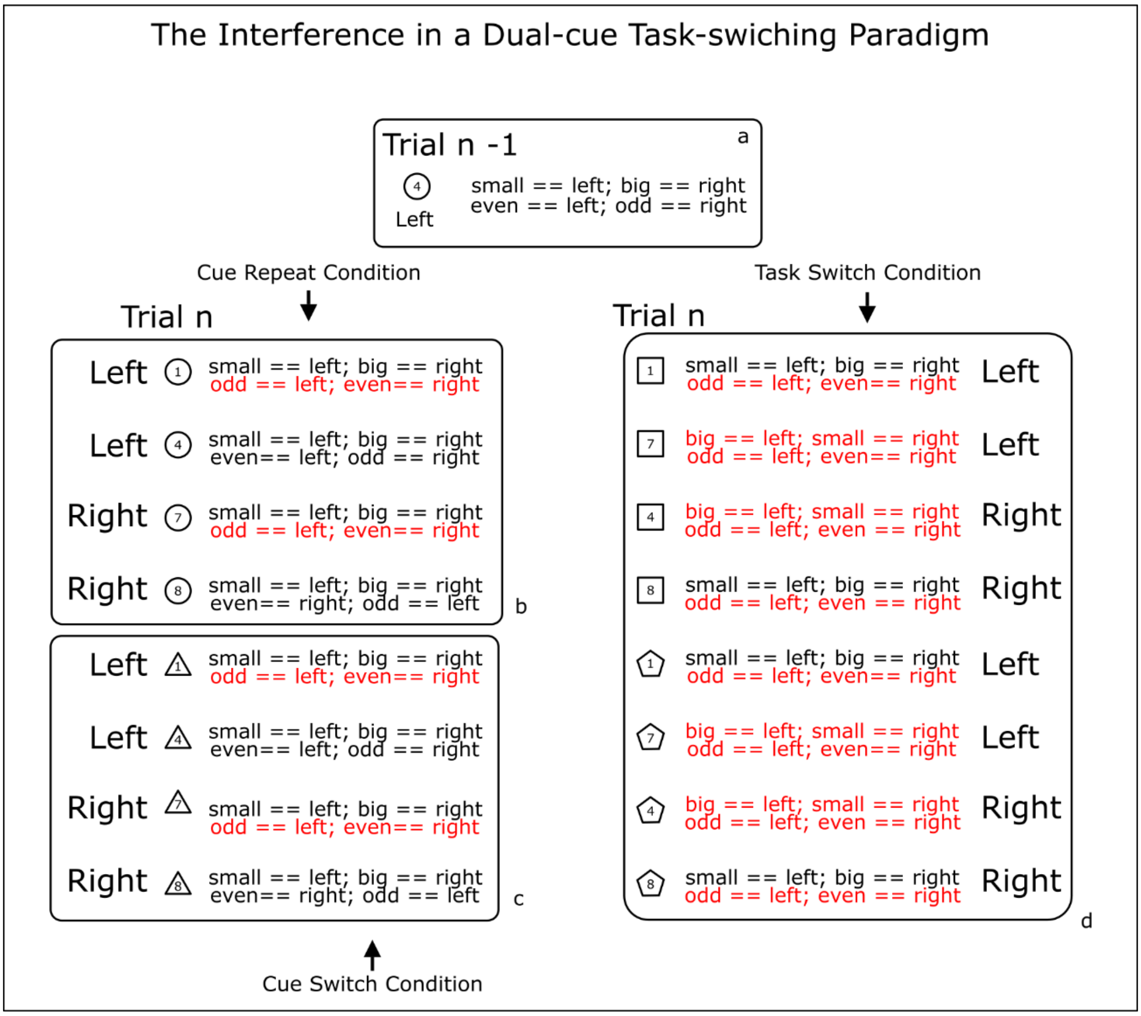

Figure 5.9. Calculate the interference from a typical dual-cue task-switching paradigm.

It is important to point out that, on average, a cue-repeat trial had 1 unit of interference. In other words, my modified interference account predicts, that after controlling the task-switching strategy, interference between different features can only produce task-switching costs but not cue-switching costs. However, Forrest et al. (2014) did not report cue-switching costs. We do not know whether or not they found any cue-switching costs. Although there is no direct evidence to refute the modified proactive interference account any cue-switching costs would disprove this account.

5.3.3 Summary

In summary, applying the same cue-stimulus combinations as in Experiments 2.1 and 2.2 but with different arrangements, I controlled the task-set reconfiguration process but demonstrated switching costs nevertheless. Based on Allport and colleagues' proactive interference account (Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000; Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006), Woodward et al.'s (2003) interpretation of the feature-response mapping, and the studies of feature response integration (Hommel 1998, 2004, 2005) I propose a simple method to approximate the amount of interference in a single trial. I demonstrated that as long as the repeat trials had on average less interference than the switch trials, significant switching costs remained.

In Experiment 5.1, switch trials no longer triggered greater task-set reconfiguration process than the repeat trials. I suggested that the proactive interference alone produced the task-switching costs. Therefore, I have shown that proactive interference can be a source of switching costs independent of the task-set reconfiguration process. This account can explain task-switching costs, even when participants do not apply a task-switching strategy. Other possible explanations will be discussed in the final chapter.

Specifically, I have demonstrated that interference is not always a by-product of a task (or task-set). In line with previous studies (Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000), I suggest that interference is the result of stimulus-response and feature-response mappings in general (Woodward et al., 2003; Hommel, 1998). In particular, I suggest that the cue also serves as a feature among different cue-stimulus combinations. Hence, each cue-stimulus-response mapping has three potential features. When a participant gives a response three binary feature-response mappings are active. Those mappings are either compatible or incompatible with the mappings formed in the previous trial. When the mappings are incompatible (reversed), they create interference. In a typical task-switching experiment with shared stimulus-sets, a switch trial has more incompatibilities, and therefore more interference, than a repeat trial.

5.3.4 Limitations and Suggestions for Future Studies

There is a small problem in the present calculations. In order to make calculations of interference simpler, I assume that each feature incompatibility (i.e., the cue, the shape, and the colour) creates the same amount of interference (1 unit per feature). However, it seems very likely that the colour-response, shape-response, and cue-response mappings produce different amounts of interference. This is not a major problem, because if we only look at the difference between the switch and repeat trials, differences between the three types of feature-response mappings may balance each other out so that the difference between the two conditions remains the same. However, if researchers want to predict interference in single trials, then this would become a problem. For example, if *trial n* and *trial m* both had one unit of interference due to *trial n - 1* and *trial m - 1*, respectively then the current method would assume that *trial n* and *m* receive the same amount of interference. However, if trial n received interference from a colour-response mapping, and trial m received interference from a shape-response mapping,the amounts of interference in t*rial n* and *trial m* might be different. The calculation method is not specific enough yet to reflect this difference. Therefore, I describe an interference task-switching paradigm that should create equal amounts of interference, as a potential future study (see Figure 5.10). The results of this paradigm may further confirm or refute the modified proactive interference account.

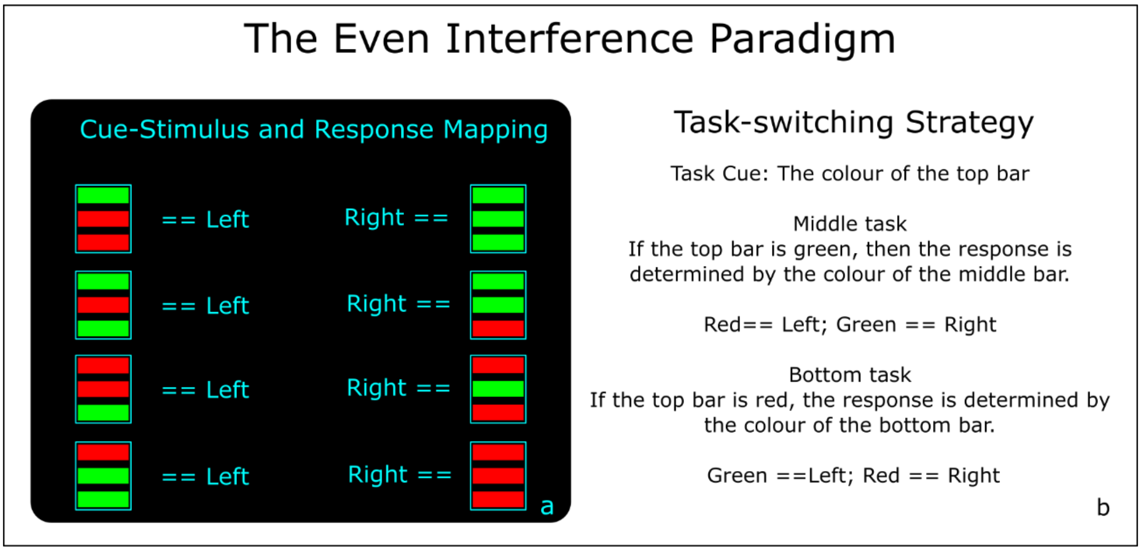

Figure 5.10. In a paradigm with balanced interferences, the three layers of information

(features) are the colour of the top, middle, and bottom bar. Each feature should produce the same amount of interference. (a) The cue-stimulus mappings. (b) The task rules.

There are also two major disadvantages of the calculation method I described above. Firstly, modified interference assumes that, when the task-switching strategy is controlled, only binary feature-response mappings trigger interference. If a feature is not binary (e.g., the feature has four different levels), I assume the feature-response mapping does not trigger interference. This assumption needs further discussion. Nevertheless, such an assumption is not groundless. Task-switching experiments typically apply only one binary response-set: identical motor responses are used in each task (e.g., press the *left* key or the *right* key). This set-up allows participants to form binary feature-response mappings. In contrast, some experiments assign different response keys to each task. For instance, an experiment might require participants to press a *right* or *left* response key in the colour task and press an upper or lower response key in the shape task (e.g., Brass et al., 2003). As the response-set has four levels, participants can never form binary feature-response mappings in such an experiment. Previous studies have suggested that, without binary response-sets, task-switching costs are usually smaller (Brass et al., 2003; Yeung & Monsell, 2003; Meiran, 2005; but also see Mueller, Swainson & Jackson, 2007).

It is reasonable to suggest that at least a certain amount of interference can be attributed to binary feature-response mappings. Future studies can examine this assumption by conducting a shared stimulus-set task-switching experiment using visual stimuli. If the experiment incorporates different response keys for each task, the modified interference account would predict no task-switching costs, once the task-switching strategy has been controlled, because there would be no binary feature-response mappings at all. Conversely, if we found any task-switching costs after controlling the task-switching strategy in such an experiment, my modified interference account would need to be modified further.

Secondly, it can be argued that I did not eliminate the task-set reconfiguration process in a genuine task-switching experiment. By changing the arrangement, I also modified the design of the paradigm altogether. Therefore, Experiment 5.1 would no longer qualify as a task-switching experiment. I assumed implicitly that the interference in Experiment 5.1 and the interference in a typical task-switching experiment would have the same source. I therefore proposed that the same calculations could predict the interference in both Experiment 5.1 and Experiments 2.1 and 2.2. However, since the paradigms differ, providing sufficient proof that both interferences have the same hidden mechanism is logically impossible. The ultimate solution is to develop a task-switching experiment in which some participants can identify the task and others cannot. However, all the participants have to identify the task-relevant features before obtaining the correct response. This is one of the goals of my future studies.

5.3.5 Alternative Methods to Test the Interference Account

Another potential methodology that can further test the interference account is EEG recording and more specifically, the study of event-related potentials (ERP). Previous ERP studies have provided evidence for both the task-set reconfiguration account and the interference account (for a recent review, see Karayanidis & Jamadar, 2014). Firstly, ERP studies consistently report a relative positive shift for switch trials compared with repeat trials (maximal over central and parietal scalp with peaks around 400–600 ms after the task cue appears). It is believed that this positive post-cue shift reflects an advance preparation process in task-switching—a process that the task-set reconfiguration account proposed (e.g., Karayanidis et al., 2003; Lavric, Mizon & Monsell, 2008). ERP studies have also shown the robust effects of task-switching after the stimuli appears. After the target stimuli appears, switch trials tend to show a larger N2 and smaller P3 than repeat trials and therefore show a broad centroparietal maximal negative waveform. These post-stimuli negative waveforms have been associated with interference and carry-over of activities from the previous to the current trial (e.g., Hsieh & Yu, 2003; Hsieh & Chen, 2006; Karayanidis et al., 2003). Further ERP studies may be able to examine the idea that we can eliminate the task-set reconfiguration process while still preserving the interference. For example, if this idea is correct, by inducing participants to perform a CSI task-switching experiment without applying a task-switching strategy, the positive post-cue shift should be reduced or eliminated, whereas the post-stimuli negativity should be unaffected.

Note

This is the last experimental chapter. The link below provides demos of my experiments (including Experiments 2.1, 3.1, 4.2 and 5.1). Although the online demos are slightly different from the real experiments (e.g., fewer experimental trials, different response keys and feedback durations), the key characteristics such as the stimuli and timelines are identical. The online demos will be updated with each version of Psytoolkit: (http://www.psytoolkit.org/#_web_based_login).

Please follow this link to visit the demo web page:

http://www.psytoolkit.org/cgi-bin/psy2.3.5/survey?s=CvN9L

Chapter 6: Can the LUT Approach Explain Animal Task-switching Behaviours?

To understand why monkeys and pigeons can perform task-switching experiments without any task-switching costs (Stoet & Snyder, 2003; Avdagic, et al., 2013; Castro & Wasserman, 2016; Meier et al., 2016; but see Caselli & Chelazzi, 2011), this thesis investigated under what conditions human participants mirror pigeons' and monkeys' task-switching behaviour——performing task-switching experiments without showing any task-switching costs. In particular, I focused on how human participants can complete a task-switching experiment without switching between two tasks. This is because many previous studies have reasoned that animals do not switch between tasks in the first place (Dreisbach et al., 2006, 2007; Meier et al., 2016; Forrest et al., 2014). Instead, it was suggested that animals memorised all stimulus-response mappings, forming a LUT. Since the LUT does not require task-set reconfiguration processes that are required by the task-switching strategy, task-switching costs can be eliminated.

However, for human participants, the LUT approach only works under certain conditions. Experiments 3.1 and 3.2 replicated the results of Dreisbach et al. (2006, 2007) and demonstrated that, as long as two separate stimulus-sets are used in a task-switching experiment, the LUT approach can always eliminate the task-switching costs. Alternatively, if the task-switching paradigm applies a shared stimulus-set, human participants cannot apply the LUT. Instead, participants seem to create novel strategies. For example, participants created the “black and white” strategy in Experiments 2.1 and 2.2 and the BF strategy in Experiments 4.1 and 4.2. Moreover, task-switching costs remained significant (e.g., Chapter 2; Forrest et al., 2014) unless semantic tasks were used as in Experiments 4.1 and 4.2.

Previous animal studies, on the other hand, usually applied task-switching experiments with visual tasks and shared stimulus-sets (e.g., Stoet & Snyder, 2003; Caselli

& Chelazzi, 2011; Avdagic, et al., 2013; Meier et al., 2016; Castro & Wasserman, 2016). I conclude that the reason animals can remove task-switching costs is not due to the elimination of the task-set reconfiguration process or to the application of an LUT approach. At least, the LUT approach does not seem to be the full story. There may be additional factors that can be attributed to differences in cognition between humans and animals.

One critical factor may be proactive interference. As suggested in Chapters 3 and 4, once the interference from the previous trial was removed and the task-set reconfiguration process was controlled, task-switching costs could be eliminated in humans. In the following sections, I will attempt to explain the behavioural differences between humans, pigeons and monkeys in task-switching experiments between humans based on the interference account. I will discuss pigeons and monkeys separately (Li, Li, Lages & Stoet, 2017).

6.1 Task-Switching in Humans and Pigeons

In this section, I will discuss two possible explanations for the difference in task-switching behaviour between humans and pigeons. The first explanation is developed from Meier et al. (2016). The second explanation is developed from Castro and Wasserman (2016). Both explanations focus on the absence of interference.

6.1.1 Missing Interference

Meier et al. (2016) suggest that, unlike human participants, who can always identify the cue and the stimulus separately, pigeons may always encode the task cue, the stimulus, and even the location of the response key as one compound (i.e., a cue + stimulus + response). Therefore, they suggest that, based on Pearce's generalisation rule (1987), humans would generate more than twice as much task-switching cost as pigeons, so that pigeons' small task-switching costs become undetectable. I speculate another possibility is that, because pigeons encode all elements together, they do not identify the task-relevant features separately, as in Meier et al.'s (2016) experiment. In other words, just as the non-

Chinese participants in Experiments 4.1 and 4.2 could not identify the semantic task features in a Chinese number, pigeons did not identify the visual task feature in a visual stimulus. Thus, I propose that the pigeons in Meier et al. (2016) did not experience enough interference from the previous trial to produce task-switching costs. Furthermore, many studies have suggested that pigeons do not have a sophisticated executive control process (e.g., Lea & Wills, 2008; Lea et al., 2009; Maes et al., 2015; Smith et al., 2011; Smith et al., 2012; Wills et al., 2009). Thus, the pigeons cannot have any task-set reconfiguration process either. In summary, since pigeons have neither proactive interference nor task-set reconfiguration processes, they do not generate task-switching costs.

One disadvantage of this explanation is that I can only assume that pigeons did not identify the task-relevant features in the task-switching experiments. However, it is difficult to test this assumption. In addition, it was pointed out in a number of studies that birds have selective attention and the ability to categorise abstract information (cf. Soto and Wasserman, 2010; Soto & Wasserman, 2014; Castro and Wasserman, 2016). It is therefore possible that pigeons can actually identify the task-relevant features and experience interference from the previous trial in a similar way to humans. Nevertheless, this is a question that is beyond the scope of this thesis.

6.1.2 Long ITIs

Castro and Wasserman (2016) argued that pigeons did not perform in their experiment without interruption. This is because, after obtaining a correct response, their pigeons had to turn around and peck the rewards. Monkeys did not have the same problem in related experiments. For example, Stoet and Snyder (2003) inserted a water tube inside the monkeys' mouths, and the reward (water) was provided to them automatically. Avdagic et al. (2013) allowed their monkeys to pick up a piece of banana by hand once they successfully finished a trial. The reward processes were relatively short: less than four seconds.

However, pigeons' pecking behaviour made the inter-trial interval (ITI) far longer than in the monkey and the human experiments. In Castro and Wasserman's study (2016), the ITI was between eight and 12 seconds. In Meier et al. (2016), the ITI was between 15 and 30 seconds. In comparison, human task-switching experiments always progress very rapidly. The usual ITI is often less than 1000 ms and studies with an ITI longer than ten seconds are very scarce. For human participants, only a small number of functional magnetic resonance imaging (fMRI) task-switching studies had such a long ITI (e.g., Barder & Carter, 2005; Ravizza & Carter, 2008). Thus, Castro and Wasserman (2016) pointed out that settings and paradigms in pigeon studies may have been less conducive to producing task-switching costs. Pigeons may exhibit task-switching costs for shorter ITIs——an issue that needs to be tested in future experiments. I therefore propose that, even if pigeons experience interference from previous trials, this effect may have faded after a long ITI.

However, in fMRI studies with long ITIs, human participants still showed significant task-switching costs (Barder & Carter, 2005; Ravizza & Carter, 2008). Since all participants received explicit task-switching instructions, the task-set reconfiguration process can explain these task-switching costs. If the ITI is more than ten seconds, and if we can rule out task-switching strategy then even human participants may show no task-switching costs.

6.1.3 Summary

I have proposed two potential explanations for the absence of task-switching costs in pigeons. The first explanation requires the additional assumption that pigeons do not process the task-relevant features in task-switching. The second explanation is relatively straightforward. I suggest that long ITIs in pigeon task-switching experiments may reduce, or even counteract, interference from the previous trial.

Nevertheless, both explanations suggest that, unlike human participants, pigeons do not experience interference from preceding trials, nor can they apply a task-set reconfiguration process. As a result, pigeons do not show any task-switching costs. So humans may mirror pigeons' behavioural characteristics in task-switching experiments if the task-relevant features (the source of interference) in the stimuli are obscured (e.g., Experiments 4.1 and 4.2).

6.2 Task-switching in Humans and Monkeys

In many studies, it was assumed that neither monkeys nor pigeons applied a human task-switching strategy in task-switching experiments (e.g., Dreisbach et al., 2006, 2007; Meier et al., 2016; Forrest et al., 2014). Instead, they assumed that animals applied the LUT approach. In fact, this is the assumption I put forward in Chapter 2. However, after carefully reviewing the literature, I think there is sufficient evidence that monkeys switch between tasks similarly to humans. For example, Stoet and Snyder (2003) introduced eleven novel stimuli, interspersed with the practiced stimuli, to monkey M2. M2 performed significantly better than the chance level (M2 was correct in 10 out of 11 novel trials), which indicates that this monkey had learned to categorise different task features from the stimuli. If this monkey had only relied on the LUT approach, then its performance should have been at chance level for the novel stimuli. Moreover, further studies have suggested that monkeys' neurons in the posterior parietal cortex encode task-set information independently of stimulus features (Stoet & Snyder, 2004).

Further evidence was provided by Avdagic et al. (2013). Since the researchers applied a simultaneous chain (SimChain) paradigm, a pure LUT account cannot fully explain the monkeys' behaviour in their study, for two reasons. Firstly, in this paradigm, stimuli and responses vary considerably across trials, without repetition. However, the LUT approach only works when the experiment repeats a minimal amount of stimulus-response mappings. Secondly, as the SimChain paradigm makes a set of items appear simultaneously in each trial, the monkeys have to make a set of responses in a particular order according to the task rule. The feedback (correct or mistake) would not be given until the last item had been responded to. Therefore, if the monkeys applied an associative learning process or the LUT approach, it would be difficult for them to realise which step went wrong, so they could not receive any meaningful reinforcement (Jensen et al. 2013). It is hard to perform a SimChain paradigm without applying task rules.

The monkeys' zero task-switching costs performances indeed raised a problem. They obviously applied the task-switching strategy. The task-switching strategy requires participants to react to the task-relevant feature, so monkeys almost certainly identified task-relevant features, and they could also have received interference from the previous trial. The proactive interference accounts would also predict that the monkeys would have task-switching costs. Nevertheless, despite the fact that they had to deal with both the task-set reconfiguration process and the interference from the previous trial, they had no task-switching costs.

I can provide some hypotheses based on the following two pieces of evidence. Firstly, Stoet and Snyder (2003) included a 650 ms CSI. Therefore, their monkeys could only eliminate residual task-switching costs. In addition to this, we know that the monkeys from Stoet and Snyder (2003) demonstrated slight, but significant, task-switching costs when the ITI was short (170 ms). However, when the ITI was long (345 ms), they had no task-switching costs. In contrast, Caselli and Chelazzi (2011) applied a 700-ms ITI and Avdagic et al. (2013) applied an ITI of four seconds. The experiments in this thesis always had an ITI of 300 ms.

Secondly, according to proactive interference account, residual task-switching costs are the result of interference triggered by previous trials. Interference decays only slowly over time (e.g., Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000). As a consequence, even if the task-set reconfiguration process has been completed, interference between trials can still produce a task-switching cost.

6.2.1 Weak Interference

I will now outline the hypotheses for weak interference. I hypotheses that unlike in humans, the interference monkeys receive from the previous trial decays very quickly. This explains why monkeys showed task-switching costs even when the ITI was only 170 ms. Although interference decays quickly, an ITI of 170 ms is too short for the interference to fade away entirely. If the ITI is longer, interference for monkeys may fade away completely so that the task-set reconfiguration process alone produces the task-switching costs. Thus, given enough preparation, task-switching costs can be eliminated. The following sections will discuss whether or not this lack of proactive interference can explain the results of another two monkey studies.

The SimChain Paradigm and Interference

Unlike Stoet and Snyder (2003), who applied a conventional task-cueing paradigm, Avdagic et al. (2013) used a SimChain paradigm to investigate monkeys' task-switching behaviour. In every trial, several circles were displayed on a touch screen simultaneously. Each circle had two dimensions: brightness, ranging from black (RGB 0, 0, 0) to light grey (RGB 220, 220, 220); and radius or size, ranging from 10 pixels (0.15 cm) to 70 pixels (1.05 cm). Three monkeys were required to touch the circles in a sequence that was determined by the task rules. For example, in the brightness task, the monkeys had to touch the circles in the order of their luminosity, i.e., the lightest first, followed by a slightly darker, until, finally: the darkest item. Similarly, in the radius task, monkeys had to press the circles in the order of their radius or size, from the smallest to the largest circle. The cue was the background colour (blue == brightness; red == radius). In the final experimental section, the monkeys in Avdagic and colleagues' study had to touch six different circles in the correct order according to the task rules (see Figure 6.1).

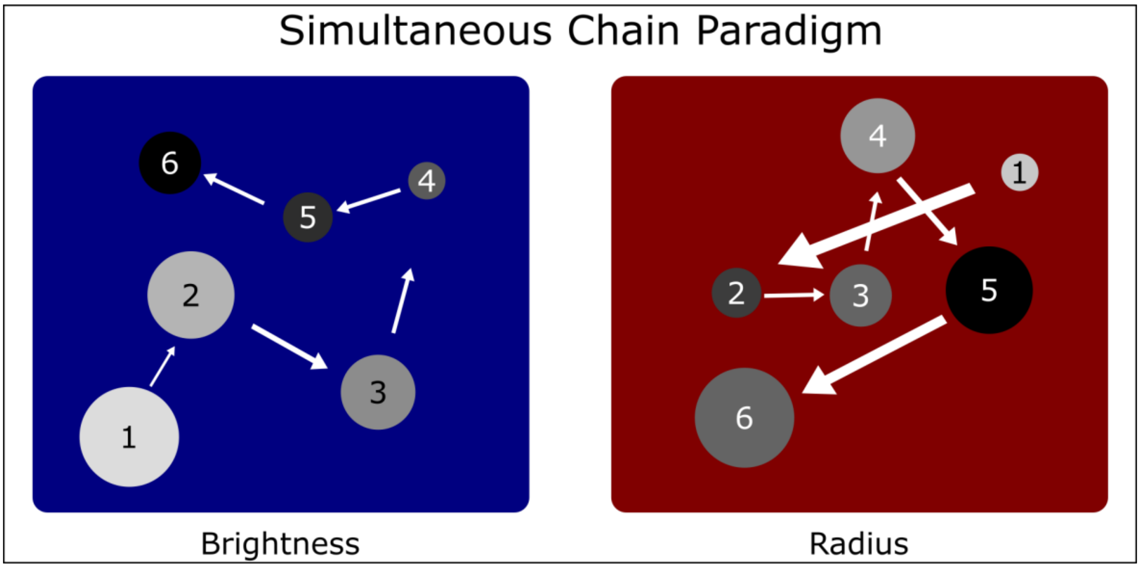

Figure 6.1. Illustration of the SimChain paradigm from Avdagic et al., 2013. In every trial, the monkeys had to touch each item (circle) in the correct order (from 1 to 6) based on the task rule. Brightness task: the lightest to the darkest circle. Radius task: the smallest to the largest circle.

The position, size, and brightness of each item was randomised in every trial, so that the same stimulus and response would not be repeated. As a consequence, applying the LUT approach was not an efficient approach. A certain degree of cognition and generalisation was required. Moreover, based on the modified interference account that I proposed in Chapter 5, SimChain paradigms do not produce the same interference as task-cueing paradigms. The reason for this is that, under a typical task-cueing paradigm, each feature from the cue-stimulus combination can only link with two possible responses, so that a binary feature-response mapping is formed (e.g., black==*left*; white==*right*). In contrast, in a SimChain paradigm, each trial has a unique response pattern so that binary feature-response mappings cannot be formed.

Some studies have considered that interference originates from stimulus-task-set associations rather than the feature (or stimulus)-response mappings (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). In these studies, both tasks may share the same stimuli. Therefore, in switch trials when the previous stimulus is repeated and when the task-set switched, the previous stimulus-task-set association can interfere with the current stimulus-task-set association, producing task-switching costs. In a SimChain experiment as suggested by Avdagic et al. (2013), however, neither response nor stimulus were repeated during the experiment, so that stimulus-task-set associations could not interfere with each other. Thus, there should have been no interference between task-sets.

Here, I suggest that SimChain paradigms do not produce the interference produced in conventional task-cueing paradigms. For a participant, whether human or monkey, task-switching costs can be eliminated if they can complete the task-set reconfiguration process quickly enough. The SimChian paradigm was originally developed to study animal learning processes (Terrace, 1984, 2005). These studies focussed on how animals can make a sequence of responses based on their understanding of abstract rules or concepts, rather than through a pure associative learning process (for a review, see Terrace, 2005). Compared with the task-cueing paradigm, the SimChain paradigm is rarely used in task-switching studies. In addition, Avdagic and colleagues (2013) did not use human participants as a control group; they only included three monkeys in their study. Therefore, we do not know whether or not human participants would demonstrate task-switching costs under this particular SimChain paradigm. It is possible that human participants might also have shown no task switching costs.

In short, by applying the SimChain paradigm, the results by Avdagic et al. (2013) may not support the idea that monkeys can switch between two tasks better than humans for two reasons. Firstly, even if monkeys experience interference similarly to humans, the SimChain paradigm does not create interference as a typical task-cueing paradigm does. Therefore, performances under the SimChain paradigm and performances under task-cueing paradigms are difficult to compare. Secondly, no evidence was provided that humans would demonstrate task-switching costs if they performed under the same paradigm as the monkeys.

Poor Performance and Task-switching Costs

In contrast to Stoet and Snyder (2003) and Avdagic et al. (2013), Caselli and Chelazzi (2011) found significant task-switching costs in monkeys. Unsurprisingly, researchers who favour the associative learning account often ignore this study. Caselli and Chelazzi (2011) applied a typical task-cueing paradigm which was very similar to that used by Stoet and Snyder (2003). Two monkeys (M1 and M2) and eight humans performed a colour task and an orientation task and switched between tasks in some trials. However, each task had four levels. For the colour task, green and yellow == *left*; red and blue == *right*. For the orientation tasks, the four levels were vertical, horizontal, and two orthogonal oblique orientations. The clockwise oblique and horizontal stimuli were linked to the *left* key; the counter-clockwise oblique and vertical stimuli were linked to the *right* key (see Figure 6.2).

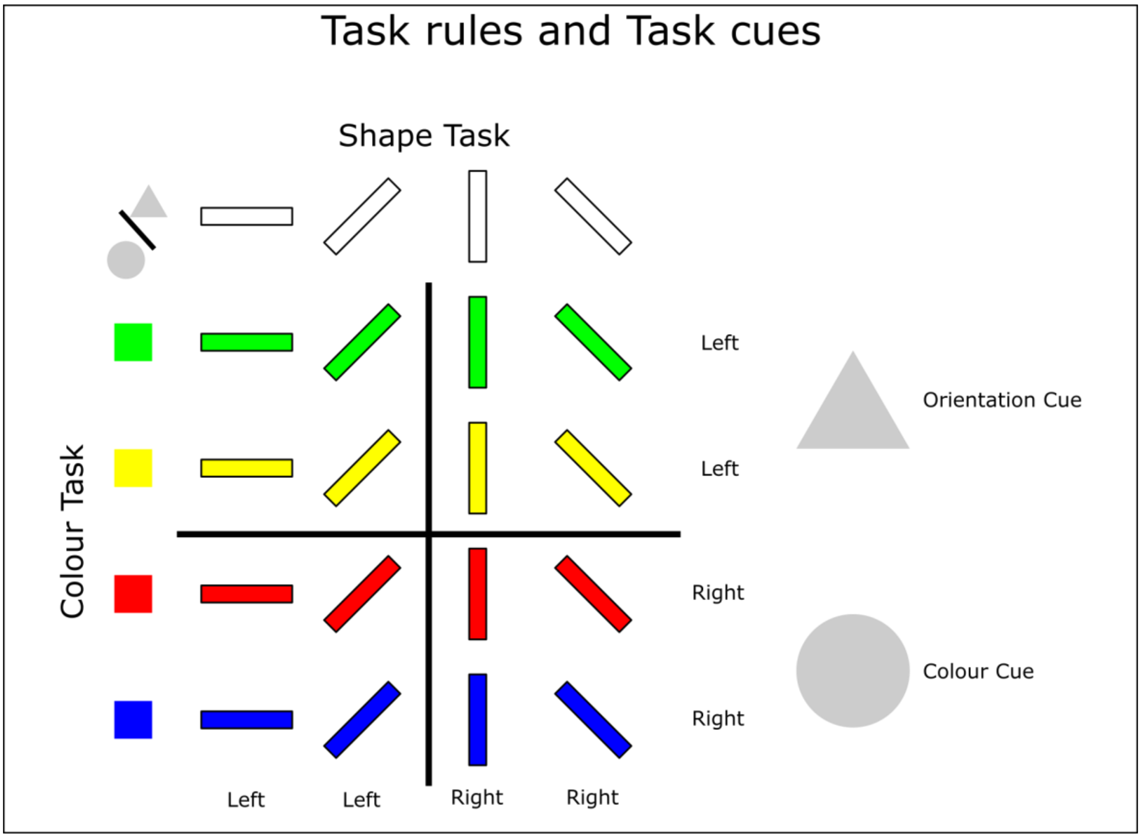

Figure 6.2. Illustration of task rules and task cues in the experiment by Caselli and Chelazzi (2011). Each task had four different levels.

In order to explain the disagreement between Stoet and Snyder (2003) and Caselli &

Chelazzi (2011), Avdagic et al. (2013) suggested that the monkeys in Caselli and Chelazzi's study were simply not well-trained enough. Stoet and Snyder's (2003) monkeys received approximately 100,000 practice trials before the final experimental section with more than 1,000 trials. In contrast, Caselli and Chelazzi (2011) did not report the number of training trials and only mentioned that subjects received "several training sessions". Moreover, Caselli and Chelazzi (2011) had considerably higher error rates (M1 = 11.1%, M2 = 18.2%) than the monkeys in Stoet and Snyder (2003) (M1 = 4.7%, M2 = 6.9%). Indeed, it would seem that the monkeys in Caselli and Chelazzi (2011) were not as well-trained as the monkeys in Stoet and Snyder (2003).

Nevertheless, Avdagic et al. (2013) did not explain why poor performance or less training would produce significantly more task-switching costs. It may be argued that in Experiments 4.1 and 4.2, non-Chinese participants performed worse than Chinese participants because they were not familiar with the Chinese numbers. Therefore, their error rates were higher than those of the Chinese participants. However, the non-Chinese participants showed no task-switching costs in Experiments 4.1 and 4.2. Poor performance does not necessarily imply increased task-switching costs.

Since both studies on monkeys included a preparation interval or CSI, I hypothesise that the task-switching costs might reflect that Caselli and Chelazzi's (2011) monkeys were not able to fully reconfigure the task-set in advance during a CSI of 700 ms. There are two factors that might have caused incomplete preparations. Firstly, with the introduction of four levels in each task, the task-sets were more complex than those of Stoet and Snyder (2003). Secondly, as explained above, the monkeys in Caselli and Chelazzi (2011) might have received less training than those of Stoet and Snyder (2003). Therefore, since the task-set reconfiguration process may not have been completed during the CSI, monkeys' task-switching costs were larger in the study by Caselli and Chelazzi (2011).

6.2.2 Suggestions for Future Research

I suggest that the monkeys' outstanding task-switching behaviour was the result of their task-set reconfiguration process. Monkeys may have experienced only very small interference between trials. Hence, once the task-set reconfiguration was completed, task-switching costs vanished. In the following section I discuss interference in general terms. It is possible that interference arose from stimuli-response mapping (e.g., Allport et al., 1994; Allport & Wylie 1999; Wylie & Allport, 2000), or from binary feature-response mapping, according to my modified interference account in Chapter 5, or that interference was created by stimulus-task-set associations (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). The main point is that these monkeys only received very limited interference from previous trials.

In order to examine this hypothesis, it would be interesting to manipulate the CSI and ITI in a monkey task-switching experiment. Firstly, we could compare task-switching behaviour in monkeys between composite conditions (CSI = 0 ms) and CSI conditions (e.g., CSI = 650 ms) with fixed ITI (e.g., 350 ms). If my hypothesis holds true, monkeys could eliminate task-switching costs in some CSI conditions but not in composite conditions. In composite conditions, it is impossible for the monkeys to reconfigure the task-sets in advance. Furthermore, task-switching costs in monkeys may be significant as soon as we reduce the ITI from 350 ms to 100 ms. Although interference may decay quickly in monkeys, according to the results of Stoet and Snyder (2003), I suggest that a 100-ms ITI is too short for interference to fade away completely.

In short, I suggest that monkeys can only eliminate task-switching costs in the 650-CSI-350-ITI condition, but not in the 0-CSI-350-ITI and 650-CSI-100-ITI conditions. In a 0-CSI-350-ITI condition, the task-set reconfiguration process is likely to produce task-switching costs. In a 650-CSI-100-ITI condition, it may be proactive interference.

6.3 Humans, Pigeons, and Monkeys

In summary, I propose that pigeons and monkeys eliminate task-switching costs in different ways (see also Figure 6.3). I propose that pigeons do not experience interference from the previous trial and that they cannot apply a rule-based strategy requiring executive control. As a result, they do not show any task-switching costs. Alternatively, I propose that task-switching costs in monkeys are primarily caused by task-set reconfiguration. Compared to proactive interference in humans, proactive interference in monkeys may decay more quickly, so that this has only a very limited impact. This difference in decay may reflect differences between species. As a consequence, if monkeys are well-trained and the preparation time is long enough, then they can complete the task-set reconfiguration process and residual task-switching costs will be eliminated. Task-switching costs in humans originate from at least two independent sources: the task-set reconfiguration process and the interference from the previous trial. Therefore, we can only eliminate human task-switching costs when both sources are controlled.

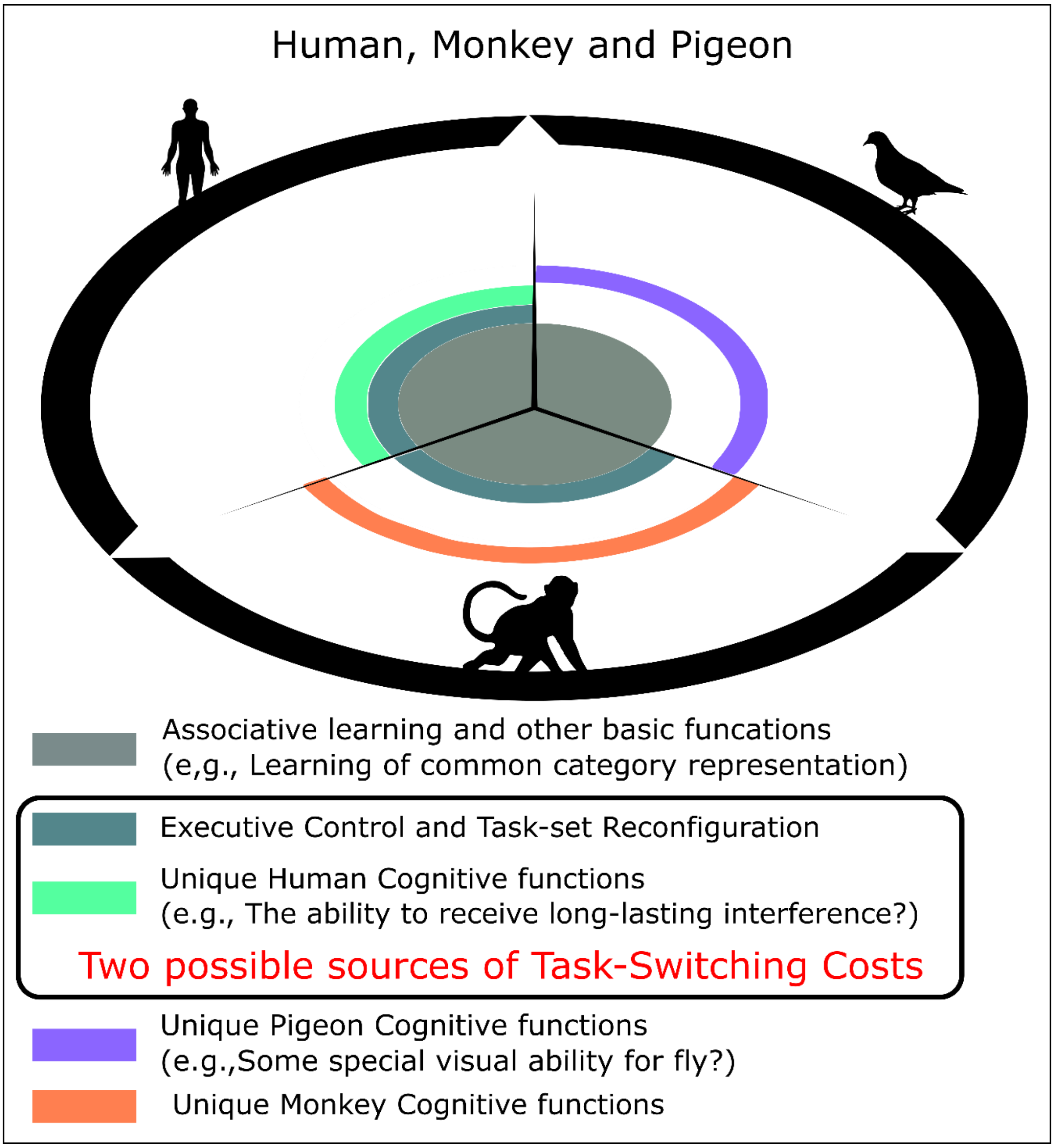

Figure 6.3. Diagram illustrating overlap of cognitive functions between humans, monkeys and pigeons. The black outer circle separates the three species. The concentric circle and coloured arches at the centre represent different cognitive functions in each species. Humans have two cognitive functions that can cause task-switching costs. Monkeys have one (i.e., the task-reconfiguration process), whereas pigeons have none.

Chapter 7: Discussion and Conclusion

This final chapter focuses on two topics. Firstly, from Section 7.1 to section 7.5**,** I will discuss alternative accounts that can explain task-switching costs in the absence of the task-switching strategy. I will compare these with the modified interference account from Chapter 5. Secondly, in section 7.6, I will summarise the current understanding of task-switching costs and outline some implications based on the results of the experiments in the present thesis.

7.1 Interference and Task-switching Costs without Task-switching

One important aim of this thesis is to explain the surprising observation of task-switching costs when participants do not switch between two tasks. In this thesis, after I controlled the task-switching strategy and the task-set reconfiguration process, task-switching costs sometimes disappeared (e.g., Experiments 4.1 and 4.2; Experiments 3.1 and 3.2), but at other times, task-switching costs remained statistically significant (e.g., Experiments. 2.1 and 2.2). In Chapter 5, I proposed a modified interference account. This account suggested that the interference from the previous trial could be triggered by binary feature-response mappings and might create task-switching costs even without participants switching between tasks. Hence, I proposed that participants could not eliminate task-switching costs unless they could eliminate both the task-set reconfiguration process and the interference from binary feature-response mappings.

For example, in Experiments 4.1 and 4.2, semantic interference was prevented by the use of Chinese numbers, especially for the non-Chinese participants, who could only perceive these as meaningless symbols. As a consequence, there were not enough binary feature-response mappings to produce interference. Hence, in Experiments 4.1 and 4.2, without participants having an explicit understanding of the task-switching strategy, both the task-set reconfiguration and the semantic interference disappeared, and task-switching costs eventually vanished, too. In contrast, Experiments 2.1 and 2.2 applied visual tasks and visual interference was unavoidable, so task-switching costs remained significant even when participants did not switch between tasks.

As demonstrated in Chapter 5, the modified interference account can explain the results of Experiments 2.1 to 5.1 and the results of Forrest et al. (2014). However, previous studies have provided several different accounts. In the following section, I will discuss whether these accounts provide alternative explanations to the proactive interference account. I will discuss the Compound Retrieval account first.

7.2. Compound Retrieval Account and Task-switching Costs

The idea that human participants can complete a task-switching experiment without applying the task-switching strategy was first proposed by Logan and colleagues (Logan & Bundesen, 2003; Logan et al., 2007; Arrington & Logan, 2004; Schneider & Logan, 2005, 2007; Logan & Schneider, 2006a, b; Logan & Schneider, 2010). In this series of studies, they proposed the compound retrieval account. This account suggests that participants might not apply the task-switching strategy. Instead, participants may apply a compound retrieval strategy: forming cue-stimulus compounds and retrieving the corresponding response for each compound from memory directly (episodically or semantically). In this case, task-switching costs in fact reveal an extra cue-encoding process. In Chapter 1, I have discussed the compound retrieval account.

Each of the experiments in the present thesis had a fairly small stimulus-set. Therefore, according to Arrington and Logan (2004), participants would have applied the compound retrieval strategy at the episodic level. More importantly, these experiments had only one cue per task, so that cue-switching costs could not be separated from the task-switching costs. The compound retrieval account predicts that, in Experiments 2.1 to 5.1, the task-switching costs will remain significant even when participants do not apply the task-switching strategy. The task-switching costs are caused by the cue-encoding disadvantage in switch trials. In other words, the tasl-switching costs are actually cue-switching costs.

Logan and colleagues' compound retrieval account can explain the results of Experiments 2.1 and 2.2: without participants having any understanding of the task rules, task-switching costs were still significant and reflected the cue-encoding process. However, in Experiments 3.1 and 3.2, the task-switching costs completely disappeared when the task-switching strategy was controlled. Based on the compound retrieval account, one may argue that in Experiments 3.1 and 3.2 a separate stimulus-sets design was applied. Therefore, when participants applied the "Look-up table" (LUT) approach, the task cue could be ignored. In contrast, many dual-cue task-switching experiments applied shared stimulus-sets (e.g., Logan, & Bundesen, 2003; Mayr & Kliegl, 2003; Schmitz & Voss, 2014).

A limitation of compound retrieval account is revealed by the Chinese number experiments in Chapter 4. Experiments 4.1 and 4.2 applied a shared stimulus-set design, just as in Experiments 2.1 and 2.2. Based on the compound retrieval account, participants would have had to identify the cue before they could obtain the correct response. Hence, participants should have had task-switching costs that reflected the cue-encoding process. However, no such task-switching costs were observed in Experiments 4.1 and 4.2. Therefore, the compound retrieval account is not compatible with the results of Experiments 4.1 and 4.2.

In typical cue-switching studies, participants were explicitly required to switch between tasks, and all participants seemed to apply the task-switching strategy. As was demonstrated in Experiments 4.1 and 4.2, there is no evidence of any cue-encoding process when participants do not apply the task-switching strategy. Therefore, in contrast to Logan et al.'s (2003) original assumption, which suggested that task-switching costs only reflect cue-switching costs caused by the compound retrieval strategy, I propose that, if cue-switching costs exist, they might be a by-product of the task-switching strategy.

Later studies have proven, with both behavioural data and with brain imaging that the cue-switching process exists independently of the task-switching process(for a review see Jost et al., 2013). In these studies, it was suggested that cue-switching costs originated from an active control process, rather than from the perceptual priming of the cue itself as originally proposed by Logan and colleagues (cf., Logan & Bundesen, 2003; Schneider & Logan, 2005). For example, it was suggested in some studies that cue-switching costs reflect an activation of the task-set representations in working memory (Mayr & Kliegl, 2003; Grange & Houghton, 2009).

I propose that there is a potential interplay between participants' strategies and the cue-encoding process. Perhaps, when participants are applying the task-switching strategy, a task cue also gives them a certain amount of task-relevant information, as previous studies have suggested (e.g., Mayr & Kliegl, 2003; Grange & Houghton, 2009). As a consequence, this cue-encoding process would require more cognitive effort, triggering cue-switching costs. Alternatively, when participants are applying novel strategies like the "bottom first" (BF) strategy, the task cue no longer represents a conventional task. As a result, the cue-encoding process would be relatively simple causing no detectable cue-switching costs.

7.3 Associative Learning and Task-switching

Some studies (e.g., Forrest et al., 2014; Dreisbach et al., 2006, 2007), especially the animal task-switching studies (e.g., Meier et al., 2016; Castro & Wasserman, 2016), have focussed on two different learning approaches. The first is an associative learning process: learning according to the outcome of each response, which allows subjects (humans or animals) to remember the links between stimuli and responses. The second is a cognitive learning process, governed by the executive control system. Using the latter learning process, subjects obtain the correct response by applying the task-switching strategy. They deduce correct the responses by applying task rules. Forrest et al. (2014) proposed that, in a conventional task-switching experiment, human subjects may apply both of these learning approaches. However, after removing the task rule-based instructions, Forrest et al. (2014) suggested that humans only apply the associative learning process. They concluded that the remaining task-switching costs must be the result of the associative learning process.

Before embarking on further discussion, one important difference between Forrest et al. (2014) and Experiments 4.1 and 4.2 needs to be addressed. It remains unknown whether or not the participants in Forrest et al. (2014) really performed the experiment in a purely associative manner. Nevertheless, based on the results of Experiments 4.1 and 4.2, I propose that the novel BF strategy is not just a post-hoc oral report, but describes a genuine rule-based strategy. Similarly to the task-switching strategy the BF strategy also requires a cognitive learning process. It seems unreasonable to assume that participants can only learn correct responses via associative learning, and to completely rule out the possibility of executive control.

In order to compare the results of Experiments 4.1 and 4.2 with previous studies (e.g., Forrest et al., 2014; Meier et al., 2016), we may assume that after controlling for task-switching as a strategy, any remaining cognitive learning process produces no task-switching costs. Moreover, if there are any task-switching costs, we would assign them to associative learning. In the following, I will discuss two different associative learning accounts and how they can explain the existence and absence of task-switching costs after controlling for task-switching.

7.3.1 The Adaptively Parametrised Error Correcting System (APECS) Model

Forrest et al. (2014) analysed their data using the APECS model. APECS is a three-layer backpropagation localist connectionist network (McLaren, Forrest & McLaren, 2012; Maclaren, 1993). Originally, the APECS model was developed to explain several associative learning effects including perceptual learning, latent inhibition, the Espinet Effect, and

Sequential Learning Problems (see McLaren, Forrest & McLaren, 2012). In the next section, I will introduce a classic example of the sequential learning effect (Barnes & Underwood, 1959; McCloskey & Cohen, 1989; Ratcliff, 1990) in order to demonstrate the link between these learning effects and task-switching.

Sequential Learning and Task-switching

Barnes and Underwood (1959) presented their participants with a list of meaningless "words" (e.g, "dax" and "teg"). Each meaningless "word" was then paired with a meaningful word: "regal", "sleek", etc. They called this list the "Regal List". During the first experimental block, participants were asked to remember the one-to-one associations between the Nonsense List and the Regal List. The idea was that, if the participants saw "dax", they needed to respond "regal", and if they saw "teg", they were asked to respond "sleek", and so on. After training, the participants were able to successfully remember all the pairs (with 100% accuracy). In the second experimental block, the participants had to remember a further list of meaningful words: "keen", "swift", etc. They called this list the "Keen List". Similarly, participants were asked to remember the associations between the Nonsense List and the Keen List (e.g., "dax" == "keen"; "teg" == "swift"). After training, the participants reached 90% accuracy.

In the final experimental block, when the participants were asked to recall the associations (or pairs) from Block 1 again, they were only able to recall 50% of the associations. This 50% decline in accuracy, together with the results of a control group suggested that it was not simply the passage of time that was responsible. Instead, if two different types of associations (i.e., the Nonsense List-Regal Lists link and the Nonsense List-Keen Lists link) are presented in alternating trials, the participants could quickly remember almost all the associations from both the Regal List and the Keen List.

The sequential learning problem shares some characteristics with task-switching. For example, consider the two lists as two different "tasks". In the alternating trial condition, the participant has to "switch" between the "Nonsense-Regal" task and the "Nonsense-Keen" task. Moreover, just as in the incongruent condition, the same stimuli were linked with different responses depending on the "task rules". APECS can cope with the sequential learning problem (MacLaren 1993; McLaren, et al., 2012).

Forrest et al. (2014) applied the same APECS method to model the results of a task-switching paradigm. They found that the APECS associative learning network would perform worse in switch trials than in repeat trials. They applied dual-cue paradigms, and it was suggested that the task-switching costs might reflect a closer associative connection between cues that indicate the same task. If the same stimulus-response links in the artificial neuron network are repeatedly activated in the presence of certain task cues, this activation can strengthen the link between the cues themselves, resulting in an associative cue equivalence. This equivalence then selectively facilitates the retrieval of a stimulus-response link in trials with equivalent cues, i.e., task-repeat trials. Therefore, when participants do not switch between two tasks, the APECS associative learning account still predicts task-switching costs.

Disadvantages of APECS

APECS is not, however, compatible with the results in Experiments 4.1 and 4.2. In these experiments, there was no evidence of any task-switching costs after controlling the task-switching strategy. One possible explanation is that the APECS model is only compatible with the dual-cue task-switching paradigm. In this paradigm, each task had two cues, and there was a total of four task cues. In contrast, Experiments 4.1 and 4.2 applied a single explicit task cue. The APECS model relies on the associations between cues representing the same task. If there is only one cue per task, the results might be different, because there would be no associations between two task cues that present the same task. For the APECS model, the problem is the pigeon studies (Castro & Wasserman 2016; Meier et al., 2016). Specifically, Meier and colleagues (2016) used a very similar experimental setup to Forrest et al. (2014). Both experiments had four cues (two for each task), four target stimuli, and two response keys.

It has been suggested that pigeons do not have executive control (e.g., Lea & Wills, 2008; Lea et al., 2009; Maes et al., 2015; Smith et al., 2011; Smith et al., 2012; Wills et al., 2009).Therefore, pigeons can only use associative learning to perform in a task-switching paradigm. If the APECS model is correct, the pigeons, like the human participants in Forrest et al.'s (2014) experiments, should demonstrate significant task-switching costs. However, no evidence of task-switching costs has been found in pigeons. I conclude that the APECS model does not fit task-switching behaviour once we have ruled out the task-switching strategy or cognitive learning process.

The following section discusses another account of associative learning: Pearce's generalisation rule (1987). As Meier et al. (2016) suggested, Pearce's generalisation rule can explain why Forrest et al. (2014) found significant task-switching costs in human participants, but none in pigeons, using an equivalent task-switching paradigm (Meier et al., 2016).

7.3.2 Pearce's Generalisation Rule

Meier and colleagues (2016) proposed that pigeons (and other animals) learn to associate stimulus configurations with responses, and these mappings can be generalised to other stimulus configurations that share similar elements. They applied Pearce's generalisation model to calculate the strength of generalisation. Pearce (1987) gives a simple rule for generalisation: G (Generalisation) = (NS × NS) / (TA × TB). In this equation: NS denotes the number of elements shared by stimulus configuration A and stimulus configuration B; TA is the total number of elements in A; and TB is the total number of elements in B. Also, Meier and colleagues assumed that, in any given trial, the pigeons identified three elements: the cue, the stimulus, and the response key (where the pigeon should peck), and that these elements formed a compound stimulus. In the following example, I illustrate how this rule can be applied in a task-switching experiment.

Suppose that in *trial n - 1*, task cue A and incongruent stimulus W appear and the correct response key is *left* (L). If a pigeon makes a correct response and receives a reward, the associative strength between the reward and the compound stimulus AWL is increased by amount δ of associative strength. If the baseline of associative strength equals V, then the current associative strength of compound AWL = V + δ.

In the subsequent *trial n*, cue B appears, but the stimulus is still W. Since W is an incongruent stimulus, the correct response is *right* (R), and L is the incorrect response. Now, the pigeon can choose between two compound stimuli: BWR (the correct one) and BWL (the incorrect one). We can assume the pigeon is more likely to pick the compound with more associative strength, rather than the compound with less associative strength. Furthermore, since compound AWL from *trial n - 1*, compound BWR, and compound BWL share common elements, the strength of association is generalised, according to Pearce's rule.

Compound BWR shares one common element with compound AWL. Applying the rule, we get: $V + [(1 \times 1) / (3 \times 3)] \times \delta = V + 1\delta /9$. In other words, in *trial n*, compound BWR (correct) inherited 1/9 of the associative strength increment from the previous trial. Compound BWL (incorrect) shares two common elements with compound AWL. Applying the rule, we get $V + [(2 \times 2) / (3 \times 3)] \times \delta = V + 4\delta/9$. Surprisingly, in *trial n*, the incorrect compound BWL has 4/9 of the associative strength increment from the previous trial, which is stronger than the correct compound with a difference of $\delta/3$ in associative strength. Therefore, the pigeon is more likely to choose the incorrect compound and make a mistake in *trial n*. Applying the same rule, we can measure the difference in associative strength between the correct compound stimulus and the incorrect compound stimulus in every trial.

Pigeons and Humans

Meier and colleagues (2016) found that for pigeons, the average associative strength difference between correct and incorrect compounds was increased by .22 δ in repeat trials compared to switch trials. Theoretically, pigeons were less likely to make a mistake in repeat trials than in switch trials. However, because δ is a relatively small increment, they suggested that a difference of .22δ between the switch and the repeat trials may be below a threshold and therefore undetectable. However, they proposed that the human participants in Forrest et al. (2014) were not encoding all three components (i.e., cue, stimuli, and response) as one compound. Instead, the human participants were encoding these three components separately. This allowed the humans to develop a cue equivalence between two cues that represent the same task, as suggested in the APECS model. Applying these assumptions, Meier and colleagues (2016) suggested that it is possible for humans to generate a significant task-switching cost using Pearce's generalisation rule (1987). They reported that for human participants, the average associative strength difference between correct and incorrect compounds is .50δ in repeat trials compared to switch trials (the difference in associative strength is more than doubled that of pigeons). Consequently, for human participants (i.e., Forrest et al., 2014), there is a strong and detectable task-switching cost. However, Meier et al. (2016) did not report their calculations. Therefore, it is difficult to replicate how they derived .50δ (see Meier et al., 2016; p. 173).

Pearce's Rule and the Results of Experiments 2.1, 2.2, 4.1 and 4.2

Meier et al. (2016) applied a dual-cue task-switching paradigm. In all of the experiments in this thesis, I used a task-cueing paradigm. Hence, each task has one task cue. Meier et al. (2016) did not report how exactly they applied Pearce's rule for human participants who encoded each element of the cue-stimulus compound separately. Hence, we do not know how Pearce's rule predicts human task-switching behaviour if there is only one cue per task. Nevertheless, it would appear that, as long as human participants encode the cue, the stimulus, and the location of the response key separately, Pearce's rule would predict constant behavioural switch costs. Human participants either do or do not indicate a significant task-switching cost. As a consequence, Pearce’s rule is not a good model for explaining the difference between Experiments 2.1 and 2.2 and Experiments 4.1 and 4.2. In Experiments 2.1 and 2.2, task-switching costs remained significant despite the absence of the task-switching strategy. In contrast, in Experiments 4.1 and 4.2, task-switching costs vanished without the task-switching strategy. It is difficult to explain this difference using Pearce’s rule.

7.4 Proactive Interference in Task-switching Experiments

In the previous sections, I have reviewed two alternative accounts in order to explain the behavioural patterns in task-switching experiments when participants do not apply the task-switching strategy. The results of Experiments 4.1 and 4.2 challenge the compound retrieval account because it predicts task-switching costs in all our experiments, which is clearly not the case.

The associative learning account, and especially Pearce’s (1987) generalisation account, as employed by Meier et al. (2016), provides a better model fit, as it can explain most of the current results. In particular, it can explain why pigeons have no task-switching costs, while human participants show task-switching costs in dual-cue task-switching experiments. However, Pearce's rule cannot explain the difference in task-switching costs between Experiments 2.1 and 2.2 and between Experiments 4.1 and 4.2, where there were no task-switching costs at all.

Based on these results, I suggested that, the modified interference account I proposed in Chapter 5 is a possible contender. Nevertheless, this account also has some disadvantages and potential loopholes. For example, to further confirm this model, future studies need to show that binary feature-response mappings can cause interference whereas other non-binary mappings cannot.

7.5 Limitations Due to Statistical Power

One major disadvantage of the present thesis is that the experiments have relatively low statistical power. In the present thesis, I sought to reduce task-switching costs. In other words, I predicted a significant difference in task-switching effects between conditions. The minimum probability acceptable for a Type 2 error is usually stated as .20, which is the probability of not rejecting a null hypothesis when it is in fact false. Accordingly, the power of the test is .80 [Power = 1 - Prob (Type 2 error)].

For a given alpha level and effect size, the power of the statistical test can be increased by a larger sample size. Hence, we can calculate the minimum requirement of the sample size in order to achieve a power of .80. For example, in Experiment 4.2, I sought to examine the task-switching effect (i.e., the factor of trial transition) in a 2 × 2 ANOVA with repeated measurements and within-between interactions. The two factors were trial transition and congruency. To reach a power of .80 (for a medium effect size f = 0.25 and alpha = 0.05), statistical power analysis (G*power version 3.1) suggested an optimal simple size of N = 24 participants. However, there were only 15 Chinese speakers and 6 non-Chinese speakers in Experiment 4.2. Therefore, this experiment is potentially underpowered. In order to reach a power of .95, a sample of N=36 would be required. Experiment 4.1 and the experiments from Chapter 3 have similar problems. Altogether, due to the low statistical power of some experiments, the result that participants cannot fully eliminate task-switching cost unless they can eliminate both the task-set reconfiguration process and interference is not conclusive. To fully confirm this explanation, future replication studies with more participants are crucial. This is one of the goals for my future studies.

7.6 Task-switching Costs: Some Speculations and Implications

Two separate mechanisms of task-switching costs have been proposed in task-switching studies: the task reconfiguration process (e.g., Monsell, 2003; Monsell & Mizon, 2006; Mayr & Kliegl, 2000; Rogers & Monsell, 1995), and task interference (e.g., Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000; Mayer & Keele, 2000; Schuch & Kochl, 2003). Later, studies proposed that in the task-cueing paradigm, at least some proportion of the task-switching cost may be the result of a cue-encoding process (Mayr & Kliegl, 2003; Grange & Houghton, 2009; Horoufchin et al., 2011; Schmitz & Voss, 2012, 2014).

These three mechanisms have been established as the origins of task-switching costs. In many studies it was assumed that all three accounts were valid and that task-switching costs are a combination of all three effects (Meiran, 2000; Koch & Allport, 2006; Gilbert & Shallice, 2002; Brown, Reynolds & Braver, 2007; Schmitz & Voss, 2012, 2014). However, based on the results of Experiments 2.1 to 5.1, I propose some interaction between these three mechanisms.

7.6.1 Relationships between Task-sets, Cue-encoding and Interference

Firstly, since, in Experiments 4.1 and 4.2, I did not detect any cue encoding after controlling the task-switching strategy, I proposed that cue-encoding effects may be only a by-product of the task-set reconfiguration process. Secondly, based on the results of Experiment 5.1, I also proposed that interference from the previous trial can be independent of the task-set reconfiguration process. On its own, this interference can create task-switching costs even when no explicit task-switching occurs. In my view this can happen because interferences are not always triggered by the task-set (Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006; Koch et al., 2010), but can also be triggered by binary feature–response mappings, as suggested in previous studies (e.g., Woodward et al. 2003; Hommel, 1998, 2005).

Thirdly, despite the fact that interference and task reconfiguration can be two independent mechanisms, it is also possible that both mechanisms interact with each other.

If the interference and task-set reconfiguration processes are two parallel processes that can generate task-switching costs, then removing the task reconfiguration process should reduce the task-switching costs. In essence, this was reported by Forrest et al. (2014). In all three experiments, they found that, after removing task-switching as a viable strategy, task-switching costs were significantly reduced.

However, in Experiments 2.1 and 2.2, I found that after controlling the task-switching strategy, task-switching costs were not reduced. One potential explanation is that the interference and the task-set reconfiguration process do not simply add up and form a summation of task-switching costs. Different degrees of interaction are possible. For example, we know that, under certain circumstances task-sets can block interference (for a review, see Dreisbach, 2012). Therefore, I suggest that, in Experiments 2.1 and 2.2, when the participants applied the task-switching strategy, during reconfiguration, the task-sets eliminated some of the interference. Thus, the power of the interference was reduced. In contrast, when the participants applied novel strategies, without activating the task-sets, interference was increased. As a result, the experimental group and the control group showed similar amounts of task-switching cost. How exactly the interference and the task-set reconfiguration process interact is a question that may be pursued in follow-up research

7.6.2 What are Task-switching Costs?

What do task-switching costs reflect in task-switching experiments? For the task-set reconfiguration process alone, the answer is straightforward. Previous studies have suggested that this process may reflect an executive control effort (Monsell, 2003; Monsell & Mizon, 2006; Mayr & Kliegl, 2000; Rogers & Monsell, 1995; Meiran, 2000; Braver, 2012). As discussed in Chapter 2, previous studies have concluded that participants with better executive control tend to have smaller task-switching costs (Cepeda, Cepeda & Kramer, 2000; Kramer, Cepeda & Cepeda, 2001; Rasmussen & Gillberg, 2001; Kray et al., 2012; Meiran, Gotler & Perlman, 2001; Kray, Li & Lindenberger 2002; Mayr & Kliegl, 2000; Barenberg et al., 2015; Kamijo & Takeda, 2010).However, if the interference from task features can be completely independent of the executive control governed task-set reconfiguration process, then the results of these studies might gain a different interpretation. For example, instead of deficiencies of executive control, it is possible that participants show larger task-switching costs because they experience stronger interference from the previous trial. Hence, measuring the individual differences of proactive interference is a promising topic for future studies.

7.6.3 A Unique Human Cognitive Function?

We have established an idea of what task-switching costs may reflect. The task-switching costs in task-cueing paradigms may relate to a combined effect of task-set reconfiguration, cue-encoding, and proactive interference from the previous trial. In this section, I will attempt to discuss a slightly different question: why do we have task-switching costs?

Based on the differences between humans and monkeys Stoet and Snyder (2007, 2009) proposed that unlike other animals, humans often need to focus on a single task. Therefore, we have may developed a unique cognitive function to prevent us from switching between tasks too easily——every time we switch to a new task, we have to make a cognitive effort. For monkeys, on the other hand, focusing on a single task is not so important. They have not developed such a cognitive function, and therefore can switch between tasks effortlessly. In other words, Stoet and Snyder (2007, 2009) suggested that human task-switching costs are not a disadvantage but rather a valuable cognitive effect that helps us to focus on the current task.

I suggested that both monkeys and humans need an additional task-set reconfiguration process when switching to a new task. The difference between the two species is the result of different interferences. For monkeys, the interference from previous actions decays very fast. However, for humans, the interference from previous actions is long-lasting. Thus, I have slightly revised Stoet and Snyder's (2007, 2009) original explanation. I propose that, to help us to focus on one task, we have developed an additional cognitive function so that the information produced by previous actions remains strong and long-lasting. As a result, every time we switch to a new task, prior information is more likely to be incompatible, resulting in delayed reactions even when the preparation period is relatively long. It is also important to mention that, although the task-switching costs can be quite substantial in task-switching experiments, this may not have such a profound impact in daily life. After all, *being forced* to switch between two tasks, back and forth continuously and rapidly, is unlikely to occur in daily life. The price of having such an additional cognitive function to help us focus on one task is in fact quite low.

7.7 Conclusion

In the present thesis, I have attempted to answer two questions. Firstly, I tried to explain the remaining task-switching costs when we rule out the task-switching strategy, I proposed a modified proactive interference account. Interference accounts in previous work have suggested that interference is the result of the relevant task-sets or stimulus-response mappings (e.g., Allport et al., 1994; Allport & Wylie, 1999; Wylie & Allport, 2000; Mayer & Keele, 2000; Schuch & Kochl, 2003; Waszak et al., 2003; Waszak & Hommel, 2007; Koch & Allport, 2006). However, the modified interference account suggests that interference can be triggered by binary feature-response mappings. In addition, I propose that proactive interference can be independent of task-set reconfiguration processes. As a result, even without explicitly switching between tasks, interference alone can produce significant task-switching costs.

Secondly, I have provided a possible explanation for the observed differences between task-switching costs in humans, pigeons, and monkeys. I have proposed that unlike pigeons and monkeys, humans have strong and long-lasting interference from previous trials in typical task-switching experiments. As a consequence, we consistently observe task-switching costs in humans, whereas monkeys and pigeons can perform in task-switching experiments without showing any task-switching costs. In order to survive and to reproduce all animals, including humans, must adapt to a complex and ever-changing environment. Thus, there is a constant demand to switch between different tasks. However, it was suggested that humans, unlike most animals, also need to focus on a single task for a prolonged period of time (Stoet & Snyder 2007, 2009). I propose that human task-switching costs may reflect the trade-off between these two needs.

References

Allport, A., Styles, E. A., & Hsieh, S. (1994). Shifting intentional set: Exploring the dynamic control of tasks. In C. Umilta & M. Moscovitch (Eds.), *Conscious and nonconscious information processing: Attention and performance XV* (pp. 421–452). Cambridge, MA: MIT Press.

Allport, A., & Wylie, G. (1999). Task-switching: Positive and negative priming of task-set. In G. W. Humphreys, J. Duncan, & A. M. Treisman (Eds.), *Attention, space and action: Studies in cognitive neuroscience* (pp. 273–296). Oxford: Oxford University Press.

Allport, A., & Wylie, G. (2000). Task-switching, stimulus–response bindings, and negative priming. In S. Monsell & J. Driver (Eds.), *Control of cognitive processes: Attention and performance XVIII* (pp. 36–70). Cambridge, MA: MIT Press.

Altmann, E. M. (2006). Task switching is not cue switching. *Psychonomic Bulletin & Review, 13,* 1016–1022.

Altmann, E. M. (2007a). Comparing switch costs: Alternating runs and explicit cuing. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 33,* 475–483. http://doi:10.1037/0278-7393.33.3.475

Altmann, E. M. (2007b). Cue-independent task-specific representations in task switching: Evidence from backward inhibition. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 33,* 892–899. http://doi:10.1037/0278-739133.5.892

Altmann, E. M. (2008). An intergrated model of cognitive control in task switching. *Psychological Review*, *115*(3), 602-639.

Altmann, E. (2011). Testing probability matching and episodic retrieval accounts of response repetition effects in task switching. *Journal Of Experimental Psychology: Learning, Memory, And Cognition*, *37*(4), 935-951. http://dx.doi.org/10.1037/a0022931

Altmann, E. M. (2013). Fine-grain episodic memory processes in cognitive control. *Zeitschrift für Psychologie*, *221*, 23-32. (Special issue: Task switching.)

Arbuthnott, K. (2008). Asymmetric switch cost and backward inhibition: Carryover activation and inhibition in switching between tasks of unequal difficulty. *Canadian Journal of Experimental Psychology/Revue Canadienne De Psychologie Expérimentale*, *62*(2), 91-100. http://dx.doi.org/10.1037/1196-1961.62.2.91

Arbuthnott, K., & Woodward, T. (2002). The influence of cue-task association and location on switch cost and alternating-switch cost. *Canadian Journal of Experimental Psychology/Revue Canadienne De Psychologie Expérimentale*, *56*(1), 18-29. http://dx.doi.org/10.1037/h0087382

Arrington, C. M., & Logan, G. D. (2004a). Episodic and semantic components of the compound-stimulus strategy in the explicit task-cuing procedure. *Memory & Cognition*, *32,* 965–978.

Arrington, C., & Logan, G. (2004b). The cost of a voluntary task switch. *Psychological Science*, *15*(9), 610-615. http://dx.doi.org/10.1111/j.0956-7976.2004.00728.x

Arrington, C. M., Reiman, K. M., & Weaver, S. M. (2014). Voluntary task switching. In J. Grange & G. Houghton (Eds.), *Task switching and cognitive control* (pp. 118-136). Oxford University Press.

Avdagic, E., Jensen, G., Altschul, D., & Terrace, H. (2013). Rapid cognitive flexibility of rhesus macaques performing psychophysical task-switching. *Animal Cognition*, *17*(3), 619-631. http://dx.doi.org/10.1007/s10071-013-0693-0

Barber, A., & Carter, C. (2005). Cognitive control involved in overcoming prepotent response tendencies and switching between tasks. *Cerebral Cortex*, *15*(7), 899-912. http://dx.doi.org/10.1093/cercor/bhh189

Barenberg, J., Berse, T., & Dutke, S. (2015). Ergometer cycling enhances executive control in task switching. *Journal of Cognitive Psychology*, *27*(6), 692-703. http://dx.doi.org/10.1080/20445911.2015.1024256

Barkley, R. (1997). Behavioral inhibition, sustained attention, and executive functions: Constructing a unifying theory of ADHD. *Psychological Bulletin*, *121*(1), 65-94. http://dx.doi.org/10.1037//0033-2909.121.1.65

Barkley, R. A., and Lombroso, P. J. (2000). Genetics of childhood disorders: XVII. ADHD, Part 1: the executive functions and ADHD. *J. Am. Acad. Child. Adolesc. Psychiatry* 39, 1064–1068. http://dx.doi.org/10.1097/00004583-200008000-00025

Barnes, J., & Underwood, B. (1959). "Fate" of first-list associations in transfer theory. *Journal of Experimental Psychology*, *58*(2), 97-105. http://dx.doi.org/10.1037/h0047507

Barrouillet, P., Bernardin, S., & Camos, V. (2004). Time constraints and resource sharing in adults' working memory spans. *Journal of Experimental Psychology: General*, *133*(1), 83-100. http://dx.doi.org/10.1037/0096-3445.133.1.83

Barutchu, A., Becker, S., Carter, O., Hester, R., & Levy, N. (2013). The role of task-related learned representations in explaining asymmetries in task switching. *Plos ONE*, *8*(4), e61729. http://dx.doi.org/10.1371/journal.pone.0061729

Bertelson (1965). Serial choice reaction-time as a function of response versus signal-and-response repetition. *Nature*, *206*(4980), 217-218. http://dx.doi.org/10.1038/206217a0

Bonnin, C., Gaonac'h, D., & Bouquet, C. (2011). Adjustments of task-set control processes: Effect of task switch frequency on task-mixing and task-switching costs. *Journal Of Cognitive Psychology*, *23*(8), 985-997. http://dx.doi.org/10.1080/20445911.2011.594435

Brass, M., Ruge, H., Meiran, N., Rubin, O., Koch, I., & Zysset, S. et al. (2003). When the same response has different meanings:. *Neuroimage*, *20*(2), 1026-1031. http://dx.doi.org/10.1016/s1053-8119(03)00357-4

Braver, T. (2012). The variable nature of cognitive control: a dual mechanisms framework. *Trends In Cognitive Sciences*, *16*(2), 106-113. http://dx.doi.org/10.1016/j.tics.2011.12.010

Braver, T., Reynolds, J., & Donaldson, D. (2003). Neural mechanisms of transient and sustained cognitive control during task switching. *Neuron*, *39*(4), 713-726. http://dx.doi.org/10.1016/s0896-6273(03)00466-5

Brown, J., Reynolds, J., & Braver, T. (2007). A computational model of fractionated conflict-control mechanisms in task-switching. *Cognitive Psychology*, *55*(1), 37-85. http://dx.doi.org/10.1016/j.cogpsych.2006.09.005

Campbell, K., & Proctor, R. (1993). Repetition effects with categorizable stimulus and response sets. *Journal of Experimental Psychology: Learning, Memory and Cognition*, *19*(6), 1345-1362.

Caselli, L., & Chelazzi, L. (2011). Does the macaque monkey provide a good model for studying human executive control? A comparative behavioral study of task Switching. *Plos ONE*, *6*(6), e21489. http://dx.doi.org/10.1371/journal.pone.0021489

Castro, L., & Wasserman, E. (2016). Executive control and task switching in pigeons. *Cognition*, *146*, 121-135. http://dx.doi.org/10.1016/j.cognition.2015.07.014

Cepeda, N. J., Cepeda, M. L., & Kramer, A. F. (2000). Task switching and attention deficit hyperactivity disorder. *Journal of Abnormal Child Psychology*, *28*, 213-226.

Descartes, R. (2008). *Discourse on the Method*. New York: Cosimo, Inc.

De Jong, R. (2000). S. An intention-activation account of residual switch costs. D. Monsell (Eds.), *Control of Cognitive Processes: Attention and Performance XVIII* (pp. 357-376), MIT Press.

Dreisbach, G. (2012). Mechanisms of cognitive control. *Current Directions In Psychological Science*, *21*(4), 227-231. http://dx.doi.org/10.1177/0963721412449830

Dreisbach, G., Goschke, T., & Haider, H. (2006). Implicit task sets in task switching? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *32*(6), 1221-1233. http://dx.doi.org/10.1037/0278-7393.32.6.1221

Dreisbach, G., Goschke, T., & Haider, H. (2007). The role of task rules and stimulus–response mappings in the task switching paradigm. *Psychological Research*, *71*(4), 383-392. http://dx.doi.org/10.1007/s00426-005-0041-3

Dreisbach, G., & Haider, H. (2008). That's what task sets are for: shielding against irrelevant information. *Psychological Research*, *72*(4), 355-361. http://dx.doi.org/10.1007/s00426-007-0131-5

Dreisbach, G., & Haider, H. (2009). How task representations guide attention: Further evidence for the shielding function of task sets. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *35*(2), 477-486. http://dx.doi.org/10.1037/a0014647

Druey, M. (2014). Response-repetition costs in choice-RT tasks: Biased expectancies or response inhibition?. *Acta Psychologica*, *145*, 21-32. http://dx.doi.org/10.1016/j.actpsy.2013.10.015

Eichelman, W. (1970). Stimulus and response repetition effects for naming letters at two response-stimulus intervals. *Perception & Psychophysics*, *7*(2), 94-96. http://dx.doi.org/10.3758/bf03210140

Forrest, C., Monsell, S., & McLaren, I. (2014). Is performance in task-cuing experiments mediated by task set selection or associative compound retrieval? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *40*(4), 1002-1024. http://dx.doi.org/10.1037/a0035981

Gade, M., & Koch, I. (2008). Dissociating cue-related and task-related processes in task inhibition: Evidence from using a 2:1 cue-to-task mapping. *Canadian Journal of Experimental Psychology/Revue Canadienne De Psychologie Expérimentale*, *62*(1), 51-55. http://dx.doi.org/10.1037/1196-1961.62.1.51

Gade, M., Schuch, S., Druey, M. D., & Koch, I. (2014). Inhibitory control in task switching. In J. Grange and G. Houghton (Eds.), *Task switching and cognitive control* (pp. 101-116). New York: Oxford.

Gilbert, S., & Shallice, T. (2002). Task switching: A PDP model. *Cognitive Psychology*, *44*(3), 297-337. http://dx.doi.org/10.1006/cogp.2001.0770

Grange, J. (2016). Temporal Distinctiveness in Task Switching: Assessing the Mixture-Distribution Assumption. *Frontiers In Psychology*, *7*. http://dx.doi.org/10.3389/fpsyg.2016.00251

Grange, J., & Houghton, G. (2014). *Task switching and cognitive control*. Oxford: Oxford University Press.

Grange, J., & Houghton, G. (2009). Cue-switch costs in task-switching: cue priming or control processes? Psychological Research, 74(5), 481-490. http://dx.doi.org/10.1007/s00426-009-0270-y

Grange, J., Kowalczyk, A., & O'Loughlin, R. (2017). The Effect of Episodic Retrieval on Inhibition in Task Switching. *Journal of Experimental Psychology: Human Perception and Performance*. http://dx.doi.org/10.1037/xhp0000411

Herd, S., O'Reilly, R., Hazy, T., Chatham, C., Brant, A., & Friedman, N. (2014). A neural network model of individual differences in task switching abilities. Neuropsychologia, 62, 375-389. http://dx.doi.org/10.1016/j.neuropsychologia.2014.04.014

Holm, S. (1979). A simple sequentially rejective multiple test procedure. *Scandinavian Journal of Statistics*, *5* (2), 65-70.

Hommel, B. (1998). Event files: Evidence for automatic integration of stimulus-response episodes. *Visual Cognition*, *5*(1-2), 183-216. http://dx.doi.org/10.1080/713756773

Hommel, B. (2004). Event files: feature binding in and across perception and action. *Trends In Cognitive Sciences*, *8*(11), 494-500. http://dx.doi.org/10.1016/j.tics.2004.08.007

Hommel, B. (2005). Feature integration across perception and action: event files affect response choice. *Psychological Research*, *71*(1), 42-63. http://dx.doi.org/10.1007/s00426-005-0035-1

Horoufchin, H., Philipp, A., & Koch, I. (2011). The dissipating task-repetition benefit in cued task switching: Task-set decay or temporal distinctiveness? *Journal of Experimental Psychology: Human Perception and Performance*, *37*(2), 455-472. http://dx.doi.org/10.1037/a0020557

Hsieh, S., & Cheng, P. (2006). Task reconfiguration and carryover in task switching: An event-related potential study. *Brain Research*, *1084*(1), 132-145. http://dx.doi.org/10.1016/j.brainres.2006.02.060

Hsieh, S., & Liu, L. (2005). The nature of switch cost: task set configuration or carry-over effect? *Cognitive Brain Research*, *22*(2), 165-175. http://dx.doi.org/10.1016/j.cogbrainres.2004.08.006

Hsieh, S., & Yu, Y. (2003). Exploring the nature of switch cost: inferences from P300 and the lateralized readiness potentials. *Brain Research Protocols*, *12*(1), 49-59. http://dx.doi.org/10.1016/s1385-299x(03)00071-0

Hübner, R., & Druey, M. D. (2006). Response execution, selection, or activation: What is sufficient for response-related repetition effects under task shifting? *Psychological Research, 70,* 245–261. http://dx.doi.org/10.1007/s00426-005-0219-8

Jersild, A. T. (1927). Mental set and shift. *Archives of Psychology, 14,* 5–81.

Jost, K., De Baene, W., Koch, I., & Brass, M. (2013). A review of the role of cue processing in task switching. *Zeitschrift Für Psychologie*, *221(1*), 5-14. http://dx.doi.org/10.1027/2151-2604/a000125

Kamijo, K., & Takeda, Y. (2010). Regular physical activity improves executive function during task switching in young adults. *International Journal of Psychophysiology*, *75*(3), 304-311. http://dx.doi.org/10.1016/j.ijpsycho.2010.01.002

Karayanidis, F., Coltheart, M., Michie, P., & Murphy, K. (2003). Electrophysiological correlates of anticipatory and poststimulus components of task switching. *Psychophysiology*, *40*(3), 329-348. http://dx.doi.org/10.1111/1469-8986.00037

Karayanidis, F., & Jamadar, S. (2014). ERP measures of proactive and reactive control processes in task-switching paradigms. In J. Grange, & G. Houghton (Eds.), *Task switching and cognitive control* (pp.201-236). Oxford: University Press.

Kessler, Y., & Meiran, N. (2010). The reaction-time task-rule congruency effect is not affected by working memory load: further support for the activated long-term memory hypothesis. *Psychological Research PRPF*, *74*(4), 388-399. http://dx.doi.org/10.1007/s00426-009-0261-z

Kiesel, A., Steinhauser, M., Wendt, M., Falkenstein, M., Jost, K., Philipp, A.M. & Koch, I. (2010*)* Control and interference in task switching—A review. *Psychological Bulletin*, *136*, 849-874. http://dx.doi.org/10.1037/a0019842

Kleinsorge, T., & Scheil, J. (2015). Incorrect predictions reduce switch costs. *Acta Psychologica*, *159*, 52-60. http://dx.doi.org/10.1016/j.actpsy.2015.05.006

Kleinsorge, T., & Scheil, J. (2016). Guessing versus choosing an upcoming task. *Frontiers In Psychology*, *7*. http://dx.doi.org/10.3389/fpsyg.2016.00396

Koch, I. (2003). The role of external cues for endogenous advance reconfiguration in task switching. *Psychonomic Bulletin & Review*, *10*(2), 488-492. http://dx.doi.org/10.3758/bf03196511

Koch, I., & Allport, A. (2006). Cue-based preparation and stimulus-based priming of tasks in task switching. *Memory & Cognition*, *34*(2), 433-444. http://dx.doi.org/10.3758/bf03193420

Koch, I., Gade, M., & Philipp, A. (2004). Inhibition of response mode in task switching. *Experimental Psychology*, *51*(1), 52-58. http://dx.doi.org/10.1027/1617-3169.51.1.52

Koch, I., Gade, M., Schuch, S., & Philipp, A. (2010). The role of inhibition in task switching: A review. *Psychonomic Bulletin & Review*, *17*(1), 1-14. http://dx.doi.org/10.3758/pbr.17.1.1

Koch, I., Prinz, W., & Allport, A. (2005). Involuntary retrieval in alphabet-arithmetic tasks: task-mixing and task-switching costs. *Psychological Research Psychologische Forschung*, *69*(4), 252-261. http://dx.doi.org/10.1007/s00426-004-0180-y

Kramer, A., Cepeda, N., & Cepeda, M. (2001). Methylphenidate effects on task-switching performance in attention-deficit/hyperactivity disorder. *Journal of The American Academy of Child & Adolescent Psychiatry*, *40*(11), 1277-1284. http://dx.doi.org/10.1097/00004583-200111000-00007

Kray, J., Karbach, J., Haenig, S., & Freitag, C. (2012). Can task-switching training enhance executive control functioning in children with attention deficit/-hyperactivity disorder? *Frontiers In Human Neuroscience*, *5*. http://dx.doi.org/10.3389/fnhum.2011.00180

Kray, J., & Lindenberger, U. (2000). Adult age differences in task switching. *Psychology and Aging*, *15*(1), 126-147. http://dx.doi.org/10.1037//0882-7974.15.1.126

Kray, J., Li, K., & Lindenberger, U. (2002). Age-related changes in task-switching components: The role of task uncertainty. *Brain and Cognition*, *49*(3), 363-381. http://dx.doi.org/10.1006/brcg.2001.1505

Lavric, A., Mizon, G., & Monsell, S. (2008). Neurophysiological signature of effective anticipatory task-set control: a task-switching investigation. *European Journal of Neuroscience*, *28*(5), 1016-1029. http://dx.doi.org/10.1111/j.1460-9568.2008.06372.x

Li, X., Li, B., Lages, M., & Stoet, G. (2017). Commentary: Task-Switching in Pigeons: Associative Learning or Executive Control?. *Frontiers In Psychology*, *8*. http://dx.doi.org/10.3389/fpsyg.2017.01420

Liefooghe, B., Barrouillet, P., Vandierendonck, A., & Camos, V. (2008). Working memory costs of task switching. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *34*(3), 478-494. http://dx.doi.org/10.1037/0278-7393.34.3.478

Lien, M., & Ruthruff, E. (2008). Inhibition of task set: Converging evidence from task choice in the voluntary task-switching paradigm. *Psychonomic Bulletin & Review*, *15*(6), 1111-1116. http://dx.doi.org/10.3758/pbr.15.6.1111

Logan, G., & Bundesen, C. (2003). Clever homunculus: Is there an endogenous act of control in the explicit task-cuing procedure? *Journal of Experimental Psychology: Human Perception and Performance*, *29*(3), 575-599. http://dx.doi.org/10.1037/0096-1523.29.3.575

Logan, G., & Schneider, D. (2006a). Interpreting instructional cues in task switching procedures: The role of mediator retrieval. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *32*(2), 347-363. http://dx.doi.org/10.1037/0278-7393.32.3.347

Logan, G., & Schneider, D. (2006b). Priming or executive control? Associative priming of cue encoding increases “switch costs” in the explicit task-cuing procedure. *Memory & Cognition*, *34*(6), 1250-1259. http://dx.doi.org/10.3758/bf03193269

Logan, G., & Schneider, D. (2010). Distinguishing reconfiguration and compound-cue retrieval in task switching. *Psychologica Belgica*, *50*(3-4), 413. http://dx.doi.org/10.5334/pb-50-3-4-413

Logan, G., Schneider, D., & Bundesen, C. (2007). Still clever after all these years: Searching for the homunculus in explicitly cued task switching. *Journal of Experimental Psychology: Human Perception and Performance*, *33*(4), 978-994. http://dx.doi.org/10.1037/0096-1523.33.4.978

Longman, C., Lavric, A., Munteanu, C., & Monsell, S. (2014). Attentional inertia and delayed orienting of spatial attention in task-switching. *Journal of Experimental Psychology: Human Perception and Performance*, *40*(4), 1580-1602. http://dx.doi.org/10.1037/a0036552

Los, S. (1996). On the origin of mixing costs: Exploring information processing in pure and mixed blocks of trials. *Acta Psychologica*, *94*(2), 145-188. http://dx.doi.org/10.1016/0001-6918(95)00050-x

McLaren, I. P. L. (1993). APECS: A solution to the sequential learning problem. In Proceedings of the *XVth Annual Convention of the Cognitive Science Society* (pp. 717–722). New York: Psychology Press.

McLaren, I., Forrest, C., & McLaren, R. (2012). Elemental representation and configural mappings: Combining elemental and configural theories of associative learning. *Learning & Behavior*, *40*(3), 320-333. http://dx.doi.org/10.3758/s13420-012-0079-1

MacLeod, C. (1991). Half a century of research on the Stroop effect: An integrative review. *Psychological Bulletin*, *109*(2), 163-203. http://dx.doi.org/10.1037//0033-2909.109.2.163

Mari-Beffa, P., & Kirkham, A. (2014) The mixing cost as a measure of cognitive control. In Grange, J.A.; & Houghton, G.(Eds), Task switching and cognitive control (pp.74-100). New York: Oxford

Mayr, U. (2002). Inhibition of action rules. *Psychonomic Bulletin & Review*, *9*(1), 93-99. http://dx.doi.org/10.3758/bf03196261

Mayr, U., & Keele, S. (2000). Changing internal constraints on action: The role of backward inhibition. *Journal of Experimental Psychology: General*, *129*(1), 4-26. http://dx.doi.org/10.1037//0096-3445.129.1.4

Mayr, U., & Kliegl, R. (2000). Complex semantic processing in old age: Does it stay or does it go? *Psychology and Aging*, *15*(1), 29-43. http://dx.doi.org/10.1037//0882-7974.15.1.29

Mayr, U., & Kliegl, R. (2003). Differential effects of cue changes and task changes on task-set selection costs. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *29*(3), 362-372. http://dx.doi.org/10.1037/0278-7393.29.3.362

Meier, B., Woodward, T., Rey-Mermet, A., & Graf, P. (2009). The bivalency effect in task switching: General and enduring. *Canadian Journal of Experimental Psychology/Revue Canadienne De Psychologie Expérimentale*, *63*(3), 201-210. http://dx.doi.org/10.1037/a0014311

Meier, C., Lea, S., & McLaren, I. (2016). Task-switching in pigeons: Associative learning or executive control? *Journal of Experimental Psychology: Animal Learning and Cognition*, *42*(2), 163-176. http://dx.doi.org/10.1037/xan0000100

Meiran, N. (1996). Reconfiguration of processing mode prior to task performance. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *22*(6), 1423-1442. http://dx.doi.org/10.1037//0278-7393.22.6.1423

Meiran, N. (2000). Modeling cognitive control in task-switching. *Psychological Research*, *63*(3-4), 234-249. http://dx.doi.org/10.1007/s004269900004

Meiran, N. (2005). Task rule-congruency and Simon-like effects in switching between spatial tasks. *The Quarterly Journal Of Experimental Psychology Section A*, *58*(6), 1023-1041. http://dx.doi.org/10.1080/02724980443000421

Merian, N. (2014). In J. Grange & G. Houghton, *Task Switching and Cognitive Control* (1st ed., pp. 45-73). New York: Oxford University Press.

Meiran, N., Chorev, Z., & Sapir, A. (2000). Component processes in task switching. *Cognitive Psychology*, *41*(3), 211-253. http://dx.doi.org/10.1006/cogp.2000.0736

Meiran, N., Dimov, E., & Ganel, T. (2012). Selective attention to perceptual dimensions and switching between dimensions. *Journal of Experimental Psychology: Human Perception and Performance*. http://dx.doi:10.1037/a0027638

Meiran, N., Gotler, A., & Perlman, A. (2001). Old age is associated with a pattern of relatively intact and relatively impaired task-set switching abilities. *The Journals of Gerontology Series B: Psychological Sciences and Social Sciences*, *56*(2), P88-P102. http://dx.doi.org/10.1093/geronb/56.2.p88

Meiran, N., & Kessler, Y. (2008). The task rule congruency effect in task switching reflects activated long-term memory. *Journal of Experimental Psychology: Human Perception and Performance*, *34*(1), 137-157. http://dx.doi.org/10.1037/0096-1523.34.1.137

Meiran, N., Kessler, Y., & Adi-Japha, E. (2008). Control by action representation and input selection (CARIS): a theoretical framework for task switching. *Psychological Research*, *72*(5), 473-500. http://dx.doi.org/10.1007/s00426-008-0136-8

Metzak, P., Meier, B., Graf, P., & Woodward, T. (2013). More than a surprise: The bivalency effect in task switching. *Journal of Cognitive Psychology*, *25*(7), 833-842. http://dx.doi.org/10.1080/20445911.2013.832196

Minear, M., & Shah, P. (2008). Training and transfer effects in task switching. *Memory & Cognition*, *36*(8), 1470-1483. http://dx.doi.org/10.3758/mc.336.8.1470

Monsell, S. (2003). Task Switching. *Trends in Cognitive Sciences*, *7*(3), 134-140. https://doi.org/10.1016/S1364-6613(03)00028-7

Monsell, S., & Mizon, G. (2006). Can the task-cuing paradigm measure an endogenous task-set reconfiguration process? *Journal of Experimental Psychology: Human Perception and Performance*, *32*(3), 493-516. http://dx.doi.org/10.1037/0096-1523.32.3.493

Monsell, S., Sumner, P., & Waters, H. (2003). Task-set reconfiguration with predictable and unpredictable task switches. *Memory & Cognition*, *31*(3), 327-342. http://dx.doi.org/10.3758/bf03194391

Monsell, S., Yeung, N., & Azuma, R. (2000). Reconfiguration of task-set: Is it easier to switch to the weaker task? *Psychological Research*, *63*(3-4), 250-264. http://dx.doi.org/10.1007/s004269900005

Mueller, S., Swainson, R., & Jackson, G. (2007). Behavioural and neurophysiological correlates of bivalent and univalent responses during task switching. *Brain Research*, *1157*, 56-65. http://dx.doi.org/10.1016/j.brainres.2007.04.046

Neill, W. (1997). Episodic retrieval in negative priming and repetition priming. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *23*(6), 1291-3105. http://dx.doi.org/10.1037//0278-7393.23.6.1291

Nieuwenhuis, S., & Monsell, S. (2002). Residual costs in task switching: Testing the failure-to-engage hypothesis. *Psychonomic Bulletin & Review*, *9*(1), 86-92. http://dx.doi.org/10.3758/bf03196259

Notebaert, W., & Soetens, E. (2003). The influence of irrelevant stimulus changes on stimulus and response repetition effects. *Acta Psychologica*, *112*(2), 143-156. http://dx.doi.org/10.1016/s0001-6918(02)00080-x

O’Reilly, R. (2010). The What and How of prefrontal cortical organization. *Trends In Neurosciences*, *33*(8), 355-361. http://dx.doi.org/10.1016/j.tins.2010.05.002

Pashler, H., & Baylis, G. (1991). Procedural learning: II. Intertrial repetition effects in speeded-choice tasks. *Journal of Experimental Psychology: Learning, Memory, And Cognition*, *17*(1), 33-48. http://dx.doi.org/10.1037//0278-7393.17.1.33

Pearce, J. M. (1987). A model for stimulus generalization in Pavlovian conditioning. *Psychological Review*, *94*, 61–73. http://dx.doi.org/10.1037/0033-295X.94.1.61

Pennington, B., & Ozonoff, S. (1996). Executive Functions and Developmental Psychopathology. *Journal of Child Psychology and Psychiatry*, *37*(1), 51-87. http://dx.doi.org/10.1111/j.1469-7610.1996.tb01380.x

Philipp, A., Gade, M., & Koch, I. (2007). Inhibitory processes in language switching: Evidence from switching language-defined response sets. *European Journal of Cognitive Psychology*, *19*(3), 395-416. http://dx.doi.org/10.1080/09541440600758812

Poboka, D., Karayanidis, F., & Heathcote, A. (2014). Extending the Failure-to-Engage theory of task switch costs. *Cognitive Psychology*, *72*, 108-141. http://dx.doi.org/10.1016/j.cogpsych.2014.02.003

Posner, M. I. (1978). *Chronometric explorations of mind*. Hillsdale, NJ: Erlbaum.

Posner, M. (2008). Measuring alertness. *Annals of The New York Academy of Sciences*, *1129*(1), 193-199. http://dx.doi.org/10.1196/annals.1417.011

Posner, M., & Boies, S. (1971). Components of attention. *Psychological Review*, *78*(5), 391-408. http://dx.doi.org/10.1037/h0031333

Rabbitt, P. (1968). Repetition effects and signal classification strategies in serial choice-response tasks. *Quarterly Journal of Experimental Psychology*, *20*(3), 232-240. http://dx.doi.org/10.1080/14640746808400157

Rasmussen, P., & Gillberg, C. (2001). Natural Outcome of ADHD With Developmental Coordination Disorder at Age 22 Years: A Controlled, Longitudinal, Community-Based

Study. *Journal of The American Academy of Child & Adolescent Psychiatry*, *39*(11), 1424-1431. http://dx.doi.org/10.1097/00004583-200011000-00017

Ravizza, S., & Carter, C. (2008). Shifting set about task switching: Behavioral and neural evidence for distinct forms of cognitive flexibility. *Neuropsychologia*, *46*(12), 2924-2935. http://dx.doi.org/10.1016/j.neuropsychologia.2008.06.006

Rogers, R., & Monsell, S. (1995). Costs of a predictible switch between simple cognitive tasks. *Journal of Experimental Psychology: General*, *124*(2), 207-231. http://dx.doi.org/10.1037//0096-3445.124.2.207

Rubin, O., & Meiran, N. (2005). On the origins of the task mixing cost in the Cuing Task-Switching Paradigm. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *31*(6), 1477-1491. http://dx.doi.org/10.1037/0278-7393.31.6.1477

Rubinstein, J., Meyer, D., & Evans, E. (2000). Executive Control of Cognitive Processes in Task Switching. *Journal of Experimental Psychology: Human Perception and Performance*, *27* (4), 763-797. http://dx.doi.org/10.1037/0096-1523.27.4.763

Schmitz, F., & Voss, A. (2012). Decomposing task-switching costs with the diffusion model. *Journal of Experimental Psychology: Human Perception and Performance*, *38*(1), 222-250. http://dx.doi.org/10.1037/a0026003

Schmitz, F., & Voss, A. (2014). Components of task switching: A closer look at task switching and cue switching. *Acta Psychologica*, *151*, 184-196. http://dx.doi.org/10.1016/j.actpsy.2014.06.009

Schneider, D. (2015). Isolating a mediated route for response congruency effects in task switching. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *41*(1), 235-245. http://dx.doi.org/10.1037/xlm0000049

Schneider, D. (2016). Investigating a method for reducing residual switch costs in cued task switching. *Memory & Cognition*, *44*(5), 762-777. http://dx.doi.org/10.3758/s13421-016-0590-2

Schneider, D. (2017). Phasic alertness and residual switch costs in task switching. *Journal of Experimental Psychology: Human Perception and Performance*, *43*(2), 317-327. http://dx.doi.org/10.1037/xhp0000318

Schneider, D., & anderson, J. (2010). Asymmetric switch costs as sequential difficulty effects. *The Quarterly Journal of Experimental Psychology*, *63*(10), 1873-1894. http://dx.doi.org/10.1080/17470211003624010

Schneider, D., & Logan, G. (2005). Modeling task switching without switching tasks: A short-term priming account of explicitly cued performance. *Journal of Experimental Psychology: General*, *134*(3), 343-367. http://dx.doi.org/10.1037/0096-3445.134.3.343

Schneider, D., & Logan, G. (2006). Hierarchical control of cognitive processes: Switching tasks in sequences. *Journal of Experimental Psychology: General*, *135*(4), 623-640. http://dx.doi.org/10.1037/0096-3445.135.4.623

Schneider, D., & Logan, G. (2007). Task switching versus cue switching: Using transition cuing to disentangle sequential effects in task-switching performance. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *33*(2), 370-378. http://dx.doi.org/10.1037/0278-7393.33.2.370

Schneider, D., & Logan, G. (2015). Chunking away task-switch costs: a test of the chunk-point hypothesis. *Psychonomic Bulletin & Review*, *22*(3), 884-889. http://dx.doi.org/10.3758/s13423-014-0721-3

Schuch, S., & Koch, I. (2003). The role of response selection for inhibition of task sets in task shifting. *Journal of Experimental Psychology: Human Perception & Performance*, *29*(1), 92-105. http://dx.doi.org/10.1037//0096-1523.29.1.92

Sergeant, J., Geurts, H., & Oosterlaan, J. (2002). How specific is a deficit of executive functioning for attention-deficit/hyperactivity disorder? *Behavioural Brain Research*, *130*(1-2), 3-28. http://dx.doi.org/10.1016/s0166-4328(01)00430-2

Shahar, N., & Meiran, N. (2014). Differential contribution of task conflicts to task switch cost and task mixing cost in alternating runs and cued task-switching: evidence from ex-Gaussian modeling of reaction time distributions. *Psychological Research*, *79*(2), 259-266. http://dx.doi.org/10.1007/s00426-014-0569-1

Soto, F., and Wasserman, E. (2010). Error-driven learning in visual categorization and object recognition: a common-elements model. *Psychol. Rev.* 117, 349–381. http://dx.doi.org/ 10.1037/a0018695

Soto, F., & Wasserman, E. (2014). Mechanisms of object recognition: what we have learned from pigeons. *Frontiers In Neural Circuits*, *8*. http://dx.doi.org/10.3389/fncir.2014.00122

Spector, A., & Biederman, I. (1976). Mental set and mental shift revisited. *The American Journal of Psychology*, *89*(4), 669. http://dx.doi.org/10.2307/1421465

Stoet, G., & Snyder, L. (2003). Executive control and task-switching in monkeys. *Neuropsychologia*, *41*(10), 1357-1364. http://dx.doi.org/10.1016/s0028-3932(03)00048-4

Stoet, G., & Snyder, L. (2004). Single neurons in posterior parietal cortex of monkeys encode cognitive set. *Neuron*, *42*(6), 1003-1012. http://dx.doi.org/10.1016/j.neuron.2004.06.003

Stoet, G., & Snyder, L. (2007). Extensive practice does not eliminate human switch costs. *Cognitive, Affective, & Behavioral Neuroscience*, *7*(3), 192-197. http://dx.doi.org/10.3758/cabn.7.3.192

Stoet, G., & Snyder, L. (2009). Neural correlates of executive control functions in the monkey. *Trends In Cognitive Sciences*, *13*(5), 228-234. http://dx.doi.org/10.1016/j.tics.2009.02.002

Strobach, T., Liepelt, R., Schubert, T., & Kiesel, A. (2011). Task switching: effects of practice on switch and mixing costs. *Psychological Research*, *76*(1), 74-83. http://dx.doi.org/10.1007/s00426-011-0323-x

Sudevan, P., & Taylor, D. (1987). The cuing and priming of cognitive operations. *Journal of Experimental Psychology: Human Perception And Performance*, *13*(1), 89-103. http://dx.doi.org/10.1037//0096-1523.13.1.89

Terrace, H. (1984). Simultaneous chaining: the problem it poses for traditional chaining theory. In M. Commons, R. Herrnstein & A. Wagner, *Quantitative analyses of behavior: discrimination processes* (pp. 115-138). Cambridge, MA: Ballinger.

Terrace, H. (2005). The simultaneous chain: a new approach to serial learning. *Trends In Cognitive Sciences*, *9*(4), 202-210. http://dx.doi.org/10.1016/j.tics.2005.02.003

Vandierendonck, A., Liefooghe, B., & Verbruggen, F. (2010). Task switching: Interplay of reconfiguration and interference control. *Psychological Bulletin*, *136*(4), 601-626. http://dx.doi.org/10.1037/a0019791

Verbruggen, F., Liefooghe, B., Vandierendonck, A., & Demanet, J. (2007). Short cue presentations encourage advance task preparation: A recipe to diminish the residual switch cost. *Journal Of Experimental Psychology: Learning, Memory, And Cognition*, *33*(2), 342-356. http://dx.doi.org/10.1037/0278-7393.33.2.342

Wasylyshyn, C., Verhaeghen, P., & Sliwinski, M. (2011). Aging and task switching: A meta-analysis. *Psychology And Aging*, *26*(1), 15-20. http://dx.doi.org/10.1037/a0020912

Waszak, F., & Hommel, B. (2007). The costs and benefits of cross-task priming. *Memory & Cognition*, *35*(5), 1175-1186. http://dx.doi.org/10.3758/bf03193487

Waszak, F., Hommel, B., & Allport, A. (2003). Task-switching and long-term priming: Role of episodic stimulus–task bindings in task-shift costs. *Cognitive Psychology*, *46*(4), 361-413. http://dx.doi.org/10.1016/s0010-0285(02)00520-0

Welford AT. 1980. Choice reaction time: basic concepts. In: Welford AT, editor. Reaction times. New York, NY: Academic Press. p 73–128.

Wong, J., & Leboe, J. (2009). Distinguishing between inhibitory and episodic processing accounts of switch-cost asymmetries. *Canadian Journal Of Experimental Psychology/Revue Canadienne De Psychologie Expérimentale*, *63*(1), 8-23. http://dx.doi.org/10.1037/a0013222

Woodward, T., Meier, B., Tipper, C., & Graf, P. (2003). Bivalency is Costly: Bivalent Stimuli Elicit Cautious Responding. *Experimental Psychology*, *50*(4), 233-238. http://dx.doi.org/10.1026//1618-3169.50.4.233

Wylie, G., & Allport, A. (2000). Task switching and the measurement of "switch costs". *Psychological Research*, *63*(3-4), 212-233. http://dx.doi.org/10.1007/s004269900003

Yeung, N., & Monsell, S. (2003). Switching between tasks of unequal familiarity: The role of stimulus-attribute and response-set selection. *Journal of Experimental Psychology: Human Perception And Performance*, *29*(2), 455-469. http://dx.doi.org/10.1037/0096-1523.29.2.455

Zinke, K., Einert, M., Pfennig, L., & Kliegel, M. (2012). Plasticity of Executive Control through Task Switching Training in Adolescents. *Frontiers In Human Neuroscience*, *6*. http://dx.doi.org/10.3389/fnhum.2012.00041

Zmigrod, S., & Hommel, B. (2013). Feature Integration across Multimodal Perception and Action: A Review. *Multisensory Research*, *26*(1-2), 143-157. http://dx.doi.org/10.1163/22134808-00002390

Appendix A

It is shown how interference in each trial is calculated in Chapter 5. This includes interference in Experiment 5.1, Experiments 2.1 and 2.2, and the hypothetical dual-cue task-switching paradigm from Chapter 5.

Abbreviations:

CO==colour cue | SH==shape cue

WH==White | BL==Black

CI==Circle | HE==Hexagon

For example, a colour cue with black circle is CO BL CI.

Res = Response

FRM = feature-response mapping

L = Left; R = Right

UoI = Unit of interference

Conditions

Gray cell = Switch Condition

White cell = Repeat Condition

Interference in Experiment 5.1

No	Trial n -1 Stimulus	Res	FRM	Trial n Stimulus	Res	FRM	UoI
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
1	BL	Left	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	HE		HE=L;CI=R	HE		HE=L;CI=R	0
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
2	BL	Left	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	HE		HE=L;CI=R	CI		CI=L;HE=R	1
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
3	BL	Left	BL = L;	WH	Left	WH=L;BL=R	1

			WH=R				
	HE		HE=L;CI=R	CI		CI=L;HE=R	1
	CO		CO=L; SH=R	CO		SH=L;CO=R	1
4	BL	Left	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	HE		HE=L;CI=R	HE		CI=L;HE=R	1
	CO		CO=L; SH=R	SH		SH=L;CO=R	1
5	BL	Left	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	HE		HE=L;CI=R	HE		HE=L;CI=R	0
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
6	BL	Left	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	HE		HE=L;CI=R	HE		CI=L;HE=R	1
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
7	BL	Left	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	HE		HE=L;CI=R	CI		HE=L;CI=R	0
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
8	BL	Left	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	HE		HE=L;CI=R	CI		HE=L;CI=R	0
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
9	BL	Left	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	CI		CI=L;HE=R	HE		HE=L;CI=R	1
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
10	BL	Left	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	CI		CI=L;HE=R	CI		CI=L;HE=R	0
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
11	BL	Left	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	CI		CI=L;HE=R	CI		CI=L;HE=R	0
	CO		CO=L; SH=R	CO		SH=L;CO=R	1
12	BL	Left	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	CI		CI=L;HE=R	HE		CI=L;HE=R	0
	CO		CO=L; SH=R	SH		SH=L;CO=R	1
13	BL	Left	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	CI		CI=L;HE=R	HE		HE=L;CI=R	1
	CO		CO=L; SH=R	SH		CO=L; SH=R	0

14	BL	Left	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	CI		CI=L;HE=R	HE		CI=L;HE=R	0
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
15	BL	Left	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	CI		CI=L;HE=R	CI		HE=L;CI=R	1
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
16	BL	Left	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	CI		CI=L;HE=R	CI		HE=L;CI=R	1
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
17	WH	Left	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	CI		CI=L;HE=R	HE		HE=L;CI=R	1
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
18	WH	Left	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	CI		CI=L;HE=R	CI		CI=L;HE=R	0
	CO		CO=L; SH=R	CO		CO=L; SH=R	0
19	WH	Left	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	CI		CI=L;HE=R	CI		CI=L;HE=R	0
	CO		CO=L; SH=R	CO		SH=L;CO=R	1
20	WH	Left	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	CI		CI=L;HE=R	HE		CI=L;HE=R	0
	CO		CO=L; SH=R	SH		SH=L;CO=R	1
21	WH	Left	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	CI		CI=L;HE=R	HE		HE=L;CI=R	1
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
22	WH	Left	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	CI		CI=L;HE=R	HE		CI=L;HE=R	0
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
23	WH	Left	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	CI		CI=L;HE=R	CI		HE=L;CI=R	1
	CO		CO=L; SH=R	SH		CO=L; SH=R	0
24	WH	Left	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	CI		CI=L;HE=R	CI		HE=L;CI=R	1
	CO		SH=L;CO=R	CO		CO=L; SH=R	1
25	WH	Right	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	HE		CI=L;HE=R	HE		HE=L;CI=R	1
	CO		SH=L;CO=R	CO		CO=L; SH=R	1

26	WH	Right	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	HE		CI=L;HE=R	CI		CI=L;HE=R	0
	CO		SH=L;CO=R	CO		CO=L; SH=R	1
27	WH	Right	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	HE		CI=L;HE=R	CI		CI=L;HE=R	0
	CO		SH=L;CO=R	CO		SH=L;CO=R	0
28	WH	Right	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	HE		CI=L;HE=R	HE		CI=L;HE=R	0
	CO		SH=L;CO=R	SH		SH=L;CO=R	0
29	WH	Right	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	HE		CI=L;HE=R	HE		HE=L;CI=R	1
	CO		SH=L;CO=R	SH		CO=L; SH=R	1
30	WH	Right	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	HE		CI=L;HE=R	HE		CI=L;HE=R	0
	CO		SH=L;CO=R	SH		CO=L; SH=R	1
31	WH	Right	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	HE		CI=L;HE=R	CI		HE=L;CI=R	1
	CO		SH=L;CO=R	SH		CO=L; SH=R	1
32	WH	Right	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	HE		CI=L;HE=R	CI		HE=L;CI=R	1
	SH		SH=L;CO=R	SH		SH=L;CO=R	0
33	WH	Left	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	HE		HE=L;CI=R	HE		HE=L;CI=R	0
	SH		SH=L;CO=R	SH		CO=L; SH=R	1
34	WH	Left	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	HE		HE=L;CI=R	HE		CI=L;HE=R	1
	SH		SH=L;CO=R	SH		CO=L; SH=R	1
35	WH	Left	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	HE		HE=L;CI=R	CI		HE=L;CI=R	0
	SH		SH=L;CO=R	SH		CO=L; SH=R	1
36	WH	Left	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	HE		HE=L;CI=R	CI		HE=L;CI=R	0
	SH		SH=L;CO=R	CO		CO=L; SH=R	1
37	WH	Left	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	HE		HE=L;CI=R	HE		HE=L;CI=R	0

	SH		SH=L;CO=R	CO		CO=L; SH=R	1
38	WH	Left	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	HE		HE=L;CI=R	CI		CI=L;HE=R	1
	SH		SH=L;CO=R	CO		CO=L; SH=R	1
39	WH	Left	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	HE		HE=L;CI=R	CI		CI=L;HE=R	1
	SH		SH=L;CO=R	CO		SH=L;CO=R	0
40	WH	Left	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	HE		HE=L;CI=R	HE		CI=L;HE=R	1
	SH		CO=L; SH=R	SH		SH=L;CO=R	1
41	BL	Right	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	HE		CI=L;HE=R	HE		HE=L;CI=R	1
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
42	BL	Right	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	HE		CI=L;HE=R	HE		CI=L;HE=R	0
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
43	BL	Right	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	HE		CI=L;HE=R	CI		HE=L;CI=R	1
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
44	BL	Right	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	HE		CI=L;HE=R	CI		HE=L;CI=R	1
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
45	BL	Right	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	HE		CI=L;HE=R	HE		HE=L;CI=R	1
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
46	BL	Right	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	HE		CI=L;HE=R	CI		CI=L;HE=R	0
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
47	BL	Right	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	HE		CI=L;HE=R	CI		CI=L;HE=R	0
	SH		CO=L; SH=R	CO		SH=L;CO=R	1
48	BL	Right	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	HE		CI=L;HE=R	HE		CI=L;HE=R	0
	SH		CO=L; SH=R	SH		SH=L;CO=R	1
49	BL	Right	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	CI		HE=L;CI=R	HE		HE=L;CI=R	0
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
50	BL	Right	WH=L;BL=R	BL	Right	WH=L;BL=R	0

	CI		HE=L;CI=R	HE		CI=L;HE=R	1
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
51	BL	Right	WH=L;BL=R	BL	Right	WH=L;BL=R	0
	CI		HE=L;CI=R	CI		HE=L;CI=R	0
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
52	BL	Right	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	CI		HE=L;CI=R	CI		HE=L;CI=R	0
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
53	BL	Right	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	CI		HE=L;CI=R	HE		HE=L;CI=R	0
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
54	BL	Right	WH=L;BL=R	BL	Left	BL = L; WH=R	1
	CI		HE=L;CI=R	CI		CI=L;HE=R	1
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
55	BL	Right	WH=L;BL=R	WH	Left	WH=L;BL=R	0
	CI		HE=L;CI=R	CI		CI=L;HE=R	1
	SH		CO=L; SH=R	CO		SH=L;CO=R	1
56	BL	Right	WH=L;BL=R	WH	Right	BL = L; WH=R	1
	CI		HE=L;CI=R	HE		CI=L;HE=R	1
	SH		CO=L; SH=R	SH		SH=L;CO=R	1
57	WH	Right	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	CI		HE=L;CI=R	HE		HE=L;CI=R	0
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
58	WH	Right	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	CI		HE=L;CI=R	HE		CI=L;HE=R	1
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
59	WH	Right	BL = L; WH=R	BL	Right	WH=L;BL=R	1
	CI		HE=L;CI=R	CI		HE=L;CI=R	0
	SH		CO=L; SH=R	SH		CO=L; SH=R	0
60	WH	Right	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	CI		HE=L;CI=R	CI		HE=L;CI=R	0
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
61	WH	Right	BL = L; WH=R	BL	Left	BL = L; WH=R	0
	CI		HE=L;CI=R	HE		HE=L;CI=R	0
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
62	WH	Right	BL = L;	BL	Left	BL = L; WH=R	0

			WH=R				
	CI		HE=L;CI=R	CI		CI=L;HE=R	1
	SH		CO=L; SH=R	CO		CO=L; SH=R	0
63	WH	Right	BL = L; WH=R	WH	Left	WH=L;BL=R	1
	CI		HE=L;CI=R	CI		CI=L;HE=R	1
	SH		CO=L; SH=R	CO		SH=L;CO=R	1
64	WH	Right	BL = L; WH=R	WH	Right	BL = L; WH=R	0
	CI		HE=L;CI=R	HE		CI=L;HE=R	1

In summary, all repeat conditions had a total of 34 units of interference. There were 32 different repeat conditions; thus, on average, each repeat trial had 1.0625 units of interference from the previous trial. All switch trials received a total of 54 units of interference. On average, each switch trials had 1.68 units of interference. Switch – Repeat = .62

Interference in Experiments 2.1 and 2.2

No	Trial n -1 Stimulus	Res	FRM	Trial n Stimulus	Res	FRM	UoI 1 or 0
			SH=L; CO = R			SH = L; CO = R	0
1	SH WH CI	L	WH=L; BL = R	SH WH CI	L	WH = L; BL = R	0
			CI= L; HE = R			CI = L; HE = R	0
			SH=L; CO = R			SH = L; CO = R	0
2	SH WH CI	L	WH=L; BL = R	SH BL CI	L	BL= L; WH = R	1
			CI= L; HE = R			CI = L; HE = R	0
			SH=L; CO = R			CO = L; SH = R	1
3	SH WH CI	L	WH=L; BL = R	SH WH HE	R	BL = L; WH = R	1
			CI= L; HE = R			CI = L; HE = R	0
			SH=L; CO = R			CO = L; SH = R	1
4	SH WH CI	L	WH=L; BL = R	SH BL HE	R	WH = L;BL= R	0
			CI= L; HE = R			CI = L;HE = R	0
			SH=L; CO = R			CO = L; SH = R	1
5	SH WH CI	L	WH=L; BL = R	CO WH HE	L	WH = L; BL = R	0
			CI= L; HE = R			HE = L;CI = R	1
			SH=L; CO = R			CO = L; SH = R	1
6	SH WH CI	L	WH=L; BL = R	CO WH CI	L	WH = L; BL = R	0

			CI= L; HE = R			CI = L;HE = R	0
			SH=L; CO = R			SH = L; CO = R	0
7	SH WH CI	L	WH=L; BL = R	CO BL HE	R	WH = L; BL = R	0
			CI= L; HE = R			CI = L; HE = R	0
			SH=L; CO = R			SH = L; CO = R	0
8	SH WH CI	L	WH=L; BL = R	CO BL CI	R	WH = L; BL = R	0
			CI= L; HE = R			HE = L; CI = R	1
			SH = L; CO = R			SH = L; CO = R	0
9	SH BL CI	L	BL= L; WH = R	SH WH CI	L	WH = L; BL = R	1
			CI = L; HE = R			CI = L; HE = R	0
			SH = L; CO = R			SH = L; CO = R	0
10	SH BL CI	L	BL= L; WH = R	SH BL CI	L	BL= L; WH = R	0
			CI = L; HE = R			CI = L; HE = R	0
			SH = L; CO = R			CO = L; SH = R	1
11	SH BL CI	L	BL= L; WH = R	SH WH HE	R	BL = L; WH = R	0
			CI = L; HE = R			CI = L; HE = R	0
			SH = L; CO = R			CO = L; SH = R	1
12	SH BL CI	L	BL= L; WH = R	SH BL HE	R	WH = L;BL= R	1
			CI = L; HE = R			CI = L;HE = R	0
			SH = L; CO = R			CO = L; SH = R	1
13	SH BL CI	L	BL= L; WH = R	CO WH HE	L	WH = L; BL = R	1
			CI = L; HE = R			HE = L;CI = R	1
			SH = L; CO = R			CO = L; SH = R	1
14	SH BL CI	L	BL= L; WH = R	CO WH CI	L	WH = L; BL = R	1
			CI = L; HE = R			CI = L;HE = R	0
			SH = L; CO = R			SH = L; CO = R	0
15	SH BL CI	L	BL= L; WH = R	CO BL HE	R	WH = L; BL = R	1
			CI = L; HE = R			CI = L; HE = R	0
			SH = L; CO = R			SH = L; CO = R	0
16	SH BL CI	L	BL= L; WH = R	CO BL CI	R	WH = L; BL = R	1
			CI = L; HE = R			HE = L; CI = R	1
			CO = L; SH = R			SH = L; CO = R	1
17	SH WH HE	R	BL = L; WH = R	SH WH CI	L	WH = L; BL = R	1
			CI = L; HE = R			CI = L; HE = R	0
			CO = L; SH = R			SH = L; CO = R	1
18	SH WH HE	R	BL = L; WH = R	SH BL CI	L	BL= L; WH = R	0
			CI = L; HE = R			CI = L; HE = R	0
			CO = L; SH = R			CO = L; SH = R	0

19	SH WH HE	R	BL = L; WH = R	SH WH HE	R	BL = L; WH = R	0
			CI = L; HE = R			CI = L; HE = R	0
			CO = L; SH = R			CO = L; SH = R	0
20	SH WH HE	R	BL = L; WH = R	SH BL HE	R	WH = L;BL= R	1
			CI = L; HE = R			CI = L;HE = R	0
			CO = L; SH = R			CO = L; SH = R	0
21	SH WH HE	R	BL = L; WH = R	CO WH HE	L	WH = L; BL = R	1
			CI = L; HE = R			HE = L;CI = R	1
			CO = L; SH = R			CO = L; SH = R	0
22	SH WH HE	R	BL = L; WH = R	CO WH CI	L	WH = L; BL = R	1
			CI = L; HE = R			CI = L;HE = R	0
			CO = L; SH = R			SH = L; CO = R	1
23	SH WH HE	R	BL = L; WH = R	CO BL HE	R	WH = L; BL = R	1
			CI = L; HE = R			CI = L; HE = R	0
			CO = L; SH = R			SH = L; CO = R	1
24	SH WH HE	R	BL = L; WH = R	CO BL CI	R	WH = L; BL = R	1
			CI = L; HE = R			HE = L; CI = R	1
			CO = L; SH = R			SH = L; CO = R	1
25	SH BL HE	R	WH = L;BL= R	SH WH CI	L	WH = L; BL = R	0
			CI = L;HE = R			CI = L; HE = R	0
			CO = L; SH = R			SH = L; CO = R	1
26	SH BL HE	R	WH = L;BL= R	SH BL CI	L	BL= L; WH = R	1
			CI = L;HE = R			CI = L; HE = R	0
			CO = L; SH = R			CO = L; SH = R	0
27	SH BL HE	R	WH = L;BL= R	SH WH HE	R	BL = L; WH = R	1
			CI = L;HE = R			CI = L; HE = R	0
			CO = L; SH = R			CO = L; SH = R	0
28	SH BL HE	R	WH = L;BL= R	SH BL HE	R	WH = L;BL= R	0
			CI = L;HE = R			CI = L;HE = R	0
			CO = L; SH = R			CO = L; SH = R	0
29	SH BL HE	R	WH = L;BL= R	CO WH HE	L	WH = L; BL = R	0
			CI = L;HE = R			HE = L;CI = R	1
			CO = L; SH = R			CO = L; SH = R	0
30	SH BL HE	R	WH = L;BL= R	CO WH CI	L	WH = L; BL = R	0
			CI = L;HE = R			CI = L;HE = R	0
			CO = L; SH = R			SH = L; CO = R	1
31	SH BL HE	R	WH = L;BL= R	CO BL HE	R	WH = L; BL = R	0
			CI = L;HE = R			CI = L; HE = R	0

32	SH BL HE	R	CO = L; SH = R WH = L;BL= R CI = L;HE = R	CO BL CI	R	SH = L; CO = R WH = L; BL = R HE = L; CI = R	1 0 1
33	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	0 0 0
34	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	0 0 1
35	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	1 0 1
36	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	CO BL CI	R	SH = L; CO = R WH = L; BL = R HE = L; CI = R	1 0 0
37	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	SH WH CI	L	SH = L; CO = R WH = L; BL = R CI = L; HE = R	1 0 1
38	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	SH BL CI	L	SH = L; CO = R BL= L; WH = R CI = L; HE = R	1 1 1
39	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	SH WH HE	R	CO = L; SH = R BL = L; WH = R CI = L; HE = R	0 1 1
40	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	SH BL HE	R	CO = L; SH = R WH = L;BL= R CI = L;HE = R	0 0 1
41	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	0 0 1
42	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	0 0 0
43	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	1 0 0
44	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	CO BL CI	R	SH = L; CO = R WH = L; BL = R HE = L; CI = R	1 0 1

45	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	SH WH CI	L	SH = L; CO = R WH = L; BL = R CI = L; HE = R	1 0 0
46	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	SH BL CI	L	SH = L; CO = R BL= L; WH = R CI = L; HE = R	1 1 0
47	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	SH WH HE	R	CO = L; SH = R BL = L; WH = R CI = L; HE = R	0 1 0
48	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	SH BL HE	R	CO = L; SH = R WH = L;BL= R CI = L;HE = R	0 0 0
49	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	CO WH HE	L	CO = L; SH = R WH = L; BL = R HE = L;CI = R	1 0 1
50	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	CO WH CI	L	CO = L; SH = R WH = L; BL = R CI = L;HE = R	1 0 0
51	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	0 0 0
52	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	CO BL CI	R	SH = L; CO = R WH = L; BL = R HE = L; CI = R	0 0 1
53	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	SH WH CI	L	SH = L; CO = R WH = L; BL = R CI = L; HE = R	0 0 0
54	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	SH BL CI	L	SH = L; CO = R BL= L; WH = R CI = L; HE = R	0 1 0
55	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	SH WH HE	R	CO = L; SH = R BL = L; WH = R CI = L; HE = R	1 1 0
56	CO BL HE	R	SH = L; CO = R WH = L; BL = R CI = L; HE = R	SH BL HE	R	CO = L; SH = R WH = L;BL= R CI = L;HE = R	1 0 0
57	CO BL CI	R	SH = L; CO = R WH = L; BL = R	CO WH HE	L	CO = L; SH = R WH = L; BL = R	1 0

			HE = L; CI = R			HE = L;CI = R	0
			SH = L; CO = R			CO = L; SH = R	1
58	CO BL CI	R	WH = L; BL = R	CO WH CI	L	WH = L; BL = R	0
			HE = L; CI = R			CI = L;HE = R	1
			SH = L; CO = R			SH = L; CO = R	0
59	CO BL CI	R	WH = L; BL = R	CO BL HE	R	WH = L; BL = R	0
			HE = L; CI = R			CI = L; HE = R	1
			SH = L; CO = R			SH = L; CO = R	0
60	CO BL CI	R	WH = L; BL = R	CO BL CI	R	WH = L; BL = R	0
			HE = L; CI = R			HE = L; CI = R	0
			SH = L; CO = R			SH = L; CO = R	0
61	CO BL CI	R	WH = L; BL = R	SH WH CI	L	WH = L; BL = R	0
			HE = L; CI = R			CI = L; HE = R	1
			SH = L; CO = R			SH = L; CO = R	0
62	CO BL CI	R	WH = L; BL = R	SH BL CI	L	BL= L; WH = R	1
			HE = L; CI = R			CI = L; HE = R	1
			SH = L; CO = R			CO = L; SH = R	1
63	CO BL CI	R	WH = L; BL = R	SH WH HE	R	BL = L; WH = R	1
			HE = L; CI = R			CI = L; HE = R	1
			SH = L; CO = R			CO = L; SH = R	1
64	CO BL CI	R	WH = L; BL = R	SH BL HE	R	WH = L;BL= R	0
			HE = L; CI = R			CI = L;HE = R	1

In summary, all repeat conditions had a total of 32 units of interference. There were 32 different repeat conditions; thus, on average, each repeat trial had 1.0 unit of interference from the previous trial. All switch trials received a total of 48 units of interference. On average, each switch trials received 1.5 units of interference. Switch – Repeat = .5

Interference in Dual-Cue Task-Switching Paradigm

	Trial n -1			Trial n			UoI
No	Stimulus	Res	FRM	Stimulus	Res	FRM	
1	circle		small=L;big=R	circle		small=L;big=R	0
	1	Left	odd=L;even=R	1	Left	odd=L;even=R	0

2	circle 1	Left	small=L;big=R odd=L;even=R	circle 4	Left	small=L;big=R even=L;odd=R	0 1
3	circle 1	Left	small=L;big=R odd=L;even=R	circle 7	Right	small=L;big=R even=L;odd=R	0 1
4	circle 1	Left	small=L;big=R odd=L;even=R	circle 8	Right	small=L;big=R odd=L;even=R	0 0
5	circle 1	Left	small=L;big=R odd=L;even=R	triangle 1	Left	small=L;big=R odd=L;even=R	0 0
6	circle 1	Left	small=L;big=R odd=L;even=R	triangle 4	Left	small=L;big=R even=L;odd=R	0 1
7	circle 1	Left	small=L;big=R odd=L;even=R	triangle 7	Right	small=L;big=R even=L;odd=R	0 1
8	circle 1	Left	small=L;big=R odd=L;even=R	triangle 8	Right	small=L;big=R odd=L;even=R	0 0
9	circle 1	Left	small=L;big=R odd=L;even=R	square 1	Left	small=L;big=R odd=L;even=R	0 0
10	circle 1	Left	small=L;big=R odd=L;even=R	square 4	Right	big=L;small=R odd=L;even=R	1 0
11	circle 1	Left	small=L;big=R odd=L;even=R	square 7	Left	big=L;small=R odd=L;even=R	1 0
12	circle 1	Left	small=L;big=R odd=L;even=R	square 8	Right	small=L;big=R odd=L;even=R	0 0
13	circle 1	Left	small=L;big=R odd=L;even=R	pentagon 1	Left	small=L;big=R odd=L;even=R	0 0
14	circle 1	Left	small=L;big=R odd=L;even=R	pentagon 4	Right	big=L;small=R odd=L;even=R	1 0
15	circle 1	Left	small=L;big=R odd=L;even=R	pentagon 7	Left	big=L;small=R odd=L;even=R	1 0
16	circle 1	Left	small=L;big=R odd=L;even=R	pentagon 8	Right	small=L;big=R odd=L;even=R	0 0
17	circle 4	Left	small=L;big=R even=L;odd=R	circle 1	Left	small=L;big=R odd=L;even=R	0 1
18	circle 4	Left	small=L;big=R even=L;odd=R	circle 4	Left	small=L;big=R even=L;odd=R	0 0

19	circle		small=L;big=R	circle		small=L;big=R	0
	4	Left	even=L;odd=R	7	Right	even=L;odd=R	0
20	circle		small=L;big=R	circle		small=L;big=R	0
	4	Left	even=L;odd=R	8	Right	odd=L;even=R	1
21	circle		small=L;big=R	triangle		small=L;big=R	0
	4	Left	even=L;odd=R	1	Left	odd=L;even=R	1
22	circle		small=L;big=R	triangle		small=L;big=R	0
	4	Left	even=L;odd=R	4	Left	even=L;odd=R	0
23	circle		small=L;big=R	triangle		small=L;big=R	0
	4	Left	even=L;odd=R	7	Right	even=L;odd=R	0
24	circle		small=L;big=R	triangle		small=L;big=R	0
	4	Left	even=L;odd=R	8	Right	odd=L;even=R	0
25	circle		small=L;big=R	square		small=L;big=R	0
	4	Left	even=L;odd=R	1	Left	odd=L;even=R	1
26	circle		small=L;big=R	square		big=L;small=R	1
	4	Left	even=L;odd=R	4	Right	odd=L;even=R	1
27	circle		small=L;big=R	square		big=L;small=R	1
	4	Left	even=L;odd=R	7	Left	odd=L;even=R	1
28	circle		small=L;big=R	square		small=L;big=R	0
	4	Left	even=L;odd=R	8	Right	odd=L;even=R	1
29	circle		small=L;big=R	pentagon		small=L;big=R	0
	4	Left	even=L;odd=R	1	Left	odd=L;even=R	1
30	circle		small=L;big=R	pentagon		big=L;small=R	1
	4	Left	even=L;odd=R	4	Right	odd=L;even=R	1
31	circle		small=L;big=R	pentagon		big=L;small=R	1
	4	Left	even=L;odd=R	7	Left	odd=L;even=R	1
32	circle		small=L;big=R	pentagon		small=L;big=R	0
	4	Left	even=L;odd=R	8	Right	odd=L;even=R	1
33	circle		small=L;big=R	circle		small=L;big=R	0
	7	Right	even=L;odd=R	1	Left	odd=L;even=R	1
34	circle		small=L;big=R	circle		small=L;big=R	0
	7	Right	even=L;odd=R	4	Left	even=L;odd=R	0
35	circle		small=L;big=R	circle		small=L;big=R	0
	7	Right	even=L;odd=R	7	Right	even=L;odd=R	0

36	circle 7	Right	small=L;big=R even=L;odd=R	circle 8	Right	small=L;big=R odd=L;even=R	0 1
37	circle 7	Right	small=L;big=R even=L;odd=R	triangle 1	Left	small=L;big=R odd=L;even=R	0 1
38	circle 7	Right	small=L;big=R even=L;odd=R	triangle 4	Left	small=L;big=R even=L;odd=R	0 0
39	circle 7	Right	small=L;big=R even=L;odd=R	triangle 7	Right	small=L;big=R even=L;odd=R	0 0
40	circle 7	Right	small=L;big=R even=L;odd=R	triangle 8	Right	small=L;big=R odd=L;even=R	0 1
41	circle 7	Right	small=L;big=R even=L;odd=R	square 1	Left	small=L;big=R odd=L;even=R	0 1
42	circle 7	Right	small=L;big=R even=L;odd=R	square 4	Right	big=L;small=R odd=L;even=R	1 1
43	circle 7	Right	small=L;big=R even=L;odd=R	square 7	Left	big=L;small=R odd=L;even=R	1 1
44	circle 7	Right	small=L;big=R even=L;odd=R	square 8	Right	small=L;big=R odd=L;even=R	0 1
45	circle 7	Right	small=L;big=R even=L;odd=R	pentagon 1	Left	small=L;big=R odd=L;even=R	0 1
46	circle 7	Right	small=L;big=R even=L;odd=R	pentagon 4	Right	big=L;small=R odd=L;even=R	1 1
47	circle 7	Right	small=L;big=R even=L;odd=R	pentagon 7	Left	big=L;small=R odd=L;even=R	1 1
48	circle 7	Right	small=L;big=R even=L;odd=R	pentagon 8	Right	small=L;big=R odd=L;even=R	0 1
49	circle 8	Right	small=L;big=R odd=L;even=R	circle 1	Left	small=L;big=R odd=L;even=R	0 0
50	circle 8	Right	small=L;big=R odd=L;even=R	circle 4	Left	small=L;big=R even=L;odd=R	0 1
51	circle 8	Right	small=L;big=R odd=L;even=R	circle 7	Right	small=L;big=R even=L;odd=R	0 1
52	circle 8	Right	small=L;big=R odd=L;even=R	circle 8	Right	small=L;big=R odd=L;even=R	0 0

53	circle		small=L;big=R	triangle		small=L;big=R	0
	8	Right	odd=L;even=R	1	Left	odd=L;even=R	0
54	circle		small=L;big=R	triangle		small=L;big=R	0
	8	Right	odd=L;even=R	4	Left	even=L;odd=R	1
55	circle		small=L;big=R	triangle		small=L;big=R	0
	8	Right	odd=L;even=R	7	Right	even=L;odd=R	1
56	circle		small=L;big=R	triangle		small=L;big=R	0
	8	Right	odd=L;even=R	8	Right	odd=L;even=R	0
57	circle		small=L;big=R	square		small=L;big=R	0
	8	Right	odd=L;even=R	1	Left	odd=L;even=R	0
58	circle		small=L;big=R	square		big=L;small=R	1
	8	Right	odd=L;even=R	4	Right	odd=L;even=R	0
59	circle		small=L;big=R	square		big=L;small=R	1
	8	Right	odd=L;even=R	7	Left	odd=L;even=R	0
60	circle		small=L;big=R	square		small=L;big=R	0
	8	Right	odd=L;even=R	8	Right	odd=L;even=R	0
61	circle		small=L;big=R	pentagon		small=L;big=R	0
	8	Right	odd=L;even=R	1	Left	odd=L;even=R	0
62	circle		small=L;big=R	pentagon		big=L;small=R	1
	8	Right	odd=L;even=R	4	Right	odd=L;even=R	0
63	circle		small=L;big=R	pentagon		big=L;small=R	1
	8	Right	odd=L;even=R	7	Left	odd=L;even=R	0
64	circle		small=L;big=R	pentagon		small=L;big=R	0
	8	Right	odd=L;even=R	8	Right	odd=L;even=R	0
65	triangle		small=L;big=R	circle		small=L;big=R	0
	1	Left	odd=L;even=R	1	Left	odd=L;even=R	0
66	triangle		small=L;big=R	circle		small=L;big=R	0
	1	Left	odd=L;even=R	4	Left	even=L;odd=R	1
67	triangle		small=L;big=R	circle		small=L;big=R	0
	1	Left	odd=L;even=R	7	Right	even=L;odd=R	1
68	triangle		small=L;big=R	circle		small=L;big=R	0
	1	Left	odd=L;even=R	8	Right	odd=L;even=R	0
69	triangle		small=L;big=R	triangle		small=L;big=R	0
	1	Left	odd=L;even=R	1	Left	odd=L;even=R	0

70	triangle 1	Left	small=L;big=R odd=L;even=R	triangle 4	Left	small=L;big=R even=L;odd=R	0 1
71	triangle 1	Left	small=L;big=R odd=L;even=R	triangle 7	Right	small=L;big=R even=L;odd=R	0 1
72	triangle 1	Left	small=L;big=R odd=L;even=R	triangle 8	Right	small=L;big=R odd=L;even=R	0 0
73	triangle 1	Left	small=L;big=R odd=L;even=R	square 1	Left	small=L;big=R odd=L;even=R	0 0
74	triangle 1	Left	small=L;big=R odd=L;even=R	square 4	Right	big=L;small=R odd=L;even=R	1 0
75	triangle 1	Left	small=L;big=R odd=L;even=R	square 7	Left	big=L;small=R odd=L;even=R	1 0
76	triangle 1	Left	small=L;big=R odd=L;even=R	square 8	Right	small=L;big=R odd=L;even=R	0 0
77	triangle 1	Left	small=L;big=R odd=L;even=R	pentagon 1	Left	small=L;big=R odd=L;even=R	0 0
78	triangle 1	Left	small=L;big=R odd=L;even=R	pentagon 4	Right	big=L;small=R odd=L;even=R	1 0
79	triangle 1	Left	small=L;big=R odd=L;even=R	pentagon 7	Left	big=L;small=R odd=L;even=R	1 0
80	triangle 1	Left	small=L;big=R odd=L;even=R	pentagon 8	Right	small=L;big=R odd=L;even=R	0 0
81	triangle 4	Left	small=L;big=R even=L;odd=R	circle 1	Left	small=L;big=R odd=L;even=R	0 1
82	triangle 4	Left	small=L;big=R even=L;odd=R	circle 4	Left	small=L;big=R even=L;odd=R	0 0
83	triangle 4	Left	small=L;big=R even=L;odd=R	circle 7	Right	small=L;big=R even=L;odd=R	0 0
84	triangle 4	Left	small=L;big=R even=L;odd=R	circle 8	Right	small=L;big=R odd=L;even=R	0 1
85	triangle 4	Left	small=L;big=R even=L;odd=R	triangle 1	Left	small=L;big=R odd=L;even=R	0 1
86	triangle 4	Left	small=L;big=R even=L;odd=R	triangle 4	Left	small=L;big=R even=L;odd=R	0 0

87	triangle 4	Left	small=L;big=R even=L;odd=R	triangle 7	Right	small=L;big=R even=L;odd=R	0 0
88	triangle 4	Left	small=L;big=R even=L;odd=R	triangle 8	Right	small=L;big=R odd=L;even=R	0 1
89	triangle 4	Left	small=L;big=R even=L;odd=R	square 1	Left	small=L;big=R odd=L;even=R	0 1
90	triangle 4	Left	small=L;big=R even=L;odd=R	square 4	Right	big=L;small=R odd=L;even=R	1 1
91	triangle 4	Left	small=L;big=R even=L;odd=R	square 7	Left	big=L;small=R odd=L;even=R	1 1
92	triangle 4	Left	small=L;big=R even=L;odd=R	square 8	Right	small=L;big=R odd=L;even=R	0 1
93	triangle 4	Left	small=L;big=R even=L;odd=R	pentagon 1	Left	small=L;big=R odd=L;even=R	0 1
94	triangle 4	Left	small=L;big=R even=L;odd=R	pentagon 4	Right	big=L;small=R odd=L;even=R	1 1
95	triangle 4	Left	small=L;big=R even=L;odd=R	pentagon 7	Left	big=L;small=R odd=L;even=R	1 1
96	triangle 4	Left	small=L;big=R even=L;odd=R	pentagon 8	Right	small=L;big=R odd=L;even=R	0 1
97	triangle 7	Right	small=L;big=R even=L;odd=R	circle 1	Left	small=L;big=R odd=L;even=R	0 1
98	triangle 7	Right	small=L;big=R even=L;odd=R	circle 4	Left	small=L;big=R even=L;odd=R	0 0
99	triangle 7	Right	small=L;big=R even=L;odd=R	circle 7	Right	small=L;big=R even=L;odd=R	0 0
100	triangle 7	Right	small=L;big=R even=L;odd=R	circle 8	Right	small=L;big=R odd=L;even=R	0 1
101	triangle 7	Right	small=L;big=R even=L;odd=R	triangle 1	Left	small=L;big=R odd=L;even=R	0 1
102	triangle 7	Right	small=L;big=R even=L;odd=R	triangle 4	Left	small=L;big=R even=L;odd=R	0 0
103	triangle 7	Right	small=L;big=R even=L;odd=R	triangle 7	Right	small=L;big=R even=L;odd=R	0 0

104	triangle 7	Right	small=L;big=R even=L;odd=R	triangle 8	Right	small=L;big=R odd=L;even=R	0 1
105	triangle 7	Right	small=L;big=R even=L;odd=R	square 1	Left	small=L;big=R odd=L;even=R	0 1
106	triangle 7	Right	small=L;big=R even=L;odd=R	square 4	Right	big=L;small=R odd=L;even=R	1 1
107	triangle 7	Right	small=L;big=R even=L;odd=R	square 7	Left	big=L;small=R odd=L;even=R	1 1
108	triangle 7	Right	small=L;big=R even=L;odd=R	square 8	Right	small=L;big=R odd=L;even=R	0 1
109	triangle 7	Right	small=L;big=R even=L;odd=R	pentagon 1	Left	small=L;big=R odd=L;even=R	0 1
110	triangle 7	Right	small=L;big=R even=L;odd=R	pentagon 4	Right	big=L;small=R odd=L;even=R	0 1
111	triangle 7	Right	small=L;big=R even=L;odd=R	pentagon 7	Left	big=L;small=R odd=L;even=R	1 1
112	triangle 7	Right	small=L;big=R even=L;odd=R	pentagon 8	Right	small=L;big=R odd=L;even=R	0 1
113	triangle 8	Right	small=L;big=R odd=L;even=R	circle 1	Left	small=L;big=R odd=L;even=R	0 0
114	triangle 8	Right	small=L;big=R odd=L;even=R	circle 4	Left	small=L;big=R even=L;odd=R	0 1
115	triangle 8	Right	small=L;big=R odd=L;even=R	circle 7	Right	small=L;big=R even=L;odd=R	0 1
116	triangle 8	Right	small=L;big=R odd=L;even=R	circle 8	Right	small=L;big=R odd=L;even=R	0 0
117	triangle 8	Right	small=L;big=R odd=L;even=R	triangle 1	Left	small=L;big=R odd=L;even=R	0 0
118	triangle 8	Right	small=L;big=R odd=L;even=R	triangle 4	Left	small=L;big=R even=L;odd=R	0 1
119	triangle 8	Right	small=L;big=R odd=L;even=R	triangle 7	Right	small=L;big=R even=L;odd=R	0 1
120	triangle 8	Right	small=L;big=R odd=L;even=R	triangle 8	Right	small=L;big=R odd=L;even=R	0 0

121	triangle 8	Right	small=L;big=R odd=L;even=R	square 1	Left	small=L;big=R odd=L;even=R	0 0
122	triangle 8	Right	small=L;big=R odd=L;even=R	square 4	Right	big=L;small=R odd=L;even=R	1 0
123	triangle 8	Right	small=L;big=R odd=L;even=R	square 7	Left	big=L;small=R odd=L;even=R	1 0
124	triangle 8	Right	small=L;big=R odd=L;even=R	square 8	Right	small=L;big=R odd=L;even=R	0 0
125	triangle 8	Right	small=L;big=R odd=L;even=R	pentagon 1	Left	small=L;big=R odd=L;even=R	0 0
126	triangle 8	Right	small=L;big=R odd=L;even=R	pentagon 4	Right	big=L;small=R odd=L;even=R	1 0
127	triangle 8	Right	small=L;big=R odd=L;even=R	pentagon 7	Left	big=L;small=R odd=L;even=R	1 0
128	triangle 8	Right	small=L;big=R odd=L;even=R	pentagon 8	Right	small=L;big=R odd=L;even=R	0 0

The rest half repeats. On average, each cue switch and cue repeat trial had .5 units of interference from the previous trial. In addition, each task switching trial had 1.0 unit of interference from the previous trial. The modified interference account would predict significant task-switching costs but no cue-switching costs.

Appendix B

ANOVAs Results of Experiment 2.1

RT ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Participant Group (experimental, control)

I used the default aov () function in R, the code for this is:

Test = aov (RT ~ Transition* Congruency* Group + Error (Subjects / (Transition* Congruency)))

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	864286	864286	1.786	0.189
Residuals	38	18392119	18392119		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	1279918	1279918	39.201	2.5e-07 ***
Trial Transition × Participant Group	1	75831	75831	2.323	0.136
Residuals	38	1240712	32650		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	393302	393302	22.439	3.01e-05 ***
Congruency × Participant Group	1	32389	32389	1.848	0.182
Residuals	38	666045	17528		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	2752	2752	0.271	0.606
TT × Cong × Group	1	3057	3057	0.301	0.586
Residuals	38	385965	10157		

ER ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Participant Group (experimental, control)

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	0.2161	0.21609	10.21	0.00281 **
Residuals	38	0.8041	0.02116		

DF	Sum Sq	Mean Sq	F	P

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.00011	0.0001147	0.103	0.749
Trial Transition × Participant Group	1	0.00167	0.0016697	1.506	0.227
Residuals	38	0.04213	0.0011088		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	0.31298	0.31298	41.872	1.29e-07 ***
Congruency × Participant Group	1	0.05535	0.05535	7.404	0.00976 **
Residuals	38	0.28404	0.00747		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	0.00666	0.006664	4.600	0.0384*
TT × Cong × Group	1	0.00268	0.002678	1.849	0.1819
Residuals	38	0.05505	0.001449		

ANOVAs Results of Experiment 2.2

RT ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Participant Group (experimental, control)

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	42688	42688	0.384	0.541
Residuals	24	2664635	111026		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	234173	234173	42.216	1.02e-06 ***
Trial Transition × Participant Group	1	14017	14017	2.527	0.125
Residuals	24	133128	5547		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	70744	70744	8.331	0.00812 **
Congruency × Participant Group	1	1865	1865	0.220	0.64352
Residuals	24	203795	8491		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	9071	9071	3.718	0.0657
TT × Cong × Group	1	8325	8325	3.412	0.0771

Residuals	24	58550	2440

ER ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Participant Group (experimental, control)

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	0.06754	0.06754	6.711	0.016 *
Residuals	24	0.24155	0.01006		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.01608	0.016076	7.845	0.00991 **
Trial Transition × Participant Group	1	0.00110	0.001095	0.535	0.47179
Residuals	24	0.04918	0.002049		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	0.10259	0.10259	47.001	4.33e-07 ***
Congruency × Participant Group	1	0.00014	0.00014	0.064	0.802
Residuals	24	0.05239	0.00218		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	0.000509	0.0005088	0.401	0.533
TT × Cong × Group	1	0.000000	0.0000000	0.000	1.000
Residuals	24	0.030464	0.0012693		

ER ~ Task Type (novel shape, novel colour) × Participant Group (experimental, control)

Code: Test = aov (ER ~ Task* Group + Error (Subjects / Task))

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	0.6578	0.6578	73.7	8.86e-09 ***
Residuals	24	0.2142	0.0089		

	DF	Sum Sq	Mean Sq	F	P
Task Type	1	0.3531	0.3531	19.55	0.000181 ***
Task Type × Participant Group	1	0.1830	0.1830	10.13	0.004001 **
Residuals	24				

ANOVAs Results of Experiment 3.1

RT ~ Trial Transition (Repeat, Switch) × Stage (Stage 2, Stage 3)

Code: Test = aov (RT ~ Transition* Stage + Error (Subjects / (Transition* Stage)))

	DF	Sum Sq	Mean Sq	F	P
Stage	1	847209	847209	19.52	0.000584 ***
Residuals	14	607625	43402		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	58416	58416	19.78	0.000552 ***
Residuals	14	41345	2953		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Stage	1	39396	39396	16.45	0.00118 **
Residuals	14	33528	2395		

ER ~ Trial Transition (Repeat, Switch) × Stage (Stage 2, Stage 3)

	DF	Sum Sq	Mean Sq	F	P
Stage	1	0.000659	0.0006590	4.061	0.0635
Residuals	14	0.002272	0.0001623		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.000238	0.0002377	0.485	0.497
Residuals	14	0.006859	0.0004899		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Stage	1	0.000514	0.0005140	2.248	0.156
Residuals	14	0.003202	0.0002287		

ANOVAs Results of Experiment 3.2

RT ~ Trial Transition (Repeat, Switch) × Stage (Stage1, Stage 2, Stage 3)

	DF	Sum Sq	Mean Sq	F	P
Stage	2	3811600	1905800	58.36	1.38e-12 ***
Residuals	40	1306163	32654		

	DF	Sum Sq	Mean Sq	F	P

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	138552	138552	33.38	1.18e-05 ***
Residuals	20	83021	4151		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Stage	2	87474	43737	13.37	3.57e-05 ***
Residuals	40	130826	3271		

ER ~ Trial Transition (Repeat, Switch) × Stage (Stage1, Stage 2, Stage 3)

	DF	Sum Sq	Mean Sq	F	P
Stage	2	0.00108	0.0005411	0.256	0.776
Residuals	40	0.08468	0.0021169		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.005213	0.005213	11.53	0.00287 **
Residuals	20	0.009041	0.000452		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Stage	2	0.001019	0.0005094	0.849	0.435
Residuals	40	0.023990	0.0005997		

ANOVAs Results of Experiment 4.1

RT ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Participant Group (Chinese, non-Chinese)

Code: Test = aov (RT ~ Transition* Congruency* Group + Error (Subjects / (Transition* Congruency)))

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	22056	22056	0.284	0.598
Residuals	26	2016147	77544		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	64607	64607	10.49	0.00327 **
Trial Transition × Participant Group	1	78811	78811	12.80	0.00139 **
Residuals	26	160068	6156		

	DF	Sum Sq	Mean Sq	F	P

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	505611	505611	35.87	2.53e-06 ***
Congruency × Participant Group	1	393459	393459	27.91	1.59e-05 ***
Residuals	26	366518	14097		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	5224	5224	1.378	0.251
TT × Cong × Group	1	2836	2836	0.748	0.395
Residuals	26	98581	3792		

ER ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Participant Group (Chinese, non-Chinese)

	DF	Sum Sq	Mean Sq	F	P
Participant Group	1	0.1228	0.12280	9.251	0.00532 **
Residuals	26	0.3451	0.01327		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.00064	0.000637	0.428	0.5190
Trial Transition × Participant Group	1	0.00488	0.004878	3.276	0.0819
Residuals	26	0.03872	0.001489		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	0.22019	0.22019	102.87	1.58e-10 ***
Congruency × Participant Group	1	0.09407	0.09407	43.95	4.95e-07 ***
Residuals	26	0.05565	0.00214		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	0.00499	0.004986	2.906	0.1001
TT × Cong × Group	1	0.00527	0.005271	3.072	0.0914
Residuals	26	0.04461	0.001716		

ANOVAs Results of Experiment 4.2

Non-Chinse speaker in Composite Condition:

RT ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent)

Code: Test = aov (RT ~ Transition* Congruency + Error (Subjects / (Transition* Congruency)))

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	1166	1166	0.121	0.742
Residuals	5	48085	9617		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	1027252	1027252	20.19	0.00644 **
Residuals	5	254405	50881		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	1817	1817	0.45	0.532
Residuals	5	20162	4032		

ER ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent)

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.002361	0.0023606	3.254	0.131
Residuals	5	0.003627	0.0007255		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	0.16807	0.16807	17.75	0.00838 **
Residuals	5	0.04734	0.00947		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	0.000277	0.0002771	0.347	0.582
Residuals	5	0.003995	0.0007990		

Non-Chinse speaker in CSI and SCI Conditions:

RT ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Cue-stimulus Sequence (CSI, SCI)

Code: Test = aov (RT ~ Transition* Congruency* Sequence + Error (Subjects / (Transition* Congruency* Sequence)))

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	699	698.7	0.701	0.427
Residuals	8	7971	996.		

	DF	Sum Sq	Mean Sq	F	P

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	2614243	2614243	37.88	0.000273 ***
Residuals	8	552093	69012		

	DF	Sum Sq	Mean Sq	F	P
Cue-Stimulus Sequence	1	1032718	1032718	43.11	0.000176 ***
Residuals	8	191631	23954		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	2970	2970.3	6.076	0.039 *
Residuals	8	3911	488.9		

	DF	Sum Sq	Mean Sq	F	P
TT × Cue-Stimulus Sequence	1	111	111.0	0.211	0.658
Residuals	8	4201	525.1		

	DF	Sum Sq	Mean Sq	F	P
Cong × Cue-Stimulus Sequence	1	244815	244815	75.28	2.42e-05 ***
Residuals	8	26016	3252		

	DF	Sum Sq	Mean Sq	F	P
TT × Cong × Sequence	1	171	170.8	0.36	0.565
Residuals	8	3795	474.4		

ER ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Cue-stimulus Sequence (CSI, SCI)

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.0002359	0.0002359	1.361	0.277
Residuals	8	0.0013862	0.0001733		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	0.18889	0.18889	28.07	0.000731 ***
Residuals	8	0.05385	0.00673		

	DF	Sum Sq	Mean Sq	F	P
Cue-Stimulus Sequence	1	0.00106	0.001064	0.213	0.657
Residuals	8	0.03992	0.004990		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	0.000392	0.0003922	0.563	0.475
Residuals	8	0.005576	0.0006970		

	DF	Sum Sq	Mean Sq	F	P
TT × Cue-Stimulus Sequence	1	0.000837	0.0008372	0.724	0.42
Residuals	8	0.009255	0.0011569		

	DF	Sum Sq	Mean Sq	F	P
Cong × Cue-Stimulus Sequence	1	0.00446	0.004459	0.881	0.375
Residuals	8	0.04049	0.005062		

	DF	Sum Sq	Mean Sq	F	P
TT × Cong × Sequence	1	0.000003	0.0000027	0.003	0.959
Residuals	8	0.007721	0.0009651		

Chinese Group

RT ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Cue-stimulus Sequence (Com, CSI, SCI)

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	3141	3141	1.268	0.279
Residuals	14	34672	2477		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	2478997	2478997	65.02	1.25e-06 ***
Residuals	14	533751	38125		

	DF	Sum Sq	Mean Sq	F	P
Cue-Stimulus Sequence	2	7836133	3918066	124.8	1.13e-14 ***
Residuals	28	878893	31389		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	960	960.2	0.513	0.486
Residuals	14	26211	1872.2		

	DF	Sum Sq	Mean Sq	F	P

	DF	Sum Sq	Mean Sq	F	P
TT × Cue-Stimulus Sequence	2	292	146	0.039	0.961
Residuals	28	103649	3702		

	DF	Sum Sq	Mean Sq	F	P
Cong × Cue-Stimulus Sequence	2	57162	28581	4.83	0.0158 *
Residuals	28	165695	5918		

	DF	Sum Sq	Mean Sq	F	P
TT × Cong × Sequence	2	6728	3364	2.254	0.124
Residuals	28	41789	1492		

ER ~ Trial Transition (Switch, Repeat) × Congruency (Congruent, Incongruent) × Cue-stimulus Sequence (Com, CSI, SCI)

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.000709	0.0007093	0.647	0.435
Residuals	14	0.015345	0.0010961		

	DF	Sum Sq	Mean Sq	F	P
Congruency	1	0.5271	0.5271	52.14	4.42e-06 ***
Residuals	14	0.1415	0.0101		

	DF	Sum Sq	Mean Sq	F	P
Cue-Stimulus Sequence	2	0.10461	0.05231	15.29	3.24e-05 ***
Residuals	28	0.09577	0.00342		

	DF	Sum Sq	Mean Sq	F	P
Trial Transition × Congruency	1	0.001968	0.001968	1.844	0.196
Residuals	14	0.014942	0.001067		

	DF	Sum Sq	Mean Sq	F	P
TT × Cue-Stimulus Sequence	2	0.00372	0.001861	1.071	0.356
Residuals	28	0.04862	0.001737		

	DF	Sum Sq	Mean Sq	F	P
Cong × Cue-Stimulus Sequence	2	0.01096	0.005481	2.358	0.113
Residuals	28	0.06507	0.002324		

	DF	Sum Sq	Mean Sq	F	P
TT × Cong × Sequence	2	0.00786	0.003929	1.931	0.164
Residuals	28	0.05698	0.002035		

ANOVAs Results of Experiment 5.1

RT ~ Trial Transition (Switch, Repeat) × Stimuli Type (Normal, Exception)

Code: Test = aov (RT ~ Transition* Stimuli + Error (Subjects / (Transition* Stimuli)))

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	175563	175563	27.5	0.000206 ***
Residuals	12	76599	6383		

	DF	Sum Sq	Mean Sq	F	P
Stimuli Type	1	185083	185083	8.171	0.0144 *
Residuals	12	271811	22651		

	DF	Sum Sq	Mean Sq	F	P
TT × Stimuli Type	1	27506	27506	8.024	0.0151 *
Residuals	12	41134	3428		

ER ~ Trial Transition (Switch, Repeat) × Stimuli Type (Normal, Exception)

	DF	Sum Sq	Mean Sq	F	P
Trial Transition	1	0.003608	0.003608	3.242	0.0969
Residuals	12	0.013353	0.001113		

	DF	Sum Sq	Mean Sq	F	P
Stimuli Type	1	0.01241	0.01241	14.77	0.00234 **
Residuals	12	0.01009	0.00084		

	DF	Sum Sq	Mean Sq	F	P
TT × Stimuli Type	1	0.001275	0.0012746	1.531	0.24
Residuals	12	0.009993	0.0008327		