Cognitive inhibition in task switching: Exploring the n–2 repetition cost

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A thesis submitted for the degree of Doctor of Philosophy

December 2018
Keele University

Abstract

The current thesis provides novel insights into the n–2 repetition cost, a task-switching behavioural effect thought to reflect cognitive inhibition. The n–2 repetition cost is widely used as a measure of group and individual differences in inhibitory control; however, there was uncertainty around its reliability and the extent to which it reflects cognitive inhibition. Overall, the current thesis indicates that the n–2 repetition cost is unlikely to be a measure of cognitive inhibition. Experiment 1 showed that the n–2 repetition cost has low reliability at an individual level, which questions its use as a measure of individual differences in inhibitory control. Experiments 2–4 demonstrated that the n–2 repetition cost is considerably modulated by a non-inhibitory mechanism (i.e., episodic retrieval), which means that the common understanding that the cost reflects cognitive inhibition is incomplete. Furthermore, Experiments 5–7 show that the n–2 repetition cost seems not affected by cognitive resources as it is not modulated by working memory load; however, from Experiments 8–9 it cannot be said for certain that the cost is not modulated by attentional resources, because a trend for the cost to be smaller under conditions exhausting attentional resources was present. In terms of the individual differences in the n–2 repetition cost, this thesis shows that they are not explained with processing speed, depressive rumination, working memory capacity, or day-to-day distractibility trait (Experiments 1, 4, 7, 8–9). And, computational modelling analyses (ex-Gaussian and diffusion modelling; Simulation Studies 1–4) support the view that the n–2 repetition cost is mainly due to non-inhibitory mechanisms. Taken together, the current thesis indicates that the n–2 repetition cost is unlikely to reflect cognitive inhibition, and if used as such should be interpreted with caution.

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Acknowledgements

Writing my acknowledgments fills me with sadness and happiness at the same time; sadness because it marks the end of a chapter in my life, and happiness because I feel a great sense of achievement to have come to this point. However, I would not be here if it was not for the people that supported me in pursuing my ambition of becoming a researcher.

It is with a great pleasure that I would like to acknowledge my deepest thanks and recognition to my supervisor Dr Jim Grange. Under Jim's guidance I was able to advance my research skills, and make a significant contribution to knowledge. Jim is truly an inspiration and it was a joy to work with him. Throughout my PhD Jim was an exceptional role-model, and I am very pleased to have worked with such a good person and a great academic.

As life goes, any challenges we face are better overcome with support from friends and family. Therefore, I would like to extend my thanks to Bartosz Kaminski, my life-companion of nearly 18-years; he is truly my rock! I would also like to thank Be and her family for their never-ending encouragement; Helen, Hannah, Malwina and Katie for being friends through time and distance; and Alice, Nick, Sammyh, Sarah, Eleonora, and Suzi for making my time at Keele University wonderful.

I would also like to express my gratitude to Dr Alexandra Lamont and Dr Andrew Rutherford—the interviewing panel at my PhD interview—for recognising my potential. Moreover, I would like to thank Dr Rutherford for agreeing to be my second supervisor, as well as for his support and advice. Finally, I would like to thank for the Faculty Studentship provided by the Keele University, without which my project would not have been possible.

Agi Kowalczyk

Chapter 1 Introduction and Literature Review

Introduction

The following introduction and literature review's aim is to introduce the reader to general concepts of controlled behaviour, the way psychologists understand it and assess it. First, broad examples of controlled behaviour's function in day-to-day life will be provided; next, current understanding of some of the general processes behind controlled behaviour will be discussed. This will be followed by presenting how controlled behaviour can be studied using task-switching paradigms, and summarising what we have learnt from early and modern task-switching research. This prelude will be used to then introduce and discuss experimental effects associated with task-switching research, with the effect of interest to the current thesis being the n–2 repetition cost—in the current thesis referred to as the n–2 task repetition cost—which will be considered in detail. With that, a concept of inhibition, its different types, and why the n–2 task repetition cost is thought of as inhibitory effect will be discussed. This effect has been replicated extensively, and has become a measure of inhibitory processes; as such, the n–2 task repetition cost is an important effect, investigated by many, and used for group and individual differences research. The current thesis identifies and addresses gaps in research surrounding the n–2 task repetition cost. The critical questions that are asked in individual chapters of this thesis, overall, challenge the n–2 task repetition cost's value as a measure of individual differences in inhibitory control, and the extent to which it reflects inhibitory mechanisms.

People go through their lives completing many different tasks every single day. Some of these tasks can be carried out automatically—that is, not requiring much thinking (e.g., well-practiced tasks such as brushing teeth, making coffee)—and other tasks require more attention and effort (e.g., replying to emails, food shopping). Which task gets to be performed in each moment, in general, is determined by context and a goal one may hold in mind; for example, in the morning making a cup of tea may be more appropriate than making a cocktail, unless we are on a holiday. Overall, the efficiency with which tasks are performed depends on many factors, from available time, cognitive ability, distractors, and health, just to mention a few. In general, humans are very good at behaving in a goal-oriented and context-specific manner; an important aspect of that is the ability to adapt our goals and modify behaviour depending on circumstances. A simple example of that is shifting between tasks; for instance, when we are writing an email we can pause that action to answer a ringing phone, only to return to writing that email later. That is, we do not have to finish writing an email before we can answer a phone; if we could not disengage from temporarily not relevant goals or behaviours, we could not react to and interact with our environment efficiently.

The ability to alter our actions and intentions to match constantly changing demands of our world is an important aspect of being a human, one that is argued to have played an essential role in our evolution (Geary, 2004). For example, back in the days when humans had to forage for food, we had to be able to identify the most fruitful areas, and discontinue visiting locations which no longer provided food. An example more fitted for the 21st century is changing a PIN on our credit card; we can re-learn a new code, in place of the old one to access our money. In both scenarios, we have to disengage from old knowledge, as well as to learn and retain new knowledge to adapt our behaviour; it may take some time to perfect new actions, but the important feature of the provided examples is the ability to modify behaviour, which humans are very good at.

Context-appropriate and goal-oriented behaviour also requires the ability to overcome prepotent responses (i.e., over-learnt/ automatic responses), and to ignore irrelevant information in our environment. So, for example, learning a new PIN, initially we may make some mistakes typing a new one, because the old PIN can be so over-learnt that being exposed to a cash machine will trigger automatic retrieval of the old PIN, at which point we must exert some control and overcome the urge to type the old one. Or, whilst doing grocery shopping, when looking for a bag of sugar, we can ignore other items we could buy but are not relevant in that moment. That is, we can control which goals and actions are performed shifting from one to another depending on the context.

When we investigate closer how simple and complex tasks are performed, it becomes obvious that what may seem as effortless behaviour (i.e., automatic yet controlled cup of tea making in the morning) arises as a product of intricate interactions between mental processes. Psychologists wonder about how controlled and stable yet flexible behaviour is produced; that is, how it is that we can focus on a goal in a given moment yet we can shift our attention from that goal and modify our behaviour—referred to as stability-flexibility dilemma (Goschke, 2000). Proposed theories explaining this dilemma are not perfect, but they are a good attempt to begin to understand controlled behaviour, as it will be demonstrated in this chapter.

Executive Functions

The most general theory on how controlled behaviour is produced refers to a system of mental processes known as Executive Functions (EF), which is a group of cognitive mechanisms that are thought to govern goal-oriented and context-specific behaviours (Burgess, 1997; Logan, 1985; Ozonoff, Pennington, & Rogers, 1991; Shallice, Burgess, & Robertson, 1996; Stuss & Alexander, 2000). This theory assumes that the EF system is made up of three domain general functions (i.e., global mechanisms) that are non-automatic in nature and are responsible for shifting between mental states, updating and monitoring information to inform goals, and inhibiting irrelevant information (Miyake et al., 2000). These mechanisms are thought to be important for establishing, carrying out, and overcoming internally (i.e., through conscious intention) as well as externally (i.e., triggered by stimuli) driven goals. It is through EFs that we can choose not to eat a second portion of dinner even though we may be very tempted to do so.

Since all behaviours can be paired with different contexts—for example, putting a bag of sugar in a cupboard and not in a fridge—it is very important to know which action is necessary in a given moment, and EFs are believed to be crucial for selecting appropriate actions. Let us consider an example: we do not pick a pen up to write with every time we come across one; instead, we can acknowledge that a pen happens to be on our table but only use it when we need to. That is, it is only given the appropriate goal and context we choose to interact with objects around us. We live in a very stimuli-rich world, and in any given moment throughout every day, many different actions can be triggered, but yet, only certain ones get to be executed. As such, EFs are thought to coordinate low-level, automatic processes (Friedman & Miyake, 2017), so we can perform purposeful behaviour, that is to plan for, select, schedule, and monitor our actions (Banich, 2009).

Being able to carry out goal-oriented and context-appropriate behaviour (often in the face of distractors) but adapt it when needed, is not only very important for healthy functioning but can also be easily taken for granted. Clinical research offers evidence to show what happens when EFs are impaired; in general, it seems that hindered EF system is associated with less control over one's own behaviour (Archibald, Mateer, & Kerns, 2001; Iaccarino, Chieffi, & Iavarone, 2014; Lhermitte, 1983). For example, those who suffered an injury to the frontal parts of the brain may have limited control over some actions; that is, under certain circumstances, they can show reflex-like behaviours (Archibald et al., 2001; McCombe & Warren, 1983), such as: manual grasping (i.e., a compulsion to grasp and use objects when placed in one's hand), groping responses (i.e., a compulsion to touch and grasp an abject if in one's near vicinity), imitation behaviour (i.e., involuntary mimicking an examiner's behaviour without purpose), utilization behaviour (i.e., instrumentally correct use of objects in inappropriate contexts), and alien limb phenomenon (i.e., the lack of sense of control over own movements). What these conditions have in common is the impairment of the efficiency with which one can generate or control one's own behaviour; specifically, it appears that these individuals have trouble selecting relevant and ignoring irrelevant information, which results in inappropriate actions being carried out. Atypical EFs are also associated with risky behaviours, as people who engage in gambling or online-gaming have a difficulty with overcoming strong desires to carry out behaviours that are considered dysfunctional (Dong & Potenza, 2016; Li et al., 2016; Yao et al., 2015); however, individual variability in EFs and cognitive control has also been shown to occur in people without obvious medical conditions (L. Cheng & Runco, 2015; León-Domínguez, Solís-Marcos, Barrio-Álvarez, Barroso y Martín, & León-Carrión, 2017), which some researchers suggest may be driven by genetics (Friedman et al., 2008; Miyake & Friedman, 2012).

Understanding how goal-oriented and context-specific behaviour arises is important for many reasons. Such knowledge can inform us about our strengths and limitations and how they can be accounted for in different occupations (Arbula, Capizzi, Lombardo, & Vallesi, 2016; P. Cheng, Tallent, Bender, Tran, & Drake, 2017; Kim, Mayorga, & Harding, 2017; Méan et al., 2017; Sasangohar, Donmez, Easty, & Trbovich, 2017), and recruitment (Arbula et al., 2016); we can also track changes in behaviour to monitor progression of illness (M. F. Green, Kern, & Heaton, 2004; Rund, 1998), changes in aging (van Hooren et al., 2005), or if and how behaviour changes under extreme circumstances (e.g., space psychology; Baarsen, Ferlazzo, Smit, Duijn, & van der Pligt, 2011).

Task-Set

As mentioned earlier, we are surrounded by a lot of stimuli in our environment, all of which typically afford more than one action and therefore can be engaged with in many ways. It was proposed by some researchers (e.g., Jersild, 1927) that to act on a presented stimulus we have to have some prior information on what that stimulus is and what can be done with it; and some stimuli may evoke emotional responses (e.g., feeling frightened upon seeing a spider). Previous encounters with different stimuli provide us with information which is used in subsequent encounters to guide our behaviour on how and when to exert certain actions. Assuming that information associated with different stimuli is held in mind, it was suggested that such knowledge may be represented in mind as task-sets—or mental task-sets (Jerslid). In general, task-sets are not innate constructs (apart from drives; for example, hunger), but rather, they can be defined as learnt and practiced associations between some stimuli and responses, stored in our memory (e.g., when a phone rings we answer it); which other researchers have referred to as schemas (Norman & Shallice, 1986).

Jersild (1927, p.66) argued that task-sets are constantly adjusted and referred to them in the following way: “Every response which an individual makes is in a sense an act of adjustment. Every stimulus that comes to a responding organism is in a sense a test of the adaptability, the power of adjustment, possessed by that organism. The simple reaction time stimulus calls for a simple adjustment; the complex mental task calls for a more complex adjustment.” Another way Jersild invites us to think of tasks-sets is to consider them as reaction-systems or acquired habits; that is, task-sets drive our behavioural patterns which we practice and adjust over time (e.g., language). It is important that these reaction-systems form so we have ready mental states (i.e., templates) to employ when interacting with our environment and other people; without some sort of mental representations of tasks forming we would not learn patterns between stimuli and actions, could not hold goals in mind, plan for, or monitor our behaviour. As it stands, there does not seem to be a limit on what adds up to a task-set; and Jerslid argued that small task-sets can become part of larger task-sets (e.g., source schemas, Norman and Shallice, 1986) if practiced enough because new associations between smaller task-sets will form. For example, when learning to drive a car, at the beginning we may form simple tasks-sets to represent what a gearbox is and how to use it, when to use indicators or mirrors, how to operate pedals, and so on; after a while, once these separate task-sets are established in mind, we can begin to coordinate them together into a bigger task-set which is driving a car (Norman & Shallice). Individual task-sets that make up a larger task-set are thought to be interconnected in such a way that, exposure to stimuli triggering retrieval of one of the simple task-sets will spread to other associated information within the larger task-set; this way our behaviour can be more efficient because relevant knowledge can be accessed faster (Jersild). This has consequences for how human behaviour can be explained because any psychological theories attempting to understand the mechanisms of controlled behaviour must consider and account for how knowledge is represented and stored in mind, as well as how knowledge is retrieved, selected and used for action generation in the face of distractors and competing mental tasks-sets.

Task-Switching

Under the assumption that we hold mental representations of different stimuli, actions, and goals, one can begin to examine experimentally, through for example task-switching paradigms, how controlled behaviour is produced. Early observations of task-switching behaviour showed that we are less efficient when we alternate between tasks compared to when we focus on one task at a time; specifically, it was noted that when carrying out daily chores, tasks do not get completed as quickly if we switch from one chore to another, compared to when we complete one task at a time (Hollingworth & Poffenberger, 1919). Some years later, with the use of the list paradigm, it was experimentally shown that indeed, switching between tasks comes at a cost. For example, in one study (Jersild, 1927) participants were presented with two words lists; in the control-condition participants performed one task (i.e., naming a word of opposite meaning to the presented one; question-*answer*), and in the shift-condition participants were required to alternate between naming a word opposite and verb-object tasks (e.g., sing-*song* ,Woodworth & Wells, 1911). The speed and accuracy at which participants completed each words list were measured and compared. It was found that shifting between two tasks led to slower and less accurate performance, providing further evidence that switching between task-sets leads to inefficient performance, compared to when one task-set is repeated; however, people that were more efficient doing the single task, adapted better to alternating between two tasks.

Jersild (1927) pointed to the control system being an important factor for efficient shifting between tasks (although control was not defined). It was proposed that during task-repetition trials performance is better because the task-set stays the same, compared to task-alternating trials during which a task-set has to be updated on each trial; as such, task-alternating takes longer due to the process of updating which requires control. Jersild also noted that the costs associated with switching tended to decrease when his participants had more practice switching between given sets, drawing attention to practice being another potentially important factor in task-switching. This was interpreted in the following way: If individual tasks have separate mental representations in the form of task-sets, alternating between those task-sets calls for a larger task-set that can facilitate alternating between the smaller tasks; once the new task-set is practiced enough (i.e., becomes automatised), performance is more efficient (e.g., equivalent to performing task-repetitions, or as seen in the example of learning to drive a car). This interpretation fitted in with some earlier work on interference and adaptability (Culler, 1912), which showed that adaptation to new associations between stimuli and behaviour (e.g., having to re-learn new response keys on previously practised and well performed task) takes time, and that interference (e.g., less efficient performance switching between compared to repeating tasks) is an incident of an automatisation process. Moreover, holding in mind established task-sets was proposed to create a state of readiness for tasks, and if the established task-set is combined with a new task-set, the state of readiness is disrupted, and hence performance is impaired. And lastly, a link between scores on intelligence and the ability to switch between tasks are made (Jersild, 1927); specifically, people who scored higher on intelligence tests showed smaller costs when shifting between tasks compared to those who scored less. As such, it was suggested that the efficiency with which one switches between tasks (or learns a new task-set) is reflective of the ability to adapt to new situations—that is, the efficiency with which adjustments are made to tasks-sets—and so task-switching was referred to as a test of adaptability (Jerslid; Culler). To summarise, early research on task-switching points to the importance of defining and accounting for mental representations of tasks in theories of how efficient task-switching is achieved; but also, it draws attention to control needed to perform tasks, practice and atomisation, as well as individual differences.

Modern experimental psychology continued to offer insights into the mechanisms responsible for efficient task-switching, and why we are less efficient switching compared to repeating tasks (Koch & Brass, 2013; Monsell, 2003). The most popular method to test how tasks are switched between in a controlled manner is to use adaptations of early task-switching paradigms; there are many different types of this method (e.g., mixed-tasks vs. single-tasks blocks, predictable/ unpredictable/ voluntry task-switching; Kiesel et al., 2010; Monsell, 2003) with the most popular type being a cued task-switching paradigm, where participants switch between or repeat tasks, and each task is preceded with a cue informing them which task must be performed on the current trial. Modern task-switching tests are delivered on a computer (rather than with a paper and pen method, or using a typewriter, as it was done in early studies) and participants are instructed to perform as fast and as accurately as they can. Performance on these tests is assessed through the speed of reaction times (RTs) and accuracy recorded by the computer, where responses typically are measured with accuracy to 1ms.

Switch-cost. Early task-switching experiments' findings that we are less efficient switching between tasks compared to when we repeat a task has been confirmed many times by modern research (Allport, Styles, & Hsieh, 1994; Culler, 1912; Grange & Houghton, 2014; Jersild, 1927; Luce, 1986; Rogers & Monsell, 1995), and this effect is nowadays known as the switch-cost (SC, Rogers & Monsell, 1995). An example of a modern task-switching paradigm which produces a SC is the following: participants may be presented with digits between 1–9 one at a time (apart from a digit 5) on the screen, and asked to switch or repeat between parity (i.e., odd or even) and magnitude judgment (higher vs lower than 5, Koch & Allport, 2006). Before each task an appropriate cue is presented (e.g., words ‘parity’ or ‘magnitude’); to give responses, participants typically are required to press one of two available keys (e.g., Z for odd/ lower than 5, or M for even/ higher than five). In terms of explanations on the source of the SC, there are a few different theories (Kiesel et al., 2010; Monsell, 2003; Vandierendonck, Liefooghe, & Verbruggen, 2010), with the main ones being: the reconfiguration (Rogers & Monsell, 1995), task-set inertia (TSI, Allport et al., 1994), priming (Altmann, 2005; Logan & Schneider, 2006; Logan et al., 2007), and inhibition theory (Allport & Wylie, 1999; Astle, Jackson, & Swainson, 2006, 2012; Goschke, 2000; Koch, Gade, Schuch, & Philipp, 2010; Kuhns, Lien, & Ruthruff, 2007; Lien, Ruthruff, & Kuhns, 2006; Meiran, Hsieh, & Chang, 2011; Meiran, Hsieh, & Dimov, 2010; Verbruggen, Liefooghe, Szmalec, & Vandierendonck, 2005).

Reconfiguration theory. The reconfiguration theory (Rogers & Monsell, 1995) argues that the SC arises as a result of a endogenously driven reconfiguration (i.e., intentional reconfiguration) of task-set which takes place between a switch from one task to another; and the reason performance is faster and more accurate in task-repetitions is because as a task is being repeated, there is no need for a reconfiguration to take place—however, there is some evidence that reconfiguration is needed for both, task-switches as well as task-repetitions (Koch, 2005). By the reconfiguration theory, the reconfiguring of a task-set takes time, as such with more time between tasks (or following a cue), there should be more time for completion of task-set reconfiguration, and hence the SC should be reduced. In studies that varied the cue-stimulus interval (CSI) it has been shown that given enough preparation time (i.e., extending the CSI) the SC is considerably reduced (Meiran, 1996; Monsell, 2003; Nicholson, Karayanidis, Davies, & Michie, 2006; Rogers & Monsell, 1995); this means that with sufficient time following a cue, a task-set can be reconfigured more effectively before a response is required. In support with reconfiguration theory, computational modelling studies showed that a modelling parameter linked to early-phase processing in task-switching (i.e., reconfiguration) taking place in working memory (WM)—an important aspect of task-switching (Liefooghe, Barrouillet, Vandierendonck, & Camos, 2008; Vandierendonck, 2012)—reflects well the SC (Schmitz & Voss, 2012, 2014; Weeda, Van der Molen, Barcelo, & Huizinga, 2014).

However, the reconfiguration theory does not fully explain the SC; specifically, longer preparation does not remove the SC completely—as the reconfiguration theory would predict—leaving what is referred to as the residual SC (Meiran, 1996; Monsell, 2003). Practice (Kramer, Hahn, & Gopher, 1999; Strobach, Liepelt, Schubert, & Kiesel, 2012, but see also Logan & Schneider, 2006a; Meiran & Kessler, 2008), foreknowledge (Sohn & Carlson, 2000), providing explicit cues (Koch, 2003; D. W. Schneider, 2016), and controlling for priming of stimulus/ response/ cue repetitions (Schmidt & Liefooghe, 2016), do not seem to remove the residual SC. Some argue that the residual SC is due to participants failing to prepare regardless of preparation time (DeJong, 2000a; Meiran & Chorev, 2005; Nieuwenhuis & Monsell, 2002)—preparation being successful on some but unsuccessful on other trials likely due to attentional differences. That is, advanced preparation is optional because the task can also be performed well without it, in which case performance is expected to be slower because the task-set reconfiguration takes place when a stimulus is presented; as such, the SC is likely to be due to a mixture of prepared and unprepared trials. And, comparing performance between three conditions where participants perform task-repetitions, task-switches at long CSI and task-switches at short CSI, when looking at the whole RTs distribution, performance under long CSI on switch-trials at fast RTs is similar to no-switch trials, and performance on switch-trials at slow RTs akin to performance under short CSI; this means, that when the task-set is well prepared (i.e., the reconfiguration is complete in advance) the SC is smaller.

Another theory asserts that preparation cannot be fully completed without the presentation of the stimulus (Mayr & Kliegl, 2003; Rogers & Monsell, 1995; Rubinstein, Meyer, & Evans, 2001); and there is also some evidence to suggest that the residual SC is largely generated through response selection processes rather than task-set switching processes (Allport & Wylie, 2000; Hunt & Klein, 2002)—for example, when eye-saccadic/ anti-saccadic measurements are used instead of motor responses to measure t ask-switching performance (which are more complex to execute)—the residual SC can be removed (Hunt & Klein, 2002). Moreover, stimulus-response (S-R) mappings—the pairing of the stimulus and response (e.g., press Z-key for parity numbers)—have also been shown to affect the SC (Altmann, 2011; Cooper & Marí-Beffa, 2008; Dreisbach, Goschke, & Haider, 2007; R. Hübner & Druey, 2005); with repeating S-R mappings facilitating performance (i.e., reducing SC) and switching S-R mappings impairing performance (i.e., increasing SC). Together, it is taken as evidence that preparation is limited at reducing SCs; this questions how well reconfiguration theory can explain the SC.

Task-set inertia. Another popular theory explaining the SC, and the residual SC, is called TSI (Allport et al., 1994; Allport & Wylie, 2000). Rather than the SC being endogenously driven, supporters of TSI argue that the SC is largely due to carry-over effects, for example automatic memory (i.e., priming) and interference processes; computational modelling studies support that view, providing evidence that the TSI is linked to a parameter sensitive to carry-over effects, the drift rate (Schmitz & Voss, 2012; Weeda et al., 2014). It is thought that in task-switches, once a given task is completed, its activation does not stop instantly in place for the activation of the new task; instead, the just completed task's activation decays gradually interfering with activation of the new task, and this remaining activation is being inhibited making returning to that task slower. By TSI theory it is speculated that if a task repeats, performance can be facilitated (i.e., through positive priming), but during task-switches abandoned task is inhibited (i.e., negatively primed), which means that re-activating previously inhibited task will be impaired. Important evidence for this is the asymmetric SC which comes from studies where participants switch between tasks of unequal difficulty; the easy task is dominant because it can be activated quickly—it tends to be the better practiced task, for example word reading in the Stroop test—and the difficult task is non-dominant because it is more difficult to activate—and it tends to be less practiced, for example colour naming in the Stroop test. In practice it means that for switches from difficult to easy tasks the SC is larger compared to switches from easy to difficult tasks. It is important to note that positive and negative priming effects are assumed to persist across trials. As an easy task is performed (which is activated easily being the dominant task), the difficult task is inhibited. To switch from an easy to a difficult task, activation of the difficult task—which takes time being a non-dominant, hence a difficult task—is affected by persisting strong activation of the easy task, which is resolved by activating the difficult task and inhibition of the easy task, leading to large SCs. But reactivating the easy task—even though affected by persisting activation of the difficult task and previous inhibition of the easy task—is strong because it is a dominant task, hence less affected by carry-over effects, leading to smaller SCs

(Allport et al., 1994). The asymmetric SC can be reduced with preparation (Yeung & Monsell, 2003b), but practice does not eliminate it (Strobach et al., 2012).

The observation that the SC is reduced with longer preparation, by the TSI theory is due to dissipating activation of previously relevant task-set (Allport et al., 1994) rather than the reconfiguration/ activation of the new task-set (Rogers & Monsell, 1995). However, there is evidence that both reconfiguration and passive dissipation may be at play in task-switching affecting the SC. That is, in a study that used a cuing-paradigm (Meiran, Chorev, & Sapir, 2000), the CSI as well as the response-cue interval (RCI) were manipulated, with the CSI thought to be linked to preparatory (i.e., reconfiguration), and the RCI thought to be linked to passive dissipation (i.e., TSI) mechanisms. It was found that, extending the RCI reduced the SC—per the TSI theory—but increasing the CSI further reduced the SC—as per reconfiguration theory.

Focusing on response stage and what takes place after a response is made, by the TSI theory the magnitude of the SC reflects the time needed for the cognitive system to resolve interference between task-sets; and the extent to which task-sets interfere with each other in large seem to depend on S-R mappings. Specifically, S-R mappings may stay partially active after a switch, even when ample time for preparation is provided, leading to conflicts in response selection—which may explain why practice does not eliminate the asymmetric SC. For example, smaller SCs are seen for switching between tasks with dissimilar S-R mappings (i.e., leading to less interference) compared to similar S-R mappings (Allport et al., 1994); and there is evidence that memory of priming of S-R mappings can have a lasting impact (Pösse, Waszak, & Hommel, 2006). The importance of S-R mappings in task-switching has been highlighted by other studies too; for example, performance following trials on which participants are asked to withhold their response (i.e., No-

Go trials) is characterised by no SC (Verbruggen et al., 2005); and exploring preparatory stages in task-switching—following Go and No-Go trials—using electroencephalography (EEG), provided evidence that S-R mappings and their repetition/ switch are processed differently by the brain (Astle et al., 2006; Mueller, Swainson, & Jackson, 2007)—Go and No-Go trials are associated with lateral parietal positive activation (linked to attention, processing of stimuli), whereas No-Go trials only are associated with late frontal negative activation (linked to action decision, reward, punishment). The EEG evidence for differential brain processing following making and withholding a response is a good illustration of how behavioural data can be informed by brain imaging studies; in this case, drawing attention to S-R mapping as an important factor in task-switching and associated effects.

Inhibition. Another set of theories put an emphasis on inhibitory mechanisms leading to SCs. In general, inhibition is thought to resolve conflicts which are triggered by interference which could be from previously attended task, overlapping task-features or S-R mappings; overall, inhibition is thought to aid processing of task-relevant information against task-irrelevant information. There seems to be quite a lot of evidence to suggest that SCs may be caused by different types of inhibition; for example, lateral inhibition (Schuch & Koch, 2003), reactive inhibition (Koch, 2008; Monsell, Sumner, & Waters, 2003; Wendt, Luna-Rodriguez, Reisenauer, Jacobsen, & Dreisbach, 2012) proactive-like type of inhibition (Allport et al., 1994; Meiran, 1996; Monsell & Mizon, 2006), or a combination of both, proactive and reactive inhibition in SC (Arbula et al., 2016; Bugg & Braver, 2016; Costa & Friedrich, 2012; Tarantino, Mazzonetto, & Vallesi, 2016; Vandierendonck et al., 2010; Whitson et al., 2014; Yu, Chan, Chau, & Fu, 2017).

The different types of inhibition will be discussed later; for now it is important to appreciate that the predominant number of reports on links between the SC and inhibition point to evidence for inhibition targeting response-stage, either through automatic inhibition of selected/ executed responses (Astle et al., 2006; Meiran et al., 2010; Schuch & Koch, 2003; Verbruggen et al., 2005), or by resolving potential conflicts, such as incongruent responses (Goschke, 2000). Some argue for different types of inhibition (Astle et al., 2012); for example, inhibition of a task-set that affords an incongruent response (Meiran, Hsieh, et al., 2011), or inhibition of an abandoned task (Kuhns et al., 2007; Mayr & Keele, 2000). Evidence for different types of inhibition would suggest that it may be an adaptive phenomenon—inhibition applied selectively, when interference is likely to occur—however this argument is objected (Lien et al., 2006). That is, it is known that SCs increase for switches between tasks sharing some features (e.g., bivalent stimuli) compared to switches between tasks not sharing features (i.e., univalent stimuli) (Allport et al., 1994; Rogers & Monsell, 1995). This is believed to be the case because bivalent stimuli's mental representations (i.e., task sets) may overlap, therefore the retrieval of an appropriate task-set might be more difficult, compared to univalent stimuli. For bivalent stimuli the correct task-set has to be selected among competing task-sets, hence it takes longer to be performed (Allport & Wylie, 1999). However, Lien and colleagues showed that SCs were not larger when switching to previously inhibited more difficult task (i.e., bivalent task) compared to an easier task (i.e., univalent). If bivalent tasks required more inhibition—as it would be predicted assuming that inhibition deals with incongruency type of effects—then we would expect larger SCs for that type of switch compared to switches free from incongruency effects.

Inhibition is definitely a mechanism that should be considered when investigating controlled behaviour as literature reviews illustrate (Kiesel et al., 2010;

Koch et al., 2010; Monsell, 2003); however, so far it is not clear what exactly is inhibited, or when and why this inhibition occurs, as results can vary considerably between paradigms. Inhibition in task-switching will be discussed further in detail in the next section, first, other theories explaining and important factors modulating SC will be mentioned.

Alternative theories. The reconfiguration, TSI, and inhibition theories do not explain the SC fully. For example, task-repetitions are assumed not to require reconfiguration compared to task-switches, as such, extended preparation benefits should be specific to task-task-switches, but there is evidence that task-predictability can benefit task-repetitions and task-switches (Gotler, Meiran, & Tzelgov, 2003; Heuer, Schmidtke, & Kleinsorge, 2001; Koch, 2001, 2005); these results are against what the reconfiguration theory would predict. And, the asymmetric SC is not always present when switching between tasks of unequal difficulty as TSI theory would assume (Experiment 5, Allport et al., 1994). And when it comes to inhibition, researchers cannot agree on what is being inhibited and when. As such, SC does not seem to be explained by endogenous or exogenous mechanisms separately; but, theories that account for a combination of endogenous as well as exogenous processes and how they interact may be more appropriate (Mayr & Kliegl, 2003; Meiran, 1996; Monsell, 2003; Rubinstein et al., 2001; Ruthruff, Remington, & Johnston, 2001; Schmitz & Voss, 2012; Sohn & Carlson, 2000; Yeung & Monsell, 2003a). For example, Mayr and Kliegl used a BI paradigm with a 2:1 cue-tasks mapping (i.e., each task had two cues) and manipulated whether the cue, or the task repeated/ switched across the trials; they found that a large proportion of the SC was due to cue-switches. Moreover, cue-switches costs, but not task-switches costs, were sensitive to practice and preparation effects, whereas task-switches costs were sensitive to response-priming effects and task-set inhibition. It is thought that a task-set is automatically retrieved from long-term memory (LTM) into WM where conflicts (i.e., task-set, response) can lead to interference resolved with inhibition; which suggests a two-stage processing during task-switching (also known as multiple-components model).

Computational modelling studies show nicely that endogenous and exogenous processes may contribute differently to the SC (Karayanidis et al., 2009; Schmitz & Voss, 2012, 2014; Weeda et al., 2014), supporting multiple-components theories (e.g., Ruthruff et al., 2001). A method known as diffusion modelling (Ratcliff & McKoon, 2008) uses a number of different parameters ascribed to distinct processing mechanisms to describe reaction time (RT) performance; two of the main parameters are the drift rate, and the non-decisional parameter. In terms of the SC, it seems that different experimental factors (e.g., preparation, cues, and predictability) affect diffusion modelling parameters differently highlighting exogenous and endogenous mechanisms' contribution to the SC.

The non-decisional parameter—typically associated with basic encoding, WM processing and response execution (Voss, Rothermund, & Voss, 2004)—seems to be sensitive to preparation; specifically, this parameter is increased for switch-trials with less time for preparation compared to switch-trials with lengthier preparation time (Schmitz & Voss, 2012, 2014). Since the non-decisional parameter is affected by preparation, it can be classed as an early processing phase parameter; and that would imply that preparation can begin without the presentation of the stimulus (e.g., it can begin with the cue presentation), which could mean that the non-decisional parameter can be sensitive to higher cognitive processes (e.g., advanced preparation) and not only basic encoding or response execution processes (e.g., Karayanidis et al., 2009). It could also mean, that the non-decisional parameter indexes the time required to retrieve a new-task set/ S-R mappings, and with more preparation time, task-sets and S-R mappings can be retrieved more successfully. Another aspect of the SC and non-decisional parameter observed is that increases in non-decisional parameters seem to be task-switch specific; that is, the non-decisional parameter does not appear to be affected by cue-switches (Schmitz & Voss, 2014).

The carry-over effects such as the TSI, are linked to the drift rate parameter, which typically reflects differences between conditions to do with how efficiently information needed for response selection accumulates; as such, lower drift rates are linked to more difficult tasks compared to easier tasks (Voss et al., 2004), and are linked to higher intelligence/ WM capacity (Schmiedek, Oberauer, Wilhelm, Süß, & Wittmann, 2007). In general, the drift rate indicates relative task readiness or how efficiently a response is selected—with higher rates characterising readier tasks—which can be affected by many factors, top-down as well as bottom-up ones (e.g., task difficulty, predictability, stimuli characteristics, arousal, and priming). In task-switching the drift rate is decreased for task-switches compared to task-repetitions, and even smaller for short CSI compared to long CSI, and for predictable compared to unpredictable task-switches (Schmitz & Voss, 2012, 2014); also, the type of cues (i.e., how informative they are) does not seem to affect task-switches but influence task repetition performance as reflected in the drift parameter (Karayanidis et al., 2009). As such, it was proposed that at short CSI the TSI has particularly strong effect on drift rate through positive and negative priming, at longer CSI the relative readiness is further improved by preparation; this shows the importance of automatic (i.e., carry over effects like TSI) and control mechanisms (e.g., reconfiguration).

Computational modelling of task-switching shows that controlled and automatic processes contribute to SC, and behavioural research points to factors that should be taken into account when explaining the SC. The main contenders are for example, theories emphasising priming and episodic retrieval effects in task-switching (Altmann, 2011; Druey, 2014; Friedman, Miyake, Robinson, & Hewitt, 2011; Logan & Bundesen, 2003; Logan & Schneider, 2006b; Logan, Schneider, & Bundesen, 2007; Waszak, Hommel, & Allport, 2003), importance of cues (Jost, De Baene, Koch, & Brass, 2013), decay of task set activation (Allport et al., 1994; Altmann & Gray, 2008; Meiran et al., 2000; Sohn & Anderson, 2001), and temporal distinctiveness (Grange, 2016; Grange & Cross, 2015; Horoufchin, Philipp, & Koch, 2011).

Priming. Performance on cognitive tasks (including task-switching) can be considerably affected by priming—subconscious memory process—and automaticity (Logan, 1985, 1988). Associations between environmental factors and action are stored in memory and retrieved when necessary, making our actions more efficient (Hommel, 2000, 2004, Logan, 1985, 1988). Priming can facilitate or impair our behaviour through automating activation of declarative and procedural memories upon seeing a given stimulus (e.g., remembering our PIN when using a cash machine). This unconscious memory effect is an important factor in human behaviour because in general, it can make us more efficient responding to and interacting with our environment as well as other humans (e.g., Bargh, Chen, & Burrows, 1996; Doyen, Klein, Pichon, & Cleeremans, 2012). Positive priming is when behaviour is facilitated through previous exposure to stimuli and associated actions (e.g., reading), whereas negative priming is a slower behaviour due to previous exposure to stimuli (e.g., slower reaction to previously ignored stimuli) (Tipper, 1985; Tulving & Schacter, 1990).

Task-switching performance has been shown to be sensitive to priming effects because it involves repetitive exposure to pairing of stimuli and responses leading to positive as well as negative priming (e.g., tasks' shared perceptual features and/ or responses may become inhibited if not relevant in a given moment) (Allport & Wylie, 1999, 2000; Altmann, 2011; Grange & Houghton, 2010; Hommel, 1998, 2004; Horoufchin, Philipp, & Koch, 2011; Koch & Allport, 2006; Logan & Bundesen, 2003; Neill, 2007; Schmidt & Liefooghe, 2016; Schneider & Logan, 2005; Waszak, Hommel, & Allport, 2003; Woodward & Meier, 2003). For instance, there is evidence (Waszak et al., 2003) that if a presented stimulus was previously relevant for another task, task-switching performance is less efficient—as measured by a larger SC—relative to when a stimulus was not previously relevant for another task. This was taken as evidence that processing of stimuli associated with other tasks leads to interference due to competing stimuli's associations, which has support in other studies (Druey, 2014; Schmidt & Liefooghe, 2016). Responses in task-switching also can be primed (Gade & Koch, 2007; Schmidt & Liefooghe, 2016); for example, evidence suggests that if response keys overlap across different tasks (i.e., in bivalent tasks, where a task can afford more than one response category), it leads to larger inhibitory effects compared to when responses do not overlap (i.e., in univalent tasks, where a task can afford one response category). Together these findings were taken as supporting evidence for the view that positive and negative priming effects play an important role in successful task-switching.

Episodic retrieval. Evidence from research into priming effects are very relevant to task-switching as shown in previous paragraph; further evidence for this comes from research investigating a memory mechanism known as episodic retrieval—thought to be behind the negative priming effect (Neill, 1997).

Episodic retrieval can be described as an operation by which the most recent exposure to a given stimulus (i.e., declarative and procedural memories; for example, task’s parameters such as stimulus features, distractors, response made) is retrieved automatically upon seeing that stimulus again, either facilitating or impairing performance. It has been speculated and supported experimentally (Hommel, 1998, 2004, 2009), that performing a task results in this task’s parameters being integrated in the form of a memory trace (also known as an event-file), which can be retrieved as triggered by being exposed to the same stimulus again (i.e., a retrieval of the most recent episode). If upon seeing a given stimulus a retrieved response does not match the currently required action, by the episodic retrieval account, it should lead to impaired performance because a new response will have to be generated against the one that was initiated automatically through episodic retrieval; however, if the retrieved task’s parameters match the demands of the current task, performance should be facilitated (Hommel, 1998, 2000, 2004).

Some of priming effects seen in task-switching have been already linked to episodic retrieval mechanisms (Altmann, 2011; Grange, 2016; Horoufchin et al., 2011; Logan & Schneider, 2006b; Mayr & Kliegl, 2003; Schmidt & Liefooghe, 2016; Waszak et al., 2003).

Cues. A major aspect of tasks-switching is the use of cues; they have to be discussed further because our understanding of cues’ role in task-switching may influence the models that explain SC (Logan & Bundesen, 2003). Cues are important because they trigger the retrieval of relevant task-set; as such, the role of cues in task-switching has been investigated extensively (Jost et al., 2013), and a few trends established will be discussed next.

Cues that have—what can be considered weak associations with stimuli—are linked to larger SCs (Arbuthnott, Woodward, & Columbia, 2002); for instance, if participants switch between parity (i.e., odd/ even) and letter tasks (i.e., vowel/consonant), if tasks are cued by abstract cues (e.g., symbols), the SCs are larger compared to if the tasks are cued with transparent cues (e.g., “odd/even”, “vowel/ consonant”). The weak cue-stimulus association refers to the idea that the relevant association is not well established in memory compared to a strong cue-stimulus association which is well practiced and formed; therefore, weakly associated cues will not be as efficient at retrieving a given task-set compared to cues strongly associated with stimulus (see also, Gade & Koch, 2007; Logan & Schneider, 2006b; Mayr & Kliegl, 2003).

Also, in a recently reported study (Kleinsorge & Scheil, 2017), pre-cues were incorporated into a task-switching paradigm, and they were shown to reduce SC. Specifically, two cue conditions were created, where in one condition a standard cue was used before each trial, and in the second condition participants saw a pre-cue as well as the standard cue. The standard cue informed participants that one of four possible tasks would become relevant (i.e., number judgment: odd/ even or lower/ higher than five; letter judgment: first/ second half in alphabet or vowel/ consonant), whereas the pre-cue informed participants that one of two task-sets would be cued by the standard cue; this way, participants knew that only two out of four task-sets would be relevant. Since the SC was reduced in the pre-cue condition compared to the standard cue condition, it was suggested that pre-cue reduced the number of possible tasks to be considered, therefore the number of competing task sets was reduced, which meant that the relevant task-set could be retrieved more efficiently. Similar effects have been shown to be present in experiments which varied the certainty of incoming tasks (see also, Lange, Seer, Müller, & Kopp, 2015; Mayr, 2006), where more certainty on which task would become relevant next leads to smaller SC compared to when there is less certainty. This can be taken as evidence for a strategic/ adaptive type of processing.

Moreover, cues used in task-switching can be influenced by priming effects (Logan & Schneider, 2006; Mayr & Kliegl, 2003), which is an important modulating factor seen in task-switching. For instance, if two cues per tasks are used (Logan & Schneider, 2006b) with some cue-pairs semantically associated within each task (e.g., king/ queen cues for a parity task; salt/ pepper cues for magnitude task), other cues associated between tasks (cues king/ salt for parity; cues queen/ pepper for magnitude task), and some cues not semantically associated within or between tasks (cues king/ salt for parity task; cues night/ verb for magnitude task, performance is the most efficient for task-repetitions under semantically or associatively primed cues. In terms of whether a cue or a task repeats or alternates, performance was the fastest for cues repetitions, and the slowest for cues alternations.

The studies mentioned above illustrate nicely that cues are important for task-switching efficiency, and that if cues are not informative (e.g., Koch, 2003), or are associated with other tasks, performance is affected adversely.

Inhibition

Before the second behavioural effect seen in task-switching will be discussed, another important concept will be addressed, inhibition (Miyake et al., 2000). This mechanism is thought to operate to stop or supress a given process, for example, firing neurons, thoughts, or behaviour; as such, inhibition has been shown to be present at cellular, neural, as well as the behavioural level (Aron, 2007). In psychology inhibition in general, can be described as "… the stopping or overriding of a mental process, in whole or in part, with or without intention" (MacLeod, 2007, p.5). However, over the years, inhibition as a psychological concept has become a controversial topic, mainly because—unlike in physiology and neuroscience—in psychology an agreement on the role of inhibition in human behaviour has not been settled yet, as inhibition is difficult to define and measure, and many diverse methods are used to assess it producing different results (Amer, Campbell, & Hasher, 2016; Anderson & Levy, 2007; Aron, 2007; Bruce Morton, 2010; Gorfein & MacLeod, 2007; Meiran, 2010; Morton, Ezekiel, & Wilk, 2011). As it will be shown in the coming paragraphs, there is a considerable amount of experimental evidence suggesting that there does not seem to be one type of inhibition (Aron, 2007; Costa & Friedrich, 2012; Howard, Johnson, & Pascual-Leone, 2014; Littman & Takács, 2017); instead, when and how much inhibition is applied can depend on exogenous as well as endogenous factors such as genetic predispositions, developmental/ individual differences, tasks' difficulty, practice, general health, and age (Amer et al., 2016; Friedman et al., 2008; Wilkinson & Yang, 2016).

In psychology, researchers seem to talk about inhibition in diverse ways, and it can lead to confusion at times (MacLeod, Dodd, Sheard, Erin, Wilson, & Bibi, 2003; Noreen, MacLeod, & Kim, 2015; Rey-Mermet, Gade, & Oberauer, 2017), especially if different studies focus on different aspects of inhibition; for example, how is it triggered, what its function is, what is being inhibited, what is it modulated by, how does it manifest in brain imaging—an issue that has been well addressed in the book "The concept of inhibition in cognition" (MacLeod, 2007). This is because, depending on what is being studied, inhibition can be defined differently, leading to inconsistencies in interpretation. Some emphasise the difference between interfering information or response being inhibited (Bunge, Dudukovic, Thomason, Vaidya, & Gabrieli, 2002; Colzato et al., 2008; Wyatt & Machado, 2013a, 2013b), others talk about instructed compared to adaptive flexibility (Peters & Crone, 2014), or anticipatory and conflict/ interference driven inhibition (Whitson et al., 2014), but in task-switching research academics focus on inhibition's automatic and cognitive facets (Kiesel et al., 2010; Koch et al., 2010). For the current thesis, a few specific types of inhibition will be introduced and discussed—lateral, reactive, and proactive inhibition—together with evidence for and against those types in task-switching.

Lateral Inhibition. From research looking at cognition in chimpanzees we know that they do not seem to be able to overcome automatic responses (i.e., the prepotent responses) (Houghton & Tipper, 1996). That is, in an experimental setting chimpanzees can learn to point to a numeral 2 instead of 4 to get a better reward—where the numeral 2 leads to receiving two and the numeral 4 leads to receiving four candies. However, if instead of numerals candies are presented to choose from—where pointing to two candies leads to receiving four as a reward—subjects always reach for four candies, regardless of how much training they get. That is, they do not seem to be able to overcome the automatic response that the four candies evoke, which in this case is that they see four against two candies and four seems more rewarding. Humans on the other hand, are very good at overcoming undesired or currently not relevant responses, and we seem to be equipped with mechanisms—for example inhibition—preventing irrelevant actions being acted out (Tipper, Howard, & Houghton, 1998). If we are faced with a stimulus that evokes an irrelevant action, the irrelevant neuronal activations are thought to be inhibited through lateral inhibition to facilitate correct behaviour (Houghton & Tipper, 1994); a mechanism which has been confirmed to operate in single-cell recordings in animals (Blakemore & Tobin, 1972; Moran & Desimone, 1985; Schall & Hanes, 1993; Sillito, 1975). Lateral inhibition is a biological mechanism which, at a point of a response being selected, inhibits currently not relevant representations triggered through spreading of relevant representations activation; that is, the way neurons are connected makes it so that if a single neuron or population of neurons become excited, this excitation will spread to neighbouring neurons. To limit the excitation to the neurons that are receiving direct stimulation (i.e., that are triggered by stimuli), the neighbouring neurons are inhibited via inter-neurons; through that inhibition, activations of relevant neurons are “sharpened” and stronger than neighbouring activations; consequently, aiding an appropriate action being generated. For example, if multiple responses are paired to one stimulus, neural activations of those different S-R mappings’ may be overlapped, hence when a stimulus is presented and requires a response, the activation of mental representation of that task has to be limited to excitation of relevant representations, this is so the correct action is selected (e.g., Houghton, Tipper, Weaver, & Shore, 1996). As such, lateral inhibition is sensitive to the strength of competing responses, but relevant representations’ activations can be enhanced through attention, which leads to irrelevant activations being supressed more.

Lateral inhibition is automatic in nature, and plays an important role in overcoming prepotent responses to ensure controlled behaviour (Houghton et al., 1996; Norman & Shallice, 1986; Tipper, Howard, & Houghton, 2000). That is, our visuomotor system is crucial for learning associations between visually perceived stimuli and possible actions that go with these stimuli (Bunge, 2004; Goodale, 1998, 2014); and it seems that the way action is selected upon seeing previously encountered stimuli is determined through the strength of activations these stimuli trigger. Specifically, stimuli with their associated actions have mental representations—that is, neuronal activations related to stimuli and action synchronise when they co-occur—and these activations are triggered upon seeing given stimuli (Rizzolatti et al., 1981); this means, that merely attending to stimuli briefly or covertly can trigger associated activations, including weakly associated ones (Houghton et al., 1996).

Reactive Inhibition. However, lateral inhibition is not always effective, and in certain situations another type of inhibition is needed, namely reactive inhibition (also known as self-inhibition) (Tipper et al., 2000). Reactive inhibition seems to be a global mechanism (although, there is evidence on selective reactive inhibition see Jahfari, Stinear, Claffey, Verbruggen, & Aron, 2010) needed for stopping an ongoing or started action, and as such, reactive inhibition affects the motor system in response to exogenous trigger (e.g., stopping a car at read light or withholding a response upon seeing a STOP signal during task-switching) (Aron, 2011; Braver, 2012; Braver, Paxton, Locke, & Barch, 2009). Reactive inhibition can also be triggered by prior stimuli processing (Houghton & Tipper, 1994), and so if it is detected that activated mental representations pose a high risk of causing interference (e.g., overlapping responses), these representations are inhibited more through reactive inhibition, compared to mental representations characterised by low-risk of interference. Or, if irrelevant representations are too active—which often leads to a conflict in generating action—they are supressed more, which takes place via inhibitory self-feedback connections. That is, if too much irrelevant activation is detected, a self-feedback mechanism applies more inhibition to the irrelevant activations, or/ and after a task/ response is selected it becomes inhibited, hence reactive inhibition is also known as self-inhibition (Grange, Juvina, & Houghton, 2013; Tipper et al., 2000; Wyatt & Machado, 2013a, 2013b). As such, reactive inhibition is transient and is believed to be an important mechanism for monitoring and resolving conflicts; it allows for post-event corrections which makes it important for sequential actions (Houghton & Tipper, 1994). This type of inhibition has been referred to as a residual aftereffect of processing that is not intended (Logan, 1994).

Despite the importance of reactive inhibition in controlled behaviour, it has its limitations; namely, this type of inhibition is thought to affect the motor system globally, and it does not seem to be suitable for actions which require decisional time (i.e., "hold your horses" scenario, Aron, 2011). That is, instead of behaving reactively (i.e., in response to external stimulus) we often need partial or selective control whilst we plan for actions we want to carry out; in other words, we sometimes have to keep our goal in mind before making a response. So, in general, reactive inhibition does not seem to be suitable to deal with control of cognitive processes; however, there is some evidence from brain imaging studies that reactive control can affect a preparatory stage of cognitive processing (Chikazoe et al., 2009; Hester et al., 2004; Jahfari et al., 2010; Zandbelt et al., 2008).

Proactive Inhibition. Proactive inhibition is thought to be an intentional type of inhibition; that is, proactive inhibition is stopping of action or a thought prepared in anticipation of interference, and as such, proactive inhibition is thought to maintain goal-directed preparedness (Braver, 2012; Braver et al., 2009; Elchlepp, Lavric, Chambers, & Verbruggen, 2016). Proactive inhibition is believed to be superior to reactive inhibition (Suzuki & Shinoda, 2015); that is, it is generated by specific mental goals rather than being triggered by external stimuli, as such, it can be more selective compared to reactive inhibition which is more global (Aron, 2011). It would appear that, proactive inhibition is thought to be a top-down mechanism, hence superior to rather automatic lateral and reactive inhibition, and so it makes sense that it has been shown to be important for adopting strategies (Koch, Philipp, & Gade, 2006; Logan, 1985; Logan & Bundesen, 2003; D. W. Schneider & Logan, 2005) for efficient performance of cognitive tasks, and for controlled behaviour in day-to-day life (Arbula et al., 2016; Bugg & Braver, 2016).

N–2 Task Repetition Cost

Despite a lot of research being conducted into the source of the SC, the evidence is inconsistent and it is difficult to settle on one explanation. In an attempt to further explore one of the theories explaining the SC, researchers (Mayr & Keele, 2000) developed a new paradigm, the Backward Inhibition (BI) paradigm, to test specific predictions around inhibition. Typically in task-switching experiments two tasks are used to either switch between or repeat, but the BI paradigm was designed to accommodate switching between three tasks (Mayr & Keele, 2000); an example of the BI paradigm is as follows (Mayr, 2002). Participants switch between three spatial transformations, vertical, horizontal, and diagonal, where each task has its own cue. Every trial is composed of a cue being presented first, followed by a target, followed by response (Figure 1.1, right hand side example). The cue appears above a black frame, and the target appears in one of the inner corners of that frame; spatial transformation of the target involves indicating which corner of the frame the target would move to (from where it is) if it was to be moved in the direction cued. In Figure 1.1 is an example for the horizontal task (as cued by the triangle); the target appeared in the lower left corner, and so if it was to be spatially transformed according to the horizontal task, it would end up in the bottom right corner. To make a response participants press one of four keys on the numeric part of the keyboard (1, 2, 4, and 5); where each corner of the frame has a key assigned to it (1-bottom left; 2-bottom-right; 4-upper left; and 5-upper right). In this example participants would have to press key-2 as the correct answer; once a response is made, after a short while, another cue appears followed by another target, and so on. Examples of other BI paradigms are presented in Figure 1.1.

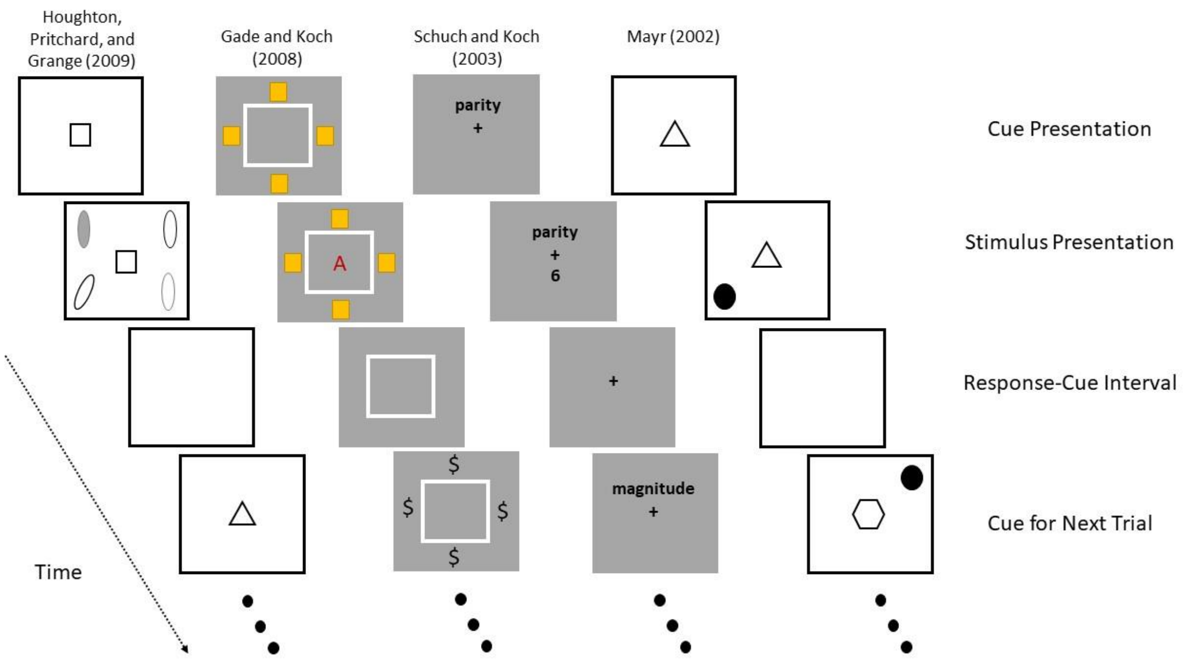

Figure 1.1 Examples of task-switching paradigms used in this thesis.

Mayr and Keele (2000) posited that carry-over effects defined by Allport et al. (1994) were important to be investigated further to understand better inhibition's role in task-switching and SCs. Stronger activation of a relevant task-set among alternative task-sets and decaying task-set's activation, is unlikely to be sufficient to deal with interference caused by competing activations; instead, it was proposed that inhibition of temporarily not relevant task-set is employed to aid selection of a new task-set. That is, as a conscious shift from a given task to another task is made, inhibition was hypothesised to be the mechanism that helps to disengage from the no-longer relevant task, so a new task can be successfully activated. As such, Mayr and Keele predicted that returning to a recently inhibited task, should be slower and less accurate, compared to performing a task that was not recently inhibited. If inhibition was present in task-switching, it was argued that it would be evidence for cognitive control influencing endogenous control of goal-shifting. The BI paradigm is designed to test that hypothesis.

In the BI paradigm participants switch sequentially between three tasks (i.e., A, B, and C task; letters A, B, C being arbitrary labels for tasks); sometimes they come across a task that they performed recently (i.e., referred to as ABA; a task on a current trial is repeated from n–2 trial, e.g., horizontal-vertical-*horizontal*), or a task that was not performed recently (i.e., CBA; a task on a current trial is NOT repeated from n–2 trial, diagonal-vertical-*horizontal*). Typically, a task on every current trial is compared via a computer code to the task on n–2 trial, and recorded accordingly as ABA or CBA. It is a consistent finding that, just as Mayr and Keele (2000) predicted, performance for ABA sequences is on average slower and less accurate than performance on CBA sequences; this effect is known as Backward Inhibition (BI), but is also referred to as the lag–2 repetition cost, or the n–2 repetition cost. For this thesis, the BI effect will be referred to as the *n–2 task repetition cost,* to emphasise that it is a task that is being repeated across the trials.

The n–2 task repetition cost is believed to reflect cognitive inhibition by the following logic. At the point of switching away from a task, that task is inhibited so its activation does not interfere with activation of the next task; however, if a task is required soon after it was inhibited (i.e., in ABA sequences), it is less accessible, and hence performing it is slower and less accurate. This is compared to performing a task that was not inhibited recently (i.e., in CBA sequences), on which performance is faster and more accurate. If performance on task-switching was driven by activation mechanisms only (e.g., Just, 1992), it should be faster for ABA compared to CBA sequences; this is because, decaying but remaining activation of task A that is being reactivated in ABA sequence should facilitate performance making it faster and more accurate. As such, the n–2 task repetition cost is an important factor explaining goal-driven and context-appropriate action in task-switching for a long time (Kiesel et al., 2010; Koch et al., 2010), and it has become a signature of cognitive inhibition. The current thesis will identify gaps in research around the n–2 task repetition cost and address some of those gaps. Before the n–2 task repetition cost is discussed further, inhibition in task-switching will be explored first in the next few paragraphs.

N–2 task Repetition Cost and Inhibition

In terms of the n–2 task repetition cost, the original study by Mayr and Keele (2000) has been interpreted as evidence for inhibition driving the cost, but it was not certain which type of inhibition was at play. The authors proposed that lateral or self-inhibition were the likely candidates, but they leaned towards the former one. This is because, in their Experiment 2, the authors showed that the n–2 task repetition cost was larger when a distractor on n–1 trial contained features of tasks from n–2 and n–0 trial; by the self-inhibition theory, processing on n–1 trial should not affect n–2 task repetition performance, but by lateral inhibition theory, since the distractor on n–1 shared features with the n–2 task, it was likely to be automatically inhibited making performing the n–2 task less efficient. As such, the authors make a clear distinction between lateral and self-inhibition. However, they also posit that it is the abandoned task that is being inhibited; that is, that inhibition is thought to suppress abstract task representations rather than action codes, and that such inhibition requires top-down control (Experiment 3), all of which imply self-inhibition, rather than lateral inhibition.

In terms of what was being inhibited, Mayr and Keele (2000) speculated that task-set representations (i.e., higher representations) were unlikely to be affected; this is because the n–2 task repetition cost was not affected by preparation, as such the n–2 task repetition cost was argued not to be a reflection of cognitive control mechanism as such, but one that aids controlled behaviour; that is, it subserves high-level control but is a low-level process. Specifically, it appears that inhibition cannot be penetrated by other processes—such as longer preparation time or pre-planned sequences of task-switches—or be overcome at will. And, even though inhibitory mechanism seems to impair performance (and hence may not seem helpful), Mayr and Keele argued that functionally this mechanism is advantageous—as it allows disengagement from a recently-performed task—but task-switching paradigms demand unnatural processing of stimuli; that is, in real life it is unlikely that we have to sequentially go back and forth between stimuli in a rapid succession. As such, the n–2 task repetition cost was suggested to be triggered automatically by task-set competition triggered at the point of disengagement from a given task-set, and it cannot be overcome, instead it must decay gradually.

However, despite inhibition being assumed to be triggered automatically, the n–2 task repetition cost was only present in a version of the BI paradigm with informative cues compared to uninformative cues (Mayr and Keele, 2000). Informative cues are considered to directly translate to the task (i.e., “Colour” word for the colour task) informing participants which task is relevant on a given trial so an appropriate task-set can be selected, whereas if the cues are not informative, they do not inform the participant about the task in any way (i.e., “xxxxxx” for the colour task). With the task being to locate a deviant stimulus among four stimuli, in the condition with no informative cues, participants have to locate the deviant stimulus in the bottom-up manner (i.e., the task is likely to be performed without a task-set being selected), and in the condition with informative cues, the cues provide information based on which the task-set is updated in a top-down fashion, and applied to the stimulus. Assuming that informative cues lead to a task being performed through top-down processes, it can be argued that the n–2 task repetition cost is a result of cognitive control processing, and when a task is performed in a bottom-up manner, performance is more automatic.

The studies that followed the original work by Mayr and Keele (2000) offer consistent evidence for the presence of the n–2 task repetition cost (Kiesel et al., 2010; Koch et al., 2010); there is also rather consistent evidence for lateral inhibition driving the n–2 task repetition cost (Arbuthnott, 2005, 2008b; Mayr & Keele, 2000; Schuch & Koch, 2003; Sinai, Goffaux, & Phillips, 2007). However, there is some research based on which reactive (i.e., self-inhibition; Grange et al., 2013; Grzyb & Hübner, 2012; Houghton & Tipper, 1994, 1996; Koch, 2008; MacKay, 1986), and proactive inhibition (Costa & Friedrich, 2012; Kuhns et al., 2007; Lien et al., 2006), can be linked to the n–2 task repetition cost too. Nevertheless, these results have to be taken with caution because it seems that there are inconsistencies in terms of how inhibition is argued to operate; instead, by looking at actual paradigms, results, and the way researches make their arguments, commonalities can be identified.

Lateral inhibition. One type of evidence for lateral inhibition as the source of the n–2 task repetition cost comes from a study that incorporated Go/ No-Go procedure into the BI paradigm (Schuch & Koch, 2003); it was found that if a response was not required on the n–1 trial (i.e., the No-Go condition)—regardless of preparation time—there was no n–2 task repetition cost observed, compared to when a response was required on n–1 trial. The n–1 trial is a task that is different from n–2 and n–0 tasks, therefore if a task-set was being inhibited performing n–1 task should not affect n–2 task performance, unless as a given response is made other responses inhibited, as it is predicted by lateral inhibition. Furthermore, by adopting a double-press technique—a method used to distinguish between response selection and response execution—it was shown that the n–2 task repetition cost was absent following double-press No-Go trials (i.e., no response selection; two possible responses executed) compared to following Go trials (i.e., response selection and execution). Using the double-press method was important because it made it possible to distinguish between effects of selection and execution of response on behaviour, and examine which is targeted by inhibition; results point to the importance of response selection (but not execution) being targeted by inhibition and driving the n–2 task repetition cost. Schuch and Koch proposed that the S-R mappings are targeted by inhibition; this is because—as it often is a case in task-switching experiments—allowable responses overlap between different tasks (Gade & Koch, 2007b), and after every response selection, inhibition is applied to aid recoding of S-R mapping (Meiran, 2000). That is, as a given S-R mapping is selected, the alternative/ competing S-R mappings are inhibited to increase the strength of the relevant S-R mapping; this means that accessing previously inhibited S-R is impaired, making responses less efficient. It seems that it is enough to select (i.e., activate) without executing a response, for the S-R to be inhibited, even if it is for a task different to the one on n–2 trial. This also shows that tasks share S-R mappings which makes them sensitive to lateral inhibition.

Another study investigated inhibition associated with the n–2 task repetition cost by looking at whether the cost is affected by task difficulty, and by examining the timing of the n–2 task repetition cost, as assessed with EEG (Sinai et al., 2007). Task difficulty was manipulated through introducing high and low interference environments, switching between dominant (i.e., over-learnt, easy/ semantic) and nondominant (i.e., difficult/ episodic) tasks. Switching from a dominant to a nondominant task (semantic-episodic-semantic; SES sequence) was hypothesised to reflect high interference environment and require more inhibition—as characterised by larger n–2 task repetition cost—compared to switching from a nondominant to dominant task (episodic-semantic-episodic, ESE sequence) which reflected low interference environment. In terms of the timing of the n–2 task repetition cost, of interest were ERPs (event-related potentials) at cue presentation and response execution; this was to determine whether inhibition affects preparatory or response related processes.

Sinai and colleagues (2007) found that the n–2 task repetition was larger in high interference compared low interference condition, as predicted. Also, the timing of the n–2 task repetition cost differed depending on whether it was high or low interference condition; that is, in the low interference condition (i.e., ESE), ERP effects associated with preparatory stage were observed, but in the high interference condition (i.e., SES) the ERP effects were linked to response stage. Specifically, for low interference condition, processes associated with n–2 task repetition cost (or rather processes at n-0 trial in ABA compared to CBA sequence; that is when reactivating previously inhibited task happens) were linked to increased attentional resources (reduced N2 and increased P3). The authors point out that this may be due to episodic tasks requiring stronger orientation, as they do not linger as much in WM compared to semantic tasks. For high interference condition, results indicate that semantic task (thought to be dominant) require stronger reactivation, as reflected in increased LRP-r—lateralized readiness potential for response execution (Falkenstein, Yordanova, & Kolev, 2006); this was suggested to be due to S-R mapping being more inhibited for semantic tasks.

However, there is one uncertainty that Sinai and colleagues (2007) introduce; that is, larger n–2 task repetition cost in high interference compared low interference condition was interpreted as evidence for the n–2 task repetition cost being a result of a reactive type of inhibition (p.605), but overall, they refer to lateral and reactive inhibition as being the source of the n–2 task repetition cost. As illustrated in earlier paragraphs, lateral and reactive inhibition are thought to be distinct mechanisms (e.g., Mayr & Keele, 2000; Schuch & Koch, 2003)—the lateral being a neuronal, automatic mechanism, and reactive inhibition being a mechanism dealing with interference. Because the authors manipulated the level of interference during task-switching—which they did successfully—it would mean that the reactive inhibition is more likely to explain their results, but in their abstract, they point to lateral inhibition as being at play. It can be argued that without using a double-press method, it is difficult to determine which type of inhibition was driving effects seen in Sinai and colleagues' study; The authors indeed mention that (p.597) it is uncertain if lateral inhibition supresses all competitors uniformly, or whether only the competitors which are most likely to lead to interference are inhibited. But it can be argued that reactive inhibition implies that for a competitor to be inhibited reactively, it has to be processed semantically first, at least in research of inhibition in language (D. W. Green, 1998) where inhibition is reactive and proportional to the level of activation. Tipper and colleagues make a clear distinction between lateral and reactive inhibition too (Miyake et al., 2000; Tipper, 2001; Tipper et al., 2000); whereby if a stimulus/ distractor evokes a very strong response, lateral inhibition is not enough to resolve response conflicts, so reactively self-inhibition is applied. It is important to mention at this point that researchers have to be careful making distinctions between different types of inhibition, as inconsistencies may lead to inaccurate conclusions.

Overall, it seems that lateral inhibition is a strong candidate driving the n–2 task repetition cost. For example, Koch and colleagues (2010, p.7) in their review argue that the n–2 task repetition cost is not driven by a reactive form of inhibition, such as self-inhibition of responses, because: a) n–2 task repetition costs have been shown to be present after immediate repetitions, b) n–2 task repetition cost is sensitive to characteristics on n–1 trial (Mayr & Keele, 2000; Schuch & Koch, 2003), c) there are no costs associated with immediate repetitions. Also, by lateral inhibition theory, assuming that when one task is activated, other tasks are being inhibited—where “magnitude of inhibition is determined by each node’s level of activation and the strength of its inhibitory connections to its associates” (Arbuthnott, 2008a, p.99)—in paradigms with unequal difficulty, the asymmetric costs are likely to be due to different levels of activation and inhibition between dominant and nondominant tasks, whereby dominant tasks trigger more activation and inhibition. And lastly, evidence that n–2 task repetition costs are not removed with foreknowledge (M. Hübner, Dreisbach, Haider, & Kluwe, 2003; Mayr & Keele, 2000) would also suggest that n–2 task repetition costs are driven by an automatic type of mechanism like lateral inhibition.

Reactive inhibition. As argued in the previous section, the lateral inhibition driving the n–2 task repetition cost argument has some considerable support. However, there is some evidence to suggest that lateral inhibition on its own is not enough to produce the n–2 task repetition cost (Grange et al., 2013; Sexton & Cooper, 2017). Grange and colleagues make an interesting point whereby in lateral inhibition—assuming that whilst a given task is performed, its competitors become inhibited—repeating A task on the ABA sequence should lead to n–2 task repetition *benefits* and not n–2 task repetition costs. This is because in ABA sequence, task A is being inhibited once, but for CBA sequence, task A is inhibited twice—once when task C is performed, and second time when task B is performed (Figure 1.2); as such, this would result in less inhibition on ABA versus CBA sequence, which Grange and colleagues’ model could not account for. Instead, they propose that it is self-inhibition—that is, immediate application of inhibition on the used task set—that the n–2 task repetition cost is produced by. Also, they find that if behaviourally there is no inhibition detected, it does not mean that inhibition is not present; and if no inhibition is added to the model, n–2 task repetition benefits are observed instead of n–2 task repetition costs.

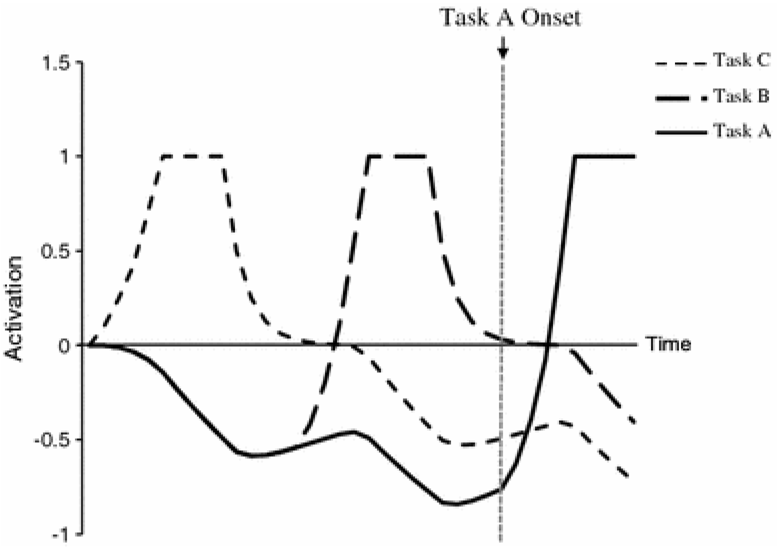

Figure 1.2 A visualisation of a simulation of lateral-inhibition dynamics across a CBA task-switching sequence from Grange, Juvina, and Houghton (2013); "…at time of task A onset (vertical grey dotted line), task A is the least active due to it being laterally inhibited at n-2 and n-1." (p. 219).

Moreover, in a study where participants had an opportunity to voluntarily choose the sequence of switched tasks (Lien & Ruthruff, 2008)—hence they did not need cues—it was found that participants avoided returning to a task from n–2 trial, but even then the n–2 task repetition cost was present. In Experiment 2 participants were allowed to repeat tasks; performance was still characterised by a preference to not return to the n–2 task, participants preferred to repeat rather than switch, and the n–2 task repetition cost was still present. This was taken as evidence that it was the task-set that was being inhibited, and that the n–2 task repetition cost is not an artefact of processing explicit cues. As such, it seems that once inhibited task-set was less likely to be selected again. This fits in with findings from the SC research. Under the hypothesis that inhibition is applied strategically, and that responses can be inhibited once executed—especially, in tasks that use bivalent-incongruent responses—in one study it was shown that the strategy participants use during task-switching seems to depend on the probability of occurrence of responses that lead to response perseverance, and as such is adapted accordingly (Grzyb & Hübner, 2013). That is, if tasks which can be classed as having high and low probability of response perseverance can be distinguished easily, a trial-by-trial strategy is applied; whereas, if high and low risk responses cannot be distinguished, the basic level of inhibition is increased for all responses.

Proactive inhibition. There are many paradigms that can be used to assess proactive inhibition (Aron, 2007); for example, the level of certainty of whether participants will perform a switch or the repetition on the next trial (Czernochowski, 2015; Yu et al., 2017), or the CSI can be manipulated (Arbula et al., 2016). The manipulation of uncertainty involves varying the extent to which participants can be certain on whether the next trial will be a switch or a repeat; that is, the condition with the highest certainty is meant to allow for proactive inhibition (i.e., participants can inhibit irrelevant and prepare relevant task-set), and in the condition with the lowest certainty, participants do not know whether a task is a switch or a repeat until they see the target (i.e., participants cannot inhibit any task because they do not know which one will become relevant, hence they cannot prepare). As such, uncertainty can be manipulated with CSI, where long CSI is meant to allow for task preparation including proactive inhibition, whereas the short CSI limits if and how much preparation can occur prior to the presentation of the stimulus. But as it was mentioned earlier, foreknowledge does not remove SC (Sohn & Carlson, 2000), and other studies seem to support the view that inhibitory effects in task-switching do not appear to be overcome with pre-knowledge (M. Hübner et al., 2003; Mayr & Keele, 2000); this would suggest that the effects ascribed to inhibition in task-switching are unlikely to be driven by proactive inhibition.

However, again, researchers have to be very clear about what they mean when they mention proactive inhibition. Proactive inhibition in task-switching is unlikely to operate by consciously inhibiting information in advance. Findings from Mayr and Keele's (2000) study have been interpreted by some as evidence for proactive inhibition (e.g., Kuhns et al., 2007), proactive in a sense of information being proactively inhibited upon disengagement, to reduce interference; this can be misleading. Proactive inhibition is related more to an early selection of information in anticipation of an event, which biases attention and other relevant processes to achieve a given goal; that is, proactive inhibition as a mechanism to maintain goal-directed preparedness (Braver, 2012; Braver et al., 2009). In task-switching paradigms adapted to include foreknowledge—even though inhibitory costs are not removed—participants clearly perform tasks well, they maintain high accuracy and speed, and they maintain their goal which is switching between or repeating tasks. As such, as some information is pre-selected, hence its activation is strengthened, other information may be inhibited; that is, proactive inhibition/ control is a mechanism by which lower-level processes are biased so information is manipulated in a certain way—mainly through directing attention to a task at hand—and as such inhibition may be indispensable part of task-switching, especially if interference or conflict is present (Elchlepp et al., 2016).

Combining inhibition types. The alternative is that lateral and reactive (i.e., self-inhibition) inhibition are not mutually exclusive, and since inhibition can be triggered by different factors, it can be flexible (Costa & Friedrich, 2012; Sdoia & Ferlazzo, 2008). For example, in an eye-tracking experiment of attention, using a version of an intermodal-preferential-looking (IPL) task, toddlers switched between semantic categories (Chow, Davies, Fuentes, & Plunkett, 2016); the testing phase involved toddlers looking at two pictures (i.e., one target-table, one distractor-flower) paired with a spoken word (“look” or “wow”), and it was of interest which picture they looked preferentially at. The presentation of the target was preceded by a prime which was semantically related (i.e., chair) or unrelated (i.e., coat) paired with its audio label, and in between the prime and the target was an intervening phase—a picture and its audio label, or a checkerboard and a sine wave tone. Results showed that switching attention from prime to an intervening word (as opposed to a tone) inhibited the prime; that is, in the tone-intervening condition (compared to the word-intervening condition), toddlers showed more preferential looking at a target after seeing a related prime than the unrelated prime. The authors interpreted their results as evidence for semantic backward inhibition (i.e., the n–2 task repetition cost); that is, as attention was switched from a related prime to an intervening word, activation of the intervening word inhibited the prime such that it did not facilitate performance. It was posited that since there were differential effects of intervening phase on primes—an effect similar to those seen in Go/ No-Go experiments (Schuch & Koch, 2003)—it is unlikely that self-inhibition only was at play; instead the authors argued that a combination of lateral and self-inhibition was more likely.

Inhibition as a “by-product”. There is another way of thinking about inhibitory effects in task-switching experiments; inhibition being a by-product of other cognitive processes (Gade & Koch, 2005; Houghton, Pritchard, & Grange, 2009; Houghton & Tipper, 1994; M. Hübner et al., 2003; Philipp & Koch, 2006; D. W. Schneider & Verbruggen, 2008; Schuch & Koch, 2003; Tipper, 2001). Specifically, as we focus on a given stimulus or a task—or as a target is selected—information outside of our focus is being inhibited automatically (akin to lateral inhibition), this is to ensure that the relevant activations are heightened. This type of inhibition can be applied at a physical and semantic level, and its strength can be adjusted depending on the level of interference (akin to self-inhibition). As such, it makes sense that in many experiments inhibition cannot be prevented or overcome (Dreisbach, Haider, & Kluwe, 2002; M. Hübner et al., 2003; Mayr & Keele, 2000). This fits in with the view that controlled behaviour can be achieved by biasing information processing (Elchlepp et al., 2016); that is, that cognitive control influences low-level mechanisms by increasing baseline activity relevant to a task, which results in response inhibition.

Goals of the PhD

The goal of this PhD was to investigate the n–2 task repetition cost further and offer novel insight into this effect. The reviewed literature clearly shows that the n–2 task repetition cost is a well-researched and established effect, and that it is likely to reflect inhibitory processing. The importance of the n–2 task repetition cost is significant because it has become a signature of cognitive inhibition in task-switching, as it has been used as such in individual (Pettigrew & Martin, 2015; Whitmer & Banich, 2007, 2012) and group differences research (Fales, Vanek, & Knowlton, 2006; Foti et al., 2015; Greenberg, Reiner, & Meiran, 2013; Lawo, Philipp, Schuch, & Koch, 2012; Mayr, 2001; Mayr, Diedrichsen, Ivry, & Keele, 2006; Moritz, Hübner, & Kluwe, 2004; Philipp & Koch, 2009; Prior, 2012; Whitmer

& Banich, 2012; Yiu-kwan, 2008). However, academics do not seem to agree entirely on what kind of inhibition, and whether it is actually inhibition, that drives the n–2 task repetition cost; and despite a lot of interest in the n–2 task repetition cost, there are a few gaps in research that needed addressing.

The first important issue to visit is the reliability of the n–2 task repetition cost. This effect has been used as a measure of individual differences; but yet, there was no study reported—before this PhD started—that looked at whether the n–2 task repetition cost is a reliable measure. This was problematic because in psychology to make meaningful conclusions for individual differences research, we have to have confidence that tests we use are reliably measuring cognitive processes of interests. As such, the first goal of the current thesis was to establish the reliability of the n–2 task repetition cost, which is addressed in Chapter 2. It can be reported in advance that the results of this study show that the n–2 task repetition cost is not a reliable measure, which questions the suitability of this effect as a measure of inhibitory control, of which consequences are discussed in detail in the respective chapter.

Another issue around the n–2 task repetition cost was to do with whether it actually measures inhibition. Indeed, this effect has been very resistant to explanations other than inhibitory ones, but there is a lot of evidence that would suggest that some of the factors that are known to modulate the n–2 task repetition cost and performance in task-switching in general, are important to revisit. For example, it seems that priming and episodic retrieval mechanisms are closely linked to the efficiency with which task-switching is performed and the extent to which inhibitory effects are present. Therefore, as a second goal of this PhD, the aim was to further explore whether and to what degree the n–2 task repetition cost is modulated by episodic retrieval (Hommel, 2004; Neill, 1997). Moreover, since in Chapter 2 the n–2 task repetition cost was shown not to be reliable, it was also of interest to examine its reliability controlling for potentially modulating effects. By gaining a better understanding of the effects of this non-inhibitory mechanism on the n–2 task repetition cost, an improved and well-informed interpretation of it can be developed. This topic is addressed in Chapter 3, where in a series of three studies, the n–2 task repetition cost is shown to be modulated to a large extent by episodic retrieval. Furthermore, it is also demonstrated that controlling for episodic retrieval, the reliability of the n–2 task repetition cost does not improve. These findings further question the extent to which the n–2 task repetition cost measures inhibition, and its usefulness as such.

The next area that was considered to be under-researched was concerned with predictors of the n–2 task repetition cost. That is, there are a lot of studies reported that looked at roles of cues, stimuli, response, RCI, or CSI, but there is no indication on what can predict the n–2 task repetition cost. Inhibitory effects outside of task-switching research (Conway & Engle, 1994; Lavie & Fox, 2000) have been shown to be dependent on WM and perceptual resources; as such, if the n–2 task repetition cost reflects inhibition, it was likely that it can be explained by individual differences related to cognitive and perceptual capacities. The third aim of this thesis was to look at whether the n–2 task repetition cost can be explained by individual differences in WM capacity and attentional/ perceptual resources. Chapter 4 looks at the relationship between the n–2 task repetition cost and WM, and whether it can be modulated by WM manipulations; in a series of three studies it was shown that the n–2 task repetition cost does not seem to be linked to WM or be modulated by it, regardless of whether it is controlled for episodic retrieval or not. Chapter 5 investigates whether and to what extent the n–2 task repetition cost is explained by individual differences in distractibility—as measured with Cognitive Failures questionnaire (CFQ, Broadbent, Cooper, FitzGerald, & Parkes, 1982)—and whether it is modulated by perceptual load. The scores on CFQ did not predict the n–2 task repetition cost (even when controlled for episodic retrieval); the results for perceptual load modulation of the n–2 task repetition cost are rather inconclusive, but in general a trend of the n–2 task repetition being reduced under conditions of exhausted attentional/ perceptual resources the n–2 task repetition was observed.

The final goal of this PhD involved looking at methods used to analyse data in task-switching experiments, and what we can learn about the n–2 task repetition cost using alternative methods. That is, typically in task-switching research data are analysed through looking at central tendencies such as mean reaction times and mean accuracy. However, this method can be considered as limited, and a more in depth understanding of performance on task-switching can be obtained using techniques such as, the whole distribution of reaction times (Heathcote, Popiel, & Mewhort, 1991) and computational modelling (Ratcliff & McKoon, 2008). Therefore, as the last goal of the current PhD, using data from four studies (three already collected and one collected specifically for computational modelling), in Chapter 6, n–2 task repetition costs were analysed with ex-Gaussian and computational modelling. Overall, the results from Chapter 6, further disprove the n–2 task repetition cost as an effect reflecting inhibitory control, and support the view that the n–2 task repetition cost is largely due to non-inhibitory, memory mechanisms.

It is important to mention that each chapter in this thesis is written in the format of a paper, and at the end of the thesis a General Discussion chapter summarises the key messages from the experimental and computational chapters. Therefore, inevitably there will be some degree of repetition throughout the chapters, but this was necessary so that each chapter could stand on its own as a paper submission.

Chapter 2 Inhibition in Task-Switching: The Reliability of the N–2 Task Repetition Cost[1][2]

Abstract

The n–2 task repetition cost seen in task-switching is an effect of slower response times performing a recently completed task (e.g. an ABA sequence) compared to performing a task that was not recently completed (e.g. a CBA sequence). This cost is thought to reflect cognitive inhibition and has been well replicated (Koch, Gade, Schuch, & Philipp, 2010). As such, the n–2 task repetition cost has started to be used as a measure of individual differences in inhibitory control (e.g. Whitmer & Banich, 2007); however, the reliability of this measure has not been investigated in a systematic manner. The current study addressed this important issue. Seventy-two participants performed three task-switching paradigms; participants were also assessed on rumination traits and processing speed (PS)—measures of individual differences potentially modulating the n–2 task repetition cost. Significant n–2 task repetition costs for each paradigm were found. However, split-half reliability tests revealed that this cost was not reliable at the individual-difference level. Neither rumination tendencies nor PS predicted this cost. It was concluded that the n–2 task repetition cost is not reliable as a measure of individual differences in inhibitory control.

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[1] This chapter has been published as Kowalczyk and Grange (2016) and is reproduced with permission of the copyright holder. Thanks to Frederick Verbruggen, Miriam Gade, Cai, Longman, and an anonymous reviewer for comments and suggestions.

[2] Kowalczyk, A. W., & Grange, J. A. (2016). Inhibition in task switching: The reliability of the n−2 repetition cost. *Quarterly Journal of Experimental Psychology*. Retrieved from http://dx.doi.org/10.1080/17470218.2016.1239750

Introduction

Cognitive control

Task-switching paradigms are used to investigate cognitive control; specifically, to assess humans' ability to exert behaviours that are context-specific, typically in an environment which affords more than one action. Cognitive control is a mental construct believed to coordinate abilities to select, process, and interpret internal and external inputs translating them into controlled actions and thoughts. For example, we can focus on one task (e.g., typing a manuscript), but if needed, we can temporarily disengage from that activity to focus on a different task (e.g., reading previously prepared notes; answering a ringing phone), only to return to the original task later. Efficient cognitive control facilitates the ability to switch our attention between different tasks, and helps us to maintain our focus on a given activity in the face of distractors when necessary.

When cognitive control works well, carrying out goal-oriented behaviours may seem effortless, but when cognitive control fails, its importance becomes apparent. Dysfunctional cognitive control may manifest as a difficulty to switch attention between tasks (e.g., persevering with a no longer relevant task), an inability to focus exclusively on one task (e.g., being distracted by irrelevant stimuli), or stimuli-driven behaviour (e.g., flicking a light switch regardless when exposed to it; Archibald et al., 2001; Lhermitte, 1983).

For a seemingly effortless goal-oriented behaviour to be effective, we must be able to distinguish between relevant and irrelevant information, as well as to attend to relevant and ignore irrelevant information. The cognitive control processes coordinating goal-oriented behaviour and modifying it when necessary are not fully understood; however, conceptually these processes are speculated to be part of a dynamic system (Goschke, 2000), which adapts behaviour in a moment-to-moment manner, by activating relevant- and inhibiting irrelevant-dimensions of tasks (Mayr & Keele, 2000).

Backward Inhibition Paradigm

Task-switching paradigms are suitable for investigating cognitive control because switching between tasks requires attending to a relevant goal, suppressing irrelevant information, and executing an appropriate action for every task; abilities thought to be regulated by cognitive control (Grange & Houghton, 2014; Kiesel et al., 2010; Monsell, 2003). In the laboratory setting, participants are presented with computerised task-switching paradigms, which typically involve rapid judgments to stimuli shown sequentially on a computer screen. The paradigm of interest to the current study (and the whole of the thesis) is the Backward Inhibition (BI) paradigm; which is believed to be particularly suitable for testing cognitive control. In this paradigm, participants switch between three tasks; for example, participants may be presented with numerical stimuli and be asked to perform tasks such as, judging whether the number is odd/even (a parity judgment), lower/higher than 5 (a magnitude judgment), or whether it is red/blue (a colour judgment). To assess task-switching performance, reaction times (RTs) and accuracy (%) are recorded and analysed. Based on that data, inferences about candidate cognitive processes associated with task-switching are made (see Grange & Houghton, 2014; Kiesel et al., 2010; and Vandierendonck, Liefooghe, & Verbruggen, 2010, for reviews).

N–2 Task Repetition Cost

Typically, in BI paradigms all three tasks are presented in such fashion that no immediate repetition of a one task is allowed (e.g., parity-parity-magnitude), meaning that participants always switch from one task to another. This means that two types of sequences of stimuli presentation occur: ABA (e.g., parity-size-parity) and CBA (e.g., magnitude-size-parity) sequences. Mayr and Keele (2000) created the BI paradigm to test a hypothesis about cognitive control in task-switching; they speculated that cognitive inhibition facilitates task-switching by temporarily inhibiting abandoned task's mental representation, when it momentarily becomes irrelevant. That is, when a given task is completed and attention shifts away from it (A→BA), that task becomes inhibited so it does not interfere or interferes less with the next task (ABA). However, inhibiting comes with a cost, because once applied inhibition cannot be overcome, it decays over time reducing inhibited task's accessibility; if a task is required soon after it was inhibited (AB→A), it should be "harder" to re-activate. As such, it was speculated that performing a task that was abandoned recently (hence inhibited) should be impaired; that is, responding to a previously inhibited task (ABA) should be characterised by slower and less accurate RTs compared to a task that was not inhibited recently (CBA). The difference in performance between ABA vs. CBA sequences is known as the n–2 task repetition cost, and this cost is thought to be driven by inhibition (Mayr & Keele, 2000); hence, the n–2 task repetition cost is argued to reflect *cognitive inhibition*.

Group Differences Research

Researchers have explored characteristics of the n–2 task repetition cost extensively (e.g., Gade et al., 2014); it has been replicated many times (for reviews Gade, Schuch, Druey, & Koch, 2014; Kiesel et al., 2010), and it has been resistant to non-inhibitory accounts (Koch et al., 2010; Mayr, 2007). Consequently, this effect has become a signature of cognitive inhibition, and has been used as such in group differences research, examples of which are provided below.

Looking at age differences, younger (average 20-24 years old) and older adults (average 66-71years old) (Lawo, Philipp, Schuch, & Koch, 2012; Mayr, 2001; for alternative results see Pettigrew & Martin, 2015) seem to have comparable inhibition when assessed with the n–2 task repetition cost. Parkinson's patients have larger inhibition (as assessed with larger n–2 task repetition cost) compared to healthy control group (Fales et al., 2006), which was interpreted as being likely due to striatal dysfunction possibly causing impaired interplay in the prefrontal cortex, associated with task-switching. In major-depressive disorder (MDD) patients' difficulty to switch between tasks was claimed to be due to inability to activate a new task-set rather than an inability to inhibit an irrelevant task-set; as depressed participants showed larger switch cost and intact BI (Whitmer & Banich, 2012). Pathological gamblers' inability to resist to gamble was argued to be due to a combination of stronger activations and weaker inhibition (Yiu-kwan, 2008). In another study, it was found that task-set selection and inhibition seem neurocognitively dissociable, with right-frontal parts of the brain being linked to inhibition, and the left-frontal brain linked to set-activation/ retrieval (Mayr et al., 2006). Children's with Williams' syndrome difficulty to process visuospatial information was proposed to be due to navigational strategy not being modulated via inhibition, making it inflexible (Foti et al., 2015). People with obsessive-compulsive disorder (OCD)—characterised by intrusive and repetitive thoughts difficult to disengage from—were showed to have spared inhibitory control, as their n–2 task repetition costs were similar to the control group (Moritz et al., 2004). Inhibition in language-switching was demonstrated not to target specific task-set aspects (e.g., cue, stimulus-response mapping) (Philipp & Koch, 2009), and inhibition in bilingual people was observed to be stronger than in monolingual people (Prior, 2012). And mindfulness was posited to improve inhibition, as indicated by larger inhibition after inducing mindfulness (Greenberg et al., 2013).

Individual Differences

As shown in the previous paragraph the n–2 task repetition cost has become a very popular tool to assess cognitive inhibition; now, studies that used the n–2 task repetition cost to investigate individual differences in inhibitory control will be introduced. These studies were an inspiration to conduct the current study. That is, despite the n–2 task repetition cost being robust, when it comes to interpreting results from individual differences studies, a potential issue was identified; the reliability of the n–2 task repetition cost was not investigated in a systematic manner—until the current study was conducted.

Whitmer and Banich (2007) looked at whether depressive rumination trait—recurrent thoughts about emotions and feelings appearing in person’s mind (Nolen-Hoeksema, 1991)—is linked to performance on task-switching. Assuming that people exhibiting depressive rumination are characterised by “attentional inflexibility”—a cognitive inflexibility, such as persevering with a given thought despite of negative feedback (Davis & Nolen-Hoeksema, 2000)—the authors speculated that this attentional inflexibility may be due to an inhibitory deficit, or deficit in task-switching ability. That is, it was hypothesised that people ruminate because they cannot block/ deactivate ruminative thoughts, or switch from one thought to another. Whitmer and Banich found a negative correlation between the n–2 task repetition cost and the tendency to engage in depressive rumination (as assessed with Ruminative Response Scale, RRS, Nolen-Hoeksema & Morrow, 1991). These findings were interpreted as evidence that depressive ruminators' attentional inflexibility—difficulty to disengage from unwanted thoughts—is due to impaired cognitive inhibition; that is, those who scored higher on RRS showed less inhibition in task-switching (i.e., smaller the n–2 task repetition cost).

Another study set out to distinguish—using brain imaging—between brain areas associated with activation vs. inhibition (Whitmer & Banich, 2012). A novel paradigm was used to investigate the relationship between behavioural differences in cognitive inhibition and brain activations during task-switching. Participants were assessed on cognitive inhibition via the n–2 task repetition cost—with larger n–2 task repetition cost indicating better inhibition—and then they were tested in the fMRI (functional magnetic resonance imaging) scanner performing task-switching between two tasks vs. repeating the same task. Isolating inhibition related brain activity is considered challenging, because existing methods do not allow for a direct measure of inhibition; despite the n–2 task repetition cost being a robust measure, the ABA vs. CBA comparison is thought to reflect the effect of reactivation of a previously inhibited task rather than inhibition, which takes place two trials prior (Dreher & Berman, 2002; Sinai et al., 2007). Moreover, inhibition is thought to be equally exerted on a task before the task B in ABA and CBA sequences, which means that at brain activations level, no differences would be expected. Therefore, for fMRI session the authors used two tasks (i.e., AB) as opposed to three (i.e., ABA) to distinguish between disengagement from a task (which is thought to be affected by inhibition), and performance on trials where no inhibition of task-set is assumed to take place (i.e., AA). Next, changes in brain activations associated with AA task sequence were hypothesised to reflect task-set activation, whereas activations changes linked to AB sequence were hypothesised to include inhibition; this is because, the authors assumed that switching from a task A to a task B requires inhibition of task A, whereas repeating a given task does not. As such, the AB task-switching sequence was assumed to include inhibitory processing and the AA sequence was assumed to show non-inhibitory processing. Next, individual differences in the n–2 task repetition cost, were regressed against brain activations associated with activity during task-switching (i.e., AB task-switching), controlling for non-inhibitory processing (i.e., during AA sequence). Looking at n–2 task repetition costs' relationship with brain activations changes recorded with fMRI, it was found that "stronger inhibition" (larger n–2 task repetition costs) was associated with increased bilateral activation of the putamen (part of the basal ganglia; "gatekeeper of working memory", e.g., McNab & Klingberg, 2007), and the supplementary motor/ premotor cortex (associated with action rules, stimulus-response mappings, response selection, e.g., Shibasaki et al., 1993), which was interpreted as evidence for these areas to be involved in task-set inhibition.

A more recent study (Pettigrew & Martin, 2015) looked at cognitive control and interference resolution of conflict in task-switching, and individual differences in working memory capacity (WMC). The n–2 task repetition cost was used as one of the measures of task-switching; specifically, inhibition in task-switching. Pettigrew and Martin speculated that, rather than the n–2 task repetition cost reflecting cognitive inhibition, they argued that it reflects an automatic, low-level inhibition (i.e., lateral inhibition). As such, it was predicted that the n–2 task repetition cost should not be explained by WMC, and this is what they found. Specifically, the n–2 task repetition cost was not predicted by WMC; however, it was correlated with WMC (the smaller the n–2 task repetition cost was, the larger the WMC was; for an alternative evidence see Grange, n.d.). Furthermore, the n–2 task repetition cost was not correlated with or explained by other measures of cognitive control: response-distractor inhibition tasks group (nonverbal Stroop task, Stroop task), and resistance to proactive interference tasks (recent negatives task, cued recall task). These results were interpreted as evidence that the n–2 task repetition cost is a reflection of an automatic mechanism, such as lateral inhibition, rather than a reactive inhibition mechanism. The n–2 task repetition cost not being explained by WMC is unexpected in light with theories which assume that it reflects cognitive inhibition (for a review see Koch et al., 2010); and theories that link inhibition to WMC (e.g., Mccabe, Mcdaniel, & Hambrick, 2010; Miyake et al., 2000). That is, if the n–2 task repetition cost reflects inhibition, we would expect it to be predicted by individual differences in WMC too.

As shown above, the n–2 task repetition cost is used widely in research for various purposes; however, this effect was utilised without knowing if it is reliable. The lack of research on reliability of the n–2 task repetition cost is problematic when it comes to interpreting results from studies that used it to assess individual differences in cognitive control. This issue is given more weight by evidence (Grange & Juvina, 2015) that the magnitude of the n–2 task repetition cost varies considerably between healthy participants—an evidence for the variation in the n–2 task repetition cost occurring naturally in non-clinical population—even after practice on the BI paradigm. That is, substantial within- and between-subject differences of practice effect on the n–2 task repetition cost were observed; as some participants' n–2 task repetition costs clearly reduced in magnitude, whereas in about a third of the subjects, this cost did not change. Moreover, not all participants showed n–2 task repetition costs. Grange and Juvina also showed, by modelling the n–2 task repetition cost, that individual differences they observed were well modelled by varying the strength of the inhibition parameter in the model; it was also speculated, but not tested, that the parameter representing the rate at which inhibition decays should be considered in future research projects. It means that the n–2 task repetition cost may vary between participants due to some participants having strong/weak inhibition and/ or their inhibition decaying at different rates.

There is still a lot to understand about the n–2 task repetition cost; one could argue that until we understand the source of the n–2 task repetition cost, we should be careful making speculations around individual differences in cognitive control assessed by it.

The Current Study

To summarise, despite the n–2 task repetition cost being well replicated, and the extensive interest from group and individual differences researchers, its reliability needed to be investigated in a systematic manner. For a test to be considered reliable, it has to produce measurements that are reproducible and consistent over a course of time (Drost, 2011; Onwuegbuzie & Daniel, 2002). This is because, if a measure is not reliable, it does not capture variations in participants' performance, which means that we cannot assess intra- and inter- individual differences in cognition over time. Also, cognitive tests are used to assess typical (i.e., healthy) as well as atypical populations (i.e., clinical); therefore, it is important that measured can provide a "baseline" of healthy behaviour/ cognitive processes, which later can become an indicator of how cognition changes when it becomes impaired, and for this, reliable measures are needed.

However, it is not uncommon for cognitive tests—used for clinical and research purposes—to not have been tested for their reliability (Bird, Papadopoulou, Ricciardelli, Rossor, & Cipolotti, 2003, 2004). When tested for reliability, some measures extensively used to assess individual differences in cognitive control have been shown to be reliable (stop-signal task: Congdon et al., 2012; go/no-go task: Leue, Klein, Lange, & Beauducel, 2013; Stroop test: Strauss, Allen, Jorgensen, &

Cramer, 2005). Whereas other measures have been shown to have low reliability (e.g., the negative priming effect; Bestgen & Dupont, 2000); therefore, the robustness of a measure should not be mistaken for reliability, and researchers should—as part of good practice—report reliability of measures they use.

Whilst this study was being conducted, a report indicating the n–2 task repetition costs' reliability appeared. Among other results, Pettigrew and Martin (2015) reported reliability of the n–2 task repetition cost; its split-half reliability ranged between .44- and .51 (Spearman-Brown corrected correlation coefficient). This is considered low because, as a rule of thumb, it is accepted that for a given measure to be considered reliable, its Spearman-Brown correlation coefficient must be at least .7 (e.g., Cronbach, 1951; Nunnally, 1967; Picardi & Masick, 2013; Revelle & Condon, 2014; Streiner, 2003); and in some circumstances a reliability of .9 may be desirable (e.g., Bird et al., 2003; Cronbach, 1951). This indication of the n–2 repetition cost low reliability made the current study even more important than originally anticipated.

In the current experiment, the reliability of the n–2 task repetition cost was examined in a systematic manner; unlike Pettigrew and Martin's (2015) study. That is, many different BI paradigms are used in task-switching, and they all seem to produce n–2 task repetition costs, despite differences in cues, stimuli and/ or response requirements. Therefore, in order for the current study's findings to be generalisable, participants performed three versions of the BI paradigm: the "Target Detection" paradigm (similar to Houghton, Pritchard, & Grange, 2009, Experiment 3); the "Visual Judgment" paradigm (similar to Gade & Koch, 2008); and the "Numeric Judgment" paradigm (similar to Schuch & Koch, 2003).

The split-half reliability method was chosen to analyse the reliability of the n–2 task repetition cost with, because the cost is sensitive to practice effects (Grange

& Juvina, 2015), and, the split-half analysis—unlike the alternative test-retest methods—requires a single exposure to the test. Also, the split-half procedure is less time consuming compared to test-retest, and since reliability testing requires collection of many data points—and because three BI paradigms would be used for the current study—for practical reasons, the split-half reliability was deemed more suitable than the alternative. Moreover, the split-half reliability has been suggested to be more suitable for examining the reliability of cognitive tests (Drost, 2011).

It is important to mention, that the split-half reliability procedure can be problematic if data are split in an arbitrary manner (e.g., reliability of odd vs. even trials); that is, one cannot be certain that the obtained reliability—which typically is a single point-estimate—is an accurate estimation of reliability, or that it is not due to the way the data were split. Therefore, an alternative way of obtaining split-half reliability coefficients was chosen, which is explained in detail in results section.

Exposing participants to three paradigms allowed for the split-half reliability of each paradigm, as well as the correlation of n–2 task repetition cost between paradigms to be explored; this is the first investigation of this type reported. Since, there is an indication that low inhibition (as indicated by small n–2 task repetition costs) is linked to higher scores on depressive rumination scale (Whitmer & Banich, 2007), and because there is evidence that participants' the n–2 task repetition cost can vary considerably (Grange & Juvina, 2015), participants' depressive rumination was assessed with the intention to control for that variable in reliability testing. Also, individual differences in processing speed (PS)—another factor potentially associated with the n–2 task repetition cost—was assessed; PS has been linked to cognition in general (Brown, Brockmole, Gow, & Deary, 2012; Stawski, Sliwinski, & Hofer, 2013), and to aging cognition (Finkel, Reynolds, McArdle, & Pedersen, 2005; Kail & Salthouse, 1994; Salthouse, 2005). That is, faster PS is associated with more efficient performance on cognitive tasks (especially requiring top-down control), and it becomes slower as we age. Moreover, PS has been linked to the Stroop effect, an effect linked to inhibition, characterised by slower performance to incongruent stimuli compared to congruent stimuli (Naber, Vedder, Brown, & Nieuwenhuis, 2016).

To anticipate the results, the n–2 task repetition cost was found to have very low reliability across all three BI paradigms.

Experiment 1

Method

Participants. This study was approved by the University Ethical Research Panel at Keele University. The sample was made up of first year Psychology students from Keele University whom participated in exchange for partial course credit. The inclusion criteria were: to be at least 18-years-old, understand spoken and written English, and have normal/ corrected-to-normal vision.

To obtain sufficient sample size for the current study, the R package "pwr" (Champely, 2009) was employed to conduct power analysis, with the expected effect size of the reliability measure. Aiming for an effect size—Pearson product-moment correlation coefficient—of $r \approx .54$ (adjusted for split-half reliability analysis; see Results section for details), and desired power of 95%, the required sample size was 38. The expected correlation coefficient was reduced to a more conservative $r = 0.4$ which at 95% power, which made the required sample of 75 participants.

Out of the ninety-four participants recruited, data of twenty-two were removed; fourteen due to accuracy on some of the BI paradigms being below required 90%; seven with incomplete data (attended ½ experimental sessions); and one with an unusually large n–2 task repetition *benefit* (> 700 milliseconds, ms).

Data of seventy-two participants (60 females; mean age = 18.76, SD = 1.07) were used for the analysis. The sample size was three participants below the intended sample size; however, the study maintained more than 94% power.

General Procedure. Three BI paradigms, a depressive-rumination questionnaire (i.e., RRS), and a PS task were completed by participants during two separate sessions, each lasting 45 minutes (1–8 days apart, *M* = 3.10, *SD* = 2.30). A Latin Square Design (3x3) was used to counterbalance BI paradigms' order; the RRS/ PS task was administered at the beginning of each session (which alternated across participants). BI paradigms were presented on a standard PC with a 17in. monitor via E-Prime v. 2.0 software (Psychology Software Tools, Pittsburgh, PA). Responses were made on a 1-ms precise USB keyboard.

Before participants practiced individual tasks, they learned the cue-task and S-R pairings; the practice session consisted of 16 trials (for each BI paradigm), and if participants made four or more errors on the first practice, the practice was repeated, but a total of two practices were allowed. If participants made an error during practices, they saw the word 'Error' (font the Courier New, size 18; 1000ms).

Task-switching paradigms: general procedure. Each BI paradigm had an identical main procedure: A single trial consisted of a cue presented for 500ms, followed by appearance of a stimulus; both disappeared when a response was made. Then, the cue for the next trial appeared 100ms after a response was recorded (response-cue interval, RCI). Participants were instructed to respond as quickly and as accurately as possible. For each BI paradigm there were four blocks of 120 trials; in between blocks participants took a self-paced short break.

To ensure the sufficient amount of data is collected (i.e., number of trials per participant) to observe a correlation, multivariate normally distributed data with known correlation (i.e., correlation coefficient $r = .7$) between two variables were generated artificially, and modelled with the Linear Ballistic Accumulator (LBA) modelling (S. D. Brown & Heathcote, 2008). Among possible number of trials (i.e., 20, 50, 100, 250, 500, 1000) the optimal number of trials at which correlation is observed, was at between 100 and 250 trials per condition (i.e., ABA and CBA sequences each); therefore, 480[3] trials per paradigm was decided as a suitable amount of data to be collected.

Within each BI paradigm the order of tasks (i.e., A, B, and C trials) was randomised, with the exception that no immediate task-repetitions were allowed—task-repetitions within a run of task-switches have been shown to reduce the magnitude on the n–2 task repetition cost (Philipp & Koch, 2006). Each current trial n was compared to the trial on n–2 trial allowing for trials to be registered as ABA or CBA sequences. See Figure 2.1 for a visualisation of the trial structure in each BI paradigm.

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[3] During data collection, due to coding error, for the Target Detection Paradigm, 360 trials were collected, instead of 480.

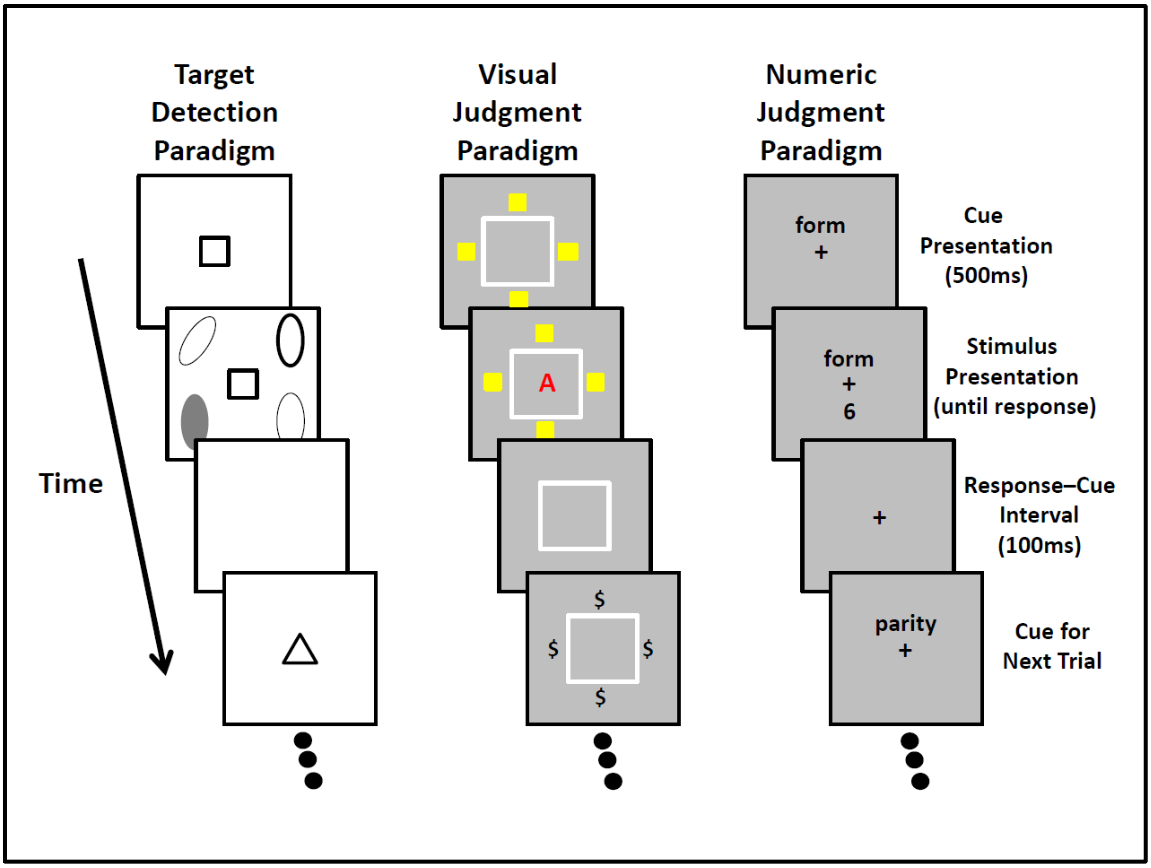


Figure 2.1 Schematic overview of the trial structure for each of the task-switching paradigms. Note the images are not to scale. This figure is reproduced from Kowalczyk & Grange (2017, p. 2422).

The Target Detection paradigm. This paradigm was based on the procedure of Houghton, Pritchard, and Grange (2009), Experiment 3; it required participants to make spatial localisation judgments of stimuli (Mayr & Keele, 2000).

The cues were shapes (triangle, square, octagon; height and width 4 cm) and targets were oval shapes with different features (see Figure 2.1). All ovals had a height of 6 cm; three ovals had widths of 2.3 cm, and one oval had a width of 3.5 cm. Cues and stimuli were presented in grayscale shading on a white background. Participants responded to the location of the target associated with the presented cue. The cues and stimuli were paired in the following way: the square cue went with the “shaded” target, the triangle cue with the “bordered” target, and the octagon cue with the “angled” target. The cue appeared in the centre of the screen, followed by four oval shapes (three possible targets and one distractor), with one oval centred within each quadrant of the screen. The position of the target on the stimuli display was randomised.

Participants responded by pressing one of four response keys depending on the location of the correct target. Each key corresponded to one corner of the screen: upper left-D, lower left-C, lower right-N, and upper right-J. Participants used their index and middle fingers of both hands to respond with; middle fingers on D/J, and index fingers on C/N.

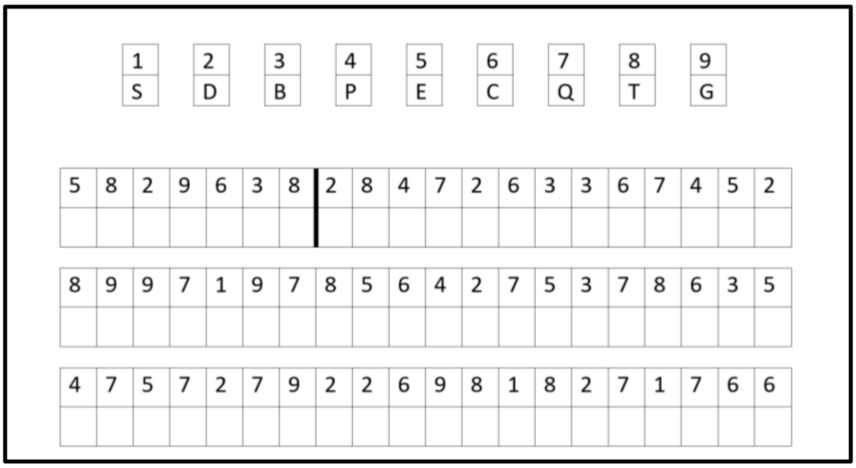
The Visual Judgments paradigm. This paradigm is based on the procedure of Gade and Koch (2008). Participants made judgments about the visual characteristic of a single multivalent stimulus. The stimulus on each trial was either the letter “A” or the number “4”, in either red or blue font; the stimulus could also be large or small. Participants judged whether the stimulus was a digit or a letter (a Form task), small or large (0.5cm vs. 1cm; a Size task), or whether it was red or blue (a Colour task).

The stimulus appeared in the centre of a white rectangle (4 cm high x 3.5 cm wide), and a task was cued by four cues of one type presented around the rectangle; each cue centred to each side of the rectangle. The $ sign cue (1 cm high) was linked with the Form task, arrows pointing up and down (1 cm high) with the Size task, and yellow squares (1 x 1 cm) with the Colour task. Participants responded with their index fingers by pressing one of two keys: “Z” (blue, small, or letter), and “M” (red, large, or digit).

The Numeric Judgments paradigm. This paradigm used the type of stimuli previously used by Schuch and Koch (2003), with central/peripheral judgment replaced by the word/digit judgment. The stimulus presented on each trial was either a digit (1, 2, 3, 4, 6, 7, 8, and 9) or a number word (one, two, three, four, six, seven, eight, and nine). Participants judged whether the stimulus was odd or even (a Parity task), whether it was lower or higher than five (a Magnitude task), or whether the stimulus was in a digit or a word form (a Form task). Task cues were the words "parity", "magnitude", and "form". Both, the cue and the stimulus, were presented on the screen in black (Courier New, size 24 font) on a grey background. The cue was presented above a central fixation cross, and the stimulus was presented below it (see Figure 2.1). Participants responded with their index fingers by pressing one of two keys: "Z" (odd, lower than five, or word), and "M" (even, higher than five, or digit).

Materials. Participants' PS (i.e., processing speed) was assessed with an adaptation of the Digit Symbol Substitution Task (e.g. van der Elst, van Boxtel, van Breukelen, & Jolles, 2006). PS has not been directly associated with the n–2 task repetition cost; however, it has been shown to predict overall cognitive abilities (Brown, Brockmole, Gow, & Deary, 2012; Stawski, Sliwinski, & Hofer, 2013), changes in aging cognition (Finkel et al., 2005; Kail & Salthouse, 1994; Salthouse, 2005), performance on the Stroop task (Naber et al., 2016), and has been shown be closely linked to the efficiency interference when processing stimuli is dealt with (Philip & Seymour, 1973; Seymour, 1974). Some even argue (Naber et al., 2016) that it is a combination of pre-set dynamics of processing efficiency and lateral inhibition rather than cognitive control, that explains the Stroop effect. Therefore, PS was deemed a potentially important factor to be controlled for in the reliability analysis of the n–2 task repetition cost.

In the Digit Symbol Substitution task, participants were given a sheet of paper on top of which nine digit-letter pairs were printed (Figure 2.2). Below those pairings was a grid with digits printed in a random order; underneath each digit participants had to write (with a pencil) the letter that corresponded to it. After a practice with seven pairs to match, participants had 120 seconds to couple as many digit-letter pairs as possible. Participants matched letters with digits in a sequential manner, without skipping any pairs; if they made a mistake, they could correct it by writing over the letter or crossing the error. Participants could score a maximum of 133 correctly matched digit-letter pairs; the higher the score, the more efficient PS is.



1	2	3	4	5	6	7	8	9
S	D	B	P	E	C	Q	T	G

5	8	2	9	6	3	8	2	8	4	7	2	6	3	3	6	7	4	5	2

8	9	9	7	1	9	7	8	5	6	4	2	7	5	3	7	8	6	3	5

4	7	5	7	2	7	9	2	2	6	9	8	1	8	2	7	1	7	6	6

Figure 2.2 An illustration of how the Digit Symbol Substitution task.

Participants’ depressive rumination trait was assessed with a short version (10 statements) of the RRS (i.e., Ruminative Response Scale)—Brooding and Reflection parts (Treynor, Gonzalez, & Nolen-Hoeksema, 2003), which has been shown to be reliable (Nolen-Hoeksema & Morrow, 1991; Thanoi & Klainin-Yobas, 2015; Whitmer & Banich, 2007). The internal reliability of the RRS in the current study was high (Cronbach’s alpha $r = .81$).

Participants answered the following question, "…*how often you do things described in each statement*", in relation to 10 statements (e.g. *Think 'What I am doing to deserve this'*?). To reflect on the extent to which participants felt each statement described them, four answers on a 1–4 scale were allowed (1- "almost never", and 4- "almost always"). The maximum possible score on the RRS was 40, and the minimum score was 10; the higher the score, the stronger the depressive rumination trait is.

Design

A within-subjects design was used to examine the reliability of the n–2 task repetition cost, with dependent variables (DVs) RTs and accuracy, and independent variables (IVs), *Task Sequence* (ABA vs. CBA) and *Paradigm* (Target Detection vs. Visual Judgment vs. Numeric Judgment). To look at whether the n–2 task repetition cost was predicted by RRS and/or PS scores, a multiple regression was conducted, with RRS and PS scores as predictors, and the RT and accuracy n–2 task repetition cost as DVs. Details of the reliability analysis procedure are outlined in the Results section.

Results

Data trimming. For all three BI paradigms, before analysis was conducted, data were trimmed. First, data of participants whose accuracy was lower than 90% in any of the BI paradigms were removed. Then, for the accuracy analysis the null trials were removed (first two trials of each block). For the RTs analysis, additionally error trials, as well as the two trials following errors were removed (those are not classifiable as an ABA or CBA sequence); this led to 6% of trials being removed (4.6%, in the Target Detection; 5.7% in the Visual Judgment; and 7.4%; in the

Numeric Judgment paradigm). For the RTs analysis, RTs faster than 150ms, and slower than 2.5 standard deviations above each participant's mean (within each paradigm) for each cell of the experimental design were removed too. Altogether, the removal of null/error trials, and RTs trimming led to 11.7 % of trials being removed (9.9 %, in the Target Detection; 11.3%, in the Visual Judgment; and 13.7%, in the Numeric Judgment).

Before conducting the reliability analysis, performance on each BI paradigm was explored with the standard RTs and accuracy analyses to assess whether n–2 task repetition costs were present. Moreover, the magnitude of n–2 task repetition costs between the three paradigms were compared to examine whether and how much this effect varied across paradigms. For mean RT and accuracy for each level of the design see Table 2.1.

Table 2.1 Mean response times (in milliseconds) and accuracy (in %) for ABA and CBA sequences for each BI paradigm. Standard errors are shown in parentheses. Note that for accuracy a negative n–2 task repetition cost reflects poorer accuracy on ABA trials.

	Response Times		Accuracy		N–2 task repetition cost	
Paradigm	ABA	CBA	ABA	CBA	RT	Accuracy
Target Detection	1014 (27)	923 (28)	97.20 (.24)	98.12 (.18)	91	-.92
Visual Judgment	1280 (41)	1175 (38)	96.94 (.24)	97.27 (.25)	105	-.33
Numeric Judgment	1323 (45)	1237 (42)	96.27 (.28)	96.27 (.31)	86	0

Response time analysis. RTs were analysed via a 2x3 repeated measures analysis of variance (ANOVA). The main effect of Task Sequence was significant, $F(1, 71) = 108.98$, $p < .001$, $\eta_g^2 = .02$, as participants were slower performing ABA (1212ms) compared to CBA sequences (1120ms). The main effect of Paradigm was also significant, $F(2, 142) = 61.19$, $p < .001$, $\eta_g^2 = .16$, the Target Detection paradigm was performed the fastest (968ms), followed by the Visual Judgement paradigm (1230ms), and the Numeric Judgement paradigm (1276ms). There was no significant interaction between Task Sequence and Paradigm, $F(2, 142) = 0.67$, $p=.51$, $\eta_g^2 < .001$, indicating an equivalent n–2 task repetition costs across the three paradigms. The density functions of the n–2 task repetition cost's distributions in each BI paradigm are depicted in Figure 2.3; as it can be seen, there was a considerable spread in the n–2 task repetition cost (i.e., n–2 task benefits and costs are present).

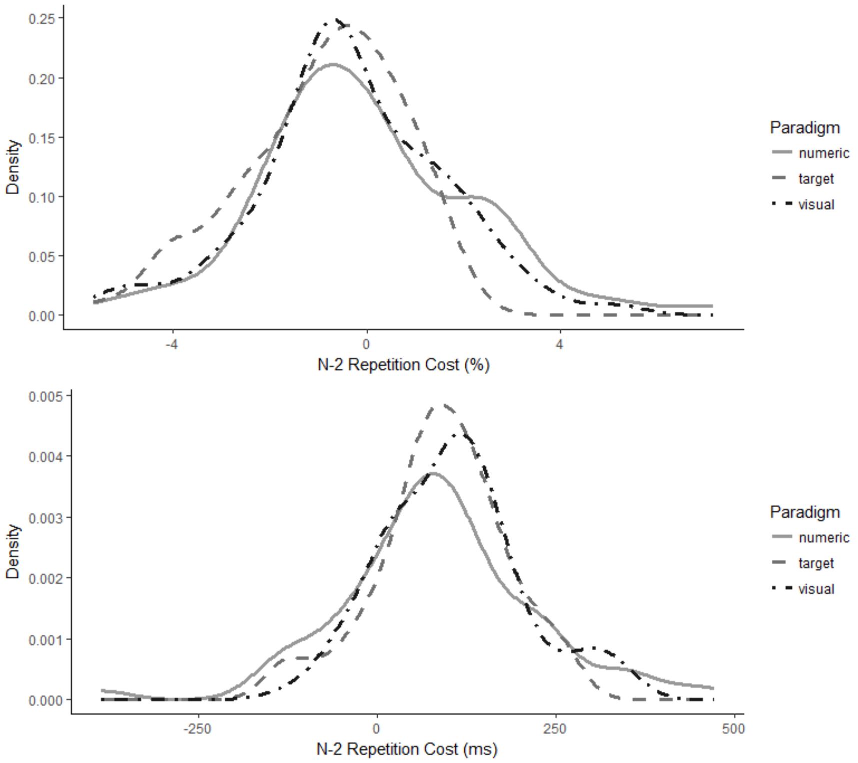


Figure 2.3 Density functions for reaction times (at the bottom) and accuracy (at the top) n–2 task repetition costs distribution for the Numeric, Visual, and Target Detection paradigm.

Accuracy analysis. A 2x3 repeated measures ANOVA for accuracy revealed that there was: a significant main effect of Task Sequence, F(1,71) = 10.22, *p* < .01, η_g^2 = .01, as participants were more accurate in CBA (97.22%) compared to ABA sequences (96.80%). The main effect of Paradigm was also significant, *F*(2, 142) = 19.07, *p* < .001, η_g^2 = .07, as participants were most accurate performing the Target Detection paradigm (97.66%), followed by the Visual Judgment paradigm (97.10%), and the Numeric Judgment paradigm (96.27%). The interaction between Task Sequence and Paradigm was significant, *F*(2,142) = 4.*01*, *p*<.05, η_g^2 = .008. The accuracy n–2 task repetition cost was significant for the Target Detection paradigm, *t*(71) = -4.71, *p* < .001, 95%CI = [-1.31, -0.53], but it was not significant for the Visual Judgement paradigm, *t*(71) = -1.42, *p* =.16, 95%CI = [-0.78, 0.13] or for the

Numeric Judgment paradigm, *t*(71) = -0.01, *p* =.99, 95%CI = [-0.52, 0.52]. See Figure 2.3 for the density functions of the distributions of accuracy n–2 task repetition cost across paradigms; again, the n–2 task repetition cost is considerably spread across participants, with n–2 task benefits and costs present.

Correlations. To examine whether participant's overall performance (RTs and accuracy) was stable across all three paradigms, a Pearson product-moment correlation analysis was conducted, which for completeness, includes individual differences (Table 2.2–2.3).

Reaction time. Mean RTs from all three BI paradigms correlated significantly: The Target Detection correlated with the Numeric Judgment (*r* = .64, *p* < .001) and the Visual Judgment paradigm (*r* = .68, *p* < .001); the Numeric Judgment correlated with the Visual Judgment paradigm (*r* = .75, *p* < .001). To note, these correlations remain significant when analysis was adjusted for multiple comparisons.

Accuracy. Overall, accuracy also correlated significantly between three paradigms: The Target Detection correlated with the Numeric Judgment (*r* = .42, *p* < .001), and the Visual Judgment paradigm (*r* = .50, *p* < .001); the Numeric Judgment correlated with the Visual Judgment paradigm (*r* = .63, *p* < .001). These correlations also remain significant when controlling for multiple comparisons.

Table 2.2 Correlation matrix for the mean response times from the three task-switching paradigms, individual differences (RRS-Depressive Rumination; P Speed-processing speed), age, sex, and the break (Gap) between the two experimental sessions.

	Numeric	Target	Visual	RRS	P Speed	Age	Sex	Gap
Numeric	—							
Target	.64**	—						
Visual	.75**	.68**	—					
RRS	.02	.10	.19	—				
P Speed	.49**	.63**	.49**	-.08	—			
Age	.35*	.25*	.30*	-.02	-.21	—		
Sex	.06	.14	.03	-.21	-.21	.11	—	
Gap	.02	-.03	.05	.12	-.02	.13	.19	—

Table 2.3 Correlation matrix for the mean overall accuracy from the three task-switching tests, individual differences (RRS-Depressive Rumination; P Speed-processing speed), age, sex, and the break (Gap) between the two experimental sessions.

	Numeric	Target	Visual	RRS	P Speed	Age	Sex	Gap
Numeric	—							
Target	.42**	—						
Visual	.50**	.63**	—					
RRS	.14	.27*	.22	—				
P Speed	.05	.08	-.07	-.08	—			
Age	-.04	.07	.01	-.02	-.21	—		
Sex	-.08	-.13	.02	-.21	-.21	.11	—	
Gap	-.17	.12	.01	.12	-.02	-.13	.19	-

Note: ***p*<.0018 (Bonferroni-corrected criterion for significance). **p*<.05

Individual differences. Next, participants' individual differences scores—PS and RRS—were examined to assess whether they correlated with RTs from task-switching. This was mainly to perform a manipulation check on PS measure; PS should correlate with overall performance, as the literature suggests.

The mean PS score was 91.41 (*SD* = 12.78, min. = 66, max. = 121)[4], and the mean RRS score was 19.04 (*SD* = 5.00, min. = 11, max. = 37). Performance on RRS did not correlate significantly with performance on the PS task (see Table 2.2). Correlation analysis between mean RTs and individual measures revealed that, the negative correlation between RTs and PS was significant, for all three task-switching paradigms (Numeric Judgment, $r = -.49$, $p < .001$; Target Detection, $r = -0.63$, $p < .001$; the Visual Judgment, $r = -.49$, $p < .001$); that is, higher PS scores were associated with faster RT. However, the RRS score did not correlate with RTs on the three BI paradigms.

Importantly, since RRS and PS were factors potentially to be controlled for in the reliability analysis, a multiple regression was conducted to examine whether, those two variables predicted n–2 task repetition costs. This analysis revealed that RT and accuracy n–2 task repetition costs were not predicted by either the RRS or the PS score in any of the three BI paradigms (see Table 2.4–2.5)[5]. As n–2 task repetition costs were not explained by individual differences, these individual differences were not controlled for in the between-paradigm correlations, or the reliability analysis.

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[4] One participant had missing data for the Processing Speed measure; to maintain power, this participant's data was kept and the missing value was imputed using the mean score for the Processing Speed test. Removing this participant changes none of the conclusions.

[5] Transforming RRS and the processing speed scores into z-scores and then inputting them into the regression analysis also yielded non-significant results.

Table 2.4 Summary of multiple regression analyses for reaction time n–2 task repetition costs as the dependent variable, and the P Speed (processing speed) and RRS scores ad independent variables.

	Predictor	*t*	*p*	β	*F*	*df*	*p*	adj.R^2
Target	RRS	-0.01	.99	-0.03				
	P Speed	0.14	.90	0.11				
	Overall model				0.01	2,68	.99	-.03
Visual	RRS	1.30	.38	2.06				
	P Speed	-0.74	.46	-0.68				
	Overall model				0.72	2,68	.49	-.01
Numeric	RRS	1.18	.24	3.91				
	P Speed	-1.13	.26	-1.45				
	Overall model				1.46	2,68	.24	.01

Table 2.5 Summary of multiple regression analyses for accuracy n–2 task repetition costs as the dependent variable, and the P Speed (processing speed) and RRS scores ad independent variables.

	Predictor	*t*	*p*	β	*F*	*df*	*p*	adj.R^2
Target	RRS	0.02	.98	0.0001				
	P Speed	-0.47	.64	0.008				
	Overall model				0.12	2,68	.89	-.03
Visual	RRS	-0.09	.93	-0.004				
	P Speed	-0.02	.98	-0.0004				
	Overall model				0.005	2,68	1.0	-.03
Numeric	RRS	1.24	.22	0.07				
	P Speed	0.31	.76	0.006				
	Overall model				0.80	2,68	.46	-.006

Between-paradigm correlation. One of the aims of the current study was to assess whether the n–2 task repetition cost correlated between the three BI paradigms. This was achieved by conducting Pearson product-moment correlations on n–2 task repetition costs across paradigms, separately for RT (Table 2.6) and accuracy (Table 2.7).

Response time. Correlation analysis revealed that the Target Detection did not correlate significantly with the Visual Judgment paradigm (r = .07, p = .57), but it correlated significantly with the Numeric Judgment paradigm (r = .25, p = .036); the Visual Judgment correlated significantly with the Numeric Judgment paradigm (r = .30, p=.01)[6]. The two correlations that were significant, do not remain significant when using Bonferroni corrections for multiple correlations.

Table 2.6 Correlation matrix for n–2 task repetition costs in response times from the three BI paradigms, individual differences (RRS-depressive rumination and P Speed-processing speed), age, sex, and the break (Gap) between the two experimental sessions.

	Numeric	Target	Visual	RRS	P Speed	Age	Sex	Gap
Numeric	—							
Target	.25*	—						
Visual	.30*	.07	—					
RRS	.15	.01	.12	—				
P Speed	-.15	.02	-.10	-.08	—			
Age	.14	-.10	-.02	-.02	-.21	—		
Sex	.06	-.04	-.05	-.21	-.21	.11	—	
Gap	.09	.07	.17	.12	-.02	-.13	.19	—

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[6] Equal trials correlation analysis: the n–2 task repetition cost correlated only between the Visual and Numeric Judgments paradigms (r = .28, p = .01). The reported correlations remained unchanged when analysis was controlled for individual differences in processing speed via partial correlations: Target Detection paradigm did not correlate with the Visual Judgement paradigm (r = .07, p = .56), but it did correlate with the Numeric Judgement paradigm (r = .25, p = .03); the Visual Judgement paradigm correlated with the Numeric Judgement paradigm (r = .30, p = .01). Note these latter correlations do not remain significant when using Bonferroni corrections for multiple correlations.

Accuracy. The accuracy n–2 task repetition costs did not correlate significantly across three paradigms: Target Detection paradigm did not correlate with the Numeric Judgment (*r* = .06, *p* = .64), or the Visual Judgment paradigm (*r* = -.15, *p* = .22); the Numeric Judgment paradigm did not correlate with the Visual Judgment paradigm (*r* = .01, *p* =.93).

Table 2.7 Correlation matrix for n–2 task repetition costs in accuracy from the three BI paradigms, individual differences (RRS-depressive rumination and P Speed-processing speed), age, sex, and the break (Gap) between the two experimental sessions.

	Numeric	Target	Visual	RRS	P Speed	Age	Sex	Gap
Numeric	—							
Target	.06	—						
Visual	.01	.15	—					
RRS	.16	.00	.03	—				
P Speed	.02	.06	.00	-.08	—			
Age	-.09	.07	.11	-.02	-.21	—		
Sex	-.18	.08	-.10	-.21	-.21	.11	—	
Gap	-.08	.07	.10	.12	-.02	-.13	.19	—

Reliability analysis. To assess the reliability of the n–2 task repetition cost (for RTs and accuracy), a split-half reliability analysis was conducted. Instead of obtaining a single-point estimate of the split-half reliability coefficient, a form of bootstrapping was conducted, where many random splits of the data were performed, and for each split a reliability estimate was calculated (see e.g. Congdon et al., 2012). First, for each participant within each paradigm, trimmed data were split randomly into two halves; for each half mean RT/ accuracy were calculated—for ABA and CBA sequences—to calculate n–2 task repetition costs for each half. Then, a Pearson product-moment correlation between n–2 task repetition costs from the two halves was conducted, and the point-estimate was stored. This procedure was repeated 500 times, allowing for a distribution of correlation coefficients.

The split-half reliability method requires the estimated reliability coefficient to be adjusted; this is because, by splitting of data in half, the total number of data points being used in the reliability analysis are reduced, which can reduce the reliability coefficient. To adjust the split-half reliability, the Spearman-Brown correction is used, which is given by

Equation 2.1

$$r_c = \frac{Nr}{1 + (N - 1)r}$$

where *r* is the Pearson product-moment coefficient and *N* is the number of "tests" being combined (for the current study there were two halves, so $N = 2$). For the current study, the reliability of r_c—the corrected reliability coefficient—of at least .7 was to be interpreted as strong reliability. With N = 2, this pertains to an uncorrected $r \approx .5385$.

The bootstrapping reliability coefficient estimation yielded some negative (uncorrected) *r* values, which indicates total lack or a very low reliability (Crawford, Sutherland, & Garthwaite, 2008), and renders the Spearman-Brown correction uninterpretable. Therefore, below the uncorrected *r* is reported, and r_c is referred to when interpreting the full results.

Response times. The results for the n–2 task repetition cost (RTs) split-half reliability analysis are depicted in Figure 2.4 in what is known as violin plots; these represent a distribution of correlation coefficients for the split-half reliability for each of the three paradigms. A violin plot is like a standard box-plot, with the addition of a rotated density function of the distribution of scores, allowing a better description of the shape of the distributions (Adler, 2005).

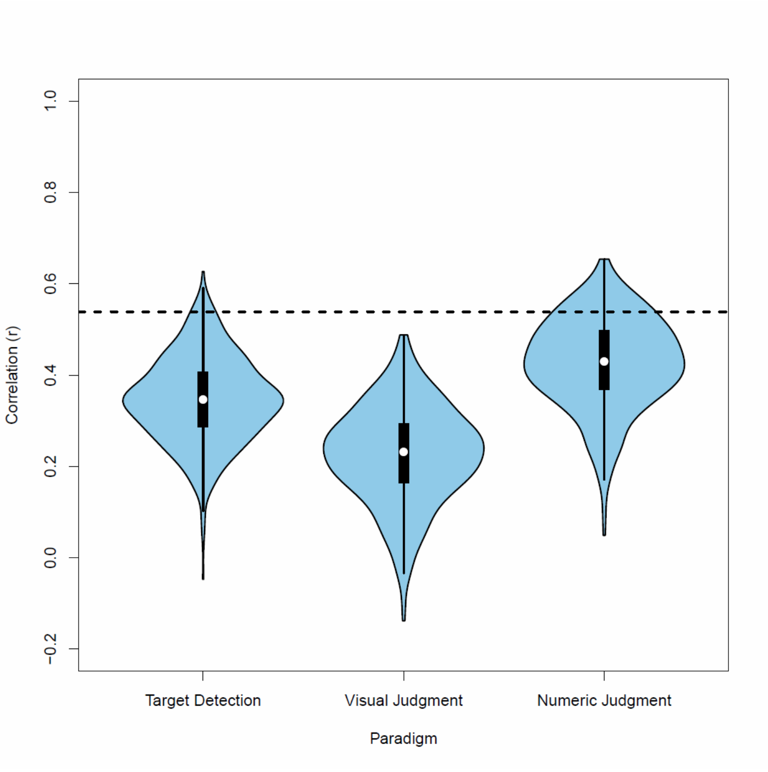

Figure 2.4 Reliability checks for the n–2 task repetition cost for response times. The plots show violin plots of the (uncorrected) bootstrapped split-half reliability estimates (correlation coefficients, *r*) each paradigm. The horizontal dashed line represents the criteria for reliability ($r \approx .5385$; Picardi & Masick, 2013; Revelle & Condon, 2014).

AS it can be seen in Figure 2.4, the median correlation coefficient values (*r*) for the split-half reliability for each of the three paradigms did not reach the criterion for reliability (as stated, equivalent to an uncorrected $r \approx .5385$). The tails of distribution of coefficients for the Target Detection and the Numeric Judgment paradigms cross the reliability criterion; however, this is not considered as strong evidence for acceptable reliability. The median values of uncorrected correlation coefficients were: $r = .35$ for the Target Detection, $r = .23$ for the Visual Judgment, and $r = .43$ for the Numeric Judgment. These translate to corrected values of $r_c = .52$ for the Target Detection, $r_c = .37$ for the Visual Judgement, and $r_c = .60$ for the Numeric Judgement paradigm.

Accuracy. The reliability analysis for the accuracy n–2 task repetition cost revealed that median values of reliability coefficients did not reach the required criterion, as depicted in Figure 2.5. The median values of uncorrected correlation coefficients were: r = .07 for the Target Detection, r = .18 for the Visual Judgment, and r = .19 for the Numeric Judgment paradigm. These translate to corrected values of r_c = .13 for the Target Detection, r_c = .31 for the Visual Judgement, and r_c = .32 for the Numeric Judgement paradigm.

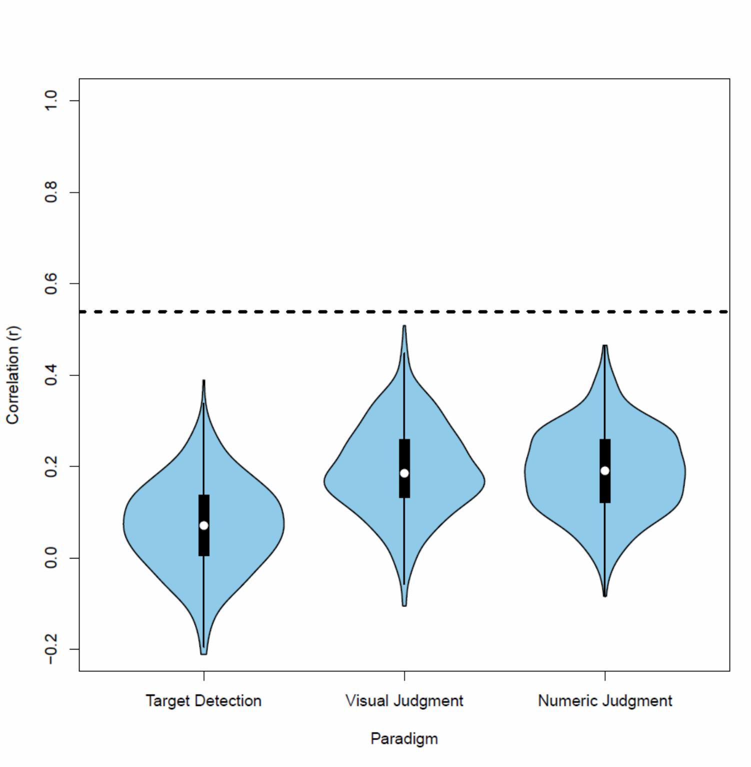

Figure 2.5 Reliability checks for the n–2 task repetition cost for accuracy. The plots show violin plots of the (uncorrected) bootstrapped split-half reliability estimates (correlation coefficients, r) each paradigm. The horizontal dashed line represents the criteria for reliability ($r \approx .5385$; Picardi & Masick, 2013; Revelle & Condon, 2014).

Exploratory analysis. Work conducted for this study was published (Kowalczyk & Grange, 2017), and the following are analyses recommended by the reviewers.

Practice effect. Participants had 16 trials of practice for each BI paradigm, which could be considered as a relatively short practice period, which could mean that participants' performance had not reached asymptote before the experimental blocks commenced. If participants did not exhibit optimal performance, it could have affected the reliability coefficient estimation results[7]; therefore, an exploratory analysis was conducted to check if the reliability analysis was higher in more practiced blocks. For each of the three paradigms, the split-half bootstrapping analysis was redone without the first block; this additional analysis revealed that the reliability results were not affected by lack of insufficient practice; that is, the correlation coefficients stayed below the required criterion.

Response times. The median values of correlation coefficients were r = .33 (r_c = .50) for the Target Detection, r = .34 (r_c = .51) for the Visual Judgment, and r = .39 (r_c = .56) for the Numeric Judgement paradigm.

Accuracy. The median values of correlation coefficients were r <.0001 (r_c = .001*)* for the Target Detection, r=.25 (r_c = .40) for the Visual Judgment, and r=.09 (r_c = .17) for the Numeric Judgment paradigm.

Order of paradigms. Another check was performed to establish whether the poor across-paradigm reliability was a result of a reduction in n–2 task repetition costs as participants progressed through the experimental sessions[8]; due to the counterbalancing, this could mask reliability effects if not controlled.

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[7] Thanks to Cai Longman for suggesting this possibility.

[8] Thanks to Cai Longman for suggesting this possibility.

To perform the necessary analysis to address this potential issue, n–2 task repetition costs for each participant were re-categorised as a function of "paradigm order" (i.e., "1st paradigm encountered"; "2nd paradigm encountered"; "3rd paradigm encountered"). A one-way ANOVA with the n–2 task repetition cost as a DV and the *paradigm order* as the IV was then carried out. This analysis revealed that there was no significant effect of order on the n–2 task repetition cost, $F(2,142) = 0.22$, $p > .8$, $\eta_g^2 = .002$; the n–2 task repetition cost for the 1st paradigm encountered had a mean of 88ms (*SE*=13), 95ms (*SE*=13) for the 2nd, and 99ms (*SE*=13) for the 3rd paradigm encountered. Therefore, poor reliability of the n–2 task repetition cost was not due to reduction in n–2 task repetition costs as participants proceeded through the experiment.

General Discussion

The n–2 task repetition cost is an effect seen in task-switching, and it is thought to reflect cognitive inhibition; the current study examined how reliable the n–2 task repetition cost is at assessing individual differences in cognitive control. Three different BI paradigms were used to measure the n–2 task repetition cost; as expected, and consistently with the existing literature, large and statistically-significant n–2 task repetition costs were found in all three paradigms. The split-half reliability of the n–2 task repetition costs in all three paradigms was low; moreover, the individual differences—i.e., RRS and PS—were not associated with n–2 task repetition costs.

The finding that split-half reliability of the n–2 the repetition cost is low, substantiates results from another study (Pettigrew & Martin, 2015). The authors of that study used a BI paradigm as part of their tests-battery to study the relationship between WM and flexible task-switching; despite not being the focus of their study, for some of the measures they report split-half reliability, and the n–2 the repetition cost' reliability was low (r_c = .44). The current study confirms that finding and extends it, as it is the first study to investigate the reliability of the n–2 the repetition cost in a systematic manner. Specifically, there are many BI paradigms and they differ in terms of cues, stimuli and response mapping; and to establish a clearer view on reliability of the n–2 task repetition cost, the current study used different BI paradigms, aiming to represent methods used in task-switching research thereby, offering more generalisable findings.

As mentioned in the introduction of this chapter, despite the n–2 task repetition cost being well replicated, its reliability was not known, which was problematic for interpretation of the n–2 task repetition cost in individual differences studies. For measures that are meant to assess individual differences, it is important that they are reliable. A reliable test has to produce measurements that are reproducible and sensitive to individual differences for any changes in performance to be observed (Drost, 2011; Onwuegbuzie & Daniel, 2002). Therefore, the finding that the n–2 task repetition cost has low reliability presents a challenge to researchers wishing to use the n–2 task repetition cost for individual differences research. A reliable tool estimates inter- and intra-person variability in a meaningful way, so it provides an indication of a person's "cognitive status" (Salthouse, 2007). Also, if a given measure taps into a single process rather than a number of different processes (unless they are related; for example, when measuring anxiety, a measure can tap into its cognitive, behavioural, and affective components of anxiety), that measure would be expected to be reliable (Streiner, 2003). Therefore, the initial implication of the finding that the n–2 task repetition cost has low reliability raises the question as to the cognitive processes "captured" by that effect; specifically, whether the n–2 task repetition cost is a measure of a single ability (or related processes), in this case, cognitive inhibition.

For example, as it was proposed by Grange and Juvina (2015), individual differences in the n–2 task repetition cost can be modelled by varying the strength of inhibition; also, it has been suggested that another aspect, the rate at which inhibition decays, is potentially important for future modelling of individual differences in inhibition. In practice, it means that there may be substantial variations in performance even between trials within one participant. The current findings suggest the n–2 task repetition cost does not seem to capture individual variation in a reliable manner, making the interpretation and meaningfulness of the results uncertain. It is possible that as-yet unidentified factors might have affected the reliability of the n–2 task repetition cost. For example, performance can naturally vary within a person due to short (across sessions), long term fluctuations (e.g., due to cognitive aging), and it can depend on how accurate the measurements are (Salthouse, 2007). Also, participants may have different levels of engagement during a single testing session, especially if the task is considered as undemanding and repetitive (Allan Cheyne, Solman, Carriere, & Smilek, 2009).

N–2 Task Repetition Cost

To provide more generalizable findings, cognitive control was assessed with three different BI paradigms, which made it possible to examine the correlation of the n–2 task repetition costs across the paradigms. This was important to establish, because if a measure is reliable and taps into the same ability—in this case inhibition—it is expected that participants' performance between paradigms should be associated. However, what was found was that whilst the group-level n–2 task repetition cost did not vary significantly across different paradigms—that is the n–2 task repetition cost was equivalent across the three paradigms—the individual n–2 task repetition costs did not correlate across paradigms. This can be interpreted as converging evidence for the lack of reliability in the measure. Specifically, if the n–2 task repetition cost was a reliable and accurate measure of an individual's inhibitory control, then individual's large n–2 task repetition cost in one paradigm should show similarly-large n–2 task repetition costs in other paradigms. If anything, some participants showed an n–2 task repetition benefit on one or two paradigms and the n–2 task repetition cost on the remaining paradigm/s. Taken together, low reliability for individual n–2 task repetition costs and partial correlation of that effect between three paradigms, indicates that the n–2 task repetition cost is an unreliable. However, the current study assumed that if the n–2 task repetition cost reflects inhibition, this inhibition is stable within participant, whereas it is possible that inhibition is not stable within individuals, and if that is the case, the BI paradigm inhibition may be not sensitive enough to those variations. Also, the n–2 task repetition cost may not be as pure a measure of cognitive inhibition as currently believed. Future research should explore potential reasons for the apparent lack of reliability.

Individual Differences

As indicated by the findings from the current study (see Figure 2.3; see also Grange & Juvina, 2015), the n–2 task repetition cost seems to vary substantially between participants. The source of these variations is uncertain, but the n–2 task repetition cost has been used to measure individual differences in cognitive inhibition. Despite the current study showing that depressive rumination nor PS were associated with the n–2 task repetition cost (although PS was correlated with RT performance), some important points came out from this study; however, the points below are made keeping in mind that the n–2 task repetition cost is not reliable, which may be the main reason for the lack of the relationship between the n–2 task repetition and depressive rumination and PS.

Trait rumination. Depressive rumination has been shown to be negatively correlated with the n–2 task repetition cost (Whitmer & Banich, 2007), but the current study failed to replicate that finding. Specifically, none of the three BI paradigms' n–2 task repetition cost was associated with trait rumination. It is uncertain yet as to why this association was not present; existing literature offers inconsistent findings, as Chen, Feng, Wang, Su, and Zhang (2016)—unlike Whitmer and Banich—also found no relationship between those two variables. The initial implication of these discrepancies is that more research is needed to establish whether and to what extent trait rumination links to cognitive inhibition.

It is plausible that the findings from the current study and those of Chen et al.(2016) reflect a genuine failure to replicate Whitmer and Banich's results (e.g., Open Science Collaboration, 2015); however, it seems that it is more likely that methodological differences may be the source of these inconsistencies. What sets Whitmer and Banich's study apart is that in their regression analysis of the n–2 task repetition cost being predicted by rumination, they controlled for the switch cost (SC)—the RT difference between task-switches vs. task-repetitions. The current study and the one by Chen and colleagues, did not control for the SC. This is a potentially important difference because the SC and the n–2 task repetition cost have been shown to be dissociable (Whitmer & Gotlib, 2012; Yiu-kwan, 2008); specifically, people's ability to activate a relevant task, may be separate from how efficiently an irrelevant task is inhibited. Also n–2 task repetition costs reduce in magnitude if immediate task-repetitions are allowed within a run of task-switches (Koch et al., 2006); therefore, to obtain the strongest n–2 task repetition costs, the current study utilised only task-switches (unlike Whitmer and Banich). However, it is possible that when investigating individual differences in cognitive control (with the n–2 task repetition cost), SC also must be accounted for.

Also, the three above mentioned studies have in common the type of sample used; that is, data for these studies were collected from healthy populations, as represented by university students' samples. Whitmer and Banich (2007) found significant correlations between n–2 task repetition costs and depressive rumination in pre-selected respondents (categorised as "low" and "high" ruminators), as well as in the not preselected sample. The current and Chen et al. (2016) studies did not classify their participants into low and high ruminators; however, as Whitmer and Banich indicate, this preselection was not necessary to observe the correlation between the n–2 task repetition cost and rumination. There is a possibility that the lack of the relationship between the n–2 task repetition cost and depressive rumination is due to low reliability of the cost, but that does not explain why Whitmer and Banich found this relationship in their study.

Processing speed. As mentioned in the introduction of this chapter, PS—though not directly linked to the n–2 task repetition cost—has been shown to predict the general efficiency with which cognitive tasks are performed. Since PS has been also linked to Stroop effect—assumed to reflect cognitive inhibition—if the n–2 task repetition cost reflects inhibitory processes, PS should explain the individual differences seen in n–2 task repetition cost too. However, the Stroop effect has been shown to be reliable (e.g., Strauss et al., 2005) unlike the n–2 task repetition cost, which may be the reason for no relationship between the cost. Nevertheless, in one of the checks it was established that PS predicted average RTs in the three BI paradigms used; this means that the PS measure successfully captured individual differences in PS and predicted performance as expected. Yet, it is unclear why PS is not related to n–2 task repetition cost. Therefore, at this stage it is uncertain whether PS not predicting the n–2 task repetition cost is due to PS not influencing inhibition or due to the cost's low reliability.

Choice of Reliability Criterion

During a review of this paper for publication, it was suggested that the reliability coefficient of .7 was too stringent for a cognitive test, as this criterion is typically used for reliability testing of psychometric measures. However, other studies that looked at the reliability of cognitive measures used a similar criterion (e.g., Dale & Arnell, 2013). Moreover, the level of reliability found in the current study has been referred to by other researchers as "moderate" (Leue, Klein, & Lange, 2013), "poor"–to–"good" (Condon et al., 2012), and "quite low" (Pettigrew & Martin, 2015). None of these are necessarily "correct", and it is up to the reader to make a judgment on how to interpret this study's results. Nevertheless, it is important to mention that a reliability of .5-.6 is acceptable in early stages research—preliminary or explorative studies—and a reliability of .8 is considered adequate for basic research—applied research, for example, looking at differences in experimental treatments—whereas, a reliability of .9 is a minimum for clinical research (Nunnally, 1967; Streiner, 2003). Taken together, literature suggests that the n–2 task repetition cost's reliability found in the current study is not sufficient for the use in individual differences research.

Reliability and Sample Type

There was a possibility that the homogenous sample used in the current study might have reduced the differential validity by reducing the variance of the n–2 task repetition cost, lowering the chances to measure reliability accurately[9]. The current study aimed to obtain data from a type of sample that is comparable to the studies reported in the literature. It was not the focus of this study to investigate how more/ less homogenous sample influences the differential validity and if it affects the variance of the n–2 task repetition cost, but this remains an interesting question for future research. However, based on observations from the current and previous studies, in a sample as homogenous as university students, there is considerable variance of the n–2 task repetition cost (e.g. Grange & Juvina, 2015; see also Figure 2.3 in the current chapter).

N–2 Task Repetition Cost and Inhibition

The current study's findings indicate that the n–2 task repetition cost has low reliability; moreover, the n–2 task repetition cost did not correlate well across the three BI paradigms. This can be interpreted as evidence that the n–2 task repetition cost is an unstable measure of inhibitory control; or that inhibition in task-switching is unstable, and the measure is not sensitive to inhibition variations. There is another potential explanation of why the n–2 task cost's reliability is low; namely, it is possible that the n–2 task repetition cost is not a "pure" measure of inhibition. Specifically, if the n–2 task repetition cost reflects a mixture of cognitive mechanisms—one of which being inhibition—then the measure would be unstable and therefore not reliable.

There are several possible factors that may confound a measure of inhibition in task-switching. For example, if in task-switching inhibition is applied to resolve interference, it is important to know where that interference comes from. This is because interference may arise as a result of lapses in attention (Allan Cheyne et al.,

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[9] Thanks to an anonymous reviewer for suggesting we discuss this.

2009), task-unrelated-thoughts (van Vugt & Broers, 2016), mismatches in episodic retrieval (Mayr, 2007; Neill, 1997), or perceptual load (Lavie, 1995), just to mention a few.

To give an example of how one of those factors could modulate performance on task-switching, let us consider episodic retrieval (Mayr, 2007; Neill, 1997). Episodic retrieval theory, when applied to task-switching, would predict: as a given task is carried out, an episodic trace of that task is stored in memory and with it the task's parameters (e.g., cue, stimulus characteristics, and response executed). If the same task is cued soon after it was performed (e.g., parity-magnitude-*parity* sequence), a memory trace of that task is automatically retrieved. So, using the example from one of the paradigms used in the current study, if the first task is a parity judgment where the stimulus is a digit "8" requiring a "Z" key response, followed by magnitude judgment, and then followed by another parity judgment; if the second parity judgment's parameters are the same as the first one (i.e., another "8" stimulus with "Z" response), that task should be performed faster—because an episodic memory trace of previously performed parity judgment will be retrieved—compared to if the second parity judgment was about a digit "3"—which is the same task to be performed but a different response (i.e., "M" response key). Episodic retrieval is not controlled for in task-switching paradigms; therefore, it is uncertain how much of the n–2 task repetition cost is due to episodic retrieval mismatches. Chapter 3 of this thesis, reports three studies that explored to what extent episodic retrieval influences the n–2 task repetition cost, and whether this modulation changes the reliability of the n–2 task repetition cost.

Difference Measures

After the paper for the current study was accepted for publication, it became apparent that there may be a very good reason for the n–2 task repetition cost to be low[10]. Namely, some literature offer evidence to illustrate that difference score measures are bound to have low reliability (Crawford et al., 2008; Cronbach & Furby, 1970; Dunlap, Kennedy, Harbeson, & Fowlkes, 1989; Kopp, 2011). That is, reliability of difference scores is “a function of the average reliability of its two components and of the correlation between them” (Kopp, p. 562; Crawford et al.). That is, if a difference score is made up of two related components, the correlation between those components will be high (or even approach the reliabilities of the two components); as such, the variance of the difference scores is likely to reflect mainly error variance. Since the n–2 task repetition cost is a difference score, the low reliability identified in the current study is likely to be due to the nature of the cost, which gives more weight to the advice concluded in the current study, which is to be cautious using the n–2 task repetition cost for individual differences research and/ or for neuropsychological assessments (Kopp; Crawford et al.). Difference scores are also prone to have negative reliability coefficients; as it was observed in the currents study. In terms of interpreting the n–2 task repetition cost, the difference scores’ reliability research would suggest that such measures are very difficult to interpret. Similar evidence has been put forward for negative priming effect (Bestgen & Dupont, 2000) and Stroop test (Strauss et al., 2005); and as such, these measures should be interpreted with caution too. Some would suggest that instead of difference scores, rate residual scores (Hughes, Linck, Bowles, Koeth, & Bunting, 2014), which take into account RTs and accuracy, are a more accurate measure of cognitive processes because they account for differences in processing speed (Rush,

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[10] Thanks to Professor Bruno Kopp for this suggestion.

Barch, & Braver, 2006). This method involves calculating a rate of correct responses per second for the conditions of interest, and these rates are put into regression (Hughes et al., 2014); however, using rate residual scores does not seem to improve the reliability of the n–2 task repetition cost in one of the studies (Pettigrew & Martin, 2015), because its reliability was low. Also, Pettigrew and Martin showed that the n–2 task repetition cost did not correlate with working memory regardless of the rate residual scores or the difference scores were used; however, but it seems that the rate residual scores method can improve the SC’s reliability (Hughes et al., 2014).

Conclusion

Data from the current study suggest the n–2 task repetition cost is not reliable as a measure of individual differences of inhibitory control. Individual differences in depressive rumination and processing speed did not predict the n–2 task repetition cost. Taken together, these results show that interpretations of the n–2 task repetition cost as a measure of individual differences in inhibitory control should be cautious until we are certain that it reflects inhibition, and it does so in a reliable manner.

Chapter 3 Inhibition in Task-Switching and Episodic Retrieval[11]

Abstract

In task-switching experiments slower reaction times returning to a task completed recently (i.e., ABA sequence) compared to performing a task that was not performed recently (i.e., CBA sequence) is known as the n–2 task repetition cost, an effect thought to reflect cognitive inhibition. Three experiments in the current study showed consistently that the n–2 task repetition cost is in large part due to non-inhibitory mechanism, episodic retrieval (Experiment 2), with the n–2 task repetition cost being larger under episodic mismatches (i.e., n–2 task repetitions with n–2 response switch) compared to episodic matches (i.e., n–2 task repetition with n–2 response repetition). Also, episodic retrieval was shown to modulate cue-transparency which in turn affected the n–2 task repetition cost (Experiment 3). Moreover, the n–2 task repetition cost, nor the episodic retrieval, were modulated by low-level perceptual mismatches/ matches of stimuli features (Experiment 4). Additionally, the n–2 task repetition cost's reliability was low with and without controlling for episodic retrieval. The main conclusion from the current study is that the n–2 task repetition cost is attributable to a combination of non-inhibitory (i.e., episodic retrieval) and inhibitory mechanisms.

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[11] An adapted version of this chapter was published (Grange et al., 2017), and components of the paper are reproduced; permission of the copyright holder granted: Copyright © 2017, American Psychological Association.

Introduction

Chapter 2 reported a study that found that the effect known as the n–2 task repetition cost—thought to reflect cognitive inhibition—has low reliability (Kowalczyk & Grange, 2017). From those results it was suggested, among other things, that the reliability of the n–2 task repetition cost may be low due to some unaccounted-for factors influencing performance, or due to the n–2 task repetition cost not being a pure measure of inhibition. It was proposed that the n–2 task repetition cost may reflect inhibition as well as other, non-inhibitory mental processes, for example, episodic retrieval. The current chapter reports a series of experiments which looked at whether and to what extent, the n–2 task repetition cost is modulated by episodic retrieval; whether controlling for episodic retrieval improves the reliability of the n–2 task repetition cost; and the effects of cue-transparency and/ or low-level, task-irrelevant stimulus features on episodic retrieval and the n–2 task repetition cost.

Cognitive Control

Every day, humans are exposed to an environment saturated with stimuli that we can act on, some of which we want to pay attention to, and some of which we want to ignore. In general, our behaviour is governed by what we attend to; therefore, to achieve our goals we must be able to distinguish between relevant and irrelevant information, and act on it appropriately. We also must be able to maintain a given goal when we encounter distractions, as well as to change our goal/ behaviour when necessary. For example, when walking to a shop to buy some bread, we may have to walk past other shops (e.g., clothes shops), meet someone we know, or receive a phone-call, all of which can distract us from the original goal. However, we can ignore distractions or we can choose to engage in them, whilst maintaining the original goal in mind. Cognitive control is thought to be a system of different mental processes working together allowing us to adapt our behaviour to match current environment's and/or our goals' demands; that is, to allow us to behave in a manner that corresponds with our intentions and is context-specific. This cognitive system is believed to coordinate and maintain our goals, at the same time allowing flexible behaviour change when necessary (Goschke, 2000).

Backward Inhibition Paradigm

One specific mental process which is thought to aid goal-oriented behaviour is inhibition. This mental process is assumed to act like 'breaks' on our intentions and actions to support selection and conduct of appropriate behaviours. In cognitive psychology, a method known as the backward inhibition (BI) paradigm has become a very popular tool to assess inhibition in task-switching (Mayr & Keele, 2000). The idea behind this paradigm is that participants switch rapidly between series of stimuli presented in close temporal succession on a computer screen. For example, they may see a series of digits (one a at a time) and react to each according to rules of one of three tasks: for a parity task, they must judge whether the digit is odd/even; for a magnitude task, if the digit is lower/higher than 5; and for a colour task, whether the digit is blue/red. The nature of the BI paradigm requires participants to switch between tasks in a controlled manner, applying correct rules to each stimulus. It is a very common finding that when participants return to a task that they performed recently (i.e., ABA sequence) they are slower and less accurate compared to when they perform a task they did not complete recently (i.e., CBA). The difference in performance—for reaction times (RTs) and accuracy—between ABA and CBA is referred to as the n–2 task repetition cost (Mayr & Keele, 2000).

It is generally accepted (see a review by Koch, Gade, Schuch, & Philipp, 2010) that the n–2 task repetition cost is due to cognitive inhibition. When we switch away from a task, that task continues to be active (Allport, Styles, & Hsieh, 1994) which is thought to interfere with the activation of the next task. Mayr and Keele speculated, that to activate a given task—against a competitor (e.g., previous task's representation)—it is not enough to strengthen activation of the currently relevant task to select it (Houghton & Tipper, 1994). To reduce the interference from an activation of the no longer relevant task, Mayr and Keele argued that irrelevant task's activation must be inhibited to aid selection of the next task (Berg & Schade, 1992). Inhibition is believed to temporarily suppress the abandoned task's representation; and if inhibited task is required soon after it was disengaged from (i.e., in ABA sequence), that task should be less accessible compared to when a current task was not abandoned recently (i.e., CBA sequence). This is exactly what Mayr and Keele found, and they speculated that it was the task-set—a set of rules on how each task is to be performed (Jersild, 1927; Rogers & Monsell, 1995)—that was being inhibited.

The reason Mayr and Keele (2000) argued that it is cognitive inhibition that the n–2 task repetition cost reflects, is that in one of their experiments (Experiment 3) this cost was shown to be driven by top-down processes. Specifically, when no cues were presented—that is, participants had to perform each task in a bottom-up manner—no n–2 task repetition cost was present, but when cues were provided, the cost was observed. Bottom-up processing allows for tasks to be performed as driven by stimulus' perceptual characteristics rather than via effortful, endogenously driven processes. Therefore, the n–2 task repetition cost is believed to arise when conscious control is applied; that is, when conscious manipulation of task-sets is required, and this conscious shifting between task-sets is supported by inhibition. Thus, the n–2 task repetition cost was deemed to be a cognitive inhibition effect, has been replicated as such many times, and seems robust against non-inhibitory explanations (e.g., Koch et al., 2010; Mayr, 2007). Moreover, the inhibition applied to task-sets is thought not to be under top-down control, because the n–2 task repetition cost is not affected by the expectancy effects (Experiment 5, Mayr and Keele); that is, knowing the sequence of tasks in advance (four at a time) does not affect the n–2 task repetition cost. But, inhibition is thought to be triggered as a result of top-down shifting between tasks, suppressing abstract task-sets, hence is referred to as cognitive inhibition.

N–2 Task Repetition Research

Under the understanding that the n–2 task repetition cost reflects cognitive inhibition, it has been used as such in individual (Whitmer & Banich, 2007) and group differences research (Dreher, Kohn, & Berman, 2001; Fales et al., 2006; Lawo et al., 2012; Mayr et al., 2006; Moritz et al., 2004; Prior, 2012). With the n–2 task repetition cost being used to assess cognitive inhibition, there is a need to understand this effect further, so we can have confidence in it. However, despite extensive research around the n–2 task repetition cost, and some evidence that the n–2 task repetition cost is observed for different response modalities (e.g., foot, finger, vocal; Philipp & Koch, 2005) suggesting diversity of inhibitory processes, some questions remain unanswered. For example, we cannot be certain yet what is being inhibited during task-switching (review, Koch et al., 2010). Early research would suggest that inhibition that drives the n–2 task repetition is applied to abandoned task’s representation (Mayr & Keele, 2000). However, there are a few other candidate processes in task-switching which may be targeted by inhibition, if we assume that the n–2 task repetition cost arises as a result of interference (Allport & Wylie, 1999, 2000; Waszak et al., 2003). Specifically, task-set is thought to provide rules on how to perform each task, but in theory, tasks' parameters (e.g., low-level representations of cues, stimuli, responses) activations can also be in conflict, which could be resolved with inhibition (Arbuthnott, 2008b; Grange & Houghton, 2010b; Houghton et al., 2009); although others would argue that inhibition is a global mechanism, rather than a selective one (Gade & Koch, 2012).

Cues. One of the low-level task's parameters which could lead to inhibition being applied are cues, which are often used in many BI paradigms. Cues have been shown to be important for retrieval of information (Jost et al., 2013), and they trigger the retrieval of a previously encountered/ learnt task-set associated with that cue (Grange & Houghton, 2010; Mayr & Kliegl, 2003; based on general episodic memory research, Tulving & Thomson, 1973). Moreover, processing of cues is also argued to contribute considerably to the switch cost (SC, Logan & Bundesen, 2003; Mayr & Kliegl, 2003), as well as the n–2 task repetition cost (Grange & Houghton, 2010b; Houghton et al., 2009).

Specifically to the n–2 task repetition cost, the extent to which a cue translates to a task (i.e., its transparency), seems to be one of the driving factors of this cost (Houghton et al., 2009); that is, the less informative the cue is (e.g., a shape cueing a "parity" task), the larger the n–2 task repetition cost, compared to informative cues (e.g., a word "parity" cueing a "parity" task), which produce smaller costs. Moreover, if half-way through the experiment participants are asked to learn new cues having already learned cue-task mappings, the n–2 task repetition cost' magnitude doubles (Grange & Houghton, 2010b). The cue transparency is thought to influence the level of inhibition applied for the following reason; assuming that upon seeing a cue task-sets are retrieved from long-term memory (LTM, Mayr & Kliegl, 2000, 2003) and loaded into working memory (WM), where it is translated into a task, the efficiency of that translation can depend on transparency of cues. That is, less informative cues require more processing in WM before a decision is made, whereas more informative cues do not require translation, or require less of it (Grange & Houghton, 2010b; Houghton et al., 2009). This results in non-transparent cues and their associated task-sets to linger in WM, leading to a conflict with other task-sets activated between task-switches, compared to transparent cues which need no translation, hence do not lead to conflicts in WM; and conflicts are assumed to be resolved with inhibition, whit less inhibition applied to transparent compared to non-transparent cues.

Response. Other area of evidence would suggest that the n–2 task repetition cost is very likely to be due to a conflict during the response stage of task processing (for a review see, Koch et al., 2010). It appears that, if a response is not required on n–1 trial (i.e., Response–noResponse–Response), the n–2 task repetition disappears, compared to when a response is required on n–1 trial (i.e., Response–Response–Response); this has been interpreted as evidence for response stage being an important factor driving the n–2 task repetition cost (Schuch & Koch, 2003, 2004). Specifically, if competing responses activations become inhibited in between switches, we would expect to see the n–2 task repetition cost; but, if a response was not made, there should be no inhibition, hence no n–2 task repetition cost, as was found by Schuch and Koch (for more evidence for this see Gade & Koch, 2007b; Grzyb & Hübner, 2013; Sinai et al., 2007).

Stimulus. It has been shown also that stimuli processing can affect performance on cognitive task-switching too, as the stimulus can become bound to the task (Waszak et al., 2003), and response (Pösse et al., 2006), and lead to interference, which younger adults seem to be better at overcoming than older adults (Mayr, 2001). Also, when a stimulus' features overlap between tasks (i.e., stimuli-bivalency) compared to when they do not (i.e., stimuli-univalency), processing of the stimulus and/ or responding to it is slower; which is referred to as the bivalency cost (Woodward & Meier, 2003). The important difference between bivalent and univalent stimuli is that, the former type affords multiple responses, whereas the latter type affords only one response. As such, processing of bivalent stimuli taking longer may be due to more reconfiguration required compared to univalent stimuli (Monsell, Yeung, & Azuma, 2000; Rogers & Monsell, 1995), but it can also be due to inter-trial effects, such as negative priming (Allport et al., 1994; Allport & Wylie, 2000). However, the source of the bivalency cost may be more complex as it has been demonstrated that stimulus' bivalency affects participants' strategy (Woodward & Meier, 2003). Specifically, it appears that if among univalent stimuli, a small number of bivalent stimuli is added, participants exercise more caution responding, their responses become slower and are more accurate. In relation to the n–2 task repetition cost, evidence shows that this cost can be present in tasks where stimulus-response mappings overlap (i.e., multivalent stimuli) (D. W. Schneider & Verbruggen, 2008), as well as for tasks that have unique stimulus-response mappings (i.e., univalent stimuli, Costa & Friedrich, 2012; Gade & Koch, 2012). Despite inconsistencies in research observations on stimuli role in task-switching, overall it can be said that stimuli can play an important role in task-switching performance, and therefore should not be discounted.

Other manipulations. Furthermore, assuming that task's individual parameters can be inhibited, and that responses are subject to inhibition, if more time passes from a response being made to the next task, or from a cue to a stimulus, the n–2 task repetition cost should be smaller, because there will be less interference between activated parameters, with decaying inhibition having a lesser influence on other parameters/ tasks. This is supported by experiments manipulating the interval between the response and cue (i.e., response-cue interval, RCI), where the longer the RCI, the smaller the n–2 task repetition cost is (Altmann, 2007; Gade & Koch, 2005; Mayr & Keele, 2000).

In terms of whether inhibition of cues processing is affected by passing time, it seems that varying the cue-stimulus-interval (CSI) does not influence the magnitude of the n–2 task repetition cost (Costa & Friedrich, 2012; Lawo et al., 2012; Mayr & Keele, 2000; Philipp & Koch, 2006); for an alternative view see Grange & Houghton (2011). If a cue processing can be influenced by inhibition, the more time passes between a cue and a stimulus, inhibition should decay, and participants should prepare better—assuming that preparation is successful (DeJong, 2000b)—leading to smaller n–2 task repetition costs, but evidence does not seem to support that view. It is unclear why, if the n–2 task repetition cost is smaller for longer RCI, the same effects are not observed varying CSI.

Episodic Retrieval

Another possible factor that may contribute to effects seen in task-switching (i.e., SC, the n–2 task repetition cost) is episodic retrieval (Neill, 1997). Episodic retrieval can be described as an operation by which the information about most recent exposure to a given stimulus (i.e., declarative and procedural memories; for example, task's parameters such as stimulus' features, distractors, response made) is retrieved automatically upon seeing that stimulus again, either facilitating or impairing performance. Specifically, it seems that performing a task results in this task's features being integrated in the form of a memory trace (also known as an event-file), which can be retrieved as triggered by being exposed to the same stimulus again (i.e., a retrieval of the most recent episode) (Hommel, 1998, 2004, 2009). For example, if upon seeing a given stimulus a retrieved response does not match the currently required action, by the episodic retrieval account, it should lead to an impaired performance because a new response will have to be generated against the one that was initiated automatically through episodic retrieval; however, if the retrieved task's parameters match the demands of the current task, performance should be facilitated (Hommel, 1998, 2000, 2004). The formation of event-files has been suggested to be present at the LTM (Tulving & Thomson, 1973) as well as at the WM level (Baddeley, 2000).

Early research suggesting existence of event-files show that response-related information is integrated with stimuli-related information (Hommel, 1998); that is, stimulus and response features can bound into a common memory structure. In one of Hommels' experiments (1a, 1b) participants were presented with a stimulus (S1)—varying in form, colour, or location—to which they had to make a simple left/ right response (R1) on some trials but not on others; a cue (e.g., in the form of a row of three pointing left/ right pointing arrows) was presented so participants knew in advance which response they were required to make (Left or Right). The R1 response was followed by a second stimulus (S2), to which participants had to make a second response which was a binary-choice (R2); this time participant did not know in advance which response they would make, but the response was made to task-relevant feature of S2. The S1 and R1 acted as primes for stimulus and response, and due to their temporal closeness, they belonged to the same event, but their pairing was not task-related. Out of the three stimulus' features, only form (e.g., stimulus being a letter O or X) was relevant to the task (i.e., R2), and for response features, only location was relevant. And, S1 and S2 features were either matched or mismatched across trials, so were R1 and R2. The authors found that repetitions of colour on its own did not seem to affect performance—this type of priming does not result in global features binding—instead, it would appear that stimulus form (i.e., task-relevant feature) bounded with response location, so did stimulus location and response location; that is, features showed binding into event-files locally and selectively. Specifically, repeating a valid stimulus-response—one presented in priming phase, even though S1 and R1 were not task-relevant—benefits performance of it; or, putting it in other words, performing of S2 and R2 was affected by features' combination on S1 and R1. This fits in with the view that performing a task leads to a memory trace of that task being stored, and then automatically retrieved when that task is encountered again facilitating performance of it (Logan, 1988). Hommel argued, and consequently showed, that colour-feature was not integrated into event-files because it was not task-relevant (Experiment 2); but when colour-feature became relevant to task, it was selectively bound into event-files with response.

For task-switching it means that if a task-set's parameters are repeated across the trials, performance should be faster (as the retrieved information matches memory trace of a task stored in LTM memory) compared to when task-set' parameters mismatch. When the episodic retrieval account was applied to explain some of the effects thought to reflect cognitive inhibition, it has been shown that these effects are attributable to a large extent to automatic episodic retrieval (Spapé & Hommel, 2008; Tipper, 2001) or even explained by it (Fox & de Fockert, 1998). However, in relation to the n–2 task repetition cost, initial investigation of episodic retrieval effects on the n–2 task repetition cost showed that it could not be attributed to, or modulated by episodic retrieval account (Mayr, 2002). In a specially prepared paradigm (Figure 3.1), in which tasks' features can be easily manipulated, Mayr showed that when episodic retrieval was controlled for, there was a numerical reduction in n–2 task repetition costs for episodic matches compared to episodic mismatches; however, episodic retrieval account did not explain the n–2 task repetition cost, as statistically the n–2 task repetition cost was not different for n–2 Response Repetitions and n–2 Response Switches. This was taken as evidence that the n–2 task repetition cost is a result of persisting inhibition rather than episodic retrieval mismatches.

Nevertheless, there is substantial evidence from experiments looking at the SC (an effect of slower responses switching between tasks compared to repeating tasks), which show that this effect is considerably modulated by episodic retrieval (Allport & Wylie, 1999; Altmann, 2011; Goschke, 2000; Horoufchin et al., 2011; Logan & Bundesen, 2003; Schmidt & Liefooghe, 2016). For example, Schmidt and Liefooghe showed that feature integration in task-switching plays an important role in how large the SC is; in Experiment 1 of their study, they manipulated individually whether within a task repetition/ switch, the stimulus and response repeated or not. For task-repetitions, performance was faster when both, the stimulus and response repeated; and when only response was repeated, performance was faster for task-repetitions but led to a cost in task-switches.

These findings have an implication for the interpretation of the source of the SC, because dominant theories argue that the SC is due to a reconfiguration of task-set between switches (Meiran, 1996; Rogers & Monsell, 1995), or due to inhibition of a task that becomes temporarily irrelevant (Allport et al., 1994), emphasising the role of cognitive control causing the SC. However, research on episodic retrieval effects on performance on task-switching show that a large part of the SC is attributable to a more automatic process, namely how task-set parameters are bound and retrieved, that is, how strong the association between the stimulus and the task is (episodic binding, Waszak et al., 2003); event-files, Pösse et al., 2006), and whether retrieved task's memory trace matches or mismatches across the trials.

More evidence for priming effects in task-switching comes from studies that looked at priming of cues (Logan & Bundesen, 2003; Mayr & Kliegl, 2003). It was noted that typically during task-switching experiments, when a task repeats, the cue repeats too, whereas when a task-switches, the cue and the task-switch; as such, it was not certain how much of the SC was due to a task-switch or a cue-switch. Using two cues per task instead of one (Logan & Bundesen, 2003; Mayr & Kliegl, 2003)—allowing for trials where only cue or task switch—it was found that cue-switches contribute largely to the SC and the n–2 task repetition cost; specifically, practice influenced mainly cue-switches, not having much effect on task-switches; preparation affected cue-switches rather than task-switches; and task-switches were sensitive to response-priming effects. Moreover, the n–2 task repetition cost was eliminated under n–2 cue-repetitions. The evidence from studies using two cues is interpreted in two ways; some argue for cue-switch costs being a result of endogenous control (Grange & Houghton, 2010a; Mayr & Kliegl, 2003), whereas others posit that the costs are driven by cue-priming (Logan & Bundesen, 2003).

The theory that SC and the n–2 task repetition cost are driven through endogenous control (e.g., Mayr & Kliegl, 2003) argues that, the role of cue is to retrieve and activate necessary information (first stage), which is then loaded into WM, where retrieved rules are applied to the stimulus resulting in response being generated (second stage). The finding that the n–2 task repetition cost is eliminated under n–2 cue repetitions was interpreted as evidence that, cue encoding and task-set retrieval are automatic and necessary before a reconfiguration in WM can take place, and these two mechanisms are distinct from each other but can interact; and that it is the task-set's rules application in WM that is inhibited (Mayr & Kliegl, 2003). In practice it means that if a task repeats on the next trial no further configuration is needed because the retrieved task-set matches the one in WM, but if a task-switches, the task-set has to be reconfigured. It is assumed that when enough time is given (i.e., at long CSIs) performance on task-repetitions and task-switches should be similar, because cue encoding and task reconfiguration should be complete under long compared to short CSI (Logan & Bundesen, 2003). The endogenous control hypothesis is supported by research looking at cue-transparency's effects on n–2 task repetition costs (Grange & Houghton, 2010a), who showed that when perceptual features within cue-switches were kept constant, the n–2 task repetition cost was present.

The theory on cue-switch costs being driven by automatic priming of cues (e.g., Logan & Bundesen, 2003) is supported by evidence from a study that used *stimulus onset asynchrony* (SOA) to examine the time course of task-switching; this is achieved by manipulating the CSI. Typically, under short CSI the SC is larger compared to long CSI (e.g., Meiran, 1996). For task-switches, if SOA is zero—the cue and the stimulus being presented simultaneously—RT captures cues encoding as well as task-switching processes; but when SOA is longer, the cues can be processed first, before the target appears; as such for longer SOA it is assumed that RTs reflect mainly task-switching processes. This time-course function was modelled (Logan & Bundesen) in two models of task-switching; the first model assumed endogenous control in task-switching, and the second model was under perceptual-priming driving task-switching performance. Under the cue-priming hypothesis, the cue and the target are stored in memory as a compound, and as such on each single trial enough information for a given response can be provided through retrieval, hence no endogenous configuration (i.e., control) should be needed. Specifically, encoded cues can be matched to previously stored cues' representations in LTM and WM; and a cue matching the information held in WM is encoded faster leading to a speedier performance compared to when a cue matches information in LTM. Under task-repetitions, the cue will match information in the WM and LTM, whereas under task-switches, information will match only information in LTM; hence, task-switches take longer to perform compared to task-repetitions. As such, cue repetition priming effects should be seen under short CSI, compared to long CSI. Logan and Bundesen showed that the model under perceptual-priming effects on task-switching is sufficient to explain differences between task-switches and task-repetitions, thereby providing evidence for exogenous mechanisms driving task-switching.

Experiment 2

Taking into account evidence on priming and episodic retrieval effects in task-switching, and noting the numerical trend for the n–2 task repetition cost to be smaller under episodic matches compared to episodic mismatches in Mayr’s (2002) study (a lack of a significant interaction is not indicative of a no modulatory effects), the aim for Experiment 2 was to replicate the study conducted by Mayr, focusing on aspects of the experiment relevant to the investigation of whether episodic retrieval modulates the n–2 task repetition cost. That is, some manipulations in Mayr’s study are not pertinent for the current experiment; for example, measuring the SC, or varying CSI and RCI. To maximise the chance of observing a robust n–2 task repetition cost, a short RCI was utilized, which has been shown to increase the n–2 task repetition cost (Gade & Koch, 2005; Grange & Houghton, 2009; Mayr & Keele, 2000); also, no immediate repetitions were allowed, as the inclusion of those has been shown to reduce the n–2 task repetition cost (Philipp & Koch, 2006).

Method

Participants. Seventy-nine participants were recruited from the School of Psychology at Keele University in exchange for partial course credits or single cash payments of £6.00. Data were used in analysis if participants had an overall accuracy of at least 90%; seventy-six participants met that criterion (mean age 21.63, SD 6.43, fifty-eight females). The inclusion criteria were to be at least 18-years-old, have normal or corrected to normal vision, and understand written and spoken English.

Stopping rule. For all three experiments performed in the current study, samples sizes were determined through what is known as Sequential Bayes Factors (SBF, Schönbrodt, Wagenmakers, Zehetleitner, & Perugini, 2014). This method, unlike the standard power analysis (null hypothesis statistical testing, NHST, i.e., using *p*-values), does not require a researcher to calculate the sample size before the experiment takes place. Instead, the results are assessed by looking at evidence data provide; which in practice means that, data are collected until the null or the alternative hypothesis is supported—BF_{10} reaches a certain threshold—or a pre-set maximum number of participants is reached. BF_{10} of 1/6 or below and of 6 or above, are considered accepted as indication compelling enough evidence to stop collecting data; this is because these thresholds have been shown to have low rates of False Positive Evidence and False Negative Evidence (Schönbrodt et al., 2014)—this guideline was used for the current experiment.

To assess when enough data are gathered to support either of the hypotheses (null or alternative), the strength of evidence is estimated through what is referred to as a Bayes Factor (BF). With BFs the relative evidence for either of the hypotheses explaining data can be quantified. The BF is defined as seen in Equation 3.1

Equation 3.1

$$BF_{10} = \frac{p(D|H_1)}{p(D|H_0)}$$

where, D refers to the actual data, H_0 is the null hypothesis, H_1 is the alternative hypothesis, and p is the probability. The BF_{10} reflects how much more likely either of the hypotheses are given the data, which is evaluated based on comparing probable models explaining patterns seen in data. For example, for Experiment 2 it was of interest if the n–2 task repetition cost is modulated by episodic retrieval, which can be modelled with: 1) null-model: the n–2 task repetition cost does not differ between episodic matches compared to episodic mismatches; or 2) alternative-model: the n–2 task repetition cost differs between episodic matches relative to episodic mismatches. For each model a BF is calculated and then compared; the numbers 1 and 0 by the BF in Equation 3.1 denote that the BF is calculated as a ratio of probability of data under the alternative hypothesis (e.g., a likely difference) compared to the probability of data under the null hypothesis (e.g., an unlikely difference). The larger the BF_{10} is than 1, the more support there is for the alternative hypothesis, and the further away BF is below 1, the more evidence there is for the null-hypothesis. The BF_{10} for Experiment 2 was estimated by comparing BF for the two probable models (null vs alternative hypothesis) with a Bayesian *t*-test (Rouder, Speckman, Sun, Morey, & Iverson, 2009).

One of the advantages of Bayesian analyses is that a prior knowledge (i.e., prior belief) can be used to inform the analysis. For example, a prior belief could be that we expect data (i.e., prior distribution of parameters in the model) to be normally distributed; and, for all three experiments in the current study, a prior on alternative hypothesis was assumed to be characterised by Cauchy distribution with scaling factor r=.707 (the true effect size), which is a *t*-distribution with a single degree of freedom (Gronau, Ly, & Wagenmakers, 2017; Rouder et al., 2009). The collected data's parameters distribution is compared with the prior belief, which gives posterior distribution, which tell us by how much our belief has to be updated; that is, how much more likely data are under the null compared to the alternative hypothesis.

The SBF analysis allows for optional stopping of data collection. As soon as "enough" evidence is gathered for either of the hypotheses (null or alternative hypothesis), data collection is stopped. However, when using SBF, it is a good practice to set a minimum and a maximum sample size before the initial analysis is conducted (Schönbrodt et al., 2015). For all the three experiments in the current study, a minimum of twenty participants were tested initially, then data collection continued until enough evidence was gathered to support either of the hypotheses, with the limitation that if evidence did not support either of the hypotheses at eighty participants, it would be concluded that the results are inconclusive, and data collection would stop. Data collection did not exceed the 80 participants mark for any of the three experiments. At seventy-nine participants (where data of seventy-six were entered into analysis), the BF_{10} reached a criterion at which data collection was planned to stop; that is, the BF_{10} was above 6 (see results section for details), in favor of the alternative hypothesis.

Apparatus and stimuli. The task-switching paradigm was presented on a standard PC with a 17in. monitor via E-Prime v. 2.0 software (Psychology Software Tools, Pittsburgh, PA). Responses were made on a 1-ms precise USB keyboard.

Backward Inhibition paradigm. The stimuli were shown in a black square frame (8cm x 8cm) on a white background; in the center of that frame a cue in the form of a word was shown (“horizontal”, “vertical”, “diagonal”; black Verdana font, size 22). The target appeared inside the frame, in one of the inner corners in the form of a black circle (1cm diameter). Responses were made on a numerical keypad, and responses were spatially–congruent to the corners of the frame; that is, each corner of the frame had one keyboard-key assigned: 1 (lower-left), 2 (lower-right), 4 (upper-left), and 5 (upper-right) were used. If the participant gave an incorrect answer, they saw “Error” message on the screen (red Verdana font, size 22).

Procedure. Participants attended a single session during which they performed a version of task-switching paradigm based on Mayr (2002). A single trial consisted of a cue, stimulus, and response. Each trial started with a presentation of a black square frame; above that frame the cue appeared (150ms); inside the frame, in one of the inner corners the target (a black circle) appeared. Participants had to make a spatial transformation of the target; that is, they had to indicate in which inner corner of the black frame, would the target move to, if it moved according to the cued task (Figure 3.1). So, if the cue was “horizontal” and the target was in the lower-left corner, it would move to the lower-right corner. In this example the participant had to press the key 2 to give the correct answer. As soon as participants made a response, the cue and stimulus they responded to disappeared; 150ms (RCI) later another cue appeared above the frame followed by another target. If the participant gave an incorrect answer, they saw the “Error” message on the screen.

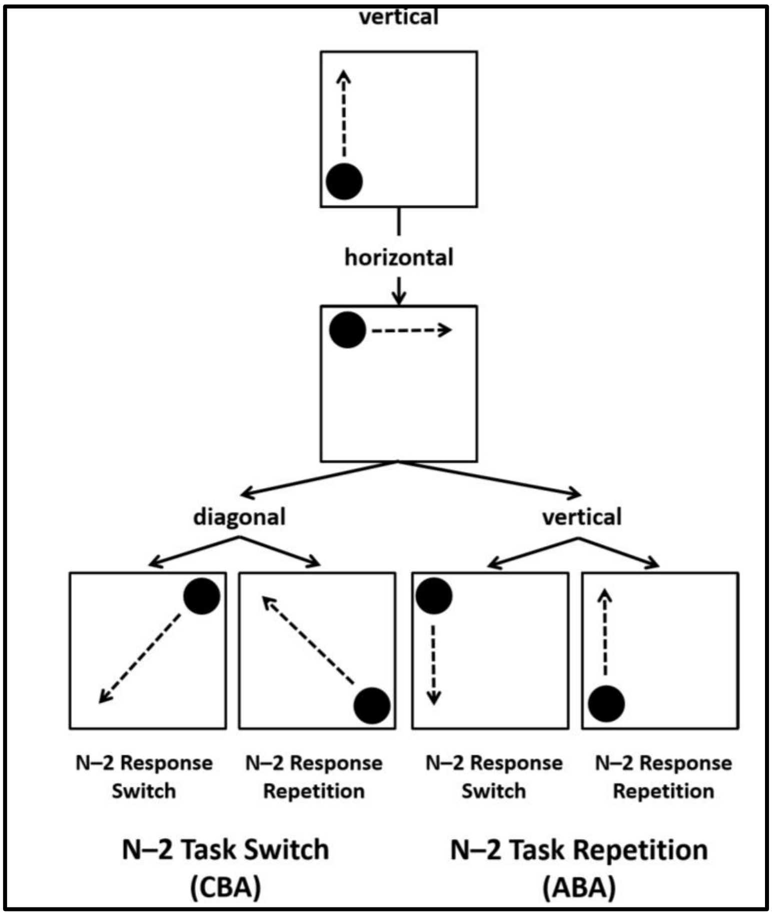


Figure 3.1 Schematic overview of Mayr's (2002) task-switching paradigm. The dotted arrows indicate correct answers and they were used only for instruction purposes.

Each participant performed 480 trials; to optimise participants' performance (i.e., to avoid participants becoming fatigued) all the trials were split into four blocks of 120 trials in each, with breaks (participant-paced) between the blocks. Participants always switched between the three different tasks (i.e., horizontal, vertical, diagonal). It was emphasised to participants that they had to respond as fast and as accurate as possible. Instructions were given to participants verbally, after which participants had a practice consisting of sixteen trials. If four or more errors during the first practice were made, participants were offered another practice with sixteen trials; however, no more than two practices were allowed per participant.

Each current trial n was compared to the task on the n–2 trial allowing for trials to be registered as ABA or CBA sequences. Importantly, the E-Prime software registered which response-key participants should press on each trial; as such, it was possible to code for whether response on n trial, was an n–2 Response Repetition (n–2 RR) or a n–2 Response Switch (n–2 RS) within ABA and CBA sequences.

Design. A within-subjects design was used to examine RTs (ms) and accuracy (%) as dependent variables, and *Task Sequence* (ABA vs. CBA), and *Response* (n–2 RR vs. n–2 RS) as independent variables.

Results

For the current study two types of analysis were conducted: the standard null-hypothesis testing (assessed via *p*-values), and a Bayesian alternative (assessed via Bayes Factors); both are reported below.

Data trimming. Before the data were put into the analysis, data trimming was performed. Initially, participants whose overall performance was below 90% were removed. Then, for accuracy analysis, the null trials were removed, as these trials were not classifiable as ABA or CBA sequence (i.e., first two trials of each block, two trials following error). For the RT analysis, further trimming was performed; that is, errors trials were removed, as well as RT faster than 150ms, and RTs slower than 2.5 standard deviation from participant’s mean. Data trimming led to 16.4% of trials being removed (Response Repetitions 15.6%, and Response Switches 16.7%).

Reaction time analysis. RTs were analysed via a 2x2 repeated measures ANOVA with two factors: *Task Sequence* (ABA vs. CBA), and *Response* (n–2 RR vs. n–2 RS). Mean and standard errors for RTs and accuracy are contained in Table 3.1. The analysis revealed that there was a significant main effect of Task Sequence, $F(1,75) = 94,14$, $p < .001$, $\eta_g^2 = .018$; that is, participants performed CBA faster (958ms) than ABA sequence (1,025ms). There was also a significant main effect of Response, $F\ (1,75) = 18.21$, $p < .001$, $\eta_g^2 = .004$; that is, performance was overall faster for n–2 RR (976ms) compared to n–2 RS (1,007ms). Most importantly, there was an interaction between the Task Sequence and Response, $F(1,75) = 9.60$, $p < .003$, $\eta_g^2 = .002$ (Figure 3.2); that is, the n–2 task repetition cost was considerably smaller for n–2 RR (48ms) compared to the n–2 task repetition cost for n–2 RS (86ms). The density functions for the n–2 task repetition cost's distributions, under n–2 RR and n–2 RS, are depicted in Figure 3.3. Individual t-tests revealed that the n–2 task repetition was significant for n–2 RR, $t(75) = 4.2$, $p < .001$, 95%CI [26,70], as well as for n–2 RS, $t(75) = 13.00$, $p < .001$, 95%CI [73, 99].

Table 3.1 Mean Response times (RTs) and accuracy (%), with standard errors, for ABA and CBA sequences, within n–2 Response Repetitions (RR) and n–2 Response Switches (RS).

	Task Sequence			
	ABA		CBA	
	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
n–2 RR	1000 (28)	96.20 (.39)	952 (28)	96.36 (.39)
n–2 RS	1050 (29)	95.02 (.35)	964 (28)	95.81 (.34)

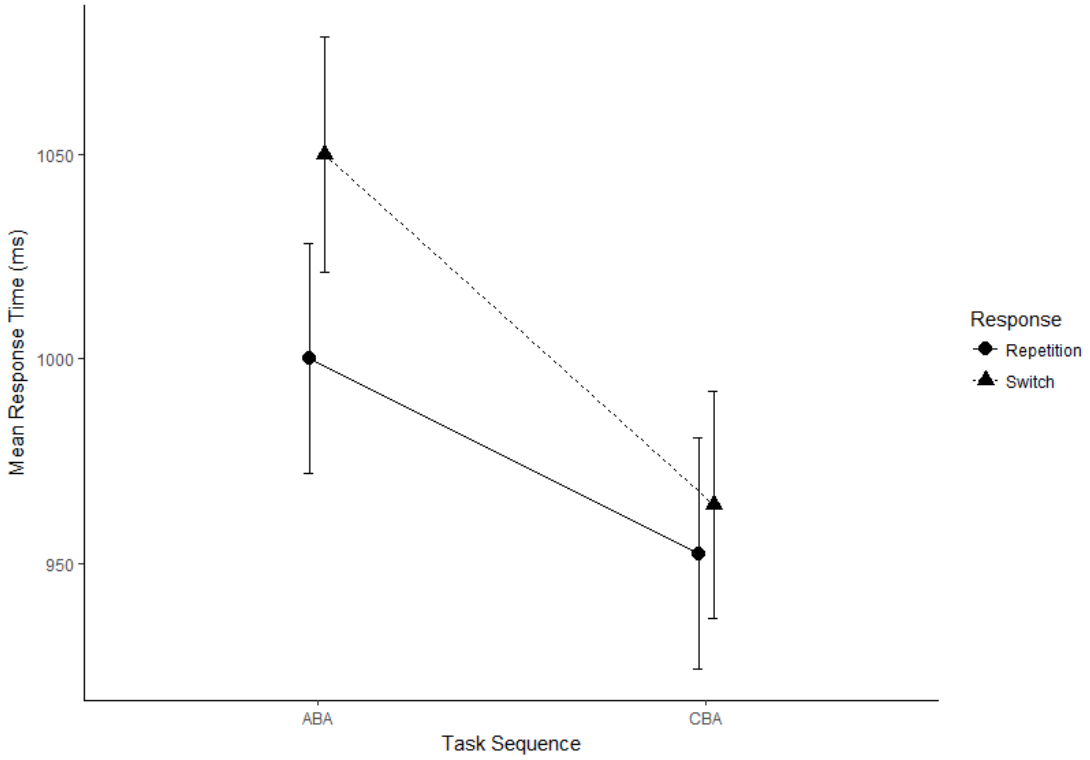


Figure 3.2 An illustration of a two-way interaction for mean response times between Task Sequence (ABA vs. CBA) with Response (n–2 Repetition vs. n–2 Switch). Error bars denote ±1 SE around the mean.

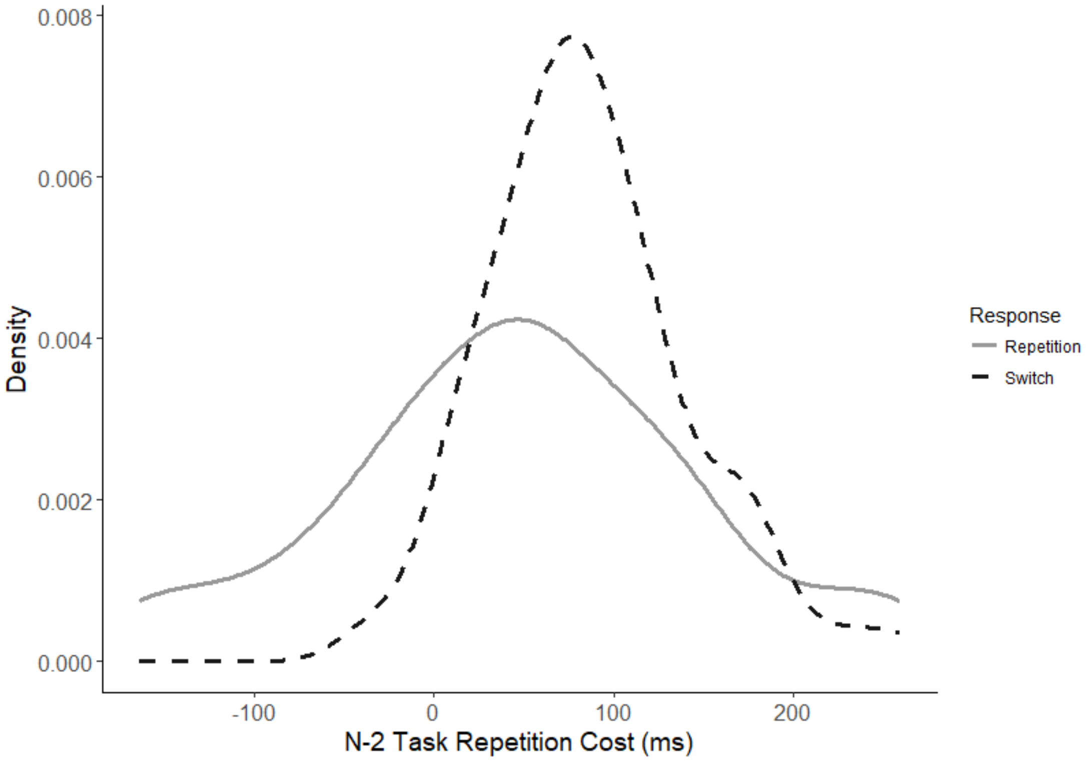


Figure 3.3 Density functions of the reaction time (RT) the n–2 task repetition costs distributions under n–2 Response Repetitions and n–2 Response Switches, calculated as RT (ABA) - RT (CBA).

***Bayesian analysis*.** The BF_{10} for the alternative-hypothesis (presence of difference of the n–2 task repetition cost between n–2 RR and n–2 RS) compared to the null-hypothesis (absence of difference of the n–2 task repetition cost between n–2 RR and n–RS) was 9.97. That is, the data are about 10 times more likely to be explained by the alternative-hypothesis (i.e., presence of difference of the n–2 task repetition cost between Response Repetition and Response Switch) than the null-hypothesis. This is considered as strong evidence for the n–2 task repetition cost being modulated by episodic retrieval.

Accuracy analysis. Accuracy was analysed via a 2x2 repeated measures ANOVA with two factors: *Task Sequence* and *Response*. The main effect of Task

Sequence was not significant, $F(1,75) = 3.36$, $p = .07$, $\eta_g^2 = .006$. The only significant result for accuracy analysis was the main effect of Response, $F(1, 75) = 12.17$, $p < .001$, $\eta_g^2 = .01$; that is, participants were more accurate performing n–2 RR (96.28%) compared to n–2 RS (95.41%). The interaction between the Task Sequence and the Response was not significant, $F(1,75) = 1.32$, $p = .25$, $\eta_g^2 = .003$. The visualization of interactions are in Figure 3.4, and density functions distributions for accuracy n–2 task repetition cost under n–2 RR and n–2 RS are provided in Figure 3.5.

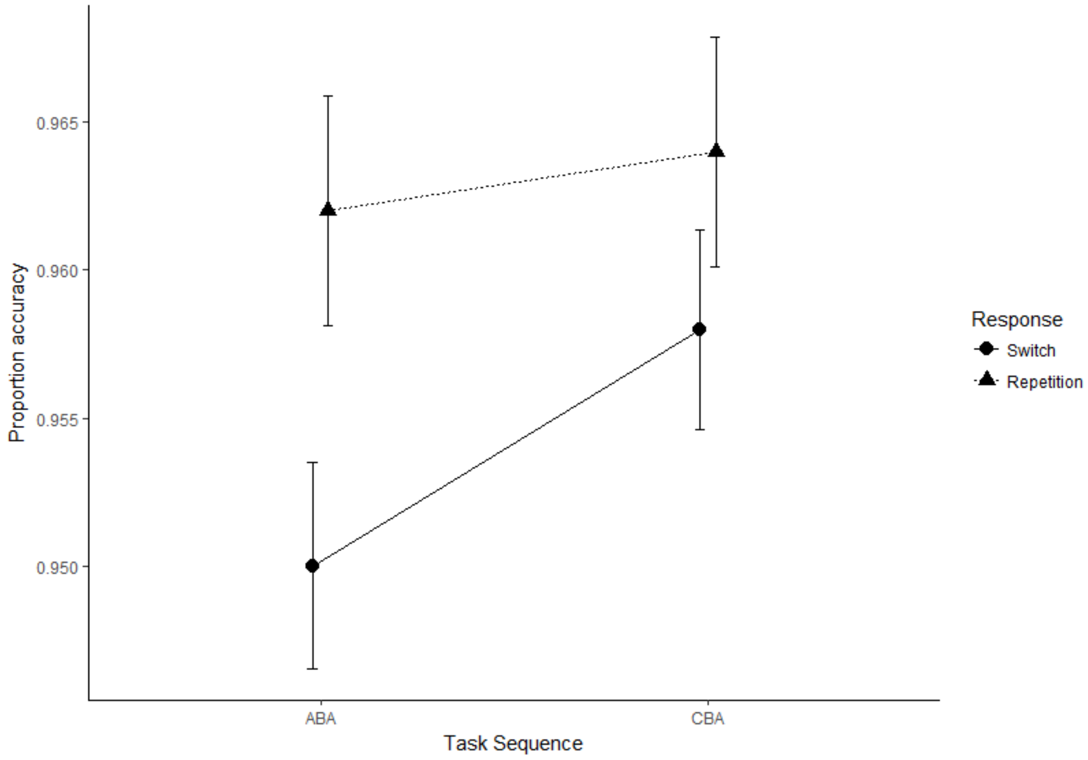


Figure 3.4 An illustration of a two-way interaction for mean accuracy between Task Sequence (ABA vs. CBA) with Response (n–2 Response Repetition vs. n–2 Response Switch). Error bars denote ±1 SE around the mean.

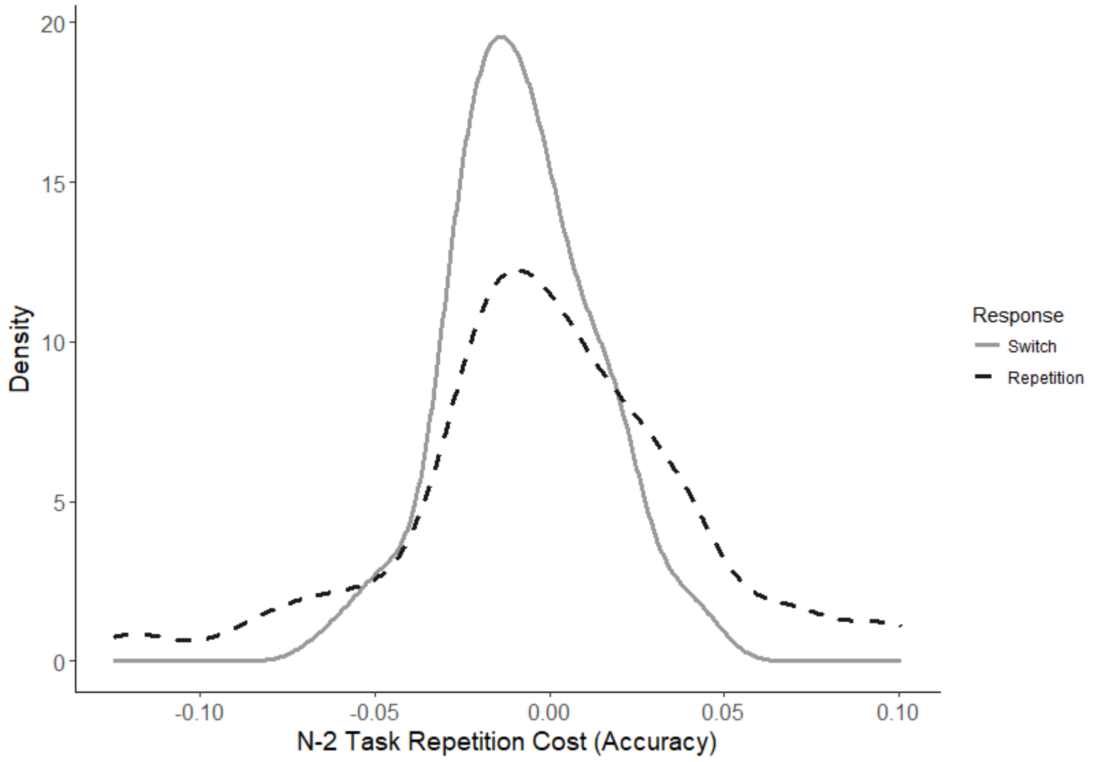


Figure 3.5 Density functions of the accuracy the n–2 task repetition costs distributions under n–2 Response Repetitions and n–2 Response Switches, calculated as % Accuracy (ABA) - % Accuracy (CBA).

Reliability analysis. There is evidence that the n–2 task repetition cost is not a reliable measure (Kowalczyk & Grange, 2017; Chapter 2), and one of the suggestions for why that reliability may be low is that perhaps the n–2 task repetition cost is not a pure measure of inhibition. Specifically, the n–2 task repetition cost may be confounded by episodic retrieval mismatches—which the current study confirmed—therefore, the reliability of n–2 task repetition was analysed to see whether controlling for episodic retrieval improves its reliability compared to that reported by Kowalczyk and Grange.

RT and accuracy n–2 task repetition costs under n–2 RR and n–2 RS were analysed for reliability using the split-half reliability analysis. For every participant data were split randomly into two halves, and n–2 task repetition costs for each half (under n–RR and n–2RS) were calculated, followed by the split-half reliability analysis being conducted (Congdon et al., 2012; Kowalczyk & Grange, 2017). The Pearson product-moment correlation analysis between the two halves was conducted for each data split; this was repeated 500 times, and then a median reliability coefficient was obtained for n–2 task repetition cost under n–2 RR and n–2 RS.

When split-half reliability is used, the total number of data points is reduced which can reduce the reliability, so to account for splitting of data, the median reliability coefficients for RTs and accuracy were adjusted using the Spearman-Brown correction, which is given by

Equation 3.2

$$r_c = \frac{Nr}{1 + (N - 1)r}$$

where r is the Pearson product-moment coefficient and N is the number of “tests” being combined (for the current study there were two halves, so $N = 2$).

Typically, it is expected that for a measure to be reliable its reliability coefficient r_c should be at least .7 (Cronbach, 1951; Picardi & Masick, 2013; Revelle, 2014), this is equivalent to $r \approx .5385$ for the uncorrected reliability coefficient. The results for the current study’s reliability analysis are seen in Figure 3.6; these graphs are violin plots (used to represent continuous distribution) which include a marker of median of the data (the white dot) with an interquartile range (black box around the white dot), as well as a double kernel probability density of the data at different values (Adler, 2005). The dashed line represents the criterion for uncorrected reliability coefficient.

Response times. The median value for the uncorrected reliability coefficient for the n–2 task repetitions cost under n–2 RR it was $r = .34$ and for the n–2 task repetition cost under n–2 RS it was $r = .15$; these translate to corrected reliability coefficients of, $r_c = .34$ (for n–2 RR) and $r_c = .26$ (for n–2 RS).

Accuracy. The median value for the uncorrected reliability coefficient for the accuracy n–2 task repetition cost under n–2 RR was $r = .27$, and for the accuracy n–2 task repetition cost under n–2 RS it was $r = .-15$; which translates to $r_c = .42$ (for n–2 RR), and $r_c = .26$ (for n–2 RS).

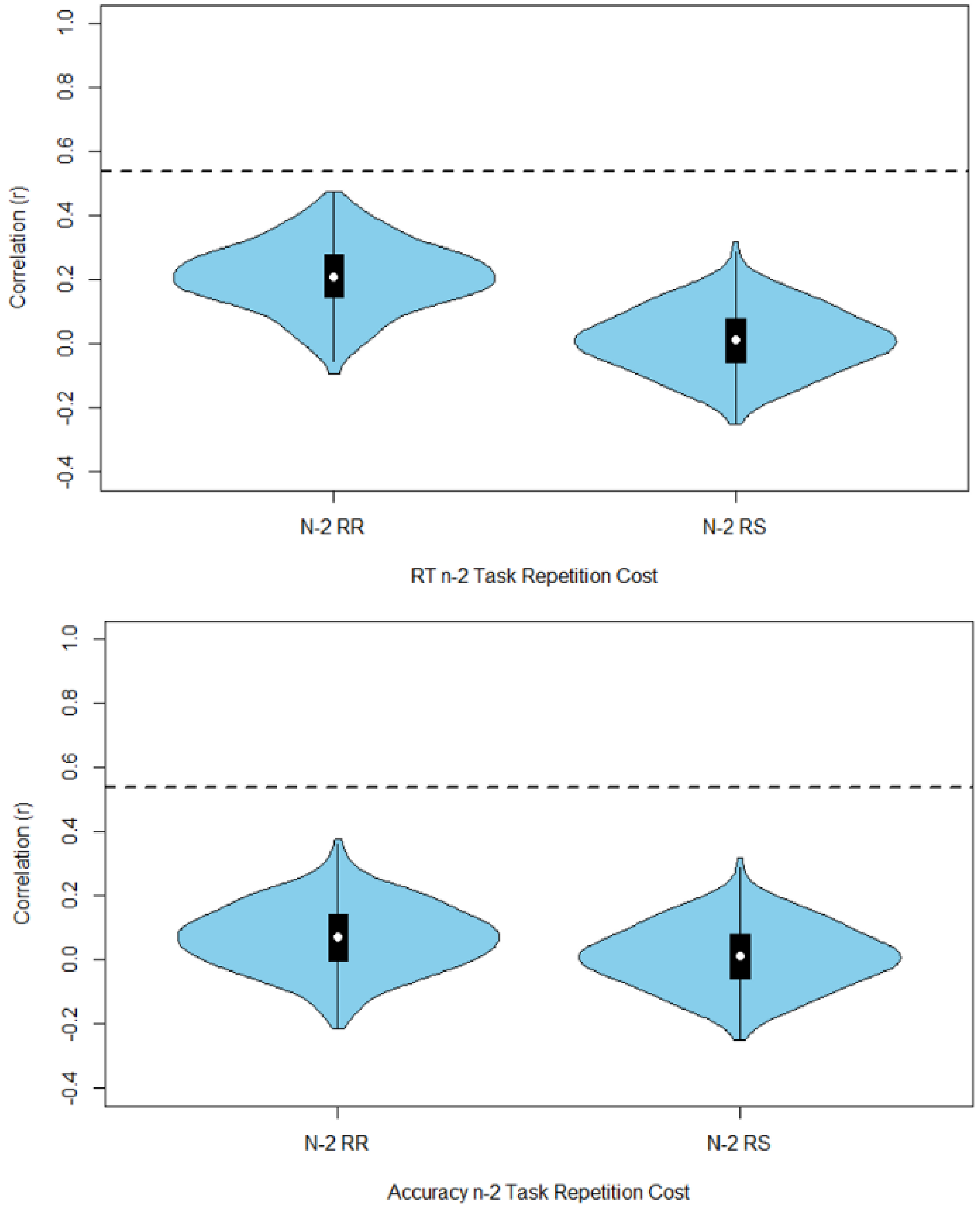


Figure 3.6 Reliability checks for the n–2 task repetition cost; reaction times at the top panel, and accuracy at the bottom panel. Violin plots represent uncorrected, bootstrapped split–half estimates of correlation coefficients (r). On the left are plots for n–2 Response Repetitions (N–2 RR), and on the right are plots for n–2 Response Switches (N–2 RS). The horizontal dashed lines represents the criteria for reliability ($r \approx .5383$; Picardi & Masick, 2013; Revelle & Condon, 2014).

Discussion

Experiment 2 was conducted to investigate if the n–2 task repetition cost is modulated by episodic retrieval. Mayr's (2002) study showed that this effect was not explained by episodic retrieval; however, a numerical trend for the n–2 task repetition cost to be smaller under episodic matches (the n–2 RR) compared to episodic mismatches (the n–2 RS) was observed. Therefore, a close replication of Mayr's experiment was run, with a few adjustments to the paradigm to ensure a strong n–2 task repetition costs. The current experiment yielded a robust n–2 task repetition cost, which was considerably smaller for n–2 RR compared to n–2 RS; which means that a large proportion of the n–2 task repetition cost was due to non-inhibitory mechanism (i.e., episodic retrieval mismatches). This means that n–2 task repetition costs reported in literature (as discussed in reviews, Kiesel et al., 2010; Koch et al., 2010), are also likely to be confounded by non-inhibitory processes, because episodic retrieval is not controlled for in BI paradigms. To summarise, the extent to which the n–2 task repetition cost measures inhibition is questioned, providing evidence against generally accepted view that this cost reflects cognitive inhibition (Koch et al., 2010; Mayr, 2002, 2007).

Experiment 3

Experiment 2 showed that performance on task-switching can be modulated by episodic retrieval; specifically, the n–2 task repetition cost—thought to reflect cognitive inhibition—was shown to be in large proportion due to episodic retrieval. This is consistent with research showing that the SC is susceptible to general priming (Altmann, 2005; Logan & Schneider, 2006b; Logan et al., 2007), and episodic retrieval effects (Allport & Wylie, 1999; Altmann, 2011; Goschke, 2000; Horoufchin et al., 2011; Logan & Bundesen, 2003); and with studies showing that stimuli and responses' features—specifically the way they are paired—can bind into event-files in memory (Hommel, 1998), priming subsequent processing of that task, facilitating or impairing performance (Schmidt & Liefooghe, 2016). As such, at this stage it is not clear whether only certain, or all parameters (e.g., cue, stimulus, response) within the BI paradigm are sensitive to episodic retrieval effects.

Experiment 3 looked at how cue-processing affects the n–2 task repetition cost when episodic retrieval is controlled for. Research on cue-transparency effects on the n–2 task repetition cost (Grange & Houghton, 2010b; Houghton et al., 2009) suggests that inhibition—measured with the n–2 task repetition cost—is triggered as a result of non-transparent cues' activations lingering in WM leading to conflicts; with transparent cues requiring no or less translation in WM hence leading to less conflicts, compared to non-transparent cues, which trigger more inhibition. However, assuming that episodic retrieval can depend on the quality of cues—how accurately and efficiently they aid retrieval—it can be argued, that since non-transparent cues are more difficult to interpret compared to transparent cues, and transparent cues are processed with no need for translation, episodic retrieval should facilitate performance to a larger extent under non-transparent compared to transparent cues.

Also, under the cue-priming hypothesis (Logan & Bundesen, 2003) a cue is stored in a cue-target compound, and it can be assumed that cues that have stronger relationship with the target (i.e., as seen in transparent cues) compared to those that have a weak association (i.e., seen in non-transparent cues), lead to more efficient retrieval. Therefore, the episodic retrieval effect should be larger under non-transparent comparted to transparent cues, as they can benefit more from episodic priming.

The cues used in Experiment 2 were relatively transparent (Houghton et al., 2009)—words “horizontal”, “vertical”, and “diagonal”—and in Experiment 3, new cues were used to represent very transparent and non-transparent cues.

Method

Participants. Sixty-six participants’ data were used in the main analysis. All participants came from the same sample pool as in Experiments 1–2. The number of participants was determined by Sequential Bayesian Factor as in Experiment 2; however, this time the compared models were different.

For Experiment 3 it was of interest whether the n–2 task repetition cost is modulated by Response and/ or Cue-Type. A Bayesian factorial analysis (Morey, Rouder, & Jamil, 2015; Rouder, Morey, Verhagen, Swagman, & Wagenmakers, 2016) was conducted with the n–2 task repetition cost as a DV, and Cue-Type (Arrows vs. Shapes) and Response (n–2 RR vs. n–2 RS) as IVs. The probable models used to describe data were: 1) the main effect of Cue-Type; 2) the main effect of Response; 3) the two main effects of Cue-Type and Response; 4) the two main effects (Cue-Type and Response) and an interaction between them. Participants were entered into each model as a random effect, and each model was compared to a common denominator which was that the n–2 task repetition cost was best explained by the random effect of participants. At sixty-six participants the BF_{10} for the best (Model 4) compared to the second best model (Model 3) was above 6 therefore data collection was stopped. The model with two main effects (Cue-Type and Response) and interaction between them (Model 4) was over seven times more likely than the model with the main effect of Cue-Type and the main effect of Response (Model 3).

Apparatus and stimuli. BI paradigms were presented on a standard PC with a 17in. monitor via E-Prime v. 2.0 software (Psychology Software Tools, Pittsburgh, PA). Responses were made on a 1-ms precise USB keyboard.

Backward Inhibition paradigm. The stimuli and the set-up of the experiment was the same as in Experiment 2, except that two types of cues were used, Shapes or Arrows (2.5 cm in width and height). For the Shapes cues, a "hexagon" was used for the vertical, a "square" for a diagonal, and a "triangle" for a horizontal task. For the Arrows cues, two vertical arrows were used for the vertical, two horizontal arrows for the horizontal, and two diagonal arrows for the diagonal task (see Figure 3.7). Shapes cues were used as non-transparent cues, and Arrows cues were used as transparent cues; where, the former type was chosen as suitable for abstract cue-target associations—which are difficult to translate into tasks—whereas the latter type was chosen as suitable for informative cue-target associations, which easily translate into tasks.

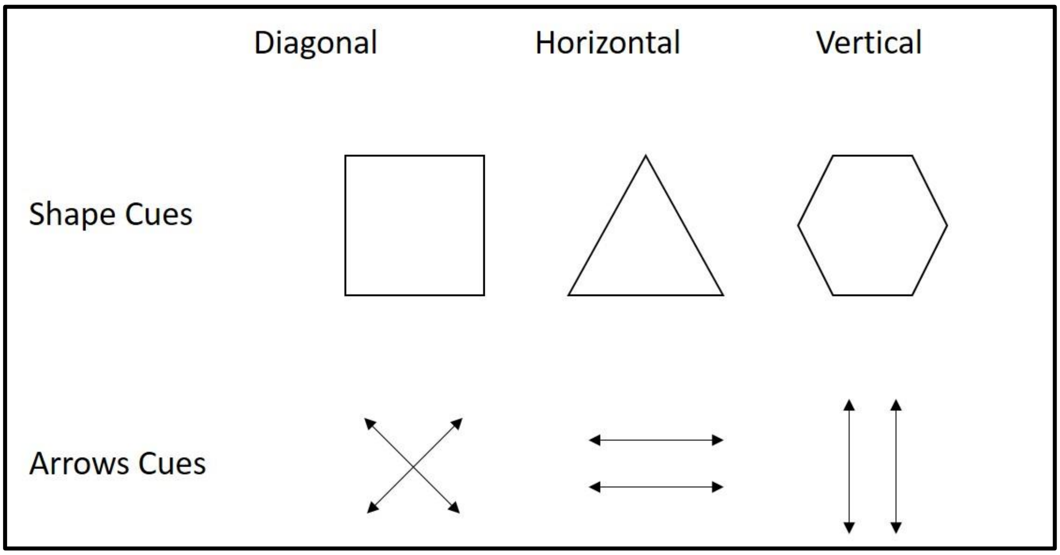


Figure 3.7 A visual representation of cues used in Experiment 3.

Procedure. Participants attended a single session during which they performed BI paradigms with two different types of cues; one half with “Shapes” and second half with “Arrows” cues. The order of these two halves was counterbalanced across participants. Before performing each BI paradigm with different cues, participants learnt relevant cue-to-task relationships and practiced them for 16 trials. If they made four or more than four errors in the first practice, they were given another practice; however, no more than two practices per participant were allowed.

For each BI paradigm (i.e., with Shapes and Arrows cues) individual participants performed 360 trials, and to optimise participants’ performance all trials were split into three blocks of 120 trials in each BI paradigm, with participant-paced breaks between the blocks. As in Experiment 2, no immediate task-repetitions were allowed (Koch et al., 2006). Participants were told to be as fast and as accurate as possible (as in Experiment 2), and the timings (i.e., cue/ stimulus etc.) were the same. Data were collected in the same way as in Experiment 2; that is, the same code for recording task sequences and responses was used.

Design. A within-subjects design was used to examine RTs (ms) as accuracy (%) as dependent variables, and *Task Sequence* (ABA vs. CBA), *Response* (n–2 RR vs. n–2 RS), and *Cue-Type* (Arrows vs. Shapes) as independent variables.

Results

Data trimming. As in Experiment 2, data trimming involved initially removing data of participants whose overall performance was below 90%; then for accuracy analysis, the null trials were removed (i.e., first two trials of each block, two trials following an error). For the RT analysis, further trimming was performed; errors trials, as well as RT faster than 150ms, and slower than 2.5 standard deviation from participant's mean were removed. Overall, 14.7% of trials were removed; 16.4% for Shapes (16.3% for n–2 RR; 16.5% for n–2 RS), and 13% for Arrow cues (12.8% for n–2 RR; 13.1% for n–2 RS).

Reaction time analysis. The three-way repeated measures ANOVA—for which descriptive statistics are in Table 3.2—yielded all main effects and interactions significant. The main effect of Cue-Type was significant, $F(1,65) = 247.98$, $p < .001$, $\eta_g^2 = .4$ (720ms for Arrow, 1067ms for Shapes); so was the main effect of Task Sequence $F(1,65) = 61.75$, $p < .001$, $\eta_g^2 = .01$ (916ms for ABA, 871ms for CBA); and the main effect of Response, $F(1,65) = 24.53$, $p < .001$, $\eta_g^2 = .006$ (888ms for n–2 RR, 909ms for n–2 RS).

In terms of interactions, there was a significant interaction between Cue-Type and Task Sequence, $F(1,65) = 15.85$, $p < .001$, $\eta_g^2 = .003$. The n–2 task repetition cost was smaller for Arrow (22ms) compared to Shapes cues (68ms); and the n–2 task repetition cost was present under Shapes, $t(130) = 6.1$, $p < .001$, 95%CI [46, 90], as well as under Arrow cues, $t(130) = 4.2$, $p < .001$, 95%CI [11, 32]. There was also a significant interaction between Cue-Type and Response, $F(1,65) = 16.1$, $p < .001$, $\eta_g^2 = .003$; namely, the difference between n–2 RR and for n–2 RS was smaller, and not significant—$t(130) = -1.6$, $p = 1$, 95%CI [-18, 2]—for Arrows (8ms) compared to Shapes cues (53ms), $t(130) = -4.7$, $p < .001$, 95%CI [-76, -31]. A third significant interaction was between Task Sequence and Response, $F(1,65) = 14.87$, $p < .001$, $\eta_g^2 = .004$; specifically, the n–2 task repetition was smaller, and not significant, under n–2 RR (20ms), $t(130) = 1.9$, $p = .06$, 95%CI [-.8, 40], compared to n–2 RS (70ms), $t(130) = 10$, $p < .001$, 95%CI [57, 83]. The interaction between the Task Sequence and Response is a replication of the results from Experiment 2, which showed that the n–2 task repetition cost was modulated by episodic retrieval. Importantly, there was significant three-way interaction between Task Sequence and Response across Cue-Types, $F(1,65) = 8.9$, $p < .005$, $\eta_g^2 = .002$, suggesting that the n–2 task repetition cost for n–2 RR and n–2 RS differed across the two Cue-Types (Figure 3.8). Separate ANOVAs for Shapes and Arrows cues, revealed the following.

Under Shapes cues, the main effect of Task Sequence was significant, $F(1,65) = 47.69$, $p < .001$, $\eta_g^2 = .02$; so was the main effect of Response, $F(1,65) = 26.94$, $p < .001$, $\eta_g^2 = .01$. And the interaction between Task Sequence and Response was significant too, $F(1,65) = 13.37$, $p < .001$, $\eta_g^2 = .008$. Individual *t*-tests showed that for n–2 RR the n–2 task repetition cost was not present, $t(65) = 1.4$, $p = .2$, 95%CI [-11, 64], but it was observed under n–2 RS, $t(65) = 10.0$, $p < .001$, 95%CI [89, 129].

Under the Arrows cues, the main effect of Task Sequence was significant, $F(1,65) = 13.12$, $p < .001$, $\eta_g^2 = .004$; but the main effect of Response was not significant, $F(1,65) = 1.99$, $p = .16$, $\eta_g^2 = .0006$. The interaction between Task Sequence and Response was significant, $F(1,65) = 4.90$, $p = .03$, $\eta_g^2 = .0007$. Individual *t*-tests revealed that for n–2 RR the n–2 task repetition cost was not present, $t(65) = 1.5$, $p = .1$, 95%CI [-4, 29], but it was observed for n–2 RS, $t(65) = 5.3$, $p < .001$, 95%CI [19, 42].

Specifically, the n–2 task repetition cost differed more between n–2 RR and n–2 RS for Shapes compared to Arrows cues (Figure 3.9); for density functions of n–2 task repetition costs under Shapes and Arrows cues see Figure 3.10.

Table 3.2 Mean Response times (RTs) and accuracy (%), with standard errors, for ABA and CBA sequences, within Response Repetitions (RR) and Response Switches (RS), for Arrows-Cues and Shapes.

	Task sequence			
	ABA		CBA	
Cue type	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
Arrows				
n–2 RR	722 (22)	98.05 (.29)	709 (20)	96.64 (.40)
n–2 RS	739 (20)	96.36 (.32)	709 (20)	97.49 (.22)
Shapes				
n–2 RR	1053 (29)	96.90 (.40)	1027 (30)	95.75 (.49)
n–2 RS	1148 (28)	94.20 (.47)	1039 (30)	96.66 (.30)

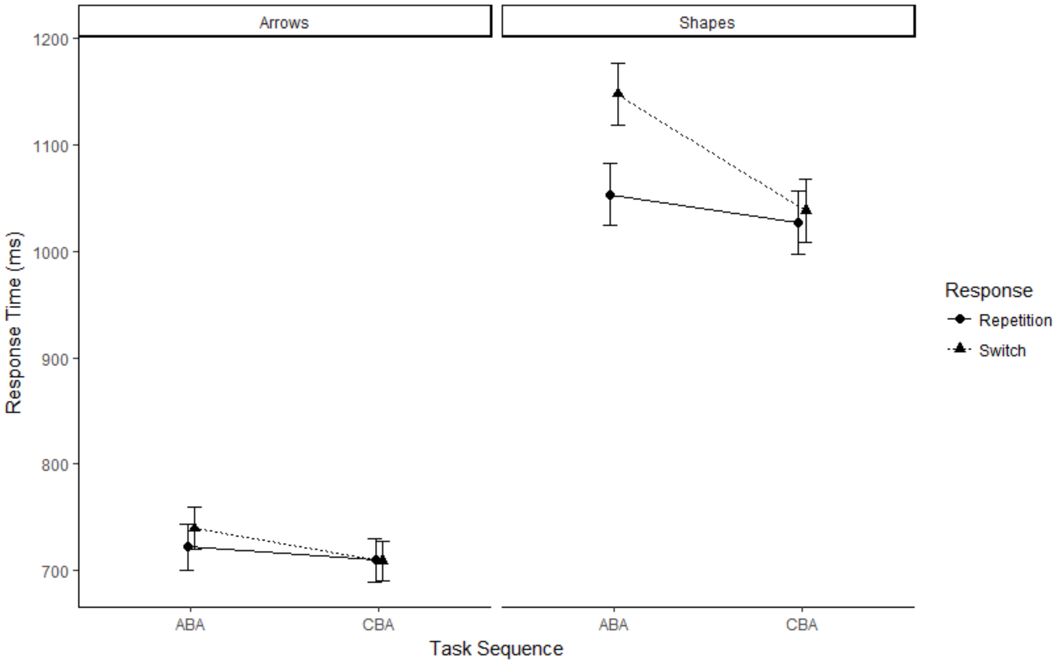


Figure 3.8 A three-way interaction between the Cue-Type, Task Sequence (ABA vs. CBA), and Response (Repetition vs. Switch). Error bars represent $\pm 1\ SE$ around the mean.

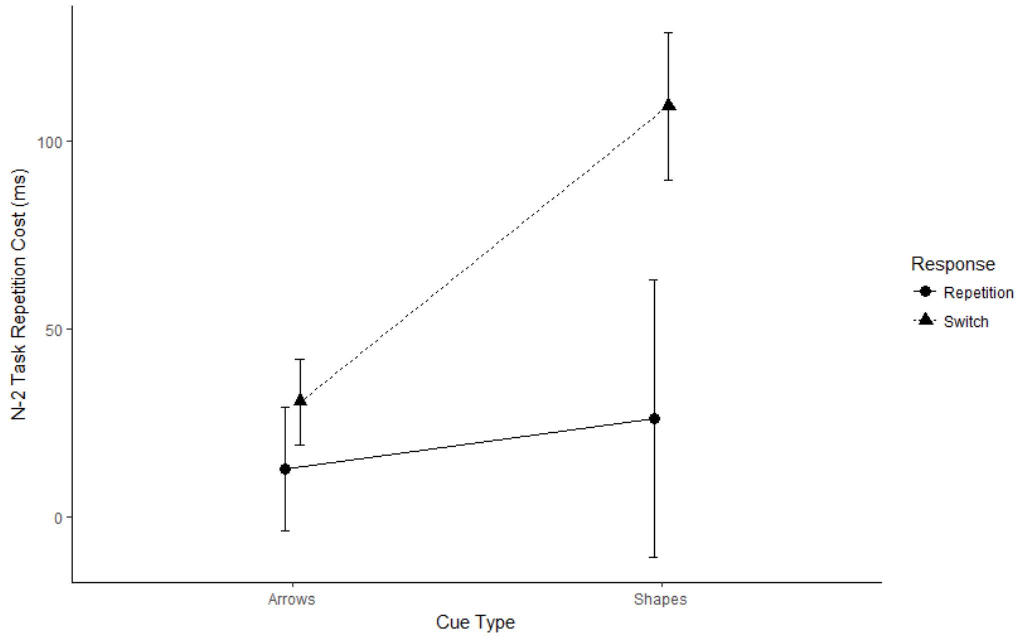


Figure 3.9 Reaction times n–2 task repetition costs for n–2 Response Repetition and n–2 Response Switch under Shapes/ Arrows cues. Error bars represent ±1 SE around the mean.

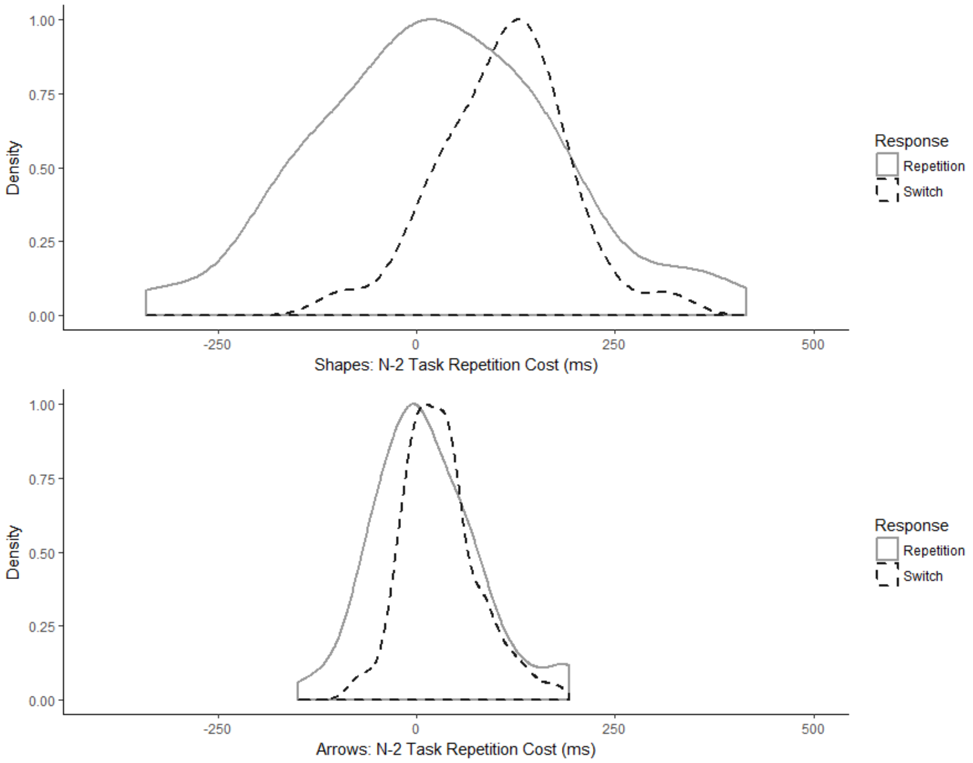


Figure 3.10 Density functions for reaction time (RT) n–2 task repetition costs distributions for n–2 Response Repetitions (Repetitions) and n–2 Response Switches (Switches), calculated as RT (ABA) - RT (CBA); on the top for Shapes cues and on the bottom for the Arrows cues.

Bayesian analysis. The model of interest was the model 4—with two main effects (Cue-Type and Response Repetition) and interaction between them—which was compared to the model with the highest BF; in this case it was model 3—with two main effects (Task Sequence and Response). Comparing these two models' BF—model 4 (BF 1,227,034)/ model 3 (BF 154,746)—gave a BF$_{10}$ of 7.93. That means, the model 4 was almost eight times more likely to account for patterns seen in data. This is considered as moderate evidence for the–2 task repetition cost being more modulated by n–2 RR under Shapes compared to Arrows cues.

Accuracy analysis. The three-way repeated measures ANOVA revealed the following results; the main effect of Cue-Type was significant, $F(1,65) = 16.58$, $p <$

.001, η_g^2 = .04 (97.13% for Arrows cues, 95.88% for Shapes cues; but the main effect of Task Sequence was not significant, *F*(1,65) = 1.38, *p* = .24, η_g^2 = .002. The main effect of Response was significant, *F*(1,65) = 12.81, *p* < .001, η_g^2 = .01 (96.81% for n–2 RR, 96.18% for n–2 RS).

The interaction between Task Sequence and Cue-Type was not significant, *F*(1,65) = 3.80, *p* = .056, η_g^2 = .004; neither was the interaction between Cue-Type and Response, *F*(1,65) = 1.33, *p* = .25, η_g^2 = .002. But the interaction between Task Sequence and Response was significant, *F*(1,65) = 51.97, *p* < .001, η_g^2 = .06; for n–2 RR there was an n–2 task repetition benefit of .013, and for the n–2 RS there was an n–2 task repetition cost of -.018. Individual *t*-tests showed that the n–2 task repetition benefit was significant under n–2 RR *t*(130) = 3.8, *p* < .001, 95%CI [.006, .20], and the n–2 task repetition cost was significant under n–2 RS, t(130) = -7.1, *p* < .001, 95CI% [-.023, -.013].

The three-way interaction between the Task Sequence, Response and Cue-Type, was not significant, *F*(1,65) = 1.69, *p* = .20, η_g^2 = .002 (see Figure 3.11 for interactions visualization, Figure 3.12 graph for averaged n–2 task repetition costs, Figure 3.13 for density functions of n–2 task repetition costs).

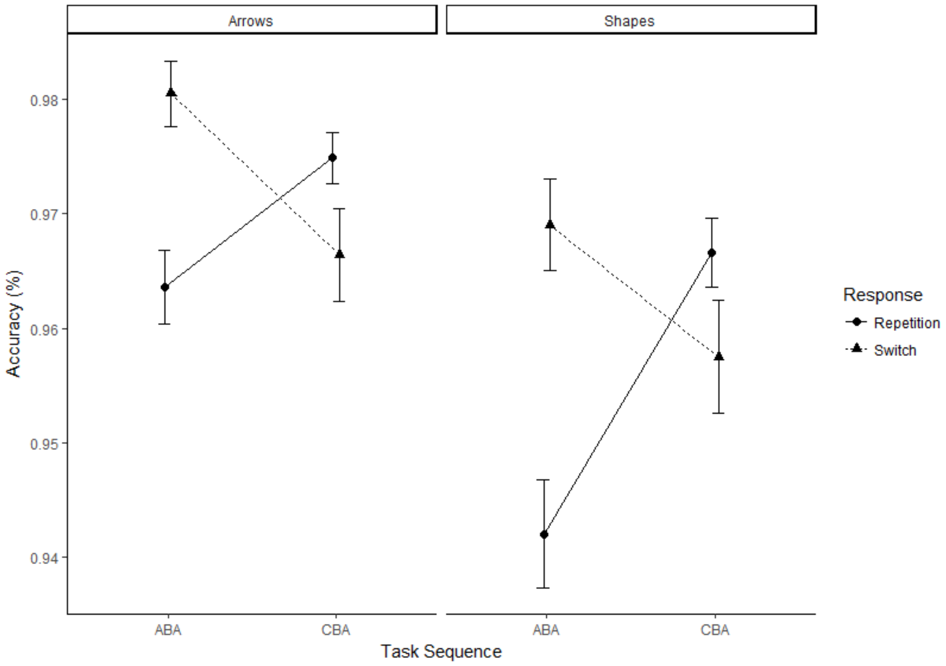


Figure 3.11 An accuracy three-way interaction between the Cue-Type (Shapes vs. Arrows), Task Sequence (ABA vs. CBA), and Response (Repetition vs. Switch). Error bars represent ±1 SE around the mean.

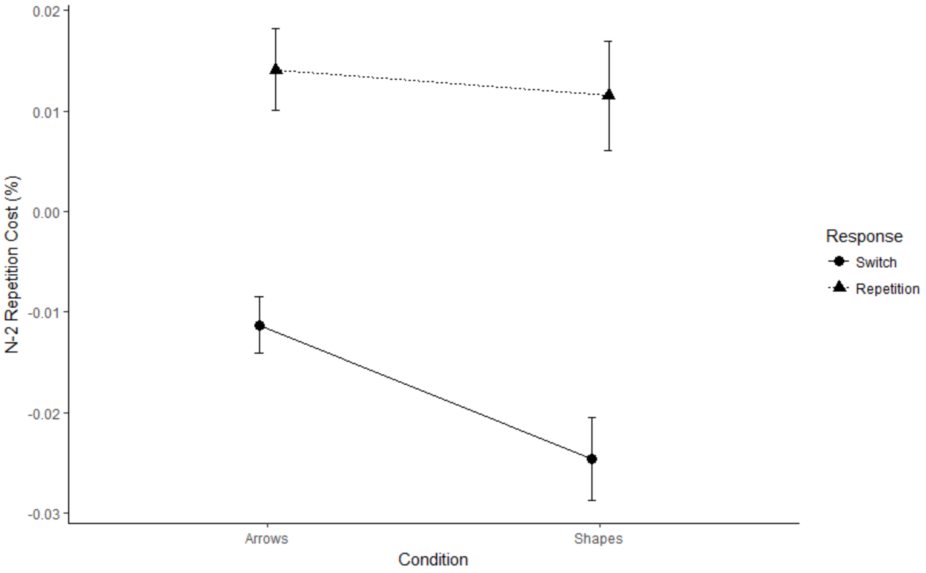


Figure 3.12 Accuracy n–2 task repetition costs for n–2 Response Repetitions and n–2 Response Switches under Shapes/ Arrows cues. Error bars represent ±1 SE around the mean.

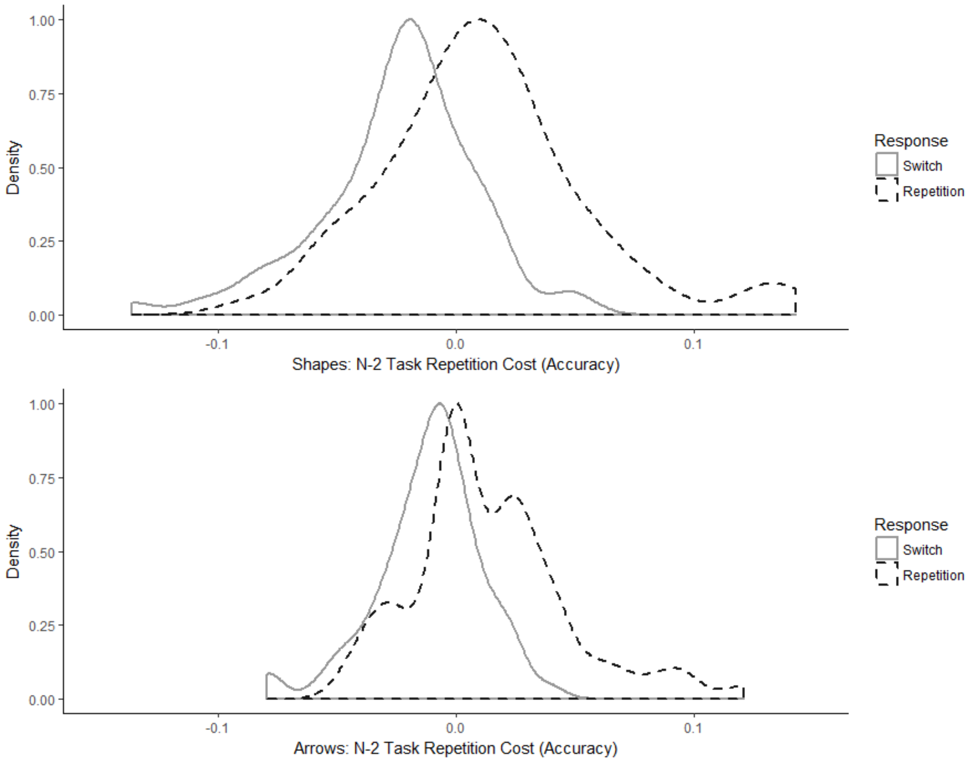


Figure 3.13 Density functions for accuracy (%) n–2 task repetition costs distributions for n–2 Response Repetitions (Repetitions) and n–2 Response Switches (Switches), calculated as Accuracy (ABA) - Accuracy (CBA); on the top for Shapes cues and on the bottom for Arrows cues.

Discussion

Experiment's 3 aim was to examine whether cue-transparency affected episodic retrieval, to establish whether the n–2 task repetition is driven by cue-transparency or episodic retrieval. Performance was overall slower (and less accurate) for ABA relative to CBA sequences; that is, the n–2 task repetition cost was replicated from Experiment 2. Importantly, the n–2 task repetition cost was modulated by episodic retrieval; that is, the n–2 task repetition cost was smaller and non-significant under episodic matches (n–2 RR) compared to episodic mismatches (n–2 RS) where the cost was present. Moreover, the cue-transparency manipulation worked because performance was less efficient under Shapes (i.e., non-transparent) compared to Arrows (i.e., transparent) cues; for example, performance was faster under transparent compared to non-transparent cues. As predicted, under Arrows cues the n–2 task repetition cost was smaller than under Shapes cues. The n–2 task repetition cost was absent under episodic matches regardless of the cue type; however, under episodic mismatches the n–2 task repetition cost was present and smaller under Arrows compared to Shape cues.

The overall episodic retrieval effect was larger under the non-transparent relative to transparent cues; that is, the RT performance differed much less between episodic matches and episodic mismatches under Arrows cues compared to Shape cues. This means that, non-transparent cues lead to more interference than Arrows cues as predicted, and episodic retrieval seems to modulate the n–2 task repetition cost more than the cue-transparency. N–2 task repetitions coupled with n–2 response repetitions result in absent n–2 task repetition cost regardless of the Cue-Type; however, n–2 task repetitions coupled with n–2 response switches lead to larger n–2 task repetition costs under Shapes cues compared to Arrows cues. This means that episodic mismatches drive the n–2 task repetition cost more than cue-transparency, because if cue-transparency drove the cost, the episodic retrieval modulation would be expected to be similar between the Cue-Types. It is not to say that cue-transparency does not matter; clearly it does, but its role in driving the n–2 task repetition cost seems different to the one originally suggested (Houghton et al., 2009).

Furthermore, the n–2 task repetition cost's magnitude was more spread for non-transparent cues compared to transparent cues; this can be interpreted as evidence that under transparent cues which are more informative performance is more consistent, compared to less non-transparent cues which are less informative. That is, under transparent cues, exact memory trace of a previously performed task's parameters can be retrieved facilitating performance, leading to efficient, effective, consistent, and fast responses; whereas, processing of non-transparent cues can involve more mechanisms—for example, complex reconfiguration, interference, and inhibition—resulting in larger variation of responses. Also, transparent cues can be assumed to have a strong relationship with the target compared to non-transparent cues, and as such are more likely to be practiced and automatic relative to non-transparent cues.

Since episodic retrieval was shown to have strong effect on the n–2 task repetition cost, with evidence that task's features can bind into one compound, and that stimulus' characteristics can be part of that compound (e.g., Hommel, 1998; Pösse et al., 2006; Waszak et al., 2003; Woodward & Meier, 2003), it was important to establish whether the n–2 task repetition cost is affected by stimulus' features. In the BI paradigm used in Experiments 2–3 the stimulus was constant across trials, therefore it is unlikely that it had any effect on participants' performance, but precisely because it did not differ across trials, it cannot be ascertained or speculated that it is not important for the n–2 task repetition cost.

Experiment 4 looked at whether the n–2 task repetition cost is affected by stimulus' features, and whether it interacts with or modulates episodic retrieval.

Experiment 4

Experiments 2 and 3 provided clear evidence that the n–2 task repetition cost is modulated by episodic retrieval of responses; that is, the n–2 task repetition cost was smaller for n–2 RR compared to n–2 RS. Experiment 3 additionally showed that the effect of episodic retrieval modulates cue-transparency effects; specifically, the episodic interference was larger for non-transparent cues (Shape) compared to transparent cues (Arrows). Experiment 4 was designed to further investigate the episodic retrieval effect on the n–2 task repetition cost, but this time looking at low-level perceptual attributes of stimuli.

Evidence from WM (Baddeley, 2000), episodic retrieval (Hommel, 2004), procedural learning (Pashler & Baylis, 1991), and task-switching research (Allport & Wylie, 2000; Koch & Allport, 2006; Schmidt & Liefooghe, 2016; Waszak et al., 2003) suggest that perceptual features of stimuli can become bounded with other task's parameters (e.g., cue, response); as such, stimulus' features may be important for manipulating and retrieving information during task-switching. If the visual characteristics of stimuli play a role in task-switching performance—bound with other parameters—it is possible that stimulus' features across trials can lead to interference, just as cue and response have been shown to do in Experiments 2-3. Therefore, it became important to investigate if the effect of episodic retrieval on the n–2 task repetition cost is modulated by low-level perceptual features of stimuli. In Experiments 2 and 3 the stimulus' perceptual attributes were task-irrelevant, participants had to attend to stimulus' location but not the low-level perceptual characteristics; also, the perceptual features of stimulus were kept the same across all trials. Experiment 4 aimed to manipulate stimulus features to investigate if low-level perceptual characteristics of the stimulus influence the n–2 task repetition cost, and/or whether it affects episodic retrieval. If episodic retrieval is not sensitive to low-level perceptual features of stimuli, it would suggest that task-irrelevant features are not retrieved, or at least, that if they are retrieved they do not interfere with episodic retrieval facilitation. Also, another possibility is that task-irrelevant stimulus' features do not bind into event-file (Hommel, 1998).

To manipulate low-level perceptual stimuli characteristics, the BI paradigm from Experiment 3 was adapted; namely, the BI paradigm variation with Shapes-cues was chosen—as it has been shown to produce the largest n–2 task repetition cost—to manipulate stimuli's features. Instead of a black circle being a target, letters were chosen as stimuli to be varied across the trials. Two conditions were created; in one participants always saw a letter A as a target (Stimulus Match), and in the second condition participants saw a different letter of the alphabet on each trial (Stimulus Mismatch).

Method

Participants. Twenty-six participants were recruited from the same population as for Experiments 1–3. Data of twenty-five participants were used in the main analysis.

The number of participants was determined by Sequential Bayesian Factor as in Experiment 3; however, this time the compared models were different. A Bayesian factorial analysis was conducted with the n–2 task repetition cost as a DV, and Stimulus and Response as IVs. The four probable models explaining data were: 1) main effect of Response; 2) main effect of Stimulus; 3) main effects of Response and Stimulus; 4) two main effects (Stimulus and Response) and interaction between them. At twenty-six participants, the BF_{10} reached the criterion of being below 1/6, in favor of the model 2 best explaining the data (see Results for details).

Apparatus and stimuli. BI paradigms were presented on a standard PC with a 17in. monitor via E-Prime v. 2.0 software (Psychology Software Tools, Pittsburgh, PA). Responses were made on a 1-ms precise USB keyboard.

Backward Inhibition paradigm. The set-up of the experiment was the same as in Experiment 2-3. Shapes cues were used (2.5 cm in width and height): a “hexagon” was used for the vertical, a “square” for a diagonal, and a “triangle” for a horizontal task. The Shapes cues were shown to generate the largest n–2 task repetition cost across Experiments 2-3, hence were deemed suitable for the current experiment. This time instead of using a black circle as the stimulus, the stimuli were letters; in the Stimulus Match condition participants saw a letter A on each trial, and in Stimulus Mismatch condition the letter changed on each trial (Figure 3.14). Allowable response key-presses were the same as in the Experiments 2–3. The “Error” message was (22 Verdana; red font).

Procedure. Participants attended a single session during which they performed BI paradigms with two different types of stimuli, one with “A” letter and one with “A-Z” letters; the order of these two was counterbalanced across participants. Before each half participants learnt relevant cue-task mappings and practiced task-switching with respective stimuli-type for 16 trials. If they made more than four errors they were given another practice with 16 trials; however, no more than two practices per participant were allowed.

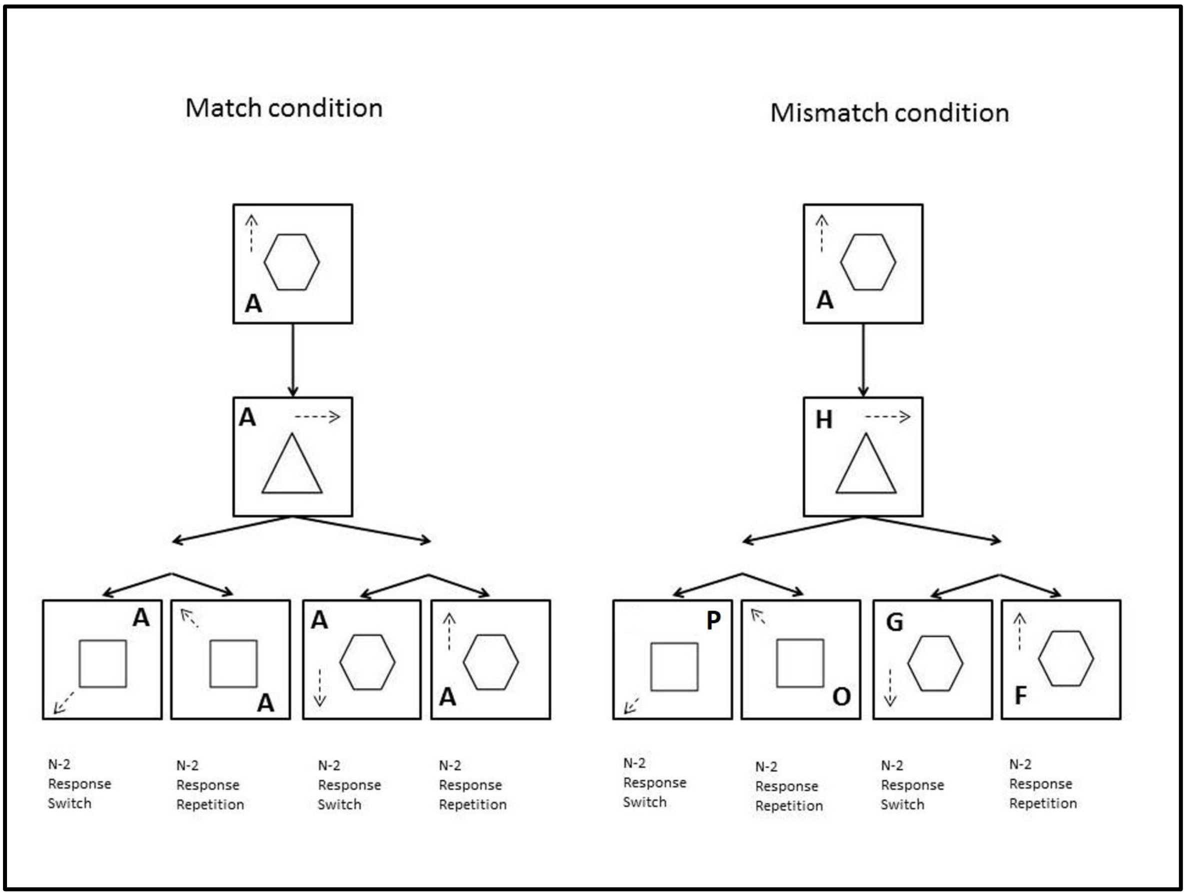


Figure 3.14 An overview of the paradigm used in Experiment 4.

As in Experiments 2-3 participants made a spatial transformation of the stimulus; that is, they had to indicate in which inner corner of the frame, would the target move to, if it moved according to cued direction. The timings of the stimulus, cue, response and intervals between these parameters, were the same as in Experiments 2–3. If the participant gave an incorrect answer, they saw the “Error” message on the screen.

Each participant performed 360 trials, and to optimise participants’ performance all the trials were split into three blocks of 120 trials in each, with participant-paced breaks between the blocks. As in the previous experiments, no immediate task-repetitions were allowed (Koch et al., 2006). The task sequences and responses were recorded as in Experiment 2–3.

Design. A within-subjects design was used to examine RTs (ms) as accuracy (%) as dependent variables, and *Task sequence* (ABA vs. CBA), *Response* (n–2 RR vs. n–2 RS), and *Stimulus* (Match vs. Mismatch) as independent variables.

Results

Data trimming. The data trimming was performed in the same manner as in Experiments 2-3. Overall, 13.5% of trials were removed because of accuracy and RTs trimming; 12.1% from Stimulus Match condition (11.2% for n–2 RR, 12.3% for n–2 RS) and 14.8% from Stimulus Mismatch condition (16.4% for n–2 RR, 14.5% for n–2 RS).

Reaction time analysis. RTs were analysed via a three-way repeated measures ANOVA with factors: *Task Sequence* (ABA vs. CBA), *Response* (n–2 RR vs. n–2 RS), and *Stimulus* (Match vs. Mismatch).

Descriptive statistics for the three-way ANOVA are in Table 3.3. The main effect of Task Sequence was significant, $F(1,24) = 35.74$, $p < .001$, $\eta_g^2 = .02$; as participants were slower for ABA (1101ms) compared to CBA (1032ms) sequence. The effect of Stimulus was not significant, $F(1,24) = 0.009$, $p = .92$, $\eta_g^2 = .0001$, neither was the effect of Response, $F(1,24) = 1.2$, $p = .28$, $\eta_g^2 = .0009$.

An interaction between Task Sequence and Response was significant, $F(1,24) = 17.04$, $p < .001$, $\eta_g^2 = .006$; the n–2 task repetition cost for n–2 RR was 29ms and not significant $t(49) =1.7$, $p = .09$, 95%CI[-5.33, 64.53], and for n–2 RS it was 108ms and significant, $t(49) =8.6$, $p < .001$, 95%CI[83.01, 133.27], which is a replication of results from Experiment 2–3. The other interactions were not significant: Stimulus and Task Sequence, $F(1,24) = 0.867$, $p = .36$, $\eta_g^2 = .0004$; Stimulus and Response, $F(1,24) = 0.66$, $p = .43$, $\eta_g^2 = .0003$; and Stimulus, Task Sequence and Response, $F(1,24) = 1.07$, $p = .31$, $\eta_g^2 = .0006$ (Figure 3.15). The averaged n–2 task repetition costs are in Figure 3.16, and density functions for the n–2 task repetition cost under Stimulus Matches and Stimulus Mismatches, for n–2 RR and n–2 RS is are depicted in Figure 3.17.

Table 3.3 Mean Response times (RTs) and accuracy (%), with standard errors, for ABA and CBA sequences, within n–2 Response Repetitions (RR) and n–2 Response Switches (RS), for Stimulus Matches and Stimulus Mismatches.

	Task sequence			
	ABA		CBA	
Stimulus Match	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
Match				
n–2 RR	1074 (51)	96.99 (.88)	1042 (51)	97.55 (.49)
n–2 RS	1124 (51)	96.60 (.42)	1038 (49)	97.86 (.56)
Mismatch				
n–2 RR	1075 (50)	95.63 (.69)	1047 (56)	97.62 (.72)
n–2 RS	1131 (49)	95.47 (.69)	1002 (45)	97.06 (.38)

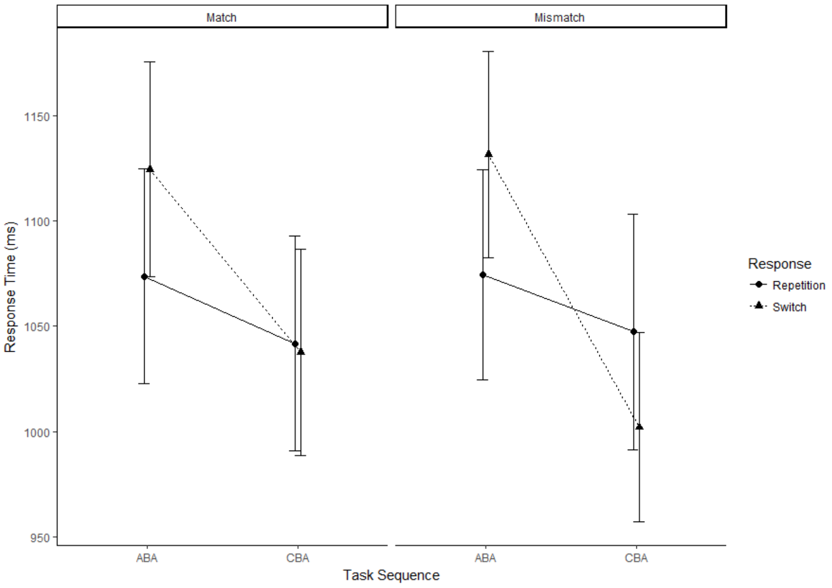


Figure 3.15 Reaction times three-way interaction between Stimulus (Match vs. Mismatch), Task Sequence (ABA vs CBA), and Response (Repetition vs. Switch). Error bars represent ±1 SE around the mean.

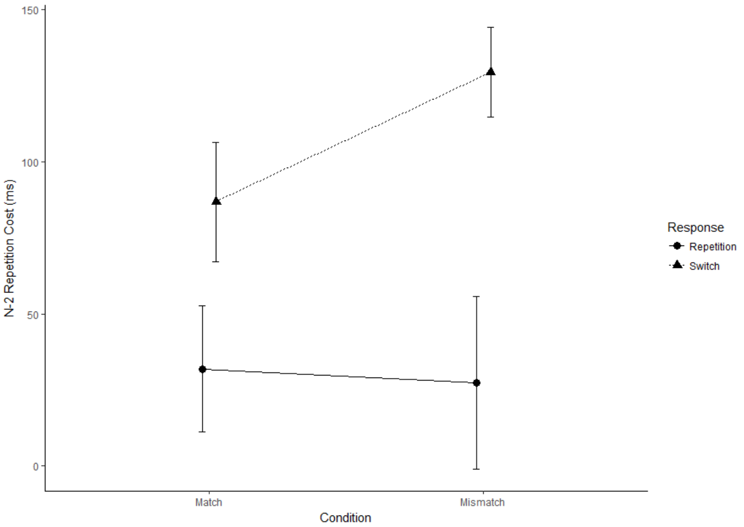


Figure 3.16 Reaction times n–2 task repetition costs for n–2 Response Repetition and n–2 Response Switch under Match/ Mismatch stimuli. Error bars represent ±1 SE around the mean.

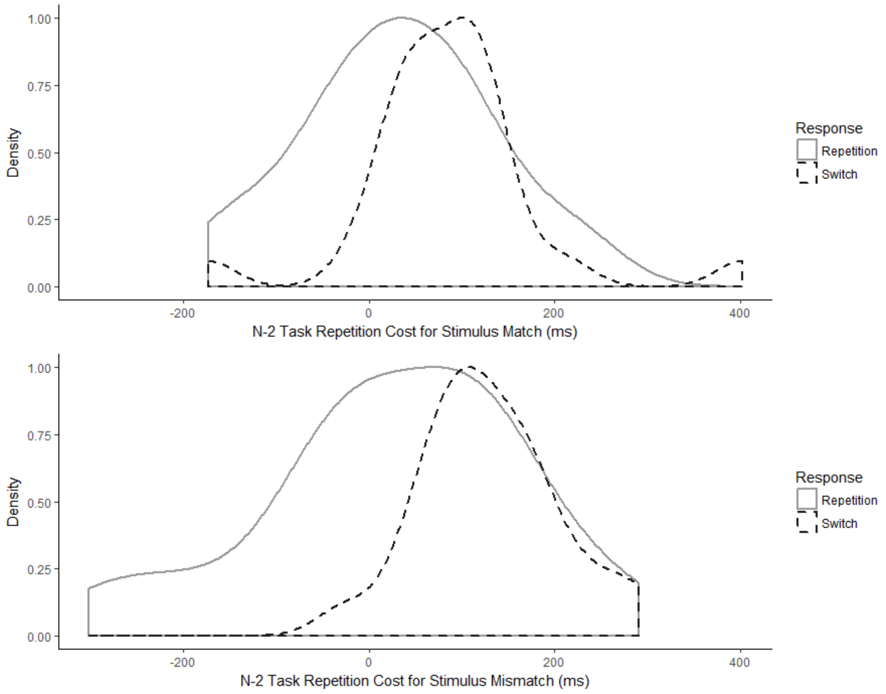


Figure 3.17 Density functions of the reaction time (RT) the n–2 task repetition costs distributions for n–2 response repetitions and n–2 response switches, calculated as RT (ABA) - RT (CBA); on the top graph for Stimulus Match and on the bottom a graph for Stimulus Mismatch.

Bayesian analysis. The model of interest in Experiment 4 was model 4—two main effects (Stimulus and Response) and interaction between them—which was compared to the next-best model, which was model 2—the main effect of Response. Comparing those two models—model 4 (BF 23.37)/ model 2 (BF 151.30)—gave the BF_{10} of 0.155. This means that model 2 is 6.46 times more likely to better explain data than model 4 (1/0.155=6.45). This is considered as moderate evidence for absence of full interaction between Response and Stimulus; meaning that the n–2 task repetition cost was not modulated by interaction between Stimulus and Response, and instead was more likely to be modulated by Response only.

Accuracy analysis. Accuracy for n–2 task repetition cost analysis revealed just one significant main effect, the effect of Task Sequence, $F(1,24) = 14.64$. $p < .001$, $\eta_g^2 = .05$ (96.17% for ABA and 97.52% for CBA sequence). The effect of Stimulus was not significant, $F(1,24) = 2.46$, $p = .13$, $\eta_g^2 = .02$, so was the effect of Response, $F(1,24) = 0.58$, $p = 45$, $\eta_g^2 = .001$.

None of the interactions were significant: Stimulus and Task Sequence, $F(1,24) = 1.33$, $\mathrm{p} = .26$, $\eta_g^2 = .005$; Stimulus and Response, $F(1,24) = 0.23$, $p = .64$, $\eta_g^2 =.0007$; Task Sequence and Response, $F(1,24) = 0.05$, $p = .82$, $\eta_g^2 = .0002$; and Stimulus, Task Sequence and Response, $F(1,24) = 0.6$, $p = .44$, $\eta_g^2 = .002$ (see Figure 3.18 for interaction visualisation; 3.19 for n–2 task repetition costs, and 3.20 for n–2 task repetition costs density functions).

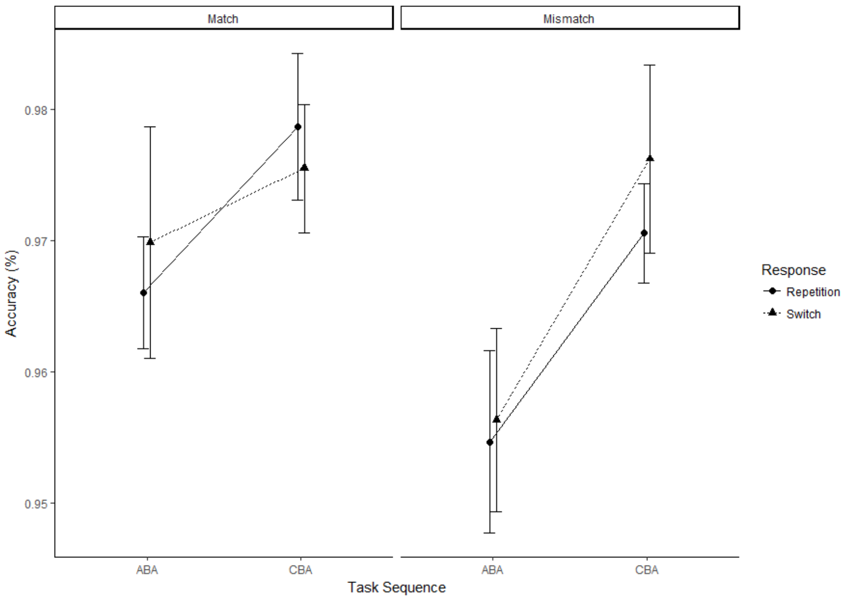


Figure 3.18 Accuracy three-way interaction between Stimulus (Match vs. Mismatch), Task Sequence, and Response. Error bars represent ±1 SE around the mean.

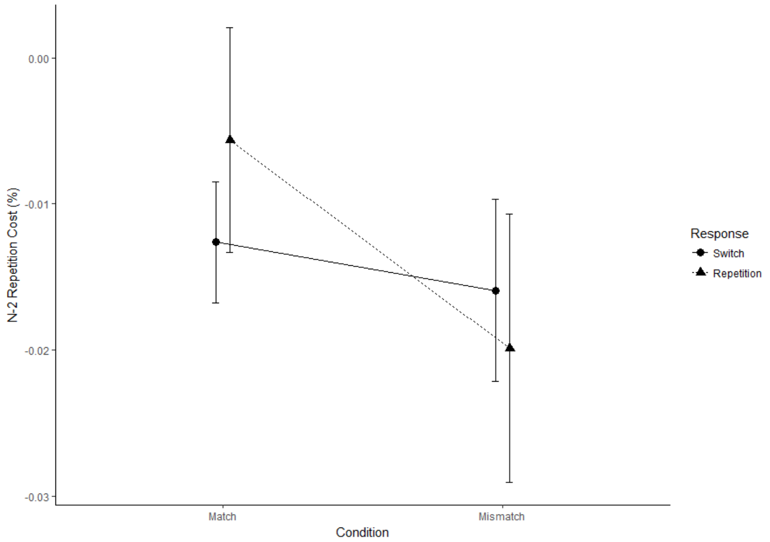


Figure 3.19 Accuracy n–2 task repetition costs for n–2 RR and n–2 RS under Match/ Mismatch stimuli. Error bars represent ±1 SE around the mean.

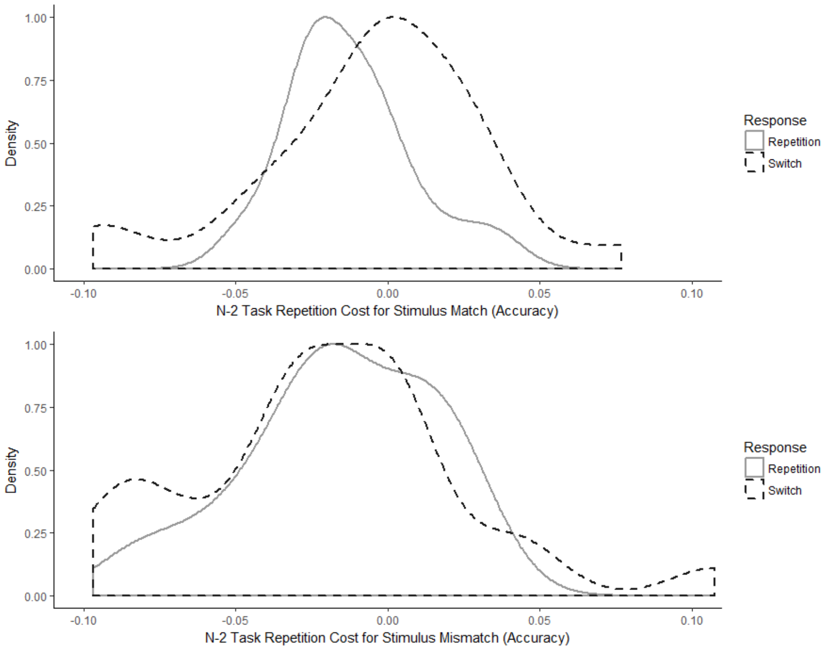


Figure 3.20 Density functions of the accuracy n–2 task repetition costs distributions for n–2 response repetitions and n–2 response switches, calculated as RT (ABA) - RT (CBA); on the top graph for Stimulus Match and on the bottom a graph for Stimulus Mismatch.

Discussion

Experiment 4 was conducted to establish whether task-irrelevant stimulus' features affect the n–2 task repetition cost and/ or episodic retrieval. The results showed that neither of those two were affected by low-level perceptual features of stimuli. This can be interpreted as evidence that task-irrelevant stimulus' low-level perceptual features do not seem to affect the n–2 task repetition cost (i.e., contribute to how much inhibition is applied). It is not to say however, that if stimuli characteristics became task-relevant (which was not tested in the current study) the inter-trial mismatches would not affect performance on task-switching (e.g., Woodward & Meier, 2003). That is, letters used in Experiment 4 (and a black circle from Experiments 2–3) do not have any task-related features so are limited as to whether they can facilitate of interfere with retrieval. Also, letters used in Experiment 4 either always repeated or never repeated, therefore a comparison of n–2 stimulus switch compared to an n–2 stimulus repetition was not possible, which can be taken as a limitation.

To address the two limitations identified in Experiment 4 a different type of stimuli could be used to explore further the low-level stimuli features' influence on the n–2 task repetition cost and/ or episodic retrieval. For example, in the BI paradigm like the one used in Experiment 4, two stimuli per task could be used, specifically two letters per task (e.g., A/ P for horizontal task; H/ S for diagonal; and K/ B for vertical task), and episodic retrieval of response could be controlled as before. Each task would be cued as usual and participants would be told to pay attention only to the location of the letter, and that it is not relevant which letter appears. Then the episodic retrieval modulatory effect on the n–2 task repetition cost would be examined under Stimulus-Switch and Stimulus-Repetition. If episodic retrieval is sensitive to stimuli low-level features, we would expect a facilitation of

Stimuli-Matches compared to Stimuli Mismatches; that is, the modulatory effect of episodic retrieval of the n–2 task repetition cost would be expected to be larger under Stimuli-Matches compared to Stimuli-Mismatches.

As such, based on these results it cannot be ascertained whether under n–2 RR, if the stimulus was repeated too, compared to if it was switched, it would lead or not to a facilitation. Numerically, n–2 task repetition costs were lower in Stimulus-Match condition compared to Stimulus-Mismatch, which indicates that the n–2 stimulus repetition benefited performance to some extent, compared to n–2 stimulus switch.

General Discussion

The current study investigated whether and to what extent episodic retrieval modulates the n–2 task repetition cost, which was explored with three experiments examining different aspects of the relationship between the n–2 task repetition cost, episodic retrieval, cue transparency, and stimulus low-level perceptual features. Experiment 2 showed that the n–2 task repetition cost is considerably reduced under episodic matches compared to episodic mismatches; in other words, ensuring n–2 RR with n–2 task repetitions across trials facilitated performance on task-switching, and reduced the n–2 task repetition cost. Experiment 3 provided evidence that the episodic retrieval modulated cue-transparency which consequently affected the n–2 task repetition cost. And, findings from Experiment 4 showed that low-level perceptual characteristics of the stimulus did not modulate the episodic retrieval or the n–2 task repetition cost.

Practical Implications

The n–2 task repetition cost has been replicated extensively and up to now has been resistant to non-inhibitory explanations (e.g., Kiesel et al., 2010). This effect has become a signature of cognitive inhibition and it has been used as such for group (Dreher et al., 2001; Fales et al., 2006; Lawo et al., 2012; Mayr et al., 2006; Moritz et al., 2004; Prior, 2012) as well as individual differences in inhibitory control (Whitmer & Banich, 2007). From such research, theoretical implications have been suggested. For example, Withmer and Banich used the n–2 task repetition cost to investigate depressive rumination tendencies and cognitive inhibition in healthy participants. Their study was designed based on the idea that depressive rumination is associated with executive functions impairments; specifically, attentional inflexibility (Davis & Nolen-Hoeksema, 2000), which was speculated to manifest itself via impaired inhibition and/or switching ability. It was found that people who scored higher on the Ruminative Response Scale (RRS)—a measure of the extent of someone having recurrent thoughts about feeling depressed—showed smaller n–2 task repetition cost, which was taken as evidence that people who have the tendency to depressively ruminate may have inhibitory deficiency; although, the authors make a point of saying that the study used a correlational analysis which does not allow for causal relationship interpretation. In another study (Whitmer & Banich, 2012) the n–2 task repetition cost was used as a behavioral marker of cognitive inhibition efficiency which was regressed against functional magnetic resonance imaging (fMRI) activations related to task-switching. The main finding of that investigation was that people who exhibited larger n–2 task repetition cost, that is people who were classed as better inhibitors, showed more activity in the basal ganglia and supplementary motor/ premotor area during task- switching, compared to task repetition. This was interpreted as these two brain areas being important for task set inhibition. Since Experiments 2-4 from the current investigation showed that the n–2 task repetition cost is considerably modulated by episodic retrieval, findings from studies using the n–2 task repetition cost as a measure of inhibition are less clear now. For example, there is evidence that the basal ganglia are important for memory processes, including episodic retrieval (Foerde & Shohamy, 2013; Scimeca & Badre, 2012); therefore, it can be speculated that the n–2 task repetition cost in the fMRI study by Whitmer and Banich's (2012) reflected episodic retrieval processes rather than inhibition. Specifically, the n–2 task repetition cost being linked to brain activity in basal ganglia and supplementary motor/ premotor area, may be due to learning and memory processes; especially since the basal ganglia seem to be important for stimulus-response associations in humans and animals (Packard & Knowlton, 2002).

The n–2 task repetition cost has also been used to assess inhibitory control in a substantial number of studies looking at group differences: in brain lesions (Mayr et al., 2006), neuroimaging (Dreher et al., 2001), healthy ageing (Lawo et al., 2012), Parkinson's disease (Fales et al., 2006), obsessive-compulsive disorder (Moritz et al., 2004), and bilingualism (Prior, 2012). These studies' findings implications are now less clear and may require revisiting accounting for episodic retrieval. For example, larger n–2 task repetition costs in accuracy in Parkinson's patients compared to controls may not be due to impaired inhibition but memory processes associated with the basal ganglia.

Moreover, there is evidence that the n–2 task repetition is not a reliable measure (Kowalczyk & Grange, 2017), which was also supported by reliability analysis in Experiment 2 of the current chapter. Kowalczyk and Grange suggested that low reliability of the n–2 task repetition cost in their study might have been due other factors not accounted for. One of those possible factors was episodic retrieval.

The current study however, showed that when episodic retrieval was controlled for the reliability of the n–2 task repetition cost was even smaller than in Kowalczyk and Grange's study. This furthers uncertainty over the n–2 task repetition cost's reliability and its usefulness as a tool assessing cognitive inhibition; therefore, the n–2 task repetition cost should be interpreted with caution.

Theoretical Implications

From a theoretical and biological point of view, it makes sense for a process like inhibition to be important for task-switching (Goschke, 2000; Mayr & Keele, 2000); or for a system by which once activated but no longer relevant task-representations' decay (Altmann & Gray, 2008). This is because, being faced with multivalent environment we often have to select among and act on stimuli effectively and accordingly; that is, we must exercise some control to disengage from and switch between mental states and behaviors dynamically, otherwise we risk being rigorous and incapable of adaptation. As such, cognitive inhibition has been deemed as the process which is very likely modulating controlled behavior; that is, a process by which we can maintain a given goal and action when needed, but also to change them if necessary. For a long time, the n–2 task repetition cost has been taken as evidence for inhibitory processes modulating task-switching and has been resistant to non-inhibitory explanations (reviews by Kiesel et al., 2010; Koch et al., 2010). However, the current study showed that the n–2 task repetition cost is clearly and considerably reduced by a non-inhibitory process, namely, episodic retrieval; therefore, this cost cannot be confidently spoken about as a measure of inhibition in task-switching.

The main theoretical implications of the current study on understanding of the n–2 task repetition cost relate to the extent to which the cost reflects inhibition, the cue-transparency role in task-switching, and low-level task-irrelevant features influence on performance.

Episodic retrieval. In terms of the involvement of cognitive inhibition in task-switching, the current study's findings point to the importance of non-inhibitory and automatic processes driving the n–2 task repetition cost to a large extent. The effect of episodic retrieval on the n–2 task repetition cost fits nicely with evidence from research on SC, which is also considerably modulated by episodic retrieval (Allport & Wylie, 1999; Altmann, 2011; Goschke, 2000; Horoufchin et al., 2011; Logan & Bundesen, 2003; Schmidt & Liefooghe, 2016). A large proportion of the SC and the n–2 task repetition cost being attributable to episodic retrieval, questions the role of cognitive control processes in task-switching, because effects ascribed to inhibition can be accounted for in large by non-inhibitory mechanisms. This means that the costs associated with task-switching are not so much due to inhibition, but due to automatic episodic retrieval of task-set parameters which do not match demand of the task at hand, which results in longer response selection and execution. It is not to say that after controlling for episodic retrieval, small residual switching costs are not attributable to inhibition, but at this stage it is uncertain what type of inhibition it is. Also, from current results it cannot be ascertained whether the residual n–2 task repetition cost is not an underestimated reflection of inhibition; that is, episodic matches may aid performance leading to shorter RTs, but it is not to say that inhibition was not applied in the first place, or that episodic mismatches lead to more inhibition compared to episodic matches.

Cue-transparency. With regards to cue-transparency (Grange & Houghton, 2010a; Houghton et al., 2009), the original research suggested that increases of the n–2 task repetition cost under non-transparent cues compared to transparent cues, were due to non-transparent cues requiring more translation to a task in WM, of which activations linger in WM leading to interference resolved with inhibition; whereas, transparent cues need less or no translation in WM, therefore do not lead to as much if any interference, hence less inhibition is applied. Cues are an important aspect of task-switching (Jost et al., 2013), they aid retrieval of information necessary to perform a given task (Mayr & Kliegl, 2003); but it seems that when episodic retrieval is controlled for, the n–2 task repetition cost is absent regardless of the Cue-Type, and only the n–2 task repetition cost not controlled for episodic retrieval varies between the Cue-Type, with the n–2 task repetition cost being larger under non-transparent compared to transparent cues. This means that, when episodic retrieval is controlled for, the cue-transparency on its own does not modulate the n–2 task repetition cost; but the effect of cue-transparency is present for the n–2 task repetition cost confounded with episodic mismatches. Specifically, when episodic retrieval is not controlled for, the effect of cue-transparency is larger under non-transparent cues compared to transparent cues, which is likely due to more episodic interference, rather than requiring more translation in WM. This because under episodic matches in non-transparent cues condition, the n–2 repetition cost was removed, but if it was not removed, that would mean that the cue-transparency effects are stronger than episodic retrieval effect.

RT performance for CBA sequence was similar under episodic matches and episodic mismatches (for both type of cues), but RT performance for ABA was particularly large under episodic mismatches under non-transparent cues. This means, that episodic mismatches impair performance which is further impaired by non-transparent cues. Specifically, it is likely that memory traces under non-transparent cues (i.e., not informative cues) cannot be effectively retrieved, which could be due to weak cue-task memory traces.

Stimulus features. Existing literature identified that stimulus' features can bind with task and response in the form of a memory compound/ event-file (Hommel, 1998; Pösse et al., 2006; Waszak et al., 2003), and that bivalency of the stimulus can lead to larger n–2 task repetition costs (D. W. Schneider & Verbruggen, 2008); but see also evidence for n–2 task repetition cost for univalent stimuli (Costa & Friedrich, 2012; Gade & Koch, 2012). The current study found that the stimulus' low-level features did not affect the n–2 task repetition cost or modulated episodic retrieval; which indicates that in the current study, stimuli were unlikely to become part of the event-files formed during task-switching. However, there was a trend for episodic retrieval facilitation to not be as efficient under stimuli mismatches compared to stimuli matches, but this pattern was not significant. It is possible that the stimulus' manipulation was not sufficient to yield anticipated effects; that is, there is evidence that for stimuli low-level features to become part of the event-file, they must be task-relevant, and in the current study the stimuli features were all task-irrelevant. In the current study, varying the low-level stimuli features during task-switching led to no effects on the n–2 task repetition cost, but future studies should address whether episodic retrieval and on the n–2 task repetition cost is affected by varying task-relevant low-level stimuli features.

Theories on the n–2 task repetition cost. In light with current study's findings, many experiments on n–2 task repetition cost reported in literature should be revisited to update previously suggested explanations and interpretations. The current study addresses only some of the factors associated with the n–2 task repetition cost, but many are still to be revisited. For example, Experiments 2-4 did not manipulate RCI or CSI, which were kept short and constant, but it is a common observation that the longer the RCI is, the smaller the n–2 task repetition cost is—predominantly interpreted as evidence for activation-decay mechanisms (Altmann & Gray, 2008); also suggested to be evidence for inhibition-decay theory (Altmann, 2007; Mayr & Keele, 2000)—so it is unknown for now whether the effect of episodic retrieval on the n–2 task repetition cost is modulated with longer RCI. Episodic retrieval is an important element of the activation-decay account; it assumes an interplay between encoding, episodic memory, decay, priming, interference, and retrieval; therefore, it seems important to control for episodic retrieval in future activation-decay models to obtain a clearer understanding of how the n–2 task repetition cost arises and what it reflects. In terms of the inhibition-decay theory, it assumes that activated task-set becomes inhibited immediately after its use (Grange et al., 2013), as such, it does not recognize episodic retrieval as a factor modulating the n–2 task repetition cost. And in terms of activation-only model—reported in Grange and Juvina's study—which produced the n–2 task repetition benefit rather than a cost, it was deemed not suitable to explain data. However, it has been noted in the current study and by other researches (Grange & Juvina, 2015; Grange, Kowalczyk, & Loughlin, 2017; Kowalczyk & Grange, 2017) that many participants show n–2 task repetition benefits instead of n–2 task repetition costs, with the number of n–2 task repetition benefits increased when episodic retrieval is controlled for. This implies that any model that attempts to explain n–2 task repetition cost should account for observation that not all participants show the costs associated with task-switching; also that, even if behaviorally it may seem that inhibition is absent or considerably reduced, it does not necessarily mean that it does not play a part in task-switching, as activations and inhibitions in between task-switches may neutralise each other (Grange et al., 2013). For now we know that the n–2 task repetition cost is greatly modulated by episodic retrieval, but it remains uncertain if and to what extent inhibition affects performance on task-switching, especially that there is evidence that episodic retrieval and inhibition are not mutually exclusive (Spapé & Hommel, 2008; Tipper, 2001).

Conclusion

The current study reports three experiments which showed that the n–2 task repetition cost is considerably modulated by episodic retrieval. Moreover, cue-transparency influences episodic retrieval increasing the n–2 task repetition cost; and, the n–2 task repetition cost and episodic retrieval are not affected by task-irrelevant, low-level stimuli perceptual features. Lastly, controlling for episodic retrieval does not improve the reliability of the n–2 task repetition cost. Taken together, these results indicate that the n–2 task repetition cost is unlikely to be a measure of inhibitory control, and if used as such should be interpreted with caution.

Chapter 4 Cognitive Inhibition in Task-Switching and Working Memory Resources

Abstract

Task-switching requires an ability to attend to relevant and ignore irrelevant stimuli, as well as to execute goal-oriented action, processes which cognitive inhibition is thought to facilitate. Computerised task-switching paradigms are used to assess cognitive inhibition as estimated via the n–2 task repetition cost (Mayr & Keele, 2000), an effect of slower performance returning to a task performed recently compared to performance on a task that was not completed recently. The n–2 task repetition cost has been used to measure individual differences in inhibitory control; however, its magnitude varies considerably between participants, the source of which is unknown. Cognitive inhibition can depend on working memory (WM) resources (Conway & Engle, 1994), as such, the current study investigated the relationship between n–2 task repetition costs (Grange, Kowalczyk, & Loughlin, 2017) and WM. Experiment 5 showed that the n–2 task repetition cost was not significantly correlated with WM capacity; and Experiments 6 and 7 revealed that n–2 task repetition costs were not modulated by WM load. These results imply that individual differences in the n–2 task repetition cost cannot be explained by WM resources, and that the cost is not modulated by WM.

Introduction

Task-switching ability is important in day-to-day life as it allows us to shift our intentions and behaviours as our goals change, ultimately making it possible for us to act appropriately in different contexts. An important aspect of efficient task-switching is the capacity to ignore goal-irrelevant or distracting information so we can focus on a task at hand; for example, if when reading a book our phone rings, we can ignore it or we can choose to answer the phone and then return to reading the book. Successful task-switching and goal-oriented actions are important for our daily functioning, because if we were unable to adapt our behaviour according to changing goals, we could be "stuck" performing one goal regardless of changes in our environment, which could be detrimental; for example, when reading a book, if we heard a fire alarm, we would have a difficulty shifting our attention from the book to the alarm to act appropriately.

An indication of what may happen if someone's goal-oriented behaviour does not work efficiently can be seen in conditions associated with frontal lobe damage, which can manifest itself in many cognitive impairments, such as difficulty in selection and organisation of context-specific action (Archibald et al., 2001). People with frontal lobe injuries may show behaviours such as manual grasping (i.e., automatic object grasping, such as grabbing a finger placed in hand), groping reflex (typically seen in infants, but can be seen in adults; for example, holding and rubbing objects placed in from of a person), imitation behaviour (i.e., imitating others' behaviour with no purpose), utilization behaviour (automatic, instrumentally correct, but exaggerated and context-inappropriate actions), alien hand phenomena (i.e., a sense of lack of control over performed actions/ movements) (Lhermitte, 1983), all of which have in common the impaired ability to perform goal-oriented and context-specific behaviour in certain situations.

Task-Switching

Even though task-switching may seem effortless, the efficiency with which we perform it determines a lot of our day-to-day functioning. In terms of what makes it possible for us to adapt our behaviour and intentions, it seems that it is likely that several different cognitive processes work together for us to be able to select actions appropriately to goals and ignore distraction, or simply for our behaviour not to be stimulus-driven. It appears that some of the important aspects of goal-oriented behaviour are the ability to maintain a given action when required, and to be able to adjust that behaviour when the goal changes; that is to have flexibility to shift attention from one task to another. Also, at the same time we have to be able to monitor the background environment so if previously irrelevant stimuli become relevant we can become aware of that as soon as it is possible so we can adapt our behaviour (Goschke, 2000). This ability to maintain a given goal but yet have the capacity to flexibly change that goal is referred to as the stability-flexibility dilemma (Goschke); which emphasises the idea that to try to understand controlled-action and the cognitive processes facilitating it, one must consider mechanisms maintaining as well as adapting behaviour in moment-to-moment fashion.

Task-switching experiments allow us to test some hypotheses we may have about the stability-flexibility dilemma and associated cognitive processes; specifically, based on participants' performance (i.e., reaction times and accuracy) we can make inferences about cognitive mechanisms assumed to facilitate task-switching. For example, from early studies using task-switching paradigms it has become apparent that we are less efficient when we switch between tasks compared to when we repeat the same task, which is known as the "switch cost" (SC, for reviews on related research see Kiesel et al., 2010; Monsell, 2003). The SC has been suggested to be due to a number of different factors (e.g., reconfiguration, task-set inertia; Monsell, 2003); however, many explanations put a large emphasis on cognitive inhibition as being one of the factors thought to contribute to the SC (e.g., Allport & Wylie, 1999; Meiran, Hsieh, & Chang, 2011; Yeung & Monsell, 2003b).

Task-switching and inhibition. In general, cognitive inhibition can be referred to as “the stopping or overriding of a mental process, in whole or in part, with or without intention” (Gorfein & MacLeod, 2007, p. 5), and it has been linked to many functional and dysfunctional behaviours (Amer et al., 2016). It seems that inhibition can influence performance on cognitive tasks and in day-to-day life negatively as well as positively (Amer et al.); but there is some evidence to suggest that low cognitive control is associated with problem solving requiring associative thinking, and analytical problem solving is linked to high level of control (L. Cheng & Runco, 2015). That is, for open-ended and creative tasks, it is beneficial for links to be made between not necessarily connected information and approach a problem from an unusual perspective (i.e., “thinking outside the box scenario”), which benefits from low cognitive inhibition; but when a problem has to be approached analytically, we benefit from focused attention which requires high cognitive inhibition (Wiley & Jarosz, 2012). However, in general, low cognitive control—for example, an inability to disengage from the no-longer-relevant information—has been shown to be linked to psychopathologies (Chamberlain, Fineberg, Blackwell, Robbins, & Sahakian, 2006; Gillan, Robbins, Sahakian, van den Heuvel, & van Wingen, 2016; Johnson, 2007; Whitmer & Gotlib, 2013).

In task-switching, a method known as the Backward Inhibition (BI) paradigm has been used to investigate cognitive inhibition specifically. In the BI paradigm participants switch back and forth between three different tasks in such a way that they either perform pure switches (referred to as an CBA sequence) or they repeat a task performed on n–2 trial (known as an ABA sequence). Performance is worse when repeating a task from an n–2 trial (i.e., ABA sequence) compared to performing a task that is not an n–2 task repetition (i.e., CBA sequence) (Mayr & Keele, 2000); this effect is known as the n–2 task repetition cost and it is thought to reflect cognitive inhibition. As we switch between tasks, a task that we switch away from becomes inhibited so its activation does not interfere with the next task, and if we return to a task that was inhibited soon after attention was disengaged from it, we should be slower performing that task because it will be under the influence of decaying inhibition.

The n–2 task repetition cost has been replicated many times and has been resistant to explanations other than inhibitory ones (for reviews see: Kiesel et al., 2010; Koch, Gade, Schuch, & Philipp, 2010); although a recent study showed that a large proportion of the n–2 task repetition cost is due to a non-inhibitory mechanism (Chapter 3; Grange et al., 2017). Since n–2 task repetition cost is so replicable it has become a signature of cognitive inhibition and has been used as such to assess group differences in healthy and clinical populations (e.g., Fales, Vanek, & Knowlton, 2006; Greenberg, Reiner, & Meiran, 2013; Moritz, Hübner, & Kluwe, 2004; Whitmer & Gotlib, 2012; Yiu-kwan, 2008). Moreover, individual differences studies link smaller n–2 task repetition costs to low (i.e., impaired) inhibition, and large n–2 task repetition costs to high cognitive inhibition (Whitmer & Banich, 2007, 2012).

Inhibition, Individual Differences, and Working Memory

Despite the robustness of the n–2 task repetition cost there are still some unanswered questions related to it. For example, it has been noted that the n–2 task repetition cost varies greatly between healthy participants (Grange & Juvina, 2015)—a pattern also noted in studies reported in Chapter 2 and 3 of this thesis—

specifically, Grange and Juvina showed substantial within- and between-subject differences in the magnitude of the n–2 task repetition costs and practice effects. Not only did the n–2 task repetition cost varied considerably between participants in the first attended session—with absent n–2 task repetition costs for some participants—not everyone benefited from the practice. That is, some subjects' n–2 task repetition cost reduced markedly with practice, whereas in about a third of the sample the practice did not reduce it. It is uncertain why the magnitude of the n–2 task repetition cost differs between participants or where these differences arise from. Nevertheless, through performing computational modelling, Grange and Juvina suggested that individual differences in the n–2 task repetition cost may be due to differences in the strength of inhibition (which was modelled successfully by varying the strength of inhibition parameter in the model); they also speculate that the rate at which inhibition decays may lead to inhibitory individual differences in task-switching (this however was not tested). Taken together, the implication is that individual differences in cognitive control (as measured by the n–2 task repetition cost) may be due to some participants having strong/ weak inhibition and/or due to differences in the speed of inhibition decay. Chapter 2 of this thesis also showed that the individual differences in the n–2 task repetition cost are not predicted by depressive rumination (contrary to Whitmer and Banich, 2007) or processing speed (Kowalczyk & Grange, 2017), and that the n–2 task repetition cost is not reliable; together, this evidence questions how meaningful individual differences in the n–2 task repetition cost are.

Working Memory and Inhibition

There is a possibility that short-term memory related mechanisms—specifically working memory (WM)—may provide some insight into the source of the individual differences in n–2 task repetition costs. Grange and Juvina (2015) hypothesised that practice would lead to smaller n–2 task repetition costs based on two theories. The first one assumes that the n–2 task repetition cost arises as a result of conflict in WM (Houghton et al., 2009); that is, cues are thought to be translated into tasks in WM, where inefficient translation of cues can lead to competition of different tasks' representations (if, for example, incorrect information is retrieved). The more exposure participants have to each task, the more efficient individual cue-task translation becomes (i.e., the cue interpretation, target identification, response selection), which eventually leads to faster responses as tasks parameters are automatically retrieved from long term memory (LTM) (Logan, 1988); in which case there should be less, if any, need for WM involvement, hence less potential interference that could lead to conflicts which need to be resolved through inhibition. The second theory assumes that practice leads to smaller n–2 task repetition costs because repeated exposure to each task will lead to an increase of the resting activation states of the tasks' mental representations of these, which can overcome temporary instances of inhibition (Grange et al., 2013). The importance of these assumptions is that they put an emphasis on memory related aspects in task-switching, about which there is little research reported.

Working memory capacity. WM is a short-term memory system thought to be crucial for information processing during tasks requiring temporary information manipulation and storage (e.g., memorising a shopping list); also, WM is believed to have limited capacity (Baddeley & Hitch, 1974) and has been suggested to be the source of individual differences in inhibition during cognitive tasks (Conway & Engle, 1994; Engle, 1994; Heitz, Unsworth, & Engle, 2005). To relate WM capacity (WMC) to the n–2 task repetition cost, we have to consider what happens during task-switching: First, information about a given task is encoded and stored in the form of what is referred to as a task-set; a task-set (Jersild, 1927; Meiran, 1996) holds information about how to perform a given task (e.g., the meaning of the cue, what the target stimulus looks like, and allowable responses), and this information is then manipulated and updated accordingly as participants perform tasks one after another (D. W. Schneider & Logan, 2014). When the cue is presented, the information about a given task is retrieved (from LTM, based on previous encounters with the task) and manipulated appropriately which is thought to take place in WM (e.g., Houghton et al., 2009; Mayr & Kliegl, 2000, 2003). It is speculated that as we switch between tasks, an interference between task-set parameters can occur; namely, an ongoing activation of a previously relevant task-set can conflict with the currently relevant task-set, and this is thought to be resolved by temporarily inhibiting the abandoned task-set (Arbuthnott, 2008a; Arbuthnott & Frank, 2000; Mayr & Keele, 2000). Assuming that task-set information is manipulated in WM, it is possible that individual differences in WMC will influence the efficiency with which task-sets are manipulated/ inhibited, and tasks are performed, which can vary between people (Engle, 1994).

WM capacity and individual differences. In general, evidence suggests (e.g., Conway & Engle, 1994; Engle, 1994; Kane & Engle, 2003) that people differ with regards to the “capacity” of their WM which seems to influence how well they do on cognitive tasks and in day-to-day life, especially if tasks are relatively demanding (e.g., tasks requiring more top-down control will rely more on WMC). Specifically, WM is believed to be responsible for how well we can attend to tasks, as well as how much information we can manipulate and temporarily hold in mind in a given moment; for example, remembering our grocery shopping list or someone’s phone number require WM. In a standard experimental WM task, participants memorise a series of items shown one a time, in between which they must perform a secondary task, known as the distractor task, which stops them from rehearsing to-be-remembered items (e.g., judging the symmetry of a presented figure). The larger the number of items recalled at the test phase, the larger WMC is thought to be available to maintain the "temporary structures" of items to-be-remembered during the distractor task; that is, Low WMC is associated with less recalled items and High WMC is associated with more recalled items. As such, WMC differs between people and it has a limit (Conwan, 2010), which makes it an important characteristic differentiating individuals on cognitive tasks. For example, when WM load (WML) is varied (from load level 0 to 3), where higher load refers to more items to be monitored and manipulated (Conway, Tuholski, Shisler, & Engle, 1999; Engle, 1996), the negative priming effect—slower response times responding to a stimulus previously ignored, thought to reflect inhibition (Tipper, 1985)—disappears under increased WML. This suggests that processes driving negative priming effect are WM dependent; moreover, it was demonstrated that people with high WMC—compared to those with low WMC—showed negative priming effect but only under WML-0, and by WML-3, performance facilitation rather than negative priming was observed. Taken together, these results support the view that, people's WMC can affect performance on cognitive tasks, cognitive effects can be WM-dependent, and that as WML increases there are less attentional resources available, hence inhibition effects decrease.

WMC and inhibition. Importantly for the current study, in a series of experiments (Conway & Engle, 1994) it was shown that the individual differences in WMC (Low vs. High WMC) are not reflected in the efficiency of activating relevant memories but rather to do with whether individuals have capacity to inhibit irrelevant activations. That is, in Experiment 1 and Experiment 2 from Conway and Engle’s study, participants were asked to memorise sets of letters—each set containing 2, 4, 6, 8, 10, or 12 letters; in Experiment 1 each letter appeared as a target in two out of four sets participants would have to recall (i.e., there was an overlap of letters across to-be-remembered sets, which could lead to interference during retrieval), and in Experiment 2 each letter appeared only in one set (i.e., no overlap of letters across to-be-remembered sets, which should not lead to interference during retrieval). Next, in both experiments, participants performed a speeded recognition task; that is, they were presented with a series of cues (i.e., numbers 2, 4, 6, 8, 10, 12 indicating the set of letters to be recalled) reflecting currently relevant set and letters to be verified as present or not in the cued set (one cue and one letter appeared at time). In one condition, the cue and the letter appeared simultaneously (i.e., the secondary-memory/ inactive-memory condition), and in the second condition, participants first saw the cue of the relevant set and then the letter (i.e., primary-memory/ active-memory condition). The secondary-memory condition was meant to create circumstances under which participants did not have time to bring to mind the appropriate set before being presented with the letter that needed to be matched to the content of the cued set; whereas the primary-memory condition allowed participants to bring to mind the required letters set before they saw which letter had to be compared against the retrieved set. In the context of task-switching, in BI paradigm cues are typically presented before the stimulus, therefore it can be assumed that participants have time to retrieve task-set information before the stimulus appears; also often bivalent stimuli are used which means that irrelevant task-set’s parameters can be triggered by the cue, as it is seen in condition of Conway and Engle’s study where letters overlap across the letter-sets.

Conway and Engle (1994) found that people did not differ in how fast they performed letter recognition in the secondary- or primary-memory conditions if to be-verified letters did not overlap across the sets. However, when the letters overlapped across the sets, people with Low WMC took longer to match the target letter against retrieved letter set under the secondary-memory condition compared to people with High WMC. This was speculated to be due to Low WMC individuals being unable to ignore irrelevant sets, forcing them to perform a serial search through sets and items to identify a relevant one, compared to High WMC individuals who can ignore irrelevant items during the search and select a relevant one faster. This was interpreted as evidence that WMC determines the level to which an individual can suppress irrelevant tasks'; that is, individuals with Low WMC have less attentional resources available to deploy inhibition, compared to individuals with High WMC who have more attentional resources, hence can inhibit irrelevant activations.

Furthermore, WM is believed not to be necessary for performance on cognitive tasks for which controlled effortful behavior is not required (i.e. bottom-up processing)—for example, in a flanker task, locating a target letter Z in a row of zeros (Shipstead, Harrison, & Engle, 2012)—and therefore, any individual differences in WMC are not expected to affect performance on those tasks. During such tasks, performance is thought to be facilitated via automatic spreading activation (i.e., activation of neuronal networks triggered by a stimulus for previously established associations) (W. Schneider & Shiffrin, 1977). Therefore, when negative priming was shown to vary depending on WMC individual differences—with Low WMC individuals showing no evidence of negative priming compared to individuals with High WMC (Engle, 1996)—it was taken as evidence that inhibition relies on WM. As such, in Low WMC individuals, the irrelevant items are speculated not to be inhibited because the primary task consumes all attentional resources (i.e., WM resources), leaving no resources left to apply to inhibition; therefore, there is no cost in performance when responding to previously ignored stimuli. This is compared to High WMC individuals who can actively inhibit to-be-ignored items because the primary task does not use all of their attentional resources, therefore when they become relevant, the performance is impaired due to persisting inhibition.

The Current Study

From the task-switching literature mixed findings are reported with regards to the relationship between WM and task-switching. Some studies investigating the SC show that WM is important for task-switching (Goschke, 2000; Meiran & Kessler, 2008; Miyake, Emerson, Padilla, & Ahn, 2004; Saeki & Saito, 2004a, 2004b); for example, the SC increases under higher WML (Baddeley, Chincotta, & Adlam, 2001; Bryck & Mayr, 2005; Emerson & Miyake, 2003; Hester & Garavan, 2005), and higher WMC is linked to smaller SC (Butler, Arrington, & Weywadt, 2011; but see also Draheim, Hicks, & Engle, 2016), However, others argue that WM and SC are not related (Kane, Conway, Hambrick, & Engle, 2007; Miyake et al., 2000; Rubin & Meiran, 2005; Sauseng et al., 2006). With regards to the n–2 task repetition cost, there is very little literature on the potential influence of WM on inhibition in task-switching; in fact, there are two publications that addressed that topic. One study was reported whilst the current study was being conducted (Pettigrew & Martin, 2015), and the second study is an unpublished investigation (Grange & Houghton, n.d.); they both showed no relationship between the magnitude of the n–2 task repetition cost and WM.

Grange and Houghton (n.d.) based their study on two opposing theories. First one argues that WM is necessary for inhibition (Conway, Tuholski, Shisler, & Engle, 1999; Engle, 1994) which predicts that people who have larger WMC should have larger inhibition (i.e., n–2 task repetition cost) compared to people with smaller WMC. The second theory says—contrary to inhibition being a resource-dependent process—that inhibition can be reactive and automatic in nature (Anderson & Levy, 2007; Arbuthnott, 2008b; Sinai, Goffaux, & Phillips, 2007), and as such should not be tied to WM resources. In support of the second theory, Grange and Houghton found that WMC was not related to the variance seen in n–2 task repetition costs; these findings were the same for a correlational analysis for the n–2 task repetition cost and Automated Operation Span (AOSPAN) measure of WM (Unsworth, Heitz, & Engle, 2005), and in an experiment which manipulated WML within a BI paradigm (based on D. W. Schneider & Verbruggen, 2008). The BI paradigm was modified in such a way that in between runs of task-switching participants saw letters (2 letters for Low WML, or 6 letters for High WML) which they had to study. There were 14 runs in each WML condition, where each run began with a presentation of letters (one letter at a time), which was followed by 12 task-switching trials; at the end of each run participants saw a probe letter, and they had to judge whether that probe letter was among the letters presented at the beginning of the run. The results showed that the number of letters participants had to remember (Low vs. High WML) did not seem to affect n–2 task repetition costs.

Pettigrew and Martin (2015) also investigated the relationship between the n–2 task repetition cost and WMC, and predicted no relationship between those two variables. They argued that the n–2 task repetition cost is not a reflection of a top-down process but rather an automatic inhibition (based on work from: Gade & Koch, 2005, 2007; Houghton et al., 2009; Schneider & Verbruggen, 2008; Schuch & Koch,

2003) or self-inhibition (Grange et al., 2013; Koch et al., 2010), therefore it should not be associated with WM. That is, this account speculates that inhibition resolves conflicts/ interference in the cognitive system (e.g., Altmann & Gray, 2002) through low-level automatic inhibition of, for example, motor responses (Arbuthnott, 2005, 2008a, 2008b; Schuch & Koch, 2003). Specifically, in order to be able to efficiently select an appropriate response, other competing responses are inhibited so they interfere less with the current goal; that is, the tasks' motor responses are inhibited rather than the whole task-sets (Cooper & Marí-Beffa, 2008; Gade & Koch, 2007; Grzyb & Hübner, 2012; Regev & Meiran, 2016; D. W. Schneider & Verbruggen, 2008; Schuch & Koch, 2003); as such, the n–2 task repetition cost would not be expected to be modulated by WM. As predicted they found no relationship between the n–2 task repetition cost and WM measures.

The current experiment initially was designed to address some limitations in Grange and Houghton's study (n.d.); that is, Grange and Houghton's investigation utilized only one measure of WMC in their correlation experiment, which is argued insufficient when assessing WM (Conway et al., 2005). As such, at this stage, it is still uncertain whether their WML manipulation did not work or whether the n–2 task repetition cost is truly not modulated by WML. Moreover, since the two studies on the relationship between WM and n–2 task repetition cost were conducted (Grange n.d.; Pettigrew et al., 2015), evidence emerged that the n–2 task repetition cost is not reliable at the individual differences level (Kowalczyk & Grange, 2017; Chapter 2 of this thesis), and that it is a confounded measure of inhibition (Grange et al., 2017; Chapter 3 of this thesis). In a series of three experiments, it was shown (Grange et al., 2017) that episodic retrieval substantially modulates performance on task-switching, influencing the magnitude of the n–2 task repetition cost. Episodic retrieval (Neill, 1997) is a process of retrieval of an event of recently encountered information upon seeing a cue that triggers that retrieval (Hommel, 1998, 2004; Logan, 1988; Logan, Schneider, & Bundesen, 2007). Specifically, it is argued that, when stimuli are encountered and acted on, a memory trace of that event is created (known as an event-file); the memory trace of an event is thought to hold information on what stimulus goes with which cue, the stimulus' appearance, and a response made (e.g., Hommel, 1998). Episodic retrieval can facilitate or hinder performance; that is, if upon seeing a cue relevant information matching a current event is retrieved, the response to the current event will be faster, compared to if retrieved information does not match the current task's demands.

In a typical BI paradigm performance is assessed based on whether a given task repeats across trials (i.e., ABA sequence) compared to when the task does not repeat across trials (i.e., CBA sequence); however, it was noted (Mayr, 2002) that when a given task repeats (i.e., ABA sequence) the responses made may differ (e.g., right-key press on first A-task, and a left-key press on second A-task). This was hypothesised to be potentially problematic for the inhibitory explanation of the n–2 task repetition cost because according to episodic retrieval theory (Neill, 1997), if upon seeing a given cue retrieved information (e.g., what is the task?; what to look for?; which response to make?) mismatches the demands of the task at hand, performance on that task will be slower. On the other hand, if the retrieved task-parameters match the demands of a task at hand, the performance on the current task should be facilitated (i.e., faster and more accurate). Grange and colleagues (see also Mayr, 2002), found that, the episodic retrieval account did not explain the n–2 task repetition cost entirely; however, controlling for response repetitions and switches within n–2 task repetitions, it was demonstrated that n–2 response repetitions substantially reduced n–2 task repetition costs compared to n–2 response switch. That means that a large proportion of the n–2 task repetition cost—typically attributable to inhibition—can be explained to a large extent by memory related processes. The reduced n–2 task repetition cost has been proposed to be a “better” (i.e., not confounded by episodic mismatches) indication of cognitive inhibition, and has been referred to as the residual n–2 task repetition cost.

The current study aimed to revisit the relationship between WMC and inhibition in task-switching, which was achieved by the following; the n–2 task repetition cost controlled for episodic retrieval was used as a measure of cognitive inhibition, WMC was assessed with three separate measures shown to reliably assess this ability, and WM was modulated during task-switching. It was speculated that, if the residual n–2 task repetition cost reflects cognitive inhibition, and if inhibition relies on WM resources, it should relate to WMC; specifically, it was expected that individuals with Low WMC should show smaller residual n–2 task repetition cost, and individuals with High WMC should show larger residual n–2 task repetition cost. However, if the residual n–2 task repetition cost is not related to WM capacity, it will indicate that inhibition—as measured via the residual n–2 task repetition cost—either is not dependent on WMC, or that perhaps, the residual n–2 task repetition cost does not reflect a top-down control process, but rather an automatic, low-level process.

Experiment 5

Method

Participants. Forty-two participants were recruited from Keele University in exchange for partial course credits. Participants were at least 18-years-old (M=19.17, SD 1.55; thirty-six females), understood spoken and written English, and had normal or corrected-to-normal vision. One participant’s data were removed due to researcher’s error, leaving forty-one participants for the main analysis.

Stopping rule. The sample size for the current study was determined via what is known as Bayes Factors (BFs) for correlation (Wetzels & Wagenmakers, 2012). Unlike the frequentists' hypothesis testing for correlation, which utilizes *p*-values to determine the significance of a relationship between variables, Bayesian inference allows for quantification of evidence for the null hypothesis (e.g., a correlation being unlikely) and experimental hypothesis (e.g., a likely correlation). To make a Bayesian inference, the null hypothesis, H_0, and the alternative hypothesis, H_1, are assigned prior probabilities—$p(H_0)$, $p(H_1)$—which are later compared to posterior probability (i.e., adjusted probability given data) of both hypotheses. This can be described as in Equation 4.1

Equation 4.1

$$BF_{10} = \frac{p(D|H_1)}{p(D|H_0)}$$

where, D refers to the gathered data, H_0 reflects the null hypothesis, H_1 reflects the alternative hypothesis, and p is the probability; the numbers 1 and 0 by the BF denote that the BF is calculated as a ratio of probability of data under the alternative hypothesis (e.g., a likely correlation) compared to the probability of data under the null hypothesis (e.g., an unlikely correlation).

The current experiment was interested in whether there is a relationship between the WMC and the residual n–2 task repetition cost, and BF_{10} informed us on at how many participants either of the hypotheses—the null (i.e., an unlikely correlation) vs. the alternative (i.e., a likely correlation)—was more probable (for which criteria are discussed later). Specifically, if the BF_{10} was below 1/6 or above 6 (Schönbrodt et al., 2014), or if at eighty participants results were inconclusive, data collection was to stop. The further away from 1 the BF_{10} is, the more compelling the evidence is, where BF_{10} larger than 1 signifies support for the alternative hypothesis, and smaller than 1 indicates that the data are better explained by the null-hypothesis. Also, it is understood that BF_{10} below 1/6 or above 6 is considered as compelling enough evidence to stop collecting data as it has been shown to have low rates of False Positive Evidence and False Negative Evidence (Schönbrodt et al., 2014).

Before the first BF_{10} was calculated, a sample size of 20 participants was tested (Schönbrodt et al., 2015). In the reported experiment, at 42^{nd} participant data collection stopped as the BF_{10} for the correlation between the residual n–2 task repetition cost and the WMC Composite Score was 0.1424, which is less than 1/6 (i.e., one of the criteria for stopping data collection); this was interpreted as evidence that data were better explained by the null hypothesis (i.e., that the correlation was unlikely). Specifically, the data were 7.02 times more likely under the null hypothesis compared to the alternative hypothesis (1/0.1424).

All statistics were conducted using an R software (R Developement Core Team, 2018). To calculate the BF_{10} for the stopping rule, first, the correlation between the residual n–2 task repetition cost and WMC was calculated (standard coefficient *r* using frequentists' method). Then, the calculated correlation coefficient (*r)* and the number of participants were entered to the JZS function (Jeffrey-Zellner-Siow, Schönbrodt, Wagenmakers, Zehetleitner, & Perugini, 2014; Wetzels & Wagenmakers, 2012), which calculates the BF_{10}. Within the JZS function, the null and alternative hypotheses are conceptualised as two linear regression models (one for a likely correlation and one for an unlikely correlation), which are compared in terms of how well each predicts the data, as expressed with $BF_{10.}$

Apparatus and stimuli. The task-switching paradigm was presented on a standard PC with a 17in. monitor via E-Prime v. 2.0 software (Psychology Software Tools, Pittsburgh, PA). Responses were made on a 1-ms precise USB keyboard.

Backward Inhibition paradigm. The stimuli were shown in a black square frame (8cm x 8cm) on a white background; in the center of the frame a cue in the form of a shape appeared—a triangle signified the “horizontal”, a hexagon the “vertical”, and a square the “diagonal” task (cues were 2.5 cm in width and height). The target appeared inside the frame, in one of the inner corners in the form of a black circle (1cm diameter). If the participant gave an incorrect answer, they saw “Error” message on the screen (red Verdana font, size 22).

WMC measures. The general information on nature of WMC tasks (Symmetry, Operation, Rotation Spans) used in this experiment is provided below; for details on the three WMC measures see the paper by Foster and colleagues (model 11, 2014; Unsworth et al., 2005). A shortened version of WMC tasks were used, which meant that there was one block of each WMC task was administered; this version of the complex span WMC tasks has been validated and shown to be reliable (Foster et al.) All three WMC tasks required participants to memorise a number of to-be-remembered items in the face of distractors (i.e., tasks performed in between to-be-remembered items) and recall memorised items in the order they were presented when cued to do so. See Figure 4.1 for visual examples of WMC tasks.

Operation span (OSpan). In the OSpan test participants were presented with letters (one at a time) which they had to memorise; in between the letters being presented, participants had to verify simple mathematical problems (e.g., (2+2) – 1 = 3). On each trial there were between 3–7 to-be-remembered items and mathematical problems presented on each trial; participants did not know how many letters and mathematical problems would be presented.

Symmetry span (SymSpan). In the SymSpan task, the to-be-remembered items were locations of red squares in a 4x4 grid of potential locations. The distractor task was to judge if shapes presented in between the to-be-remembered items, were symmetrical along their axes. Again, one to-be-remembered item followed by one distractor task were presented at a time. On each trial between 2–5 to-be-remembered items and distractor tasks were presented, and again, participants did not know how many would be presented on each trial.

Rotation span (RotSpan). In the RotSpan task the to-be-remembered items were arrows (short and long) pointing in one of eight different locations. The distractor task was to judge if presented letter, which was rotated, was correctly presented or whether it was a mirror image of the letter.

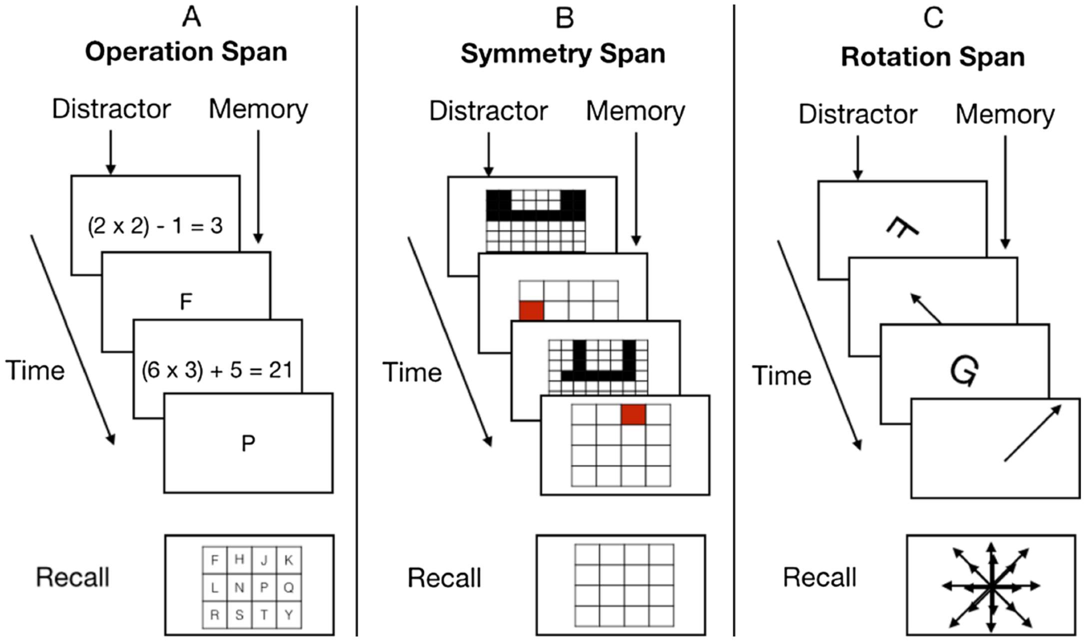


Figure 4.1 Examples of WMC taska (Operation, Symmetry, and Rotation Spans) used in the current experiment. This figure was taken from Foster et al., (2014, p.228).

Procedure. Participants attended a single session during which they performed three WMC tasks and the BI paradigm.

Backward Inhibition paradigm. A single trial consisted of a cue, stimulus, and response. Each trial started with a presentation of a black square frame inside which participants saw a cue—a shape of a square, triangle, or hexagon—for a duration of 150ms. The cue was followed by the appearance of a black circle in one of the inner corners of the frame. Participants had to make a spatial transformation of the black circle as per the cue; that is, they had to indicate in which inner corner of the black frame, would the circle move to, if it moved according to the direction that the cue represented (Figure 4.2). The numerical part of the keyboard was used for responses; each corner of the frame had one keyboard-key assigned which was spatially-congruent with the grid location; keys 1 (lower-left), 2 (lower-right), 4 (upper-left), and 5 (upper-right). So, if the cue was the hexagon and the black circle was in the lower-left corner, the correct answer would be that the black circle would move to the upper-right corner. In this example the participant had to press the key 4 to give the correct answer. As soon as the response was made, the stimulus participant made the judgment about and the cue disappeared; 150ms (RCI; response-cue interval) later another cue appeared in the center of the frame followed by another black circle. The cue was selected in a random fashion with a constraint that immediate repetitions could not occur (Philipp & Koch, 2006); the location of the target was chosen at random. If participants made an error, they saw "Error" message (1000ms).

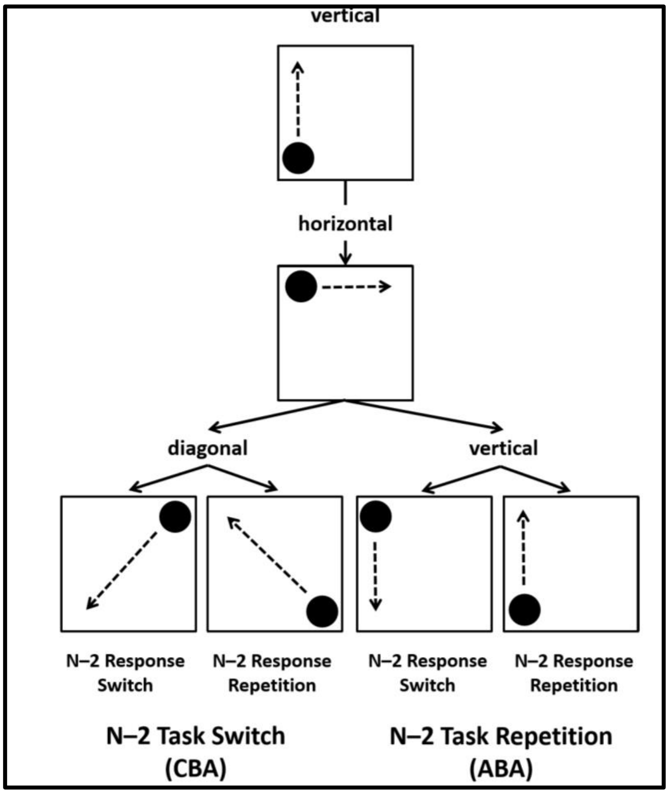


Figure 4.2 Schematic overview of the BI paradigm (Mayr, 2002). The arrows indicate correct answers and they were used only for instruction purposes. Images are not to scale.

Each participant performed 480 trials; to optimise participants' performance (i.e., to avoid participants becoming fatigued) the trials were split into four blocks of 120 trials in each, with breaks (participant-paced) between the blocks. Participants always switched between the three different judgments (i.e., horizontal, vertical, diagonal). It was emphasised to participants that they had to respond as fast and as accurate as possible. Instructions were given to participants verbally, after which participants had a practice consisting of sixteen trials; if they made four or more errors during the first practice, they were offered another practice with sixteen trials; however, no more than two practices were allowed.

The task on each current trial n was compared to the task on the n–2 trial allowing for trials to be registered as ABA or CBA sequences. Importantly, the E-

Prime software compared the response that should have been pressed to the one participants pressed which was registered as correct or incorrect, as well as which corner was the response given in on each trial which was used to code for n–2 response repetitions and n–2 response switches within ABA and CBA sequences.

WMC tasks. All instructions for each of the tasks were given on the computer screen, after which participants practiced in each WMC task, one at a time. During each WMC task participants saw one block of between two and seven (or two to five) to-be-remembered items, one at a time, between of which presentation they had to perform a distractor task. Each experimental block started with the distractor tasks, followed by the to-be-remembered item. On each WMC measure, participants' task was to memorise to-be-remembered items in the order that they were presented, at the same time maintaining their performance at 85% on the distractor task; participants recalled to-be-remembered items at the end of the run of presented items, that is, at the end of the block. Participants were given feedback on their performance on memory and distractor task throughout each block (through a score on the computer screen); maintaining high accuracy on the distractor task was important as that task was mean to stop participant rehearsing to-be-remembered items.

Based on the scores from each of the WMC measures a composite-score was created for each participant; namely, each WM measure score was Z-transformed (separately for each measure); then, for each participant, transformed scores from Operation, Rotation, and Symmetry were added and divided by 3. Obtaining Z-scores for each participant for each measure provides a better indication of each participant's ability level in a meaningful way.

Design. For the task-switching part of the experiment a within-subjects design was utilized to examine reaction times (RTs, ms) and accuracy (%) as dependent variables, and the *Task Sequence* (ABA vs. CBA) and *Response* (n–2 Response Repetition vs. n–2 Response Switch) as independent variables. To analyse the relationship between the residual n–2 task repetition cost with the WMC, a frequentist and a Bayesian version of a correlation was conducted.

Results

Data trimming. Data were trimmed for accuracy and RTs analyses. For the former one, the null-trials were removed; that is, the two trials at the beginning of each block, and two trials following an error. For the RTs analysis, as well as null trials, the error trials were removed; moreover, RTs faster than 150ms or slower than 2.5 standard deviations above each participant's mean per experimental cell were removed. All three types of data trimming have led to an overall of 11.3% trials removed (10.8% for n–2 Response Repetitions; 11.4% for n–2 Response Switches).

Reaction time analysis. RTs were analysed via a two factor repeated measures ANOVA with factors: *Task Sequence* (ABA vs. CBA) and *Response* (n–2 Response Repetition, RR vs. n–2 Response Switch, RS). Mean and standard errors for RTs and accuracy are provided in Table 4.1.

The analysis revealed that there was a significant main effect of Task Sequence, F (1, 40) = 12.30, p = .01, η_g^2 = .012; that is, the standard n–2 task repetition cost (i.e., not controlled for episodic retrieval) was present as participants responded faster performing CBA (1162ms) than ABA (1220ms). There was also a significant main effect of Response, F(1,40) = 13.17, p < .001, η_g^2 = .004; that is, performance was overall faster for n–2 RR (1174ms) compared to n–2 RS (1208ms).

The interaction between the Task Sequence and Response was significant, *F* (1, 40) = 21.66, $p < .001$, $\eta_g^2 = .001$. That is, the n–2 task repetition cost was considerably smaller for n–2 RR (8ms) compared to the n–2 task repetition cost for n–2 RS (123ms); this means that the finding from Grange et al. (2017) was replicated. Individual t-tests revealed that, the n–2 task repetition cost was not significant for n–2 RR, *t* (40) = 0.031, *p* = 1, 95%CI [-51, 53], as RTs for ABA averaged at 1175ms for ABA, and at 1166ms for CBA sequence; however it was significant for n–2 RS, *t* (40) = 8.6, $p < .001$, 95%CI [87, 141], as average RTs for ABA was 1272ms, and 1149ms for CBA sequence. That means, that controlling for Response within n–2 task repetitions reduced the standard n–2 task repetition cost to the point that it was no longer significant. See Figure 4.3 for ANOVA illustration and n–2 task repetition cost densities graphs.

Table 4.1 Mean response times (RTs) and accuracy (%), with standard error, for ABA and CBA sequences, within n–2 Response Repetitions (n–2 RR) and n–2 Response Switches (n–2 RS).

	Task Sequence			
	ABA		CBA	
	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
n–2 RR	1175 (90)	98.19 (.32)	1166 (97)	98.14 (.38)
n–2 RS	1272 (99)	96.91 (.41)	1149 (96)	98.05 (.30)

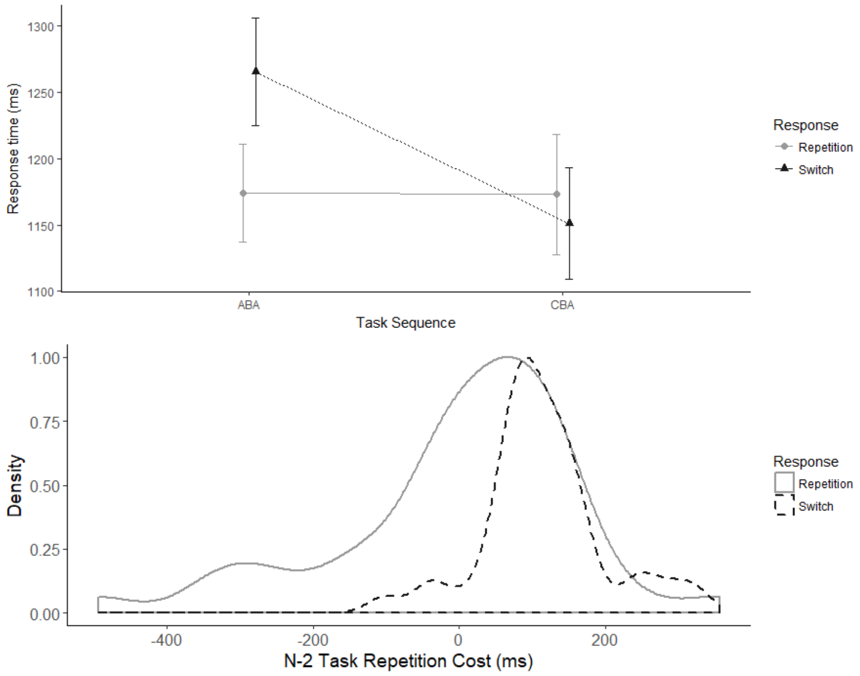


Figure 4.3 At the top: An illustration of a two-way interaction (RT; ms) between Task Sequence (ABA vs. CBA) with Response (n–2 Response Repetition vs. n–2 Response Switch). At the bottom: Density functions for RT n–2 task repetition costs n–2 Response Repetitions and n–2 Response Switches. Error bars denote ±*SE* around the mean.

Accuracy analysis. A two factor repeated measures ANOVA for accuracy revealed that the main effect of Task Sequence was “almost” significant, *F* (1, 40) = 4.04, *p* =.051, η_g^2 = .014; the overall accuracy on ABA sequence was 97.55% and 98.09% accuracy on CBA sequence. The main effect of Response was significant, *F*(1,40) = 5.97, *p* = .02, η_g^2 = .023; that is, participants were more accurate for n–2 RR (98.17%) compared to n–2 RS (97.48%).

The interaction between the Task Sequence and Response was significant, *F* (1, 40) = 5.90, *p* = .02, η_g^2=.017. Individual t-tests revealed that for n–2 RR—as it was observed in RT analysis—there was no significant difference on accuracy between ABA and CBA trials, *t* (40) = 0.15, *p* = .90, 95%CI [-0.007, 0.008];

whereas, for n–2 RS accuracy differed significantly, *t* (40) = -3.3, *p* = .002, 95%CI [-0.018, -0.004]. For n–2 RR there was an accuracy n–2 task repetition benefit of 0.05%, and for n–2 RS there was an accuracy n–2 task repetition cost of -1.14%; specifically, accuracy under n–2 RR was comparable between ABA and CBA sequences (with a tendency to lead to a benefit), but for n–2 RS there was a clear standard n–2 task repetition cost as accuracy was lower for ABA compared to CBA sequence (see Figure 4.4 for ANOVA illustration and n–2 task repetition cost densities graphs).

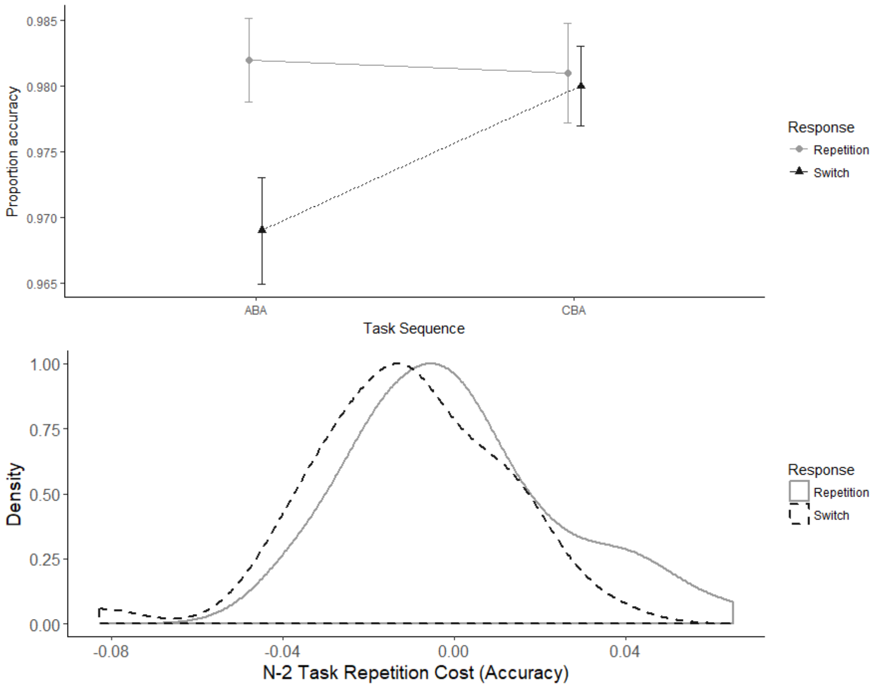


Figure 4.4 At the top: An illustration of a two-way interaction (accuracy; %) between Task Sequence (ABA vs. CBA) with Response (n–2 Response Repetition vs. n–2 Response Switch). At the bottom: Density functions for accuracy n–2 task repetition costs n–2 Response Repetitions and n–2 Response Switches. Error bars denote ±*SE* around the mean.

Standard correlations. As expected (Grange & Juvina, 2015) RT n–2 task-repetitions costs varied considerably between participants, which was true for the standard (-104–327ms) and the residual n–2 task repetition costs (-495–357ms). Looking at densities graphs for participants' individual performance (Figure 4.3–4.4), apart from n–2 task repetition costs, n–2 task repetition benefits were observed too; this was more pronounced under n–2 RR condition (in 17 participants) compared to the n–2 RS (in 3 participants). There was no significant n–2 task repetition cost under n–2 RR but the standard n–2 task repetition cost was present, and there was a considerable variance in n–2 task repetition costs between participants, therefore it was decided that the correlation analysis of WMC and the residual n–2 task repetition cost was still warranted.

The correlation between the n–task repetition controlled for episodic retrieval (i.e., the n–2 task repetition benefit) and the WMC was not significant ($r = .09, n = 41, p = .57$). The standard n–2 task repetition cost (i.e., the n–2 task repetition cost under n–2 RS) also did not correlate significantly with WMC $r = .23, n = 41, p = .14$; Figure 4.5).

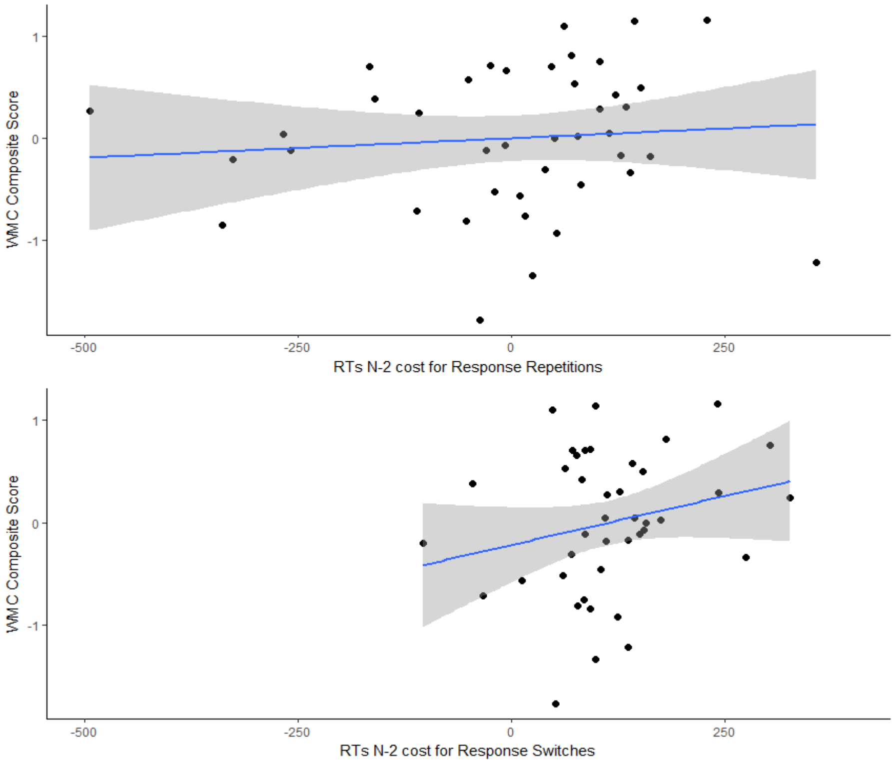


Figure 4.5 A visualization of correlations between RT n–2 task repetition costs and WMC. The lines represent the best-fitting linear regression lines, and the shaded areas reflect the 95%CI for each regression. On the top correlations for n–2 Response Repetitions are presented, and the bottom for n–2 Response Switches.

The BF_{10} for the correlation between the WM and the n–2 task repetition cost controlled for episodic retrieval was 0.1423, which means that the absence of correlation was 7.3 times more likely than the presence of it (this correlation was used as a stopping rule for data collection); for the correlation between WMC and the standard n–2 task repetition cost the BF_{10} was 0.3443; this indicates that there was an anecdotal-moderate evidence for the lack of correlation, with the absence of correlation being 2.9 times more likely than presence of it.

For accuracy, as expected (Grange & Juvina, 2015) accuracy n–2 task repetitions varied considerably between participants for the standard and the n–2 task repetition costs controlled for episodic retrieval; this was true for the standard (ranged from -0.08% to .04%) and the n–2 task repetition costs controlled for episodic retrieval (which range from -0.04 to .07%). Again, looking at individual performance, apart from n–2 task repetition costs, n–2 task repetition benefits were observed as well; this was more pronounced under n–2 RR condition (in 29 participants) compared to the n–2 RS condition (in 12 participants).

The n–2 task repetition cost controlled for episodic retrieval also did not significantly correlate with the WMC ($r = .01, n = 41, p = .95$). Whereas, the standard n–2 task repetition cost (i.e., the n–2 task repetition cost for n–2 Response Switches) did correlate significantly with WMC ($r = .34, n = 41, p = .03$); see Figure 4.6. The latter correlation did not stay significant when *p* value was adjusted for multiple tests (the corrected p-value for the correlation was .025).

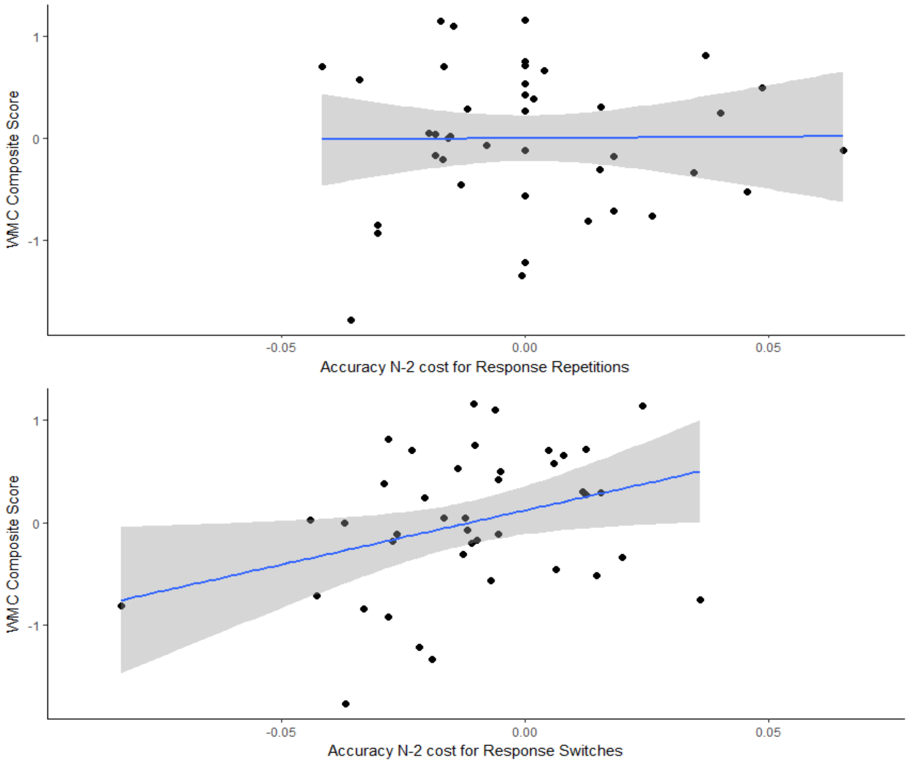


Figure 4.6 A visualization of correlations between accuracy n–2 task repetition costs and WMC. The lines represent the best-fitting linear regression lines, and the shaded areas reflect the 95%CI for each regression. On the top correlations for n–2 Response Repetitions are presented, and the bottom for n–2 Response Switches.

The BF_{10} for the correlation between the WM and the n–2 task repetition cost controlled for episodic retrieval was 0.122, which means that the absence of correlation was 8.2 times more likely than the presence of it; for the correlation between WMC and for the standard n–2 task repetition cost the was 1.275; the BF_{10} indicates that there was no decisive evidence for either of the hypotheses (i.e., the absence and present of correlation).

Bayesian correlation estimation. To strengthen the results from the correlation analysis, a Bayesian parameter estimation for correlation between the residual n–2 task repetition cost and WMC was conducted. With this alternative statistical method, a model representing a correlation is devised, where prior information (e.g., multivariate normal distribution based on means and standard deviations of the variables of interest) is utilized to estimate modelling and correlation coefficient parameters. The advantage of Bayesian correlation parameter estimation is that rather than having a single correlation coefficient point, a posterior distribution of estimated parameters and correlation are obtained with information on how the uncertainty of parameter estimates (i.e., 95% Highest-Density Interval).

To perform Bayesian parameter estimation for a correlation, the n–2 task repetition costs controlled for episodic retrieval, and WMC scores for individual participants were entered into a program called jags (Just Another Gibbs Sampler). This program takes a model of correlation between variables of interest with predictive priors on what patterns in the data are expected (e.g., how data are distributed); in this study, default priors within jags were used (e.g., that the correlation coefficient parameters are equally likely between -1 and 1, Lee & Wagenmakers, 2013). Using priors and observed data, jags estimated the n–2 task repetition cost controlled for episodic retrieval, WMC, and correlation parameters with the Markov Chain Monte Carlo (MCMC; 5000 iterations) simulation, a computer-driven sampling method. Sample values for MSMC were taken from posterior distributions of n–2 task repetition costs controlled for episodic retrieval, WMC, and correlation. The estimated parameters of n–2 task repetition cost controlled for episodic retrieval and WMC fitted the observed data well.

The results showed that, the median of the Bayesian estimated correlation coefficient posterior predictive distribution was $r = .079$; this is lower compared to the single point estimate correlation coefficient obtained in the standard correlation analysis. The BF_{10} for this correlation coefficient estimation is 0.1371, which means that the null hypothesis that there is no relationship between WMC and the n–2 task repetition cost controlled for episodic retrieval is over 7 times (1/ 0.1371) more likely than the alternative that there is a correlation between these two variables.

Discussion

Importantly for the Experiment 5, the findings from Grange and colleagues (in press.) that the n–2 task repetition cost is modulated by episodic retrieval has been replicated; that is, the n–2 task repetition cost was reduced with episodic retrieval matches (n–2 RR), compared to mismatches (n–2 RS). Specifically, the n–2 task repetition cost was reduced so much under episodic matches that performance on ABA and CBA sequence was comparable; whereas, for episodic mismatches performance was worse for ABA compared to CBA sequence (as seen in the standard n–2 task repetition cost).

The variability of the magnitude of n–2 task repetition costs fitted well with the observation from Grange and Juvina (2015) on individual differences in the n–2 task repetition cost. WMC scores also showed a good range of variability. The correlation analysis showed—and was confirmed by the Bayesian estimation of a correlation coefficient parameter—that n–2 task repetition costs were not associated with WMC; this finding supports evidence from Grange and Houghton (n.d.), and Pettigrew et al. (2015). This pattern of results was shown for both the standard and the n–2 task repetition costs controlled for episodic retrieval. However, in light with the evidence that the n–2 task repetition cost (i.e., the standard and the residual type) is not reliable at an individual level (Kowalczyk & Grange, 2017; Chapters 2-3 of this thesis), it is possible that the lack of the correlation between n–2 task repetition costs and WMC was due to the former being not reliable; also, with the n–2 task repetition cost being a difference score—which are argued to be difficult to interpret (Crawford et al., 2008)—there is an issue around if and whether n–2 task repetition cost are meaningful. As yet questions surrounding these issues are not answered, but researchers should be aware of them. Despite these uncertainties, some speculations about the results from Experiment 5 can be made.

Even though the average n–2 task repetition cost controlled for episodic retrieval was very small and was statistically non-significant, its magnitude varied considerably between participants; that is, large n–2 task repetition costs as well as n–2 task repetition benefits were observed, as well as an n–2 task repetition cost approaching zero. Therefore, the lack of significant correlation between WMC and n–2 task repetition costs controlled for episodic retrieval could not have been due to absence of individual differences in n–2 task repetition costs. The standard n–2 task repetition costs showed less variation than the n–2 task repetition costs controlled for episodic retrieval; this was mainly due to less people showing n–2 task repetition benefit (3 participants), compared to the n–2 task repetition cost controlled for episodic retrieval (17 participants). Numerically, the standard n–2 task repetition cost was more associated with WMC (in RTs and accuracy) than was the n–2 task repetition cost when controlled for episodic retrieval. At this stage it is speculative, but these results may indicate that when performance on task-switching is facilitated through episodic matches, there may be less need for WM to deal with conflicting task sets, as task sets' episodic traces are automatically retrieved.

Unlike in Grange and Houghton's (n.d.) study, the current study did not manipulate WML during task-switching; this means that, based on results from the current study, which only looked at association between the WMC and inhibition, limited conclusions can be made about the relationship between those two factors. The n–2 task repetition cost controlled for episodic retrieval not being related to WMC does not necessarily mean that this effect is not modulated by attentional resources; that is, individual differences in WMC did not seem to influence the magnitude of n–2 task repetition costs controlled for episodic retrieval; however, at this stage it cannot be ascertained whether limiting WM resources (achieved via experimental manipulation of WM load) would influence the efficiency of task-switching and/or inhibition. Specifically, it is possible that exhausting WM resources—rather than looking at individual differences in WMC—will have an effect on whether and how much inhibition is deployed.

Task-switching and articulatory suppression (AS). Task-switching research shows (Baddeley et al., 2001; Bryck & Mayr, 2005; Butler et al., 2011; Emerson & Miyake, 2003; Goschke, 2000; Meiran & Kessler, 2008; Miyake et al., 2004; Saeki & Saito, 2004a, 2004b) that increasing WML—for example, with an articulatory task which involves task-irrelevant utterances—increases the SC, which has been interpreted as evidence that WM is important for task-switching. This is thought to be because of an aspect of WM known as the phonological loop—an important mechanism for rehearsal and maintenance of a task’s representation in WM (Injoque-Ricle, Barreyro, Formoso, & Jaichenco, 2015; Kirkham, Breeze, & Marí-Beffa, 2012)—is likely to be affected adversely by AS. The phonological loop acts as inner speech which aids performance on tasks through sub-verbal rehearsal of information needed for task execution; as such, introduction of AS interferes with phonological loop because it is very difficult to perform verbalisation at the same time as rehearsing information sub-verbally, hence performance is no longer facilitated through the phonological loop (e.g., SC increases).

From research on multitasking, using a virtual environment setting (i.e., Virtual Errands Test, Law, Logie, & Pearson, 2006) it was shown that when people were required to complete different goals (i.e., simple errands) when simultaneously having to do a secondary task (e.g., AS), the performance on the primary task was negatively affected. This was interpreted as evidence that AS—which was used to increase WML—was interfering with the phonological loop and making people not being able to “rehearse” the representations of goals they needed to complete for the primary tasks. Furthermore, in the same study it was noted that the phonological loop seemed particularly important for early stages of learning a given task; that is, the primary task was most affected by AS if the primary task was not previously performed on its own. This gives further evidence that WM resources are important for shifting between goals, especially in relatively unpractised tasks. In another study (Soto & Humphreys, 2008) it has been shown that WM seems to facilitate even visual selection tasks; specifically, if AS was added as a secondary task, performance on visual search was reduced. This was taken as evidence that WM is necessary for visual selection too. Together, in general, these studies show that WML can be manipulated with AS—which interferes WM’s phonological loop—which can affect performance on cognitive tasks requiring shifting between tasks, maintaining goals in mind, and as well as task involving visual search.

Using AS and foot-tapping manipulations (Goschke, 2000; Kirkham et al., 2012), an unpublished study demonstrated that preparation does not affect the n–2 task repetition cost (Grange, n.d.). Assuming that sub-verbalisation is important for task preparation and that n–2 task repetition cost can be reduced with preparation, AS should hinder performance on cognitive tasks by influencing preparation stage in task-switching. Importantly, it was shown that task-switching performance on BI paradigm can be successfully manipulated with AS (i.e., performance was slower under AS condition compared to control condition). In terms of the n–2 task repetition cost, it was also shown that AS had no effect on n–2 task repetition cost. That is, assuming that sub-verbalisation is important for task preparation and that n–2 task repetition cost can be reduced with preparation, AS should have increased the n–2 task repetition cost, but it only slowed participants' performance; as such, it was concluded that preparation does not affect the n–2 task repetition cost.

In terms of specific mechanisms responsible for worse performance on cognitive tasks (e.g., task-switching) under conditions of AS (which are thought to hinder WM processing) can be due to a few different reasons, some of which are introduced next. For example, assuming that the phonological loop is important for retrieval of task-sets—as it can facilitate response selection (e.g., verbal self-cuing, Miyake, Emerson, Padilla, & Ahn, 2004)—if a given task's representation cannot be retrieved effectively, conflict in response selection may be present, hence more inhibition may be needed. More inhibition would also be required if the task could not be activated/ maintained effectively—that is, not activated to a stronger threshold than alternative, currently not relevant tasks' thresholds—because other task representations may lead to more interference (Bryck & Mayr, 2005; Emerson & Miyake, 2003). Alternatively, assuming that phonological loop manipulations interfere with how effectively WM works (Baddeley, 1996; Repovs & Baddeley, 2006), and that inhibition is dependent on WM (e.g., Conway & Engle, 1994), if task representations cannot be maintained through rehearsal (i.e., under AS creating high WML)—making representations weakly activated—less interference would be expected and, hence less inhibition to resolve it.

Also, assuming that WM is important for accessing a task's information form LTM (Baddeley et al., 2001; Emerson & Miyake, 2003; Miyake et al., 2004; Saeki & Saito, 2004a), those WM resources are particularly needed for performing tasks with abstract cues, that is cues which do not have pre-experimental associations to the task. This is because cues are used to retrieve task-relevant information which is then manipulated and matched to demands of a current task (Mayr & Kliegl, 2000, 2003), which is thought to take place in WM (Emerson & Miyake, 2003; Kane et al., 2007;

Mayr & Keele, 2000; Monsell, 2003). Therefore, it is believed that performance on task-switching would be expected to be most sensitive to WML manipulations in paradigms which utilise abstract cues of which interpretation requires endogenous control; and tasks which can be performed via exogenous processes (i.e., stimulus driven responses) would not require the involvement of WM, as goals can be achieved via retrieval of information directly from long term memory (LTM).

Above mentioned literature would imply that interfering with retrieval stage in task-switching (e.g., with AS) should affect preparation; and since the n–2 task repetition cost has been shown to be modulated by episodic retrieval (Grange et al., 2017; Chapter 2–4 of this thesis), it was of interest to examine whether the episodic retrieval effect on the n–2 task repetition cost is modulated by AS. Grange (n.d.) would suggest that AS does not affect the n–2 task repetition cost, however, the BI paradigm used in that study did not control for episodic retrieval, therefore it is uncertain whether it was the n–2 task repetition cost or the episodic retrieval not being affected by AS.

In an attempt to further understand if WM has a role in driving the n–2 task repetition cost (i.e., cognitive inhibition), Experiment 6 was designed to manipulate WML by introducing AS to a BI paradigm. This secondary task should interfere with WM resources (e.g., the phonological loop) affecting task-switching performance in the following ways. If the residual n–2 task repetition cost reflects inhibition—speculated to rely on WM resources (Conway & Engle, 1994)—smaller residual n–2 task repetition cost should be observed under condition of High WML compared to Low WML. If the extent to which inhibition is deployed depends on how efficiently a task's parameters are retrieved, activated, or maintained in WM, we should also see larger residual and standard n–2 task repetition costs in High WML compared to Low WML. However, if the residual n–2 task repetition cost reflects exogenous processes (i.e., automatic), we would expect to see no effect of WML on the residual n–2 task repetition cost. Also, since episodic retrieval modulates the n–2 task repetition cost more under non-transparent cues compared to transparent cues (Grange et al., 2017), the AS is expected to interfere with that modulation if episodic retrieval depends on WM involvement.

WML was manipulated by adding AS to a BI paradigm; the High WML condition required different days of the week to be uttered in a random order whilst switching between tasks, and the Low WML condition required saying the word "Sunday" whilst task-switching. The two WML conditions were meant to differ in terms of the level of the demand put on WM; in the Low WML repeating a word "Sunday" requires less control and WM manipulation than generating different day of the week in the High WML. As such, both AS conditions were expected to interfere with preparation stage of task-switching, but Low WML was meant to interfere less than the High WML. This is the first time when WML is manipulated in this manner within the BI paradigm.

Experiment 6

Method

Participants. Thirty-one participants were recruited from Keele University in exchange for partial course credits. Participants were at least 18-years-old (Mean 18.68, SD 0.70; 22 females), understood spoken and written English, and had normal or corrected-to-normal vision. Data of eight participants were removed due to incomplete data (i.e., participants whom performed only one WML condition during testing), and data of three were removed due to accuracy being lower than required 80%; a total of twenty participants were used in the main analysis.

Stopping rule. As in Experiments 2–5, the sample size for the current experiment was determined via BFs for a set of probable Bayesian factorial analysis of variance models (Rouder et al., 2016).

First, using Rouder and colleagues' method (with default priors) four different models explaining data were examined, with the n–2 task repetition cost as DV, and Response and WML as IVs. The four models were: 1) the full model—of most interest to this experiment—consisted of the main effect of Response, main effect of WML, and the interaction between them; 2) main effects of Response and WML; 3) main effect of Response; and 4) main effect of WML. In each model, the random effect of participants was added too, which was the common denominator against the models. The BF_{10} is calculated as a ratio of the model's evidence—given data—against the common denominator.

As in previous experiments which used BFs for a stopping rule, the initial sample of twenty participants collected before the initial BFs calculation. Data collection stopped when one of the criterions was reached; that is, when BF_{10} was below 1/6 or over 6, or results were inconclusive at eighty participants were tested. At the 31st participant data collection stopped as the BF_{10} was 0.08223; this is below 1/6, indicating that one of the alternative models fitted data better than the full model. Specifically, data were about eleven times more likely explained with the main effect of WML, rather than the full model (i.e., the interaction between the n–2 task repetition cost, Response, and WML.

Apparatus and stimuli. The apparatus and the stimuli in the BI paradigm were the same as in Experiment 5.

Procedure. Participants attended a single session (1 hour) during which they performed two versions of the BI paradigm adapted to manipulate WML; one with High WML and one with Low WML.

Backward Inhibition paradigm. This paradigm was the same as in Experiment 5, with exception that instead of 480 trials, participants performed 360 trials. The number of trials was changed due to time constraints. Participant-paced breaks were provided in between the blocks of 120 trials.

WM load. The WML was manipulated by introducing AS into the BI paradigm creating two conditions, High and Low WML. In the High WML condition, participants said different days of the week in a random order, one day per second. In the Low WML condition participants said the word “Sunday”, once per second. Before the practice a metronome was played for participants to get an idea of what one word per second feels like. Participants were instructed to perform task-switching and AS simultaneously, but they could not synchronise performance on both; that is, they were asked not to synchronise their responses with verbalizations. As such, they were told, if they made a mistake on task-switching, they were to continue with verbalization, and if they made a mistake verbalizing, they had to continue with task-switching. Participants stopped verbalizations during the breaks. The order of WML conditions was counterbalanced between participants.

Design. A within-subjects design was utilized to examine RTs (milliseconds) and accuracy (%) as dependent variables, and the *Task Sequence* (ABA vs. CBA), *Response* (n–2 Response Repetition, RR vs. n–2 Response Switch, RS), and WML

(High WML vs. Low WML) as independent variables. Performance was analysed via three-factor repeated measures ANOVA.

Results

Data trimming. Data of participants who scored less than 80% on accuracy were removed. This criterion is different than the one used in Experiment 5 because it was uncertain how much WML manipulation would influence performance on task-switching. Therefore, to include as much data as possible, the overall accuracy criterion was lowered from 90% to 80%. As in Experiment 5, null-trials and two trials after an error, were removed for accuracy analysis; followed with error-trials, RTs less than 150ms, and RTs slower than 2.5 standard deviations from participant's mean/ per experimental condition, being removed for RTs analysis. Overall, 33.95% of trials were removed, 27.79% from High WML (27.45% for n–2 RR; 27.89% for n–2 RS), and 20.26% for Low WML (17.09% for n–2 RR; 20.45% for n–2 RS). There were considerably more trials removed in data trimming in this investigation compared to Experiment 5; this could be attributed to WML manipulation which likely interfered with task-switching. For example, participants made more errors in Experiment 6 (accuracy around 92%) compared to Experiment 5 (98%), and since two trials after every error are removed, more trials were removed as a consequence of lower accuracy.

Reaction times analysis. For means and standard errors of RTs and accuracy (%) see Table 4.2.

A three-factor repeated measures ANOVA revealed that the main effect of Task Sequence was significant, *F* (1, 19) = 6.19, *p* = .02, η_g^2 =. 005 (ABA 1411ms; CBA 1350ms); but the main effect of Response was not significant, *F* (1, 19) = 1.20,

$p = .29$, $\eta_g^2 = .001$ (n–2 RR 1395ms; n–2 RS 1366ms). However, the main effect of WML was significant, F (1, 19) = 45.39, $p < .001$, η_g^2 =. 44, as performance was faster for Low WML (1016ms) compared to High WML (1745ms). This means that the WML manipulation worked.

The interaction between Task Sequence and Response was not significant, F (1, 19) = 0.23, $p = .64$, $\eta_g^2 < .001$; under n–2 RR the n–2 task repetition cost was 52ms (ABA, 1421ms and CBA 1369ms), and under n–2 RS the n–2 task repetition cost was 70ms (ABA 1401ms and CBA 1331ms); against a recent study that showed that the n–2 task repetition cost is modulated by episodic retrieval (Grange et al., 2017). The interaction between Task Sequence and WML was also not significant, F (1, 19) = 0.19, $p = .67$, $\eta_g^2 < 001$; under High WML the n–2 task repetition cost was 47ms (ABA 1769ms and CBA 1722ms), and under Low WML the n–2 task repetition cost was 74ms (ABA 1053ms and CBA 979ms). Response did not interact significantly with WML, F (1, 19) = 0.48, $p = .50$, $\eta_g^2 = .001$; under High WML average performance on n–2 RR was 1770ms and on n–2 RS it was 1720, whereas under Low WML performance on n–2 RR was 1011ms and on n–2 RS it was 1020ms. The three-way interaction between Task Sequence, Response and WML was also not significant, F (1, 19) = 0.17, $p = .68$, $\eta_g^2 < .001$ (Figure 4.7). RTs density functions for n–2 task repetition costs (n–2 RR and n–2 RS), for High and Low WML are depicted in Figure 4.8.

Table 4.2 Mean Response times (RTs) and accuracy (%), with standard errors, for ABA and CBA sequences, within n–2 Response Repetitions (RR) and n–2 Response Switches (RS), for High and Low WML.

	Task sequence			
	ABA		CBA	
WM load	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
High				
n–2 RR	1773 (302)	91.70 (.018)	1733 (284)	89.93 (.017)
n–2 RS	1735 (276)	90.40 (.015)	1660 (268)	91.30 (.013)
Low				
n–2 RR	1056 (126)	94.50 (.009)	960 (110)	93.90 (.015)
n–2 RS	1029 (114)	92.90 (.014)	974 (118)	95.10 (.01)

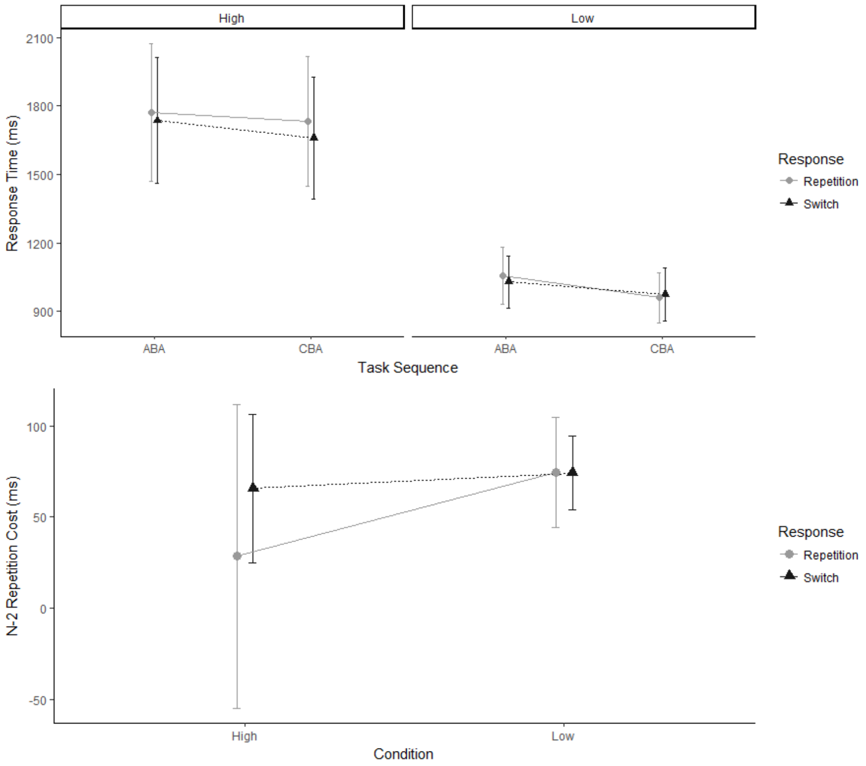


Figure 4.7 At the top, an illustration of a non-significant three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and WML (High vs Low) for RTs. At the bottom, n–2 task repetition costs varying across Response and WML. Error bars de-note ±SE around the mean.

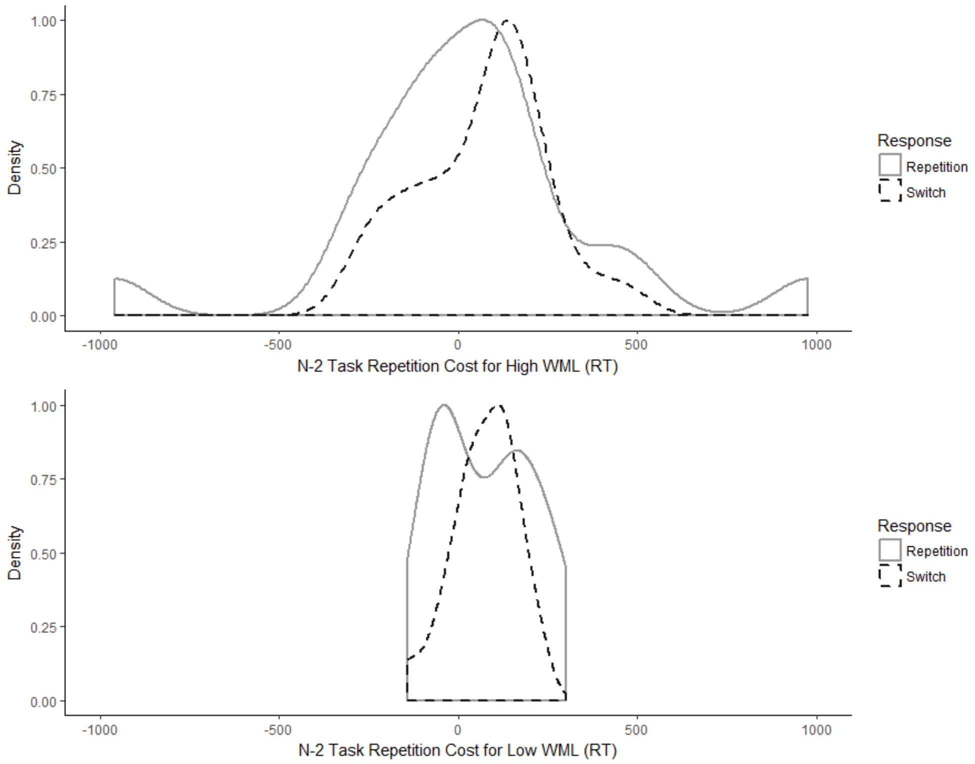


Figure 4.8 Density functions for RT n–2 task repetition costs for High and Low WML under n–2 Response Repetitions and n–2 Response Switches.

Log-RT analysis. Since there was a considerable difference in RTs between Low WML and High WML, RTs were log-transformed and a three-way ANOVA was repeated. This analysis showed a significant main effect of Task Sequence, *F* (1, 19) = 14.38, $p < .01$, $\eta_g^2 = .01$ (ABA 7.19, CBA 7.13). The main effect of Response remained non-significant, *F* (1, 19) = 0.50 $p = .49$, $\eta_g^2 < .001$ (n–2 RR 7.17, n–2 RS 7.16). And, the main effect of WML became larger, *F* (1, 19) = 69.19, $p < .001$, $\eta_g^2 = .50$ (High WML 7.42, Low WML 6.90).

The interaction between Task Sequence and Response remained non-significant, *F* (1, 19) = 0.49, $p = .49$, $\eta_g{}^2 < .001$; under n–2 RR, ABA was 7.19 and CBA was 7.14, whereas, under n–2 RS, ABA was 7.19 and for CBA it was 7.12. The interaction between Task Sequence and WML also remained non-significant, *F* (1, 19) = 1.62, $p = .22$, $\eta_g{}^2 = .002$; under High WML ABA was 7.43 compare to CBA

7.40, and under Low WML ABA was 6.94 compared to CBA of 6.87. The interaction between Response and WML remained non-significant too, *F* (1, 19) = 0.13, *p* = .72, η_g^2 > .001; for High WML n–2 RR averaged log-RT was 7.43 and for n–2 RS it was 7.41, for Low WML, n–2 RR log-RT was 6.1 and for n–2 RS it was 6.90. Lastly, the three-way ANOVA was also non-significant, *F* (1, 19) = 0.28, *p* = .61, η_g^2 <. 001 (see Figure 4.9).

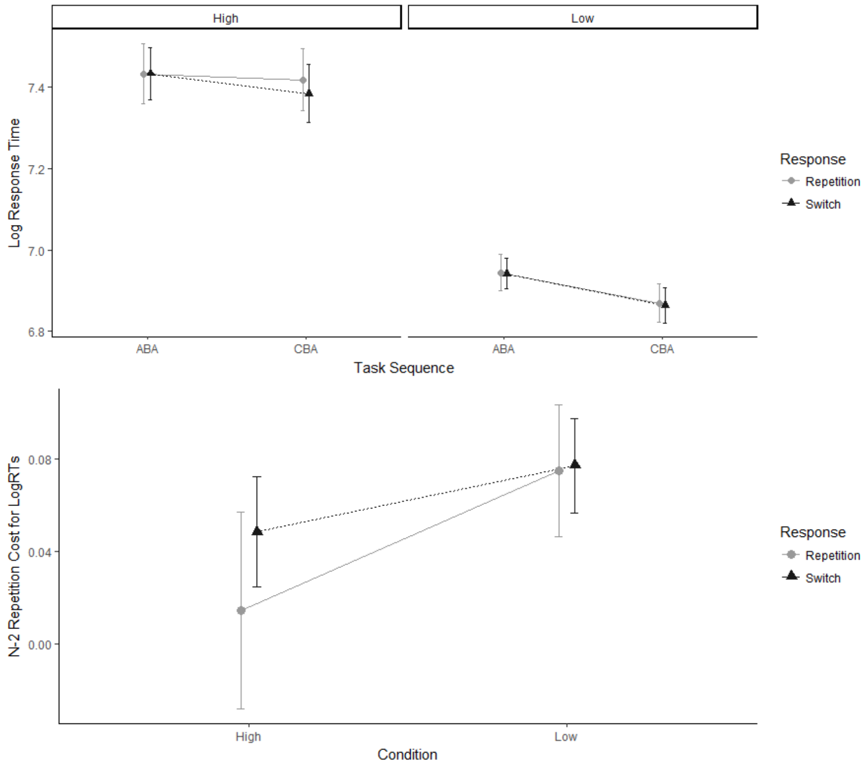


Figure 4.9 At the top, an illustration of a non-significant three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and WML (High vs Low) for log-RT. At the bottom, n–2 task repetition costs varying across Response and WML. Error bars de-note ±SE around the mean.

Accuracy. A three-factors repeated measures ANOVA for accuracy revealed the following results. The main effect of Task Sequence was non-significant, *F* (1,

19) = 0.01, *p* = .93, η_g^2 = .0001; that is, average accuracy on ABA sequence was 92.36% and 92.41% on CBA sequences. There was no main effect of Response, *F* (1, 19) = 0.04, *p* = .84, η_g^2= .0001; that is, for n–2 RR accuracy was 92.32% and for n–2 RS it was 92.44%. However, the main effect of WML was significant, *F* (1, 19) = 13.20, *p* = .002, η_g^2= .07; that is, participants were more accurate for Low WML (94.10%) compared to High WML (90.66%) condition.

There interaction between the Task Sequence and Response was significant, *F* (1, 19) = 6.10, *p* = .02, η_g^2=. 02, showing that the n–2 task repetition cost was modulated by episodic retrieval (Grange et al., 2017). Individual t-tests revealed that for n–2 RR there was no significant difference in accuracy on ABA (93.07%) vs. CBA sequences (91.57%) with a trend for n–2 task repetition benefit present, *t* (39) = 1.4, *p* = .20, 95%CI [-0.006, 0.036]; whereas, for n–2 RS there was a significant difference between accuracy on ABA (91.64%) compared to CBA (93.24%) sequences, *t* (39) = -3.1, *p* = .004, 95%CI [-0.026384, -0.005582]. The interaction between Task Sequence and WML was not significant *F* (1, 19) = 2.33, *p* = .14, η_g^2 =. 004 (under High WML accuracy on ABA was 91.01% and for CBA it was 90.31%; under Low WML accuracy was 93.70% for ABA and 94.50% for CBA); the interaction between Response and WML was also not significant, *F* (1, 19) = 0.28, *p* = .60, η_g^2 <. 001 (under High WML accuracy for n–2 RR was 90.47% and for n–2 RS it was 90.85%; whereas under Low WML for n–2 RR it was 94.18% and for n–2 RS it was 94.03%). Lastly, the three-way interaction (Task Sequence, Response, WML) was not significant, *F* (1,19) = 0.05, *p* = .82, η_g^2< .001 (Figure 4.10). Accuracy density functions for n–2 task repetition costs (n–2 RR and n–2 RS), for High and Low WML are depicted in Figure 4.11.

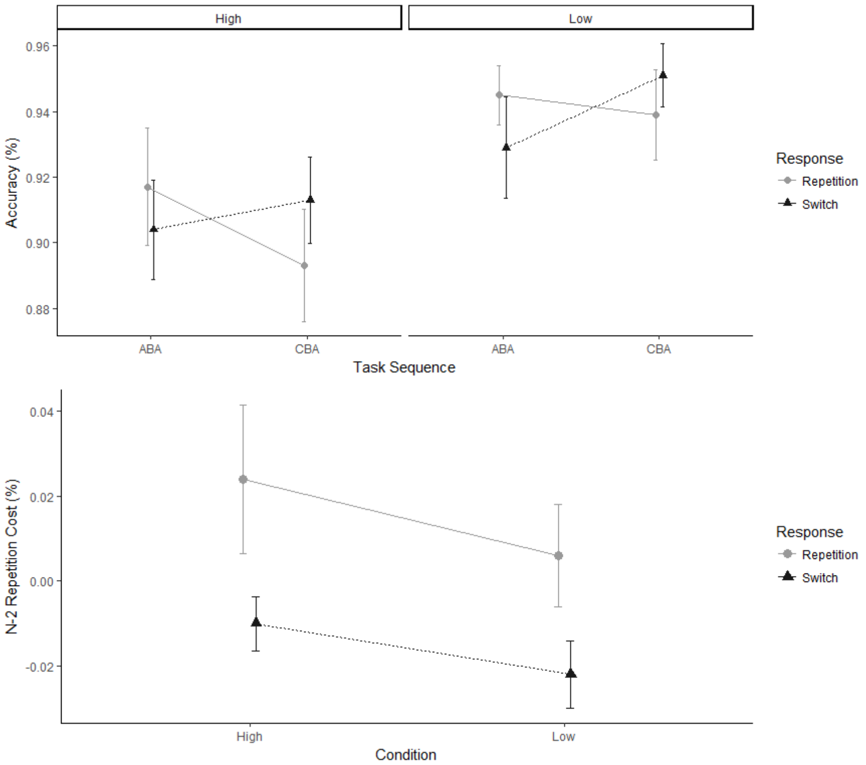


Figure 4.10 At the top, an illustration of results from the three-way ANOVA analysis of accuracy. The main effect of WML and interaction between Task Sequence and Response were significant. At the bottom, the illustration of n–2 task repetition costs for n–2 Response Repetition and n–2 Response Switches under High and Low WML conditions. Error bars de-note ±SE around the mean.

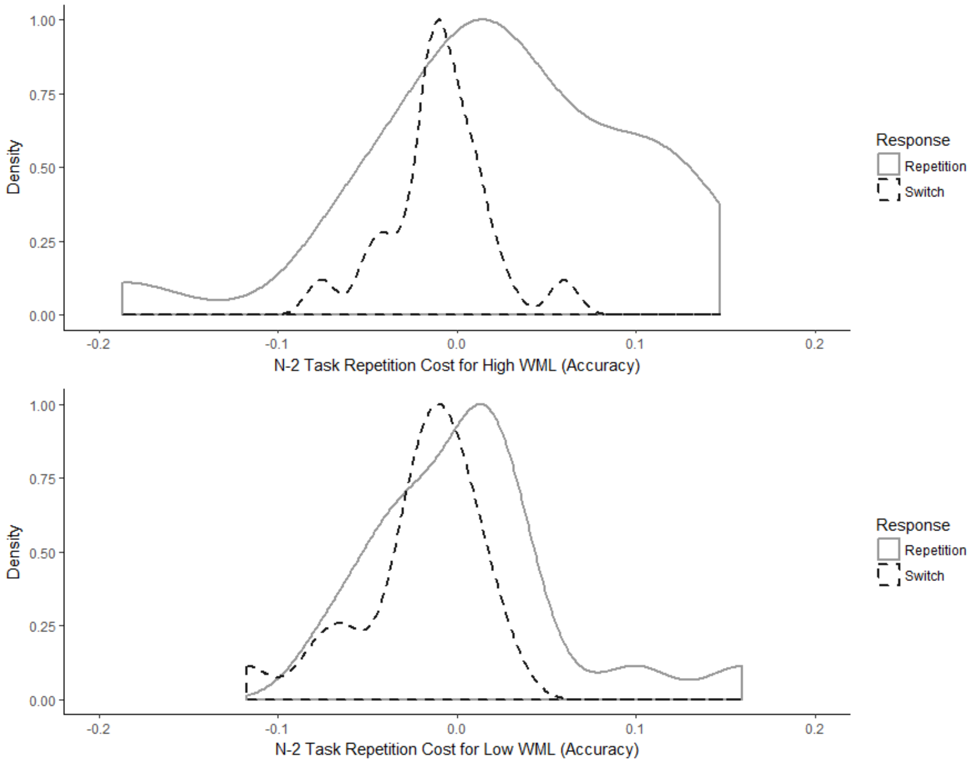


Figure 4.11 Density functions for accuracy n–2 task repetition costs for High and Low WML under n–2 Response Repetitions and n–2 Response Switches.

Bayes factors. Table 4.3 presents the results from the Bayesian comparison of n–2 task repetition costs being explained by four different models (see Stopping Rule section for details). For completeness, BF_{10} is reported for log-RT and accuracy, as well as RTs. The Bayesian analysis supports the frequentists' analysis of variance of RTs; the n–2 task repetition cost was over twelve times more likely (1/.08224) to be explained by the model with the main effect of WML than the full model (main effect of WML, main effect of Response, and interaction between them). The model with the main effect of Response was over eleven times more likely (1/.08931) than the full model. The log-RT analysis favored the model with the main effect of WML, which was over ten times more likely (1/.09508) than the full model. In terms of accuracy, the result was that accuracy n–2 task repetition costs were over six times more likely (1/.1604) explained by the main effect of WML than the full model.

Table 4.3 Bayes factors (BF) for models predicting the n–2 task repetition cost as the dependent variable and Response and WML as IVs reaction time—RTs, log-RT—and accuracy; the BFs for log-RT and accuracy, are reported for completeness. The reported models were compared to a common denominator of the n–2 task repetition cost predicted by a random effect of Participant. “+” indicates an addition of an effect, and “*” indicates an interaction.

N–2 task repetition cost	Model	BF
RTs	Response (1)	0.2442
	WML (2)	0.2652
	R + WML (3)	0.06544
	R + WML + R * WML (4)	0.02181
log-RT	Response (1)	0.2713
	WML (2)	0.6652
	R + WML (3)	0.1789
	R + WML + R * WML (4)	0.06325
Accuracy	Response (1)	0.47889
	WML (2)	6.24923
	R + WML (3)	3.13698
	R + WML + R * WML (4)	1.00258

Discussion

Experiment 6 further investigated the relationship between n–2 task repetition costs and WM; this was achieved by manipulating WML within the BI paradigm. There were two WML conditions which required participants to utter one word per second; in the High WML condition participants said out loud different days of the week in a random order, and in the Low WML condition participants said word “Sunday”. Assuming that n–2 task repetition cost (controlled for episodic retrieval) reflect inhibition which depends on WM (Conway & Engle, 1994), it was hypothesised that Low WML would not interfere with how much inhibition is deployed during task-switching, whereas the High WML condition should exhaust WM resources which should result in less WM resources left for inhibition deployment. The main prediction was that inhibition—as reflected via the n–2 task repetition cost—would be smaller under the High WML compared to Low WML condition.

The results were as follows; the n–2 task repetition cost was present at RT but not accuracy level, where performance on ABA and CBA was comparable. The WML manipulation seemed to have worked because performance was considerably slower and less accurate under High WML compared to Low WML; there was no overall effect of Response though, as performance was similar under n–2 RR and n–2 RS. There was a partial effect of Response (i.e., episodic retrieval) on the n–2 task repetition cost; that is, the accuracy n–2 task repetition cost was modulated by episodic retrieval—no n–2 task repetition cost under n–2 RR but present for n–2 RS—but for RT performance this cost was not modulated, although numerically the n–2 task repetition cost was smaller under n–2 RR compared to n–2 RS. The WML did not affect the n–2 task repetition cost, although numerically it was lower under High WML compared to Low WML; moreover, Bayesian analysis of variance for RT and accuracy performance showed that the n–2 task repetition cost was more likely explained by the WML manipulation than the alternative explanation (i.e., main effect of Response/ WML and interaction between them). In terms of the episodic retrieval, it did not seem to be affected by WML; however, numerically performance was slower under High relative to Low WML, with RTs being comparable under n–2 RR and n–2 RS in the Low WML condition but in High WML condition RTs for n–2 RR were considerably faster under n–2 RR than n–2 RS.

Experiment 6 presented with some difficulties; namely it was found that, the WML manipulation used resulted in data being “noisy”. That is, performance was much slower in High WML compared to Low WML, and almost a third of participants did not complete one of the WML conditions in the allocated time. This indicates that perhaps the WML manipulation was too difficult and might have interfered with task-switching more than intended. Also, the n–2 task repetition cost did not seem to be modulated by episodic retrieval as it has been shown by other studies (Grange et al., 2017; Experiments 2–5 of this thesis); and assuming that the residual n–2 task repetition cost is a truer reflection of inhibition—which was hypothesised to be influenced by WML in the current study—it is difficult to interpret the results of Experiment 6 clearly or confidently, because the residual n–2 task repetition cost was not present, and this is the first investigation of this sort.

Experiment 7 aimed to yet again manipulate WML but this time using what is considered an easier WML manipulation. Assuming that the High WML condition was too difficult in Experiment 6, Experiment 7 aimed to manipulate WML using a simpler AS for High WML and a motor task for Low WML. For the Low WML condition, “foot tapping” was chosen as a suitable secondary task, and for High WML “blah blah blah” AS was chosen. Foot tapping is considered a suitable control condition for the current study as it is believed not to require executive functioning, it is simple to perform (Baddeley, 1986, 1990), it has been previously used as a control task in task-switching studies looking at SC and WM (e.g., Miyake et al., 2004; Saeki et al., 2006), and it is not meant to disrupt WM’s phonological loop hypothesised to be potentially important for explaining individual differences in the n–2 task repetition cost.

For Experiment 7 it was speculated that if n–2 task repetition costs are present, they are expected to be modulated by episodic retrieval, and that inhibition (as reflected in residual n–2 task repetition cost) should be modulated by WML. Specifically, the residual n–2 task repetition cost was expected not be affected by foot tapping compared to the AS condition which was expected to decrease residual n–2 task repetition costs; that is if residual n–2 task repetition cost reflects inhibition, and if inhibition relies on WM resources.

Experiment 7

Method

Participants. Twenty participants were recruited from Keele University in exchange for partial course credits. Participants were at least 18-years-old (Mean 19.25, SD 1.25; 17 females), understood spoken and written English, and had normal or corrected-to-normal vision. All participants' data were used in analysis.

Stopping rule. As in Experiments 2–6, the sample size for the current investigation was determined via BFs; specifically, BFs were obtained through the Bayesian factorial analysis of variance (Rouder et al., 2016). The procedure and data collection stopping criteria were the same as in previous experiments.

The four models compared were: 1) the full model of the main effects of Response/ WML (i.e., Foot Tapping, FT vs. AS), and an interaction between them; 2) the main effects of WML and Response; 3) the main effect of WML:; and 4) the main effect of Response. In each model, the random effect of participants was added; and, the model explaining residual n–2 task repetition costs as random effects was used as a common denominator.

As in Experiment 1 and 2 data were collected from a sample of twenty participants, with intention to collect more data, before the initial BF analysis was conducted (Schönbrodt et al., 2015). At twenty participants, the BF_{10} for the alternative model (i.e., the full model) compared to the alternative model (i.e., the main effect of Response), was 0.159; which translates to the alternative model being 2.37 times more likely than the full model. The BF_{10} just about reached one of the two criteria (BF_{10} at < 1/6 or < 6) which is interpreted as anecdotal evidence for the alternative hypothesis. Data collection stopped at twenty participants due to ethical approval expiration before more data were collected.

Procedure. Participants attended a single 1-hour session during which they performed two BI paradigms, each with a different WML; the Foot Tapping (FT) and Articulatory Suppression (AS). The task-switching procedure was the same as in Experiments 5–6. As in Experiment 6 there were three blocks of 120 trials in each for AS and FT condition.

The AS condition involved performing BI paradigm with added verbalisation which was saying "blah blah blah" once per second. Once participants practiced verbalisation, they moved on to practicing task-switching; they were asked to carry out task-switching practice with verbalisation. As in Experiment 6, participants were asked to not synchronise the response with verbalisation.

The FT condition involved doing BI whilst tapping the right foot, aiming to make one tap per second. Participants practiced foot tapping, and then task-switching with foot tapping, before they went on to do the actual experiment.

Design. A within-subjects design was utilized to examine RTs (milliseconds) and accuracy (%) as dependent variables, and the *Task Sequence* (ABA vs. CBA), *Response* (n–2 Response Repetition, RR vs. n–2 Response Switch, RS), and WML (FT vs. AS) as independent variables. Performance was analysed via three-way repeated measures ANOVA.

Results

Data trimming. As in experiments 1–6 data were trimmed in terms of null trials, accuracy and RTs. Overall, this led to a removal of 19.4% of trials; from that, 20.8% trials were removed from AS condition (20.6% n–2 RR; 20.3% n–2 RS), and 18.1% from FT condition (21.5% n–2 RR; 20% n–2 RS).

Reaction times analysis. RTs analysis revealed that there was a main effect of Task Sequence, F (1, 19) = 24.44, $p < .001$, $\eta_g^2 = .019$, as performance was faster on CBA (947ms) compared to ABA (1012ms) sequence. Response had no significant effect on RTs, F (1,19) = 0.50, $p > .4$, $\eta_g^2 = .001$ (n–2 RR 975ms; n–2 RS 985ms). The WML also had no significant effect on RTs, F (1,19) = 0.15, $p > .7$, $\eta_g^2 = .001$ (AS 972ms ; FT 988ms).

The interaction between Task Sequence and Response was non-significant, F (1,19) = 2.42, $p > .1$, $\eta_g^2 = .002$ (n–2 task repetition cost for n–2 RR, 45ms and for n–2 RS, 85ms). The interaction between Task Sequence and WML was also non-significant, F (1,19) = 0.80, $p > .3$, $\eta_g^2 = .001$ (n–2 task repetition cost for AS, 51.3ms and for FT 78.7ms). The interaction between Response and WML was not significant, F (1,19) = 0.005, $p > .9$, $\eta_g^2 = .000$ (numerically RTs were faster for Response Repetitions; by 9.3ms for AS, and 11.9ms for FT).

The three-way interaction between the Task Sequence, Response, and WML was not significant, F (1,19) = 0.56, $p > .4$, $\eta_g^2 = .001$ (the n–2 task repetition cost was smaller for Response Repetitions for both AS and FT). The visualization of the three-way interaction, as well as n–2 task repetitions costs are displayed in Figure 4.12. For densities of n–2 task repetition costs see Figure 4.13; and for descriptive statistics, see Table 4.4.

Table 4.4 Mean Response times (RTs) and accuracy (%), with standard errors, for ABA and CBA sequences, within n–2 Response Repetitions (RR) and n–2 Response Switches (RS), for FT (foot taping) and AS (articulatory suppression).

	Task sequence			
	ABA		CBA	
WML	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
FT				
n–2 RR	1033 (134)	96.65 (.009)	952 (108)	95.10 (.01)
n–2 RS	1030 (119)	93.90 (.009)	946 (112)	95.70 (.007)
AS				
n–2 RR	970 (108)	93.20 (.016)	966 (121)	94.50 (.012)
n–2 RS	1026 (117)	92.70 (.011)	935 (104)	94.80 (.008)

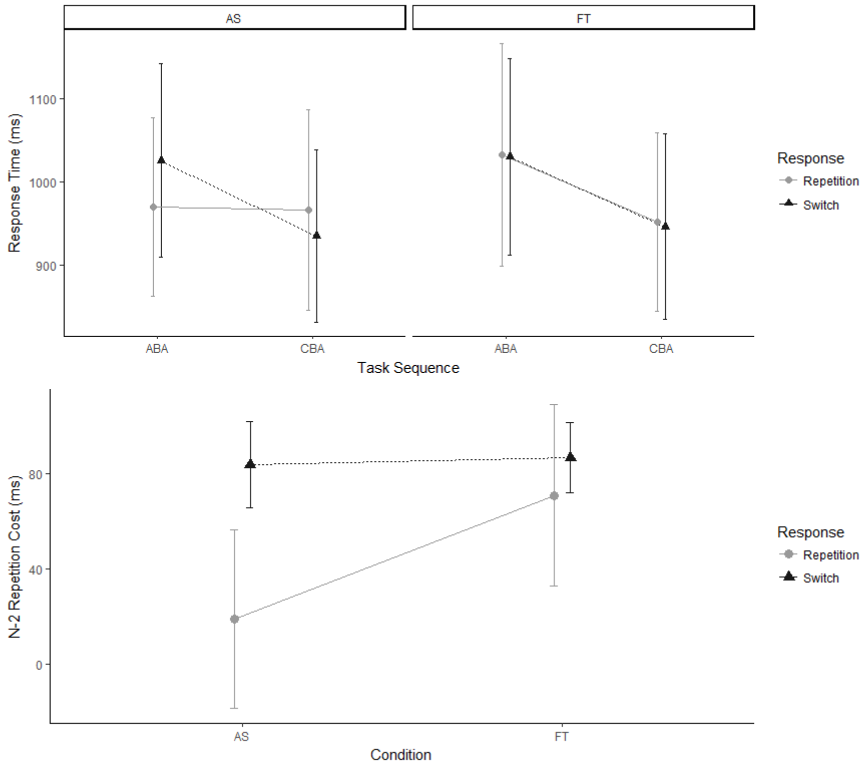


Figure 4.12 At the top, an illustration of a non-significant three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and WML (AS vs. FT) for RTs. At the bottom, n–2 task repetition costs varying across Response and WML. Error bars de-note ±SE around the mean.

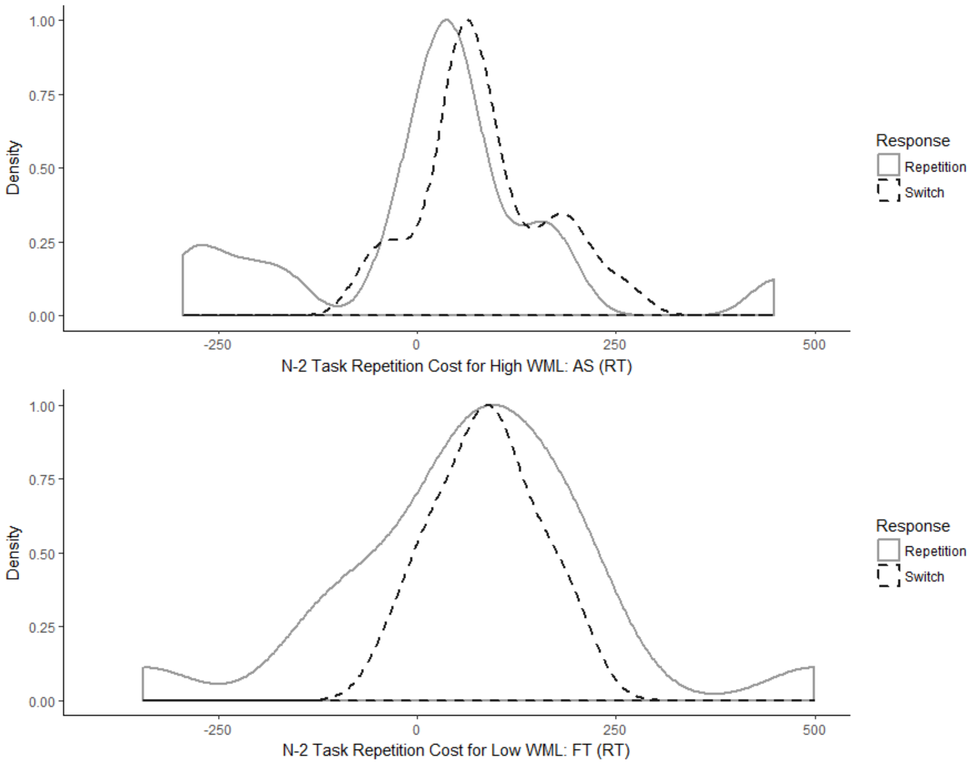


Figure 4.13 Density functions for RT n–2 task repetition costs for Articulatory Suppression (AS) and Foot Tapping (FT) under n–2 Response Repetitions and n–2 Response Switches.

Accuracy analysis. The three-way ANOVA revealed that there was a significant main effect of Task Sequence, $F(1,19) = 6.01$, $p < .02$, $\eta_g^2 = .011$, as participants were less accurate on ABA (94.05%) compared to CBA (95.04%) sequence. The main effect of Response was not significant $F(1,19) = 0.80$, $p > .38$, $\eta_g^2 = .004$ (94.80% for n–2 RR and 94.30% for n-2 RS). The WML had a significant main effect on RTs, $F(1,19) = 4.91$, $p < .04$, $\eta_g^2 = .03$, as participants had lower accuracy on task-switching under FT (93.80%) than on AS (95.30%) task.

The interaction between Task Sequence and Response was significant, $F\ (1, 19) = 7.07$, $p < .02$, $\eta_g{}^2 = .01$; for n–2 RR accuracy on ABA was 94.80% and on CBA it was 94.80%, and for n–2 RS, for ABA accuracy was 93.30% compared to CBA of 95.30%. Individual t-tests revealed that under n–2 RR there was no significant difference between ABA and CBA sequence, *t* (39) = 0.02, *p* = .98, 95%CI [-0.016, 0.016]; however, under n–2 RS there was a significant difference in accuracy between ABA and CBA sequence, *t* (39) = -3.84, *p* < .001, 95%CI [-0.030, -0.009]. The interaction between Task Sequence and WML was not significant, *F* (1, 19) = 1.50 , *p* < .24, η_g^2 = .006; under AS condition accuracy for ABA was 92.90%, and for CBA it was 94.7%, whereas under FT condition, accuracy on ABA was 95.20% and on CBA it was 95.40%. The interaction between Response and WML was not significant, *F* (1,19) = 1.30, *p* > .27, η_g^2 = .002; that is, for AS Response Repetitions was 93.80%, and Response Switches, 93.70%, whereas, for FT Response Repetitions was 95.80%, and Response Switches was 94.80%.

The three way interaction between Task Sequence, Response, and WML was also not significant, *F* (1,19) = 1.90, *p* > .18, η_g^2 = .004 (see Figure 4.14). For the density functions of accuracy n–2 task repetition cost under n–2 RR and n–2 RS, for FT and AS conditions, see Figure 4.15.

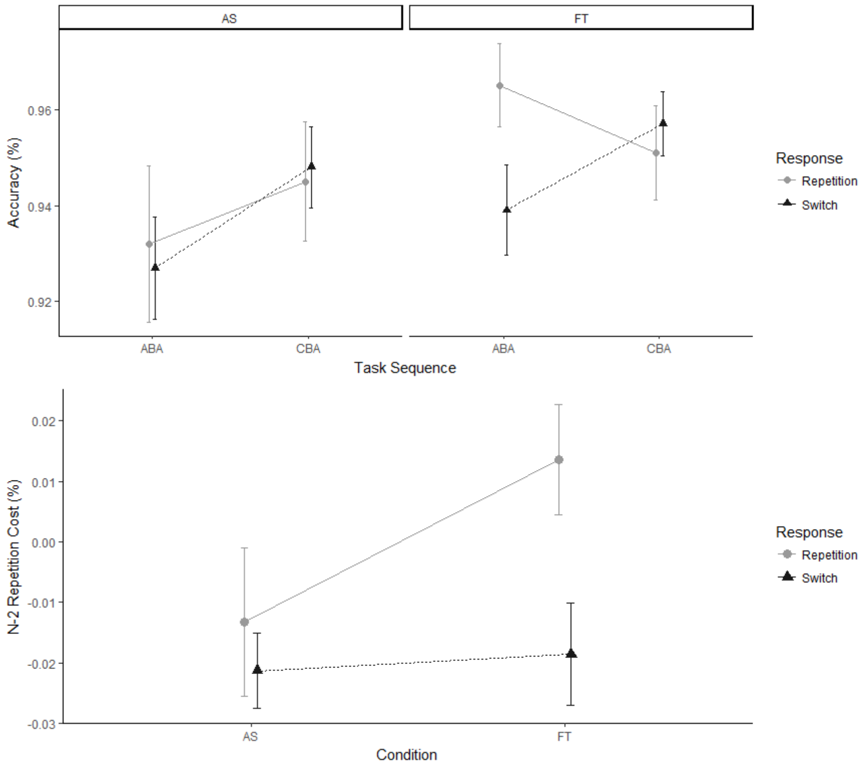


Figure 4.14 At the top, an illustration of a non-significant three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and WML (AS vs. FT) for accuracy. At the bottom, n–2 task repetition costs varying across Response and WML. Error bars de-note ±SE around the mean

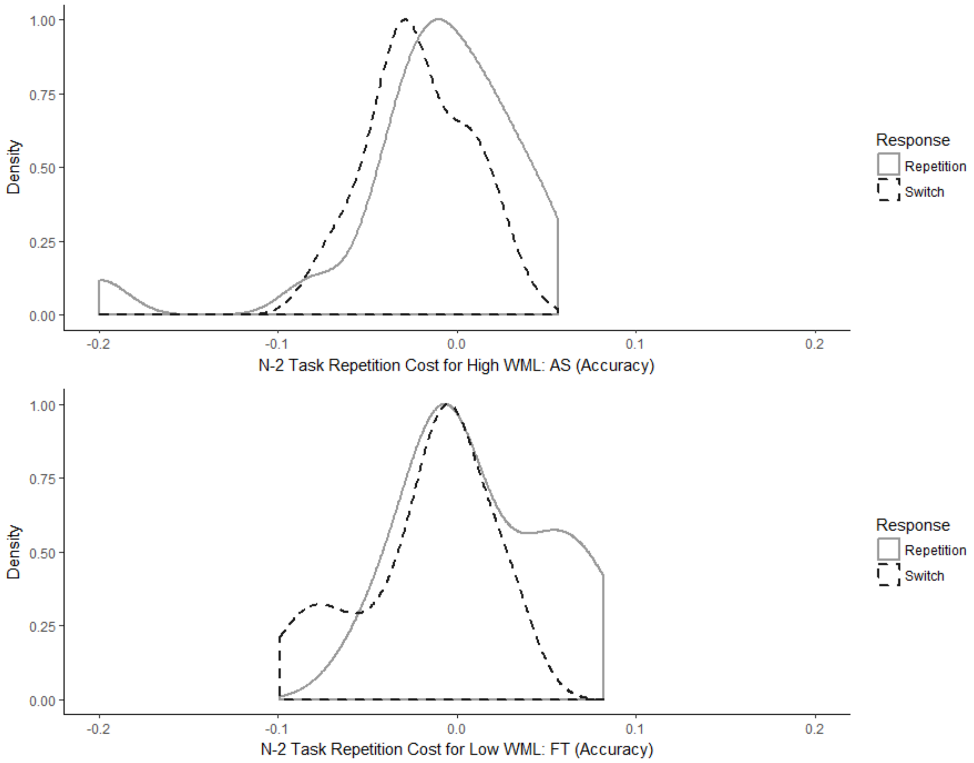


Figure 4.15 Density functions for accuracy n–2 task repetition costs for Articulatory Suppression (AS) and Foot Tapping (FT) under n–2 Response Repetitions and n–2 Response Switches.

Bayes factors. Table 4.5 contains results for BFs of the four probable models explaining n–2 task repetition costs (see Stopping Rule section). As in Experiment 6, for completeness, BFs were calculated for RTs as well as accuracy. For RTs, all four models had a BF smaller than 1; at the point of data collection being stopped the BF_{10} for a comparison of the full model (i.e., main effects of WML manipulation/ Response and an interaction between them) to the best model—which was a main effect of Response—was below 1/6, specifically 0.1593 (0.0907/ 0.5692), which means that the n–2 task repetition cost was over six times more likely (1/0.1593) to be explained by the full model than the main effect of Response. The BF_{10} for comparisons of the full model and the two other models (i.e., the main effect of WML; the main effects of WML/ Response) ranged from 0.2603 to 0.438; this is considered as anecdotal evidence for the full model being more likely than the two alternative models. Overall, since the evidence is anecdotal, the results have to be taken with caution. For accuracy, the Bayesian analysis showed that the model with the largest BF was the one with the main effect of Response (BF 1.915), followed by the model of two main effects (Response and Experimental Manipulation; BF 1.423). Comparing the full model with the model of the main effect of Response to the full model, the formal one was about twice more likely than the full model. Overall, the results Bayesian results for accuracy performance were rather inconclusive as the BF_{10} ranged from 0.6371–0.4734, which is considered as anecdotal evidence.

Table 4.5 Bayes factors for selected models predicting the n–2 task repetition cost as the DV, and Response and WML as IVs. As in Experiment 7, for completeness, Bayes factors were calculated for accuracy as well as for RTs. The reported models were compared to a denominator which was that the n–2 task repetition cost was predicted by a random factor of Participant. "+" indicates an addition of an effect, and "*" indicates an interaction.

N–2 task repetition cost	Model	BF
RTs	Response (1)	0.5692
	WML (2)	0.3485
	R +WML (3)	0.2071
	R + WML + R * WML (4)	0.0907
Accuracy	Response (1)	0.7077
	WML (2)	1.915
	R + WML (3)	1.423
	R + WML + R * WML (4)	0.9066

Discussion

Experiment 7 aimed to re-examine the relationship between inhibition (as reflected by residual n–2 task repetition costs) and WML. In Experiment 7 participants performed a BI paradigm whilst doing a secondary task; in the AS condition participants verbalised “blah blah blah” and in FT condition participants tapped their foot. The AS was expected to interfere with WM because it disrupts inner speech, whereas FT was used as a control condition WM (Baddeley, 1986, 1990) because it does not interfere with inner speech. Assuming that the residual n–2 task repetition cost reflects inhibition and that inhibition relies on WM resources, the residual n–2 task repetition cost was expected to be smaller under AS compared to FT condition.

As expected, the standard n–2 task repetition cost was present for RTs and accuracy, whereas the residual n–2 task repetition cost was only present in accuracy; however, there was a numerical trend for the residual n–2 task repetition cost in RTs too. Contrary to the proposed hypothesis, the experimental manipulation of WML did not seem to influence the residual n–2 task repetition costs (which was present only in accuracy). In terms of manipulating WML in Experiment 7, the WML manipulation used seemed to have worked as performance was statistically different between AS and FT; however unexpectedly, performance was slower in the FT condition compared to AS. It is uncertain why that would be the case, although a speculation can be made. It is possible that slower and less accurate performance under FT compared to AS, might have been due to conflict in motor responses as both the primary task (i.e., task-switching) and the secondary task (i.e., foot tapping) are motor tasks (e.g., Miyake et al., 2004)—especially as participants gave responses with their right hand and tapped their right foot. Moreover, it can be speculated that we are very well practiced at verbalising, especially nonsensical words such as “blah

blah blah", whereas tapping of the foot whilst performing cognitive tasks, even though it is considered as an easy task, is not an over practiced task and could require some level of cognitive control and coordination to maintain the tapping of the foot in a required rhythm as a secondary task.

Taken together, the results from Experiment 7 indicate that inhibition (as reflected by accuracy residual n–2 task repetition costs) does not seem to be modulated by WML as manipulated through interfering with WM's phonological loop. Overall, these results are consistent with Experiment 5 and 6 in that inhibition does not seem to depended on WM.

General Discussion

In the current study, three experiments investigated whether and to what extent cognitive inhibition (as measured via the residual n–2 task repetition cost) is related to WMC and WML. Experiment 5 examined individual differences in WMC and inhibition; whereas Experiments 6–7 investigated if manipulating WML influences inhibition. These three experiments were conducted under the hypothesis that the residual n–2 task repetition cost reflects cognitive inhibition which relies on WM resources (Conway & Engle, 1994; Conway et al., 1999). The results from all three experiments suggest that inhibition in task-switching is not associated with or modulated by WM; the results are taken as evidence that inhibition in task-switching (as measured with the residual n–2 task repetition cost) is not dependent on WM resources.

Individual Differences

Studies that looked at the relationship between cognitive inhibition (as measured with n–2 task repetition cost) and WM (Grange & Houghton, n.d.;

Pettigrew & Martin, 2015) showed no relationship between the two; however, they used a measure of inhibition confounded by episodic retrieval mismatches, that is, the n–2 task repetition cost not controlled for episodic retrieval (Grange et al., 2017). This meant that the results from those studies—in light with new evidence (Grange et al.)—were difficult to interpret because it was not certain if it was inhibition or episodic retrieval that was not linked to WM. The current study aimed to address that limitation.

Assuming that the n–2 task repetition cost controlled for episodic retrieval (i.e., residual n–2 task repetition cost) is a better measure of inhibition, that inhibition depends on WM resources (e.g., Baddeley, 1990; Engle, 1994), and that WM individual differences are linked to differences in inhibition (e.g., Conway et al., 1999), the residual n–2 task repetition cost was expected to be explained by and/or modulated by WM. It was found that results support evidence from Grange and Houghton, as well as Pettigrew and colleagues, which is that inhibition (as measured with the BI paradigm) does not seem to be related to WM resources. Specifically, n–2 task repetition costs (standard and controlled for episodic retrieval) were not modulated by WML or linked to WMC; as such, indicating that WM individual differences cannot explain differences in n–2 task repetition costs.

Considering individual differences in the n–2 task repetition cost a bit more, it was noted in Experiment 5 that participants varied a lot in how performance between ABA and CBA sequences differed; that is, n–2 task repetition costs and benefits, as well as no n–2 task repetition costs were observed. This is consistent with evidence from other studies (e.g., Grange & Juvina, 2015; Chapter 2–4). Overall, in Experiment 5 the n–2 task repetition cost was present, and it was modulated by episodic retrieval. However, no n–2 task repetition cost was present under episodic matches (n–2 RR)—there was actually a non-significant n–2 task repetition benefit present—compared to episodic mismatches (n–2 RS) where a statistically significant cost was observed. Therefore, the lack of the overall residual n–2 task repetition cost—which could be argued to be a sign of no inhibition—might have led to no relationship between the cost and WMC; however, this is unlikely. Contrary to studies that link smaller n–2 task repetition costs to weaker/ impaired cognitive inhibition (e.g., Fales et al., 2006; Moritz et al., 2004; Whitmer & Banich, 2007; Whitmer & Gotlib, 2012), it is concluded that since participants in the current study (which represented a healthy university population) performed task-switching efficiently and at high accuracy regardless of whether they showed n–2 task repetition costs or benefits, they had no impairment of cognitive control. Instead, it is speculated, that a behavioral lack of n–2 task repetition costs does not mean that inhibition was not deployed (Grange & Juvina, 2015), and that it is more likely that n–2 task repetition cost reflects inhibitory and non-inhibitory effects which can manifests as overall costs or benefits. For now it cannot be said confidently if and to what extend inhibition plays a role in task-switching performance characterised with n–2 task repetition benefits, which needs to be investigated further.

Moreover, the n–2 task repetition cost has been recently shown to not be a reliable measure of inhibitory control (Kowalczyk & Grange, 2017; Chapter 3, Experiment 2), which means that the lack of the association between n–2 task repetition costs (standard and controlled for episodic retrieval) can be due to low reliability of the cost. Also, the n–2 task repetition cost’s low reliability could be due to the cost being a difference score which have been shown to be not reliable and difficult to interpret (Crawford et al., 2008); this means that inhibition in task-switching may be important but the measure of it (i.e., the n–2 task repetition cost) is not suitable. Therefore, for now we cannot confidently say that inhibition in task-switching does not depend on WM, because of the limitations in how inhibition is assessed in the BI paradigm.

Theoretical Implications

Inhibition. Cognitive inhibition has been suggested to be a WM resources dependent process (Conway et al., 1999; Engle, 1994); based on this it was expected in the current study that people with larger WMC would have larger inhibition compared to people with smaller WMC. This is because in people with larger WMC there should be more resources to deploy inhibition, whereas in people with small WMC, resources are thought to be used up on a primary task, leaving no resources for inhibition to be deployed. However, since results of the current study showed no such relationship, it can be concluded that inhibition (as measured with the residual n–2 task repetition cost) does not depend on WM; that is, that inhibition does not seem to be resource dependent.

The current study gives further evidence to Pettigrew and colleagues' (2015) findings, which is that there is no relationship between inhibition (as measured with BI paradigm) and WM. This is based on theory that the n–2 task repetition cost does not reflect a top-down control type of process but rather an automatic inhibition (Gade & Koch, 2005, 2007; Houghton et al., 2009; Schneider & Verbruggen, 2008; Schuch & Koch, 2003) or self-inhibition (Grange et al., 2013; Koch et al., 2010).

Episodic retrieval. The n–2 task repetition cost was not modulated by episodic retrieval in Experiments 6–7, which was unexpected. This perhaps indicates that WML manipulation interfered with episodic retrieval facilitating task-switching. Looking only at numerical trends, the following can be speculated. If the n–2 task repetition cost controlled for episodic retrieval reflects inhibition, we would expect the cost under episodic matches to be more affected by WML than under episodic mismatches, a pattern which is observed in Experiments 6–7 (although not statistically significant). That is, it appears that under episodic mismatches the n–2 task repetition cost is comparable for high and low WML for RT and accuracy; but more importantly, there was a numerical trend for the smaller n–2 task repetition costs to be observed under high WML under episodic matches. This is as predicted by theory that inhibition is resource depended. On the other hand, if WM was important for episodic retrieval (Houghton et al., 2009) more than inhibition—because retrieved task-set information can be assumed to be manipulated in WM—we would expect the n–2 task repetition costs not controlled for episodic retrieval to be more dependent on WM than the cost controlled for episodic retrieval. There is some evidence from the current thesis to support that hypothesis. In Experiment 5 (Chapter 4) accuracy of the n–2 task repetition cost not controlled for episodic retrieval was significantly correlated with WMC, with higher WMC scored being linked to smaller accuracy n–2 task repetition cost. This can be interpreted as evidence that individuals with higher WMC were more accurate retrieving and manipulating task's parameters and matching them to the current task's demands compared to those who had smaller WMC. However, this correlation did not remain significant after adjusting for multiple testing, therefore these results have to be interpreted with caution. Nevertheless, n–2 task repetition costs not controlled for episodic retrieval were in general similar under both high and low WML (for RT and accuracy performance), which suggests they were not affected by manipulation of WML or that WML manipulation affected episodic retrieval; therefore, overall, the results seem inconsistent.

Working memory. Research from task-switching investigating the SC (Butler et al., 2011; Draheim et al., 2016; Erickson, 2008; Hester & Garavan, 2005; Meiran & Kessler, 2008; van Hooren et al., 2005) and n–2 task repetition costs (Grange & Houghton, 2009, 2010b; Grange & Juvina, 2015; Grange et al., 2013; Houghton et al., 2009), implies the importance of WM in task-switching. For example, it is believed that cues are translated into tasks in WM; that is, cues are interpreted and then relevant tasks are retrieved from LTM and activated (e.g., Houghton, Pritchard, & Grange, 2009). If cues are difficult to interpret, this leads to the standard n–2 task repetition increase. It is speculated that once a given task is completed and a new cue needs to be translated in WM, the abandoned task's representation may lead to a conflict in WM, therefore cognitive control mechanism inhibits the no-longer-relevant task representation. But if cues are informative they do not need to be translated in WM, therefore their activations will not 'linger' in WM, hence less if any inhibition is needed.

Since there is evidence that inhibition relies on WM and that it can vary between people depending on their WMC (Conway et al., 1999; Engle, 1994), it seemed plausible that residual n–2 task repetition costs may also be dependent on WM. Specifically, the BI paradigm used in the current study used what can be considered as uninformative cues, as such they need translation in WM, therefore assuming that inhibition needs WM resources, exhausting WM should decrease inhibition (as indicated by reduced residual n–2 task repetition cost); moreover, under the same prediction, people with lower WMC should also show smaller inhibition. The current study's findings do not support this speculation, as consistently with some previous work (Grange & Houghton, n.d.; Pettigrew & Martin, 2015) inhibition in task-switching was not affected by WML or related to WMC. Therefore, in terms of the role of WM in cue-translation in task-switching, it can be said that if cues are translated in WM, the efficiency with which this translation is performed does not seem to differ between High and Low WM and WMC. Although, to be certain, further studies investigating inhibition (as measured with residual n–2 task repetition cost) for informative and non-informative cues under different WM loads would have to be conducted.

WM is speculated to be important in task-switching for another reason. Research shows that phonological loop (Injoque-Ricle, Barreyro, Formoso, & Jaichenco, 2015; Kirkham, Breeze, & Mari-Beffa, 2012), which is a WM mechanism aiding retrieving and maintenance of decaying tasks' representations in WM through inner verbal rehearsal, plays a role in task-switching. Increasing WML through AS increases the SC (Emerson & Miyake, 2003; Goschke, 2000; Miyake, Emerson, Padilla, & Ahn, 2004; Saeki & Saito, 2004a, 2004b; Saeki, Saito, & Kawaguchi, 2006); therefore, it is believed that under AS conditions, phonological loop no longer aids these mechanisms, potentially leading to conflicts in WM. In the current study AS was used as a manipulation of WML but it is uncertain whether there is a specific relationship between the phonological loop and inhibition in task-switching. Assuming that phonological loop is necessary for temporary maintenance of information (Fürst & Hitch, 2000), which in task-switching would be task's representations, it would be expected that the level to which tasks' are activated in WM would influence the extent of possible conflicts in WM and how they are resolved. For example, phonological loop may be important in overcoming inhibition; that is, through phonological loop, relevant task sets' activations can be 'boosted' reducing n–2 task repetition costs. However, evidence from an unpublished study would suggest that the n–2 task repetition costs is not affected by task-relevant or task-irrelevant verbalisations used to aid and interfere with task preparation respectively (Grange, n.d.).

Taken together, the results from the current study support another theory on inhibition and WM; namely, that inhibition in task-switching is reactive and automatic in nature (Anderson & Levy, 2007; Arbuthnott, 2008b; Sinai et al., 2007, Gade & Koch, 2005, 2007; Houghton et al., 2009; Schneider & Verbruggen, 2008; Schuch & Koch, 2003), or that it is a self-inhibition mechanism (Grange et al., 2013; Koch et al., 2010); therefore, should not be tied to WM resources. This is in line with findings from two studies that looked at WM and inhibition in task-switching (Grange & Houghton, n.d.; Pettigrew & Martin, 2015).

Limitations

It is not certain that WML manipulations (i.e., task-irrelevant verbalisations) used in the current study were as successful as they could have been. It became apparent that manipulating WML within BI paradigm was not easy; the WML manipulations in Experiment 6 seemed to have interfered with task-switching and/or episodic retrieval considerably beyond anticipated effects. That is, almost a third of participants did not complete testing due to not being able to complete the experiment in given time, and even though accuracy from task-switching among remaining participants was high, their RT performance was almost twice as long in High WML compared to Low WML. This was somewhat rectified in Experiment 7; however, that study was stopped due to administrative reasons (i.e., ethical approval expiring) before more data could be collected. Therefore, the results from Experiments 6–7, despite being consistent with some evidence reported in literature, can be considered inconclusive. This is especially because WML manipulations introduced lead to weak n–2 task repetition cost which was not modulated by episodic retrieval.

Another limitation, currently somewhat beyond researcher’s control, is that the standard and residual n–2 task repetition cost have been shown not to be reliable at an individual level (Kowalczyk & Grange, 2017; Chapter 2–3, Experiments 1–2). Moreover, n–2 task repetition cost is a difference score which are in general difficult to interpret (Crawford et al., 2008). The immediate implication of this is that the lack of an observed relationship between the n–2 task repetition costs and WM could be simply due to the lack of the reliability of the cost itself.

Conclusion

The current study revisited the relationship between WM and inhibition in task-witching by: 1) using an improved measure of inhibition (the residual n–2 task repetition cost); 2) assessing WMC with three measures shown to reliably assess this ability, and 3) manipulating WML in BI paradigm. It was found that, contrary to the theory that inhibition relies on WM resources, the residual n–2 task repetition cost did not relate to WM; giving further evidence that inhibition (as measured via the residual n–2 task repetition cost) is not dependent on WM. The results can also be interpreted as evidence that, the n–2 task repetition cost does not reflect a top-down control, which typically is associated with WM, and that instead the n–2 task repetition cost reflects an automatic, low-level processing.

Chapter 5 Perceptual Load and the n–2 task Repetition Cost

Abstract

Cognitive inhibition is believed to play an important role in goal-driven and context-specific behavior. In task-switching cognitive inhibition is measured with the n–2 task repetition cost (an effect of slower and less accurate performance on ABA compared to CBA tasks sequences). The n–2 task repetition cost has been replicated well and has become a signature of cognitive inhibition; however, its source is still debatable, and the origin of individual differences seen in the cost is not known. The n–2 task repetition cost is not linked to working memory, therefore the current study aimed to examine whether this cost is modulated by another resource-limited construct, perceptual attention. Perceptual load was varied within the BI paradigm to manipulate attentional resources; high perceptual load was used to exhaust attentional resources compared to low perceptual load. Experiments' 8–9 results are somewhat inconsistent; evidence favors the n–2 task repetition cost not being modulated by perceptual load, but there is also some support for the cost being modulated by it. Also, individual differences in the n–2 task repetition cost are not related to cognitive failures scores (measured with Cognitive Failures Questionnaire) linked to individual differences in perceptual load and inhibition.

Introduction

Going through day-to-day routines, in any given moment more than one action can be performed; for example, walking into a living room, we can turn on the TV, reach for a newspaper, or pick up a phone to check emails. In general, the action we decide to select matches the current goal we have in mind; so, if the goal is to watch the TV, we can ignore other stimuli in our environment that also afford action. Moreover, if someone knocked on our door whilst we were watching the TV, we could disengage from TV watching and open the door. That is, we can shift our attention from one task to another to match a new goal. However, certain environmental factors and individual differences can make selecting appropriate action difficult. For example, people that are naturally easily distractible, have a medical or a developmental condition, may struggle to select, focus on, or switch between tasks (Broadbent et al., 1982; D'Alberto, Funnell, Potter, & Garavan, 2017; Forster & Lavie, 2009b; Iaccarino et al., 2014; Lhermitte, 1983; Lhermitte, Pillon, & Serdaru, 1986; Shahamat, Fadardi, Amir, Yazdi, & Talaei, 2016). Moreover, research into human perception suggests that under certain circumstances, ignored information is processed and can impair how efficiently a given action is performed (Lavie, 1995; van Gaal, Ridderinkhof, van den Wildenberg, & Lamme, 2009).

When carrying out context-specific actions becomes problematic, it highlights that seemingly easy and automatic behavior should not be taken for granted. Our ability to exert controlled-behavior and shift between goals are likely to be products of the cognitive systems' numerous processes interacting together; for example, when presented with some stimuli (e.g., whilst driving a car coming across a STOP road sign) we need to be able to perceive and encode that stimuli, as well as retrieve and select relevant knowledge on what to do with it to manipulate that knowledge (e.g., what the sign represents and when it applies), and use it to produce appropriate behavior (e.g., to stop the vehicle at STOP sign). Moreover, to adapt to our environment and its changing demands, we must be able to maintain desired action, as well as to adjust it if needed. This concept is known as the stability-flexibility dilemma (Goschke, 2000); it is a dilemma, because it is not certain how exactly the cognitive system manages stable behavior allowing for it to be flexible at the same time. Goschke proposes that this dilemma may be understood through looking at goal-oriented and context-specific behaviours as a product of changing moment-to-moment activations and deactivations of mental representations of different goals. Mental representations (or internal representations) of goals is a concept that is rather abstract, as researchers tend not to define it extensively in their work. Instead, it is assumed that mental representations of goals can be thought of as, for example, "memory stores, procedural knowledge, and response schemas" (Gopher, Armony, & Greenshpan, 2000, p.338), or "transient associations between mental representations of stimuli and responses in accordance to a specific goal" (Sdoia & Ferlazzo, 2008, p.322); in task-switching research this is known as a task-set (Rogers & Monsell, 1995). Assuming such a definition of mental representations of goals, cognitive psychology research nominates cognitive inhibition as a process facilitating decision making needed for goal-oriented and context-specific behavior, which is thought to be achieved through inhibition of irrelevant representations (Diamond, 2013; Friedman & Miyake, 2017; Gorfein & MacLeod, 2007; Johnson, 2007; Koch et al., 2010; Miyake et al., 2000; Steinbeis & Crone, 2016).

Task-switching research offers promising insight into whether, when, and how cognitive inhibition shapes behaviour. In experiments which require participants to switch back and forth between three tasks in a serial run (i.e. Backward Inhibition (BI) paradigm), an effect known as the n–2 task repetition cost is observed (Mayr & Keele, 2000). This effect is characterised by an overall slower and less accurate performance on tasks that were performed recently compared to tasks that were not. Any current task n is classified as an n–2 task repetition if it is the same task that was performed on the n–2 trial; and, it is classified as an n–2 task switch if the current task is not the same task as the one on n–2 trial. The former one is arbitrarily referred to as an ABA sequence and the latter one is referred to as a CBA sequence; the n–2 task repetition cost is the difference in performance between ABA and CBA sequences. The BI paradigm tests for presence of cognitive inhibition in task-switching based on the following hypothesis: cognitive inhibition facilitates the switch from one task to another by suppressing the mental representation of the no-longer relevant, abandoned task (Mayr & Keele, 2000). Inhibition is believed to be important for efficient task-switching because the abandoned task's mental representations continue to be active so they interfere with accessing a new task. However, applied inhibition makes the affected task less accessible momentarily; therefore, if that task is needed soon after being inhibited, it should be performed less efficiently, as reflected by slower and less accurate performance. This is exactly what Mayr and Keele found, providing support for the importance of cognitive inhibition in task-switching (for a review see Koch, Gade, Schuch, & Philipp, 2010).

For a long time, the n–2 task repetition cost was resistant to non-inhibitory explanations, which led to this effect becoming a signature of cognitive inhibition (Koch et al., 2010). As such, researchers have used the n–2 task repetition cost to assess cognitive inhibition in clinical populations (Fales et al., 2006; Foti et al., 2015; Mayr et al., 2006; Meiran, Diamond, Toder, & Nemets, 2011; Whitmer & Gotlib, 2012; Yiu-kwan, 2008), as well as to assess individual differences in inhibitory control (Whitmer & Banich, 2007). However, new evidence emerged showing that the n–2 task repetition cost is not reliable (Kowalczyk & Grange, 2016; also, see Chapters 2–3), and that it is considerably modulated by episodic retrieval

(Grange, Kowalczyk, & Loughlin, 2017; also, see Chapter 3). As a consequence, there is less confidence in the n–2 task repetition cost as a measure of cognitive inhibition. Moreover, findings from Chapter 4 (see also, Grange & Houghton, n.d.; Pettigrew & Martin, 2015) of this thesis indicate that the n–2 task repetition cost (standard and the residual) is not associated with Working Memory Capacity (WMC) or modulated by Working Memory Load (WML). This is contrary to the theory that cognitive inhibition is a resource dependent process (Conway & Engle, 1994; Engle, 1994; Heitz et al., 2005). This is important, because noticeable individual differences have been noted in the n–2 task repetition cost (Grange & Juvina, 2015), and WM was a promising candidate to explain those differences (Houghton et al., 2009). As such, currently, there seems to be no other evidence on the source of individual differences in the n–2 task repetition cost.

Perceptual Load

The cause of individual differences in the n–2 task repetition cost can be speculated to arise from another resource limited system, perceptual attention (also referred to as selective/ visual attention) (Broadbent, 1958; Lavie, 1995). The next few paragraphs will introduce the literature on how relevant information is selected against irrelevant information, what this process depends on, and how it links to inhibition and the n–2 task repetition cost.

Research from information selection in visual attention shows that the extent to which interference (e.g., distractors) may affect performance on cognitive tasks depends on perceptual load (Kahneman & Chajczyk, 1981; Lavie & Fox, 2000). Perceptual load characterises the level of perceptual demands a task puts on attention; for example, competing responses, the number of stimuli and distractors presented, as well as their location and distinctiveness (Forster & Lavie, 2008a, 2008b; Lavie, 1995; Lavie & Tsal, 1994). Perceptually low load is when a small display-set is presented (e.g., a single target, or a target and a distractor), compared to perceptually high load when a large display-set is presented (e.g., a target being accompanied with numerous distractors). Original research looking at how relevant information is selected against irrelevant stimuli in our environment proposed two competing theories: early-selection (Broadbent, 1958) and late-selection (Deutsch & Deutsch, 1963). Early-selection theory argues that relevant information is selected early on during processing of stimuli and irrelevant information is ignored, which is due to attentional resources being limited, hence resources are directed towards task-relevant information, leaving very little if any resources for processing of irrelevant information (Broadbent, 1958); the late-selection theory proposes that attention is automatic which means that task-relevant as well as task-irrelevant information are processed initially, and then task-relevant information is selected at later stages of processing (Deutsch & Deutsch, 1963). Since those two theories were developed, it has been shown that either of strategies for information selection can be employed given the right conditions (Lavie, 1995; Lavie & Tsal, 1994). By combining assumptions from the early and late information-selection theories—the limited and automatic nature of visual attention—it has been proposed and consequently demonstrated that ”perceptual processing is a limited resource, but it proceeds automatically until it runs out of capacity” (Lavie & Tsal, 1994, p. 185). In other words, if task does not exhaust attentional resources, all stimuli (task-relevant as well as task-irrelevant) may be processed, and if the task’s demands consumes attentional resources, only task-relevant information is likely to be processed. As such, low perceptual load (LPL) has been shown to facilitate late-selection, and high perceptual load (HPL) early-selection strategy (Lavie, 1995).

Evidence from perceptual load research has potential implications on how behavioural tasks' results are interpreted; specifically, task-switching effects (e.g., n–2 task repetition costs) may be confounded by processing of task-irrelevant information because task-switching paradigms are typically simple in presentation, under which conditions task-irrelevant information may be processed and affect cognitive effects measured. This means that under LPL assessment of cognitive performance can be confounded by interference from distractors even if participants do not consciously attend to them. However, it also means that by increasing a task's attentional demands—through varying the task's difficulty, responses, or stimuli location and distinctiveness—the attentional resources can be manipulated to allow for less or more interference effects from external distractors (Cave & Chen, 2016; Forster & Lavie, 2009b; Rorden, Guerrini, Swainson, Lazzeri, & Baylis, 2008; Wilson, Muroi, & MacLeod, 2011). Furthermore, it has also been shown that task-irrelevant information interference can also be generated internally, as in one study people reported more task-unrelated-thoughts (TUTs) under LPL compared to HPL (TUTs, Forster & Lavie, 2009). This is significant because more TUTs being reported under LPL imply that attentional resources under that condition can be allocated to task-irrelevant information in the form of intrusive thoughts, which again, may confound measures of cognitive performance. Taken together, research from perceptual attention shows that regardless of the source of distractors (internally or externally generated), they seem to be processed through the same attentional resources, and both can be manipulated with perceptual load.

The Current Study

So, how does perceptual attention and perceptual load research relate to task-switching and the n–2 task repetition cost? Currently in the literature there are no studies that investigated the influence of perceptual load on the n–2 task repetition cost; however, some speculations on how they may interact can be made.

To begin, other effects that are speculated to reflect cognitive control—such as negative priming, the Stroop effect, and response competitor—have been shown to be reduced under HPL (Forster & Lavie, 2008a; Kahneman & Chajczyk, 1981; Lavie & Fox, 2000; Rorden et al., 2008). Therefore, if the n–2 task repetition cost reflects cognitive inhibition, it can be speculated that it should be sensitive to perceptual load manipulations. Specifically, the n–2 task repetition cost would be expected to be reduced under HPL, compared to LPL, because under HPL there should be less spare attentional resources for inhibition to be activated, compared to LPL.

Distractors. By design task-switching paradigms are perceptually simple; that is, they typically involve processing a single stimulus (Altmann, 2007; Arbuthnott, 2009; Mayr, 2007) falling into the category of LPL, although some experiments introduce distractors (Houghton et al., 2009; M. Hübner et al., 2003; Mayr & Keele, 2000). This means that we cannot be certain if and to what extent distractors interfere with task-switching, potentially influencing measured cognitive processes. This means that n–2 task repetition costs in different experiments may reflect slightly different cognitive processing, even though in general it is thought to represent cognitive inhibition.

One could argue that BI paradigms, in general, are not designed to introduce distractors, therefore perceptual load should not be an issue in task-switching. Moreover, inhibition is thought to be necessary for successful task-switching to reduce the interference of alternative tasks, which are not thought of as distractors. However, even if alternative tasks cannot be classed as distractors, in BI paradigms that can be described as perceptually low, TUTs are likely (and were often anecdotally reported during the course of collecting data for this thesis). Thus, the presence of TUTs would be expected to lead to stronger inhibition during task-switching; alternatively, TUTs may exhaust attentional capacity leaving very little if any resources left for inhibition. Furthermore, in BI paradigms that introduce distractors (unintentionally or by design), the number of these irrelevant stimuli on any given trial is rather small (e.g., 3 irrelavnt items, Houghton, Pritchard, & Grange, 2009; Mayr & Keele, 2000), which has been shown to be insufficient to act as HPL (Lavie & Fox, 2000). Therefore, it is uncertain as yet, whether and to what extent inhibition—as measured with the n–2 task repetition cost—is confounded by distractors introduced to BI by design and/ or by TUTs; nevertheless, the n–2 task repetition cost would be expected to be smaller under HPL which makes it less likely for intrusive, task-irrelevant thoughts to occur during task-switching.

Episodic retrieval. The n–2 task repetition cost has been shown to be modulated by episodic retrieval (Grange et al., 2017), which was taken as evidence that the original n–2 task repetition costs were confounded by memory conflicts. That is, in standard BI paradigms, a task is classified as an n–2 task repetition or an n–2 task switch depending on whether the same cue and instructions are given or not across the tasks sequences. But, across the ABA/ CBA sequences n–2 response repetitions/ switches are not controlled for; so in ABA sequence, for the n parity task a digit “8” may be presented requiring M-key press, and on n–2 parity task repetition a digit “3” may be presented requiring the Z-key press. Recently it has been shown that when n–2 response repetitions/ switches are controlled for during n–2 task repetitions/ switches, the n–2 task repetition cost is considerably reduced under n–2 response repetitions compared to n–2 response switches (Grange et al.); and the mechanism by which this takes place is explained in the next paragraph.

Research shows that when we encounter an event a memory trace (i.e., event-file) of that event is formed (Hommel, 1998; Neill, 1997; Waszak et al., 2003); in a task-switching context a memory trace is thought to hold information on which cue was presented and what followed after the cue (e.g., stimulus' appearance and response). This memory trace can influence consequent performance of a task; that is, if previously performed task is required again, upon seeing a cue, the relevant memory trace is retrieved, and if the retrieved memory trace matches the demands of the current task (i.e., episodic match), performance is enhanced, and if the memory trace does not match the task's demands (i.e., episodic mismatch), performance is slower and less accurate (e.g., when a Z-key press response is retrieved but the M-key press is required). The reduction in the n–2 task repetition cost under episodic matches compared to episodic mismatches is taken as evidence that the n–2 task repetition cost is in large part due to non-inhibitory mechanism, namely episodic retrieval (Grange et al., 2017). However, typically, the n–2 task repetition cost is not removed with episodic matches (although see Chapter 4, Experiment 5); the remaining cost is referred to as the residual n–2 task repetition cost, though to be a better reflection of inhibition.

Currently, there seems to be no reported studies that looked at episodic retrieval and perceptual load, which makes it difficult to predict whether and how standard/ residual n–2 task repetition costs and perceptual load will interact. Because perceptual load seems to affect if and how task-relevant and task-irrelevant stimuli are perceived and processed, it can be speculated that under different perceptual loads what becomes part of memory traces (as well as what is retrieved) can differ between loads (Lavie, 2000; p.1050). For example, in task-switching under reduced perceptual processing (i.e., HPL) task-irrelevant information should be less efficiently encoded due to limited attentional resources; but in task-switching without obvious distractors (i.e., LPL), what can be assumed to be encoded is task-relevant as well as task-irrelevant information.

The following argument presents why predictions on episodic retrieval in task-switching and perceptual load are problematic. Task-switching and BI paradigms are typically simple in presentation falling under the category of LPL, therefore memory-traces from trial-to-trial can be speculated to be potentially confounded by task-irrelevant information affecting measured effects such as the n–2 task repetition cost; however, there is some evidence—from LPL-tasks—to suggest that stimuli-features do not seem to bind to memory-trace of a task if these features are not task-relevant (Chapter 3, Experiment 4; Grange et al., 2017, Experiment 3; Hommel, 1998). There is also evidence that controlling within task-switching for cue, stimulus, and response repetitions (Schmidt & Liefooghe, 2016)—using tasks that fall under the LPL—reduces an effect known as the switch cost (SC, an effect of slower performance switching compared to repeating tasks); the cues, stimuli, and responses were task-relevant in that study, therefore, assuming that tasks used in that study are indeed LPL-tasks, it is not certain whether and to what extent the SC was also affected by incidental processing of task-irrelevant information. This is because, under perceptual load theory, given the correct conditions, binding of co-existing features is automatic in nature—and as such is important for the process of learning and healthy functioning (Hommel, 2004)—therefore, it is not clear whether and how episodic retrieval or inhibition interacted with perceptual load in some of the studies (Chapter 3, Experiment 4; Grange et al., 2017).

Perhaps, the problem of applying perceptual load theory to episodic retrieval in task-switching is how interference is triggered and defined. For example, in task-switching the interference is thought to come from competing task-sets, specifically, ongoing activation of temporarily not relevant task-sets, and episodic mismatches; whereas, in perceptual load research interference comes from external or internal distractors that tend to be task-irrelevant. As mentioned earlier, it is uncertain whether in BI paradigms, whilst one of the three tasks is performed, the alternative tasks can be classed as distractors. However, despite this uncertainty, it still seems very plausible that under HPL, performance should be worse than under LPL; also inhibition (as measured with the residual n–2 task repetition cost) should be smaller than under LPL.

Individual differences. The magnitude of the n–2 task repetition cost varies between participants and the source of those is not known. Interestingly, there is evidence from perceptual load research (Forster & Lavie, 2009b) that the magnitude of the interference effect—as measured with response competitor task—can vary depending on individual differences in how people deal with distractions in day-to-day life—as measured by Cognitive Failures Questionnaire, CFQ (Broadbent et al., 1982). That is, under LPL compared to HPL, the interference effect was larger for people that scored high on the CFQ vs. people that scored low on that measure; however, under HPL, individual differences measured with CFQ diminished, and the interference effect was equal for both groups. Therefore, it is speculated that individual difference seen in the residual n–2 task repetition cost—assuming that it reflects inhibition triggered through competing task-sets interference—may also be explained with differences in the CFQ.

To summarise, the current study looks at whether and to what extent the n–2 task repetition cost—specifically the residual n–2 task repetition cost—is modulated by perceptual load, and whether individual differences seen in the residual n–2 task repetition cost can be accounted for by differences in the day-to-day distractibility trait. It is speculated that if the residual n–2 task repetition cost reflects inhibition, it should be smaller or diminished under HPL compared to LPL; and that, people who score high compared to those who score low on the CFQ should show larger residual n–2 task repetition costs under LPL vs. HPL, where residual n–2 task repetition costs should be more equal in low and high scorers on CFQ.

Experiment 8

Method

Participants. Forty-two participants were recruited from the School of Psychology at Keele University in exchange for partial course credits. The inclusion criteria were to be at least 18-years-old, have normal/ corrected to normal vision, and understand written and spoken English. Two of the participants' data were removed due to incomplete data; that is, two participants completed only one part of the experiment in the time given. There were twenty-five women, and the average age of participant was 20.9 (SD 2.13).

Stopping rule. The sample size was determined with Sequential Bayes Factors (BF) (Schönbrodt et al., 2014), which allows to quantify the strength of evidence for the null as well as the alternative hypothesis. Also, unlike with the standard power analysis (i.e., null hypothesis statistical testing, NHST, which uses *p*-values), with sequential BF, the sample is decided during testing and not before. Specifically, evidence provided by data—for either of the hypotheses—is evaluated once data collection begins.

The way evidence is evaluated is based on comparing probable models that can explain the patterns seen in data. For the current experiment it was of interest to investigate if the n–2 task repetition cost (i.e., standard and residual) is modulated by perceptual load. The four models were: 1) the full model made of two main effects (Response and Perceptual Load), and an interaction between them; 2) model with the main effect of Response and Perceptual Load; 3) the model with the main effect of Perceptual Load; and 4) the model with the main effect of Response. For each of those models, the random effect of participant was added, and each model was compared to a common denominator which was that the n–2 task repetition cost is explained by random effects of participants.

The model under which the n–2 task repetition cost is explained by random effects of participants can be classed as the prior belief, which for BF calculation is expressed as prior distribution. Next, density distributions are considered for observed data under each selected model which offers new information; based on these, posterior distribution is expressed. The posterior distribution tells researchers by how much the prior belief must be updated; that is, likelihood distributions are utilised to calculate BFs, which is a ratio of density of posterior distribution and the height of the prior distribution. See Equation 5.1; where D refers to the actual data, H_0 is the prior belief (or null hypothesis), the H_1 is the alternative hypothesis, and p is the probability. The larger the BF, the larger the difference between the prior distribution and empirical data's distribution supporting that model.

Equation 5.1

$$BF_{10} = \frac{p(D|H_1)}{p(D|H_0)}$$

Initial BFs for the current experiment were calculated at twenty participant and re-calculated after every testing-session (Schönbrodt et al., 2014). At the fortieth participant, the BF_{10} reached what is considered one of the thresholds at which data collection can be stopped; that is, the BF_{10} fell below 1/6 (the other threshold was BF_{10} of 6). That is, at fortieth participant it became apparent that the model with the main effect of perceptual Load 22.7 times more likely than the full model (i.e., the main effects of Response/ Perceptual Load and an interaction between them).

Apparatus and Stimuli. The task-switching part was presented on a 17-inch monitor PC through Psychopy (Pierce, 2007) software. Participants' responses were recorded via 1-ms precise USB keyboard.

Low perceptual load BI paradigm. For the LPL condition, the original Backward Inhibition paradigm was used (Mayr & Keele, 2000). The stimuli were shown in a black square frame (7cm x 7cm) on a white background (Figure 5.1, top panel); in the center of that frame a cue in the form of a shape appeared—a triangle signified the "horizontal" task, a hexagon indicated the "vertical" task, and a square represented the "diagonal" task (3cm in height and width; black outline with no fill). The target appeared inside the frame, in one of the inner corners in the form of a grey circle (1.2cm diameter). The "Error" message shown on the screen when a mistake was made was presented in red.

High perceptual load BI paradigm. For the HPL condition, the basic set-up was the same as in LPL; however, with an additional feature. Instead of seeing one stimulus being presented on each trial, four stimuli were shown. All stimuli presented were circles, where one of the circles was noticeably smaller than other three. The smaller circle was the target participants had to make a spatial transformation of (Figure 5.1; lower example). The manipulation for HPL in Experiment 8 was chosen as suitable—even though it had only three distractors—

because distractors did not afford responses, and the target was not obviously smaller than the stimuli making locating of the target relatively difficult; the pop-out effect is associated with efficient search but inefficient distraction rejection (Lavie & Cox, 1997). As such successful perceptual load manipulations are considered those that increase task-relevant processing.

CFQ. The CFQ is a self-reported questionnaire which measures failures in memory, perception, and motor function. This questionnaire is made up of twenty-five questions; participants are asked to consider whether any of the minor mistakes contained in questions happened to them in the last six months. For example: *"Do you read something and find that you haven't been thinking about it and must read it again?"* The available answers are: "Very often" (4), "Quite often" (3), "Occasionally" (2), "Very rarely" (1), and "Never" (0). Each of the available answer options has a score assigned to it, as seen in the brackets; the total score is calculated by adding scores for individual answers. The maximum score is 100, and the minimum score is 0. In the current experiment, participants' average score on CFQ was 47.7, with the minimum score of 23, and a maximum score of 71.

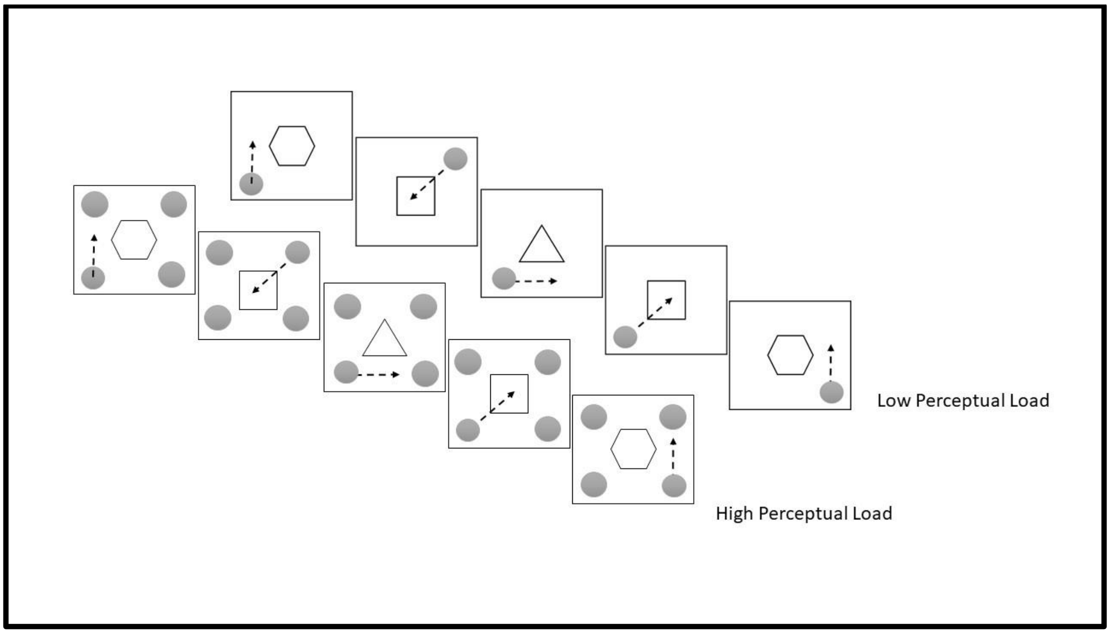


Figure 5.1 An illustration for the trial structure in High and Low Perceptual loads conditions. The arrows indicate the corners at which the correct spatial transformation was to be made; these are just for an instruction purpose, they were not included in the experimental run. Images are not to scale.

Procedure. Participants attended a single session during which they performed two task-switching tests (under LPL and HPL load) and filled in the CFQ questionnaire. The order of the task-switching tests was counterbalanced. Instructions were given to participants verbally; participants then learnt the cue-task pairing, after which they practiced task-switching for 16 trials; if they made four or more errors during the first practice, they were offered another practice with sixteen trials; however, no more than two practices were allowed.

For both task-switching conditions, each trial began with a black square frame appearing in the center of the screen inside which participants saw a cue for 150ms. The task order was random with the restriction that no immediate task-repetitions were allowed (Philipp & Koch, 2006). The cue was followed by the appearance of a single target (a grey circle) in the one of the four corners of the black frame in the LPL condition, and four circles (one in each corner) with one target and three distractors—the target was smaller than the other three circles—in the HPL. The task consisted of making a spatial transformation of the target according to the rule dictated by the cue; that is, participants had to decide in which corner the target would move to if it moved according to the direction the cue represented (Figure 5.1; arrows indicate correct spatial transformation). Participants made their responses by pressing one of the four keyboard keys on the numeric part of the keyboard which were spatially congruent with the corners of the black frame; 1 (lower-left), 2 (lower-right), 4 (upper-left), and 5 (upper-right). In both task-switching conditions the stimulus/ stimuli and the cue stayed on the screen until participants made their response. If an incorrect response was given, "Error" message appeared on the screen (1000ms).

For LPL and HPL, participants performed 480 trials in each condition. To optimise participants' performance (i.e., to avoid participants becoming fatigued) the trials were split into four blocks of 120 trials in each, with participant-paced breaks between the blocks. It was emphasised to participants that they had to respond as fast and as accurate as possible.

Design. A within-subjects design was utilised to examine RTs (ms) and accuracy (%) as dependent variables, and the Task Sequence (ABA vs. CBA), Response Repetition (n–2 Response Repetition, RR vs. n–2 Response Switch, RS), and Perceptual Load (HPL vs. LPL) as independent variables. To analyse the relationship between n–2 task repetition costs (standard and residual) and CFQ, a frequentist correlation was conducted.

Results

Data trimming. Participant’s data were included in the analysis, if they had an overall accuracy at 80% for LPL and HPL conditions; all participants had accuracy at the required level. Furthermore, null trials (i.e., trials that cannot be classified as ABA or CBA) were removed (i.e., first two trials of each block and two trials following an error). For the RT analysis, apart from null trials, error trials were additionally removed. Also, RTs faster than 150ms or slower than 2.5 standard deviations above participant’s mean per experimental cell were removed.

Reaction time analysis. For descriptive statistics from RTs analysis, see Table 5.1. There was a main effect of Task Sequence on RTs, $F(1{,}39) = 33.43, p < .001, \eta_g^2 = .01$; participants were on average slower on ABA (1246ms) compared to CBA trials (1169ms). The main effect of Response was not significant, $F(1{,}39) = 0.94, p = .34, \eta_g^2 = .0002$; the average RTs on n–2 RR was 1202ms and for n–2 RS, it was 1212ms. The Perceptual Load had a significant main effect on RTs, $F(1{,}39) = 241.37,\ p < .001, \eta_g^2 = .37$; participants were significantly faster performing task-switching under LPL (946ms) than under HPL (1468ms).

There was no significant interaction between Task Sequence and Response, $F(1{,}39) = 0.17, p = .68, \eta_g^2 = .0001$. The interaction between Task Sequence and Perceptual Load was also not significant, $F(1{,}39) = 2.12, p = .15, \eta_g^2 = .0006$. Between Response and Perceptual Load there was no significant interaction, $F(1{,}39) = 0.02, p = .89, \eta_g^2 = .000002$. The three-way interaction between Task Sequence, Response and Perceptual Load, was not significant, $F(1{,}39) = 0.09, p = .77, \eta_g^2 = .00001$ (Figure 5.2). For the densities distributions for RT n–2 task repetition costs see Figure 5.3.

Table 5.1 Mean response times (RTs) and accuracy (%), with standard error, for ABA and CBA sequences, within n–2 Response Repetitions (n–2 RR) and n–2 Response Switches (n–2 RS), under Low Personal Load (LPL) and High Perceptual Load (HPL), from Experiment 8.

	Task sequence			
	ABA		CBA	
Perceptual Load	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
LPL				
n–2 RR	982 (103)	97.20 (.005)	901 (80)	96.50 (.005)
n–2 RS	1004 (100)	95.70 (.005)	905 (93)	97.10 (.007)
HPL				
n–2 RR	1496 (142)	95.50 (.006)	1435 (114)	94.80 (.005)
n–2 RS	1504 (124)	94.30 (.006)	1438 (128)	95.20 (.005)

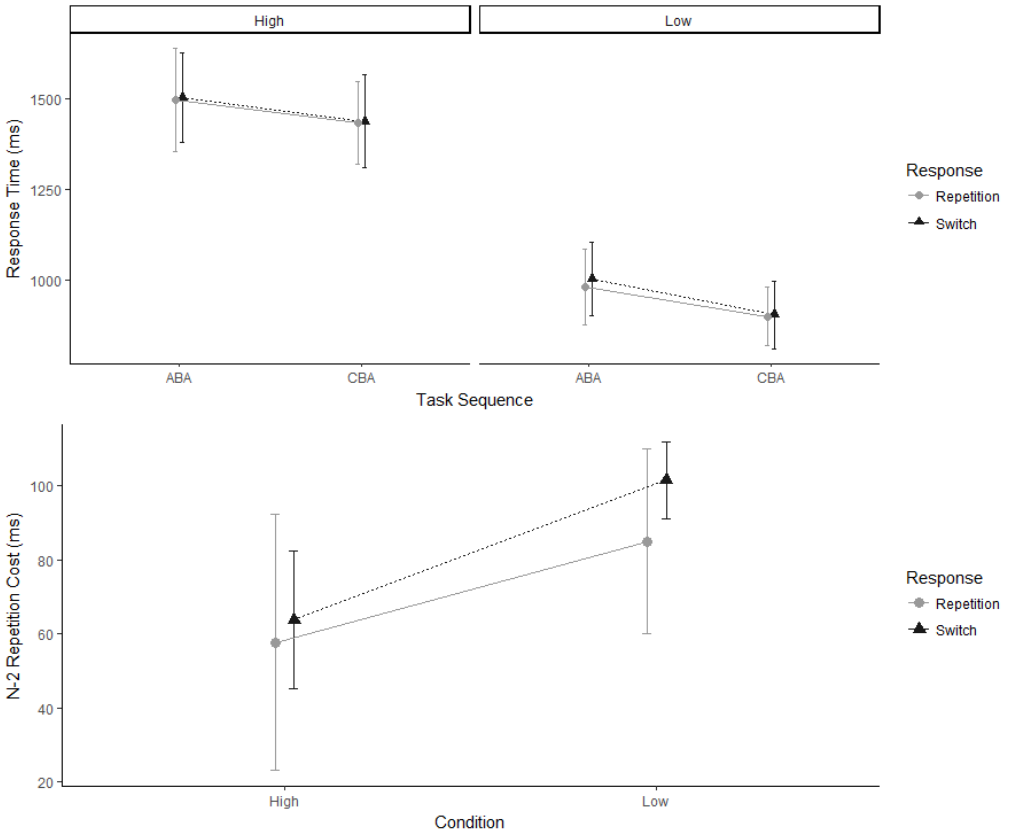


Figure 5.2 At the top, an illustration of a three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and Perceptual Load (High Perceptual Load, HPL vs. Low Perceptual Load, LPL) for RTs. At the bottom, n–2 task repetition costs varying across Response and Perceptual Load. Error bars de-note ±SE around the mean.

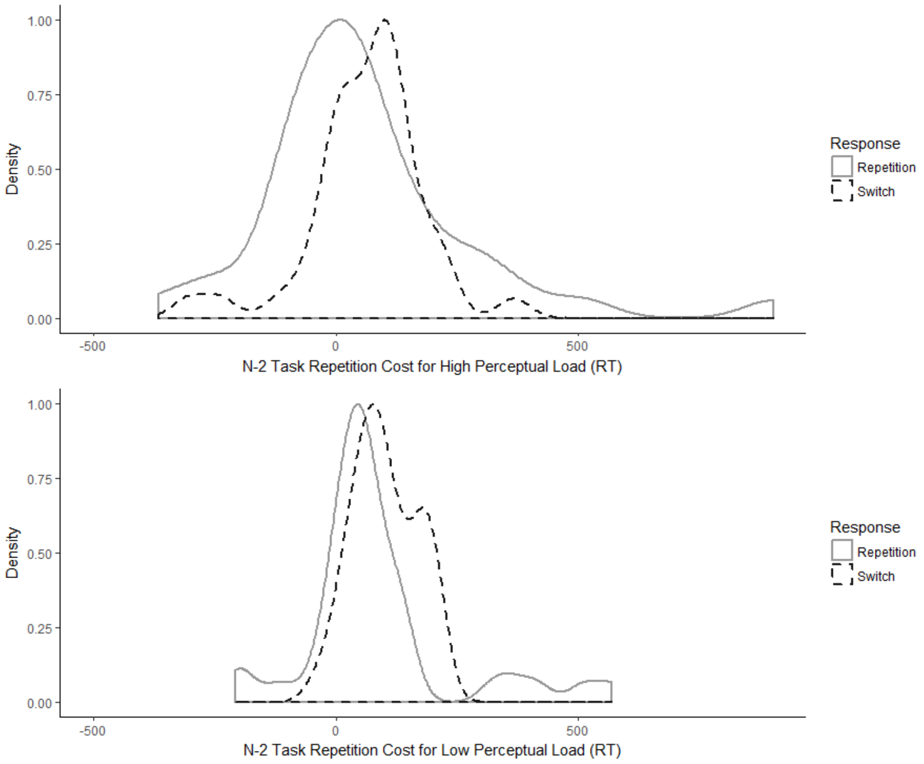


Figure 5.3 Density functions for RT n–2 task repetition costs for High and Low Perceptual Load under n–2 Response Repetitions and n–2 Response Switches.

Log-RT analysis. There was a considerable difference between RTs on task-switching under LPL compared to HPL; therefore, RTs were log-transformed and the ANOVA was repeated. For log-RT, as for the not transformed RTs, there was a main effect of Task Sequence, $F(1{,}39) = 59.00, p < .001, \eta_g^2 = .02$, for ABA average log-RT was 7.07 compared to 7.00 for CBA sequence. The main effect of Response remained not significant, $F(1{,}39) = 1.60, p = .21, \eta_g^2 = .0002$. The Perceptual Load main effect continued to be significant, $F(1{,}39) = 320.70, p < .001, \eta_g^2 =$ .41; for log-RT under HPL average RT was 7.26 compared to LPL which was 6.81.

The interaction between Task Sequence and Response remained non-significant, $F(1{,}39) = 0.86, p = .36, \eta_g^2 = .0002$. The interaction between Task Sequence and Perceptual Load became significant, $F(1{,}39) = 11.49, p < .01, \eta_g^2 =$ .003. Under HPL there was a significant difference in performance between ABA vs. CBA sequences, $t(79) = 3.7, p < .001$, 95%CI [0.02; 0.06]; and under LPL this difference was not significant, $t(79) = -0.92, p =$ .40, 95%CI [-0.03; -0.01]. The n–2 task repetition cost under HPL was 0.04 and for LPL it was 0.10. The interaction between Response and Perceptual Load remained non-significant, $F(1{,}39) = 0.02, p = .90\ \eta_g^2\ = .0000$, so did the three-way interaction between Task Sequence, Response, and Perceptual Load, $F(1{,}39) = 0.15, p = .70, \eta_g^2\ < .001$ (Figure 5.4); for density functions for log-RT n–2 task repetition cost see Figure 5.5.

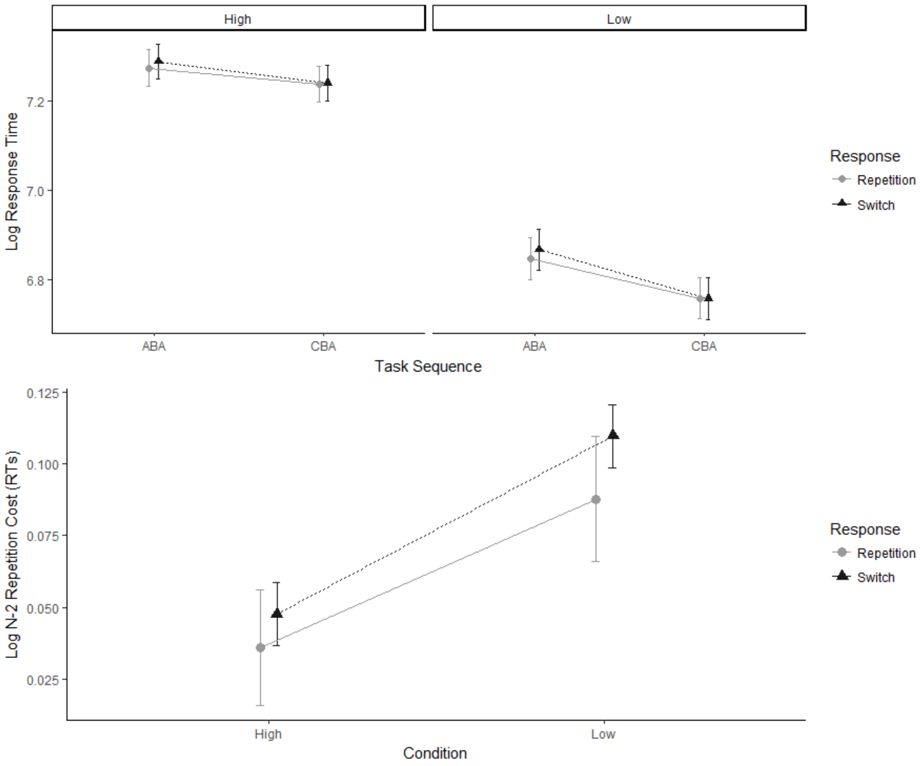


Figure 5.4 At the top, an illustration of a three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and Perceptual Load (High Perceptual Load, HPL vs. Low Perceptual Load, LPL) for log-RT. At the bottom, n–2 task repetition costs varying across Response and Perceptual Load. Error bars de-note ±SE around the mean.

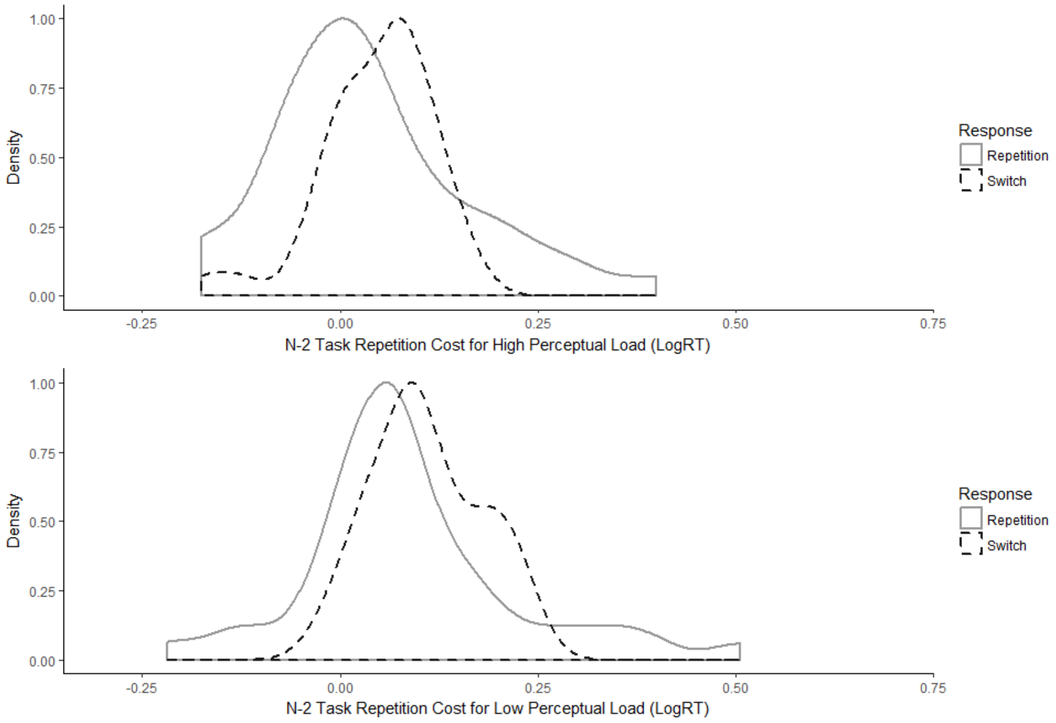


Figure 5.5 Density functions for log-RT n–2 task repetition costs for High and Low Perceptual Load under n–2 Response Repetitions and n–2 Response Switches.

Accuracy analysis. For descriptive statistics from the accuracy analysis, see Table 5.1. The main effect of Task Sequence was not significant, $F(1{,}39) = 1.02, p = .32, \eta_g^2 = .001$; participants were marginally more accurate performing the CBA (95.91%) compared to ABA (95.69%) sequence. There was no significant main effect of Response, $F(1{,}39) = 1.89, p = .18, \eta_g^2 = .004$; as participants performed comparably on n–2 RR (96.01%) and n–2 RS (95.59%). The Perceptual Load had a significant main effect on accuracy, $F(1{,}39) = 31.70, p < .001, \eta_g^2 = .06$; the accuracy was on average higher for the LPL (96.64%) compared to the HPL (94.96%).

There was a significant interaction between the Task Sequence and Response, $F(1{,}39) = 19.70, p < .001, \eta_g^2 = .02$. For n–2 RR, there was a significant difference in accuracy between ABA (96.39%) and CBA (95.63%)—that is the n–2 task repetition benefit of 0.79% was present— $t(79) = 2.00, p = .05$, 95%CI [-0.00001; 0.02]; this difference was also significant for n–2 RS—that is, the n–2 task repetition cost of -1.19% was present—$t(79) = -5.1, p = .00$, 95%CI [-0.02; -0.01], as participants were more accurate for CBA (96.18%) compared to ABA sequence (94.99%). The interaction between Perceptual Load and Task Sequence was not significant, $F(1{,}39) = 0.35, p = .56, \eta_g^2 = .0003$. The interaction between Response and Perceptual Load was also not significant, $F(1{,}39) = 0.01, p = .92, \eta_g^2 = .0004$.

The three-way interaction between Task Sequence, Response, and Perceptual Load was not significant, $F(1{,}39) = 0.29, p = .59, \eta_g^2 = .0004$ (Figure 5.6). For the densities distributions for accuracy n–2 task repetition costs see Figure 5.7.

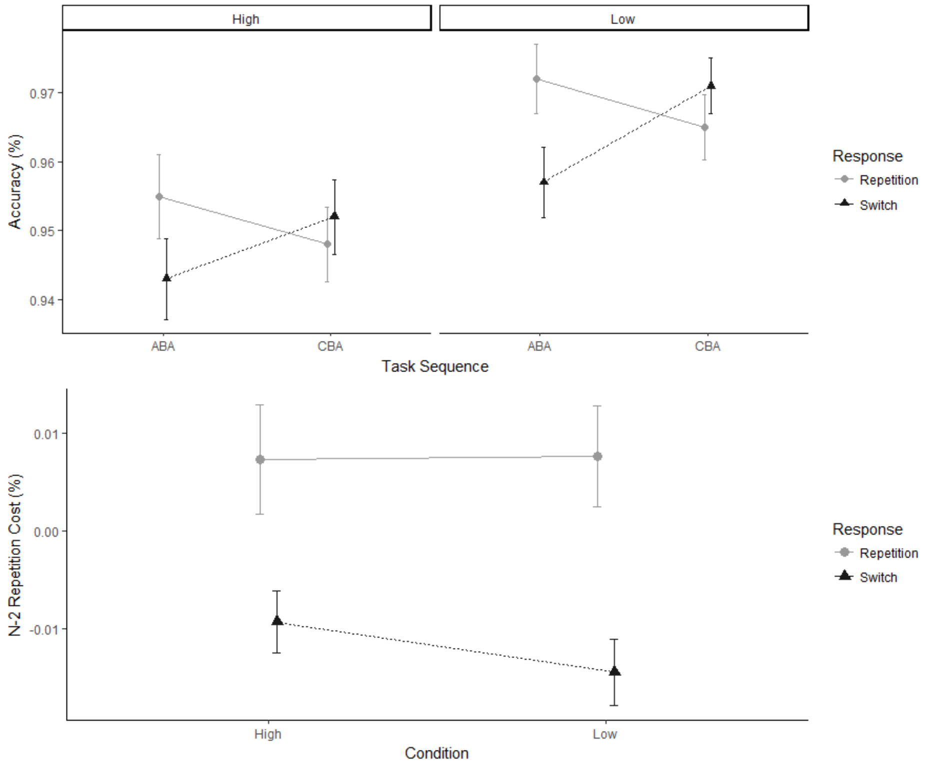


Figure 5.6 At the top, an illustration of a three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and Perceptual Load (High Perceptual Load, HPL vs. Low Perceptual Load, LPL) for accuracy. At the bottom, n–2 task repetition costs varying across Response and Perceptual Load. Error bars de-note ±SE around the mean.

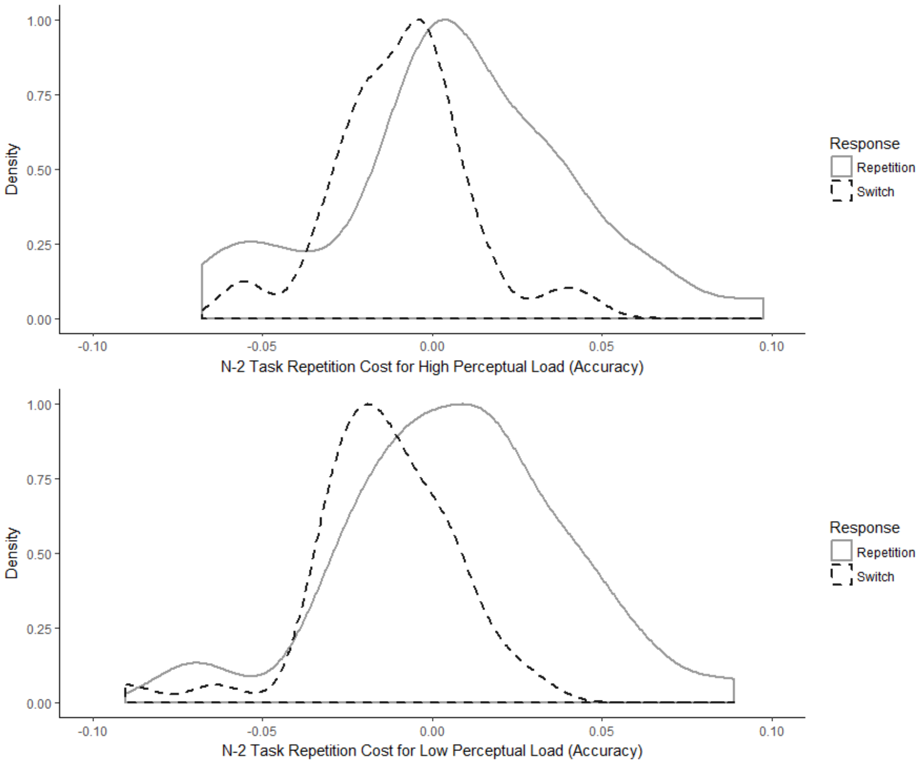


Figure 5.7 Density functions for accuracy n–2 task repetition costs for High and Low Perceptual Load under n–2 Response Repetitions and n–2 Response Switches.

Bayes Factors. BFs for individual models are contained in Table 5.2. To examine whether the n–2 task repetition cost was explained by the full model (Model 4; main effects of Response and Perceptual Load, and an interaction between them) compared to the alternative models, the BF of the full model was divided by the next best model's BF. The alternative models were: Model 1 (main effect of Response), Model 2 (main effect of Perceptual Load), and Model 3 (main effect of Response and main effect of Perceptual Load).

For RT analysis Model 2 had the highest BF among the alternative models; comparing the full model and Model 2 produced a BF_{10} of 0.04405 (0.02064/ 0.4684). This translates to strong evidence for the Model 2—that is, main effect of Perceptual Load—being 22.7 (1/ 0.04405) times more likely than the full model. Comparing the second-best model, Model 1 to the full model, produced a BF_{10} of

0.1086 (0.02064/ 0.19)—considered as substantial evidence—making the main effect of Response 9.21 times more likely (1/ 0.1086) than the full model. There was also substantial evidence for Model 3 (i.e., main effects of Response/ Perceptual Load) being over 4-times more likely than the full model, with the BF_{10} of 0.2362. Overall, the BF factorial analysis of variance for RT n–2 task repetition cost suggests that the full model is an unlikely explanation of patterns seen in n–2 task repetition costs in Experiment 8; the second best explanation was the main effect of Perceptual Load, followed by the main effect of Response explaining data better than the full model.

The BFs analysis for the log-RT n–2 task repetition cost favored Model 2 when compared to the full model (i.e., Model 4) which produced the BF_{10} of 0.0760 (5.229/ 68.82)—considered as strong evidence—making the main effect of Perceptual Load over 13 times more likely than the full model. When Model 1 was compared to the full model, the BF_{10} came out as 185.41 (5.229/ 0.02760); this means that the full model was more likely than the main effect of Response. A BF_{10} over 100 is considered as extreme/ decisive evidence, and in this case, can be considered as certain support for the full model. Also, there was substantial evidence for Model 3 (i.e., main effects of Response and Perceptual Load)—with the BF_{10} of 0.2535 (5.229/ 20.63)—being almost 4-times more likely than the full model.

With regards to BF analysis of accuracy, the results showed that the best model compared to the full model was Model 1, and when these two models were compared, the BF_{10} came out as 0.05363 (91.65/ 1709); this means there was strong evidence for the main effect of Response being over 18-times more likely (1/ 0.05363) than the full model. The second-best model, Model 3, compared to the full model produced the BF_{10} of 0.2702 (91.65/ 339.2) which is considered as substantial evidence for the model with main effects of Response/ Perceptual Load being more likely than the full model. Model 2 when compared to the full model, produced a BF_{10} of 481.61 (91.65/ 0.1903), meaning that the full model was much more likely than the main effect of Perceptual Load. Again, a BF_{10} over 100 is considered as extreme/ decisive evidence.

Table 5.2 Bayes factors for selected models predicting the n–2 task repetition cost as the dependent variable. The reported models were compared to a denominator which was that the n–2 task repetition cost was predicted by a random factor of Participant. "+" indicates an addition of an effect, and "*" indicates an interaction.

N–2 task repetition cost	Model	BF
RT	Response (1)	0.19
	Perceptual Load (2)	0.4684
	R + PL (3)	0.0874
	R + PL + R * PL (4)	0.02064
log-RT	Response (1)	0.02819
	Perceptual Load (2)	68.82
	R + PL (3)	20.63
	R + PL + R * PL (4)	5.229
Accuracy	Response (1)	1709
	Perceptual Load (2)	0.1903
	R + PL (3)	339.2
	R + PL + R * PL (4)	91.65

Correlations. For the n–2 task repetition cost (not controlled for episodic retrieval) the results as follow. For RT performance under HPL and LPL, there was no significant correlation between the scores on CFQ and n–2 task repetition costs; for HPL correlation $r = -.046, p = .80, t(38) = -0.28$, 95%CI [-0.35; 0.27], and for LPL it was, $r = .15, p = .40, t(38) = 0.93, 95\%\text{CI}\ [-0.17;\ 0.44]$. For accuracy performance, under HPL and LPL there was not a significant correlation between n–2 task repetition costs and CFQ; for HPL correlation was $r = -.07, p = .70$,

$\mathrm{t}(38) = -0.41,\ 95\%CI\ [-0.37;\ 0.25]$, and for LPL it was, $r =\ .08, p = .60, t\ (38) = 0.49, 95\%CI[-0.24; 0.38]$.

For the n–2 task repetition cost controlled for episodic retrieval the results are: Under HPL, there was no significant correlation between n–2 task repetition costs for n–RR and CFQ, $r\ = .01, p =\ .90, t(38) = 0.08, 95\%CI\ [-0.30;\ 0.32]$. or for n–2 RS, $r = -.12, p =\ .50, t(38) = -0.75, 95\%CI\ [-0.42;\ 0.20]$. For LPL, the correlation between CFQ and n–2 task repetition costs for n–2 RR was not significant, $r = .15, p =\ .40$, $\mathrm{t}(38) = 0.94, 95\%CI\ [-0.17;\ 0.44]$, as was the correlation between the CFQ n–2 task repetition cost for n–2 RS, $r =\ .06, p = .7$, $\mathbf{t(38) = 0.35,\ 95\%}\boldsymbol{CI}\ \mathbf{[-0.26;\ 0.36]}$.

Looking at accuracy, for HPL, there was no significant correlation between n–2 task repetition switching costs and CFQ for n–2 RR, $r =\ -.13, p = .90, t(38) = 0.08, 95\%CI\ [-0.30; 0.32]$; so was the correlation between the n–2 task repetition switching cost and CFQ for n–2 RS, $r = -.12, p = .50, t(38) = -0.75, 95\%CI\ [-0.42; 0.20]$. Under LPL, the correlations between n–2 task repetition costs and CFQ were non-significant for n–2 RR, $r = .15, p = .40, t(38) = 0.94, 95\%CI\ [-0.17; 0.44]$, or for n–2 RS, $r = .06, p =\ .70, t(38) = 0.35, 95\%CI\ [-0.26; 0.36]$.

Discussion

Experiment 8 looked at whether cognitive inhibition—as measured with the n–2 task repetition cost controlled for episodic retrieval—is modulated by perceptual load. Two perceptual load conditions were used (High and Low); for LPL an unmodified BI paradigm was utilised, and for HPL task-irrelevant distractors were incorporated into the BI paradigm. In both perceptual load conditions participants made a spatial judgment about the target stimulus; however, in the LPL participants saw just one stimulus and in the HPL participants had to locate the target stimulus among four stimuli that appeared on the screen, where the smaller out of the four was the target. Participants also completed the CFQ to allow an investigation of the relationship between day-to-day distractibility tendencies and inhibition. Overall, the results from Experiment 8 are inconclusive in parts but some informative observations can be made.

The standard n–2 task repetition cost was present at RT but not at accuracy level; however, there was a numerical trend for accuracy to be lower for ABA compared to CBA. Episodic retrieval on its own did not affect RT/ accuracy performance as it was similar under episodic matches (n–2 RR) and episodic mismatches (n–2 RS.) The perceptual load manipulation worked well; that is, as expected, performance was considerably slower and less accurate in HPL compared to LPL.

N–2 task repetition cost. According to standard ANOVAs, the RT n–2 task repetition cost was not modulated by episodic retrieval, or perceptual load. Bayesian analysis showed though, that n–2 task repetition costs were best explained by perceptual load manipulation. Also, Bayesian analysis provided some evidence that an interaction between episodic retreival and perceptual load, better explained n–2 task repetition cost than episodic retrieval on its own. For accuracy performance standard ANOVAs showed that the n–2 task repetition cost was modulated by episodic retrieval, and this was confirmed by Bayesian analysis; specifically, the n–2 task repetition *benefit* was observed under episodic matches, compared to the n–2 task repetition cost under episodic mismatches. As such, accuracy n–2 task repetition costs were best explained by episodic retrieval, but there was some evidence for the interaction between episodic retrieval and perceptual load better explaining the data than perceptual load on its own.

By looking at numerical trends, RT n–2 task repetition costs were lower for episodic matches compared to episodic mismatches in both, HPL and LPL conditions, with the smallest n–2 task repetition cost under episodic matches (HPL), and the highest n–2 task repetition cost under episodic mismatches (LPL). For accuracy, n–2 task repetition benefits were observed under for episodic matches in HPL and LPL; whereas, under episodic mismatches, n–2 task repetition costs were observed with the highest cost in LPL condition.

Perceptual load. The perceptual load manipulation had a strong effect on n–2 task repetition costs as performance was on average much slower in HPL compared to LPL; whereas accuracy performance was not as strongly affected by this manipulation. For RT performance there was no obvious benefit of episodic retrieval effect and instead performance was similar under episodic matches and episodic mismatches respectively in HPL and LPL. However, looking at n–2 task repetition costs, they were the smallest under HPL compared to LPL, and within those conditions they were smaller under episodic matches relative to episodic mismatches. Bayesian analysis suggested n–2 task repetition costs were better explained by an interaction between perceptual load and episodic retrieval than episodic retrieval on its own.

At the beginning of this experiment it was not certain how episodic retrieval would be affected by perceptual load, but it was speculated that under different perceptual loads what would become part of memory traces (as well as what is retrieved) could differ between loads (Lavie, 2000; p.1050). For example, under HPL task-irrelevant information should be less efficiently encoded due to limited attentional resources, but under LPL what can be assumed to be encoded is task-relevant as well as task-irrelevant information. Since, there was no obvious effect of episodic retrieval on n–2 task repetition cost in the current study, it is uncertain how perceptual load manipulation affected episodic retrieval. However, numerical trends and Bayesian results would suggest that perceptual load and episodic retrieval contributed to overall smaller n–2 task repetition costs under episodic matches compare to episodic mismatches in HPL and LPL, mechanics of which are not clear from the current results.

In accuracy performance, it is uncertain to what extent perceptual load affects episodic retrieval and/ or inhibition. For accuracy results there was a strong effect of episodic retrieval because under episodic matches n–2 task repetition benefits were observed, compared to episodic mismatches which led to n–2 task repetition cost. The n–2 task repetition benefits under episodic matches were similar in magnitude for LPL and HPL, which suggests no influence of perceptual load on n–2 task repetition costs. However, n–2 task repetition costs under episodic mismatches were larger under LPL compared to HPL. It is uncertain whether under episodic mismatches, it was the episodic retrieval or inhibition that was influenced by perceptual load. It seems that larger n–2 task repetition costs under LPL compared to HPL may be driven by the amount of interference or how interference is dealt with. That is, under LPL (assumed to be marked by spare attentional resources) episodic mismatches may lead to more interference which can be resolved with inhibition—for example, via inhibition of conflicting activations—whereas under HPL, resources are limited, therefore mismatches either lead to less interference, or less inhibition is released to deal with interference. Therefore, it can be speculated that HPL conditions reduce the amount of interference due to mismatches or triggered by it inhibition in task-switching, compared to LPL.

Individual differences. In terms of individual differences, contrary to what was expected, the scores on CFQ did not explain differences in n–2 task repetition costs (standard and controlled). It is possible that performance on task-switching was confounded by potentially too difficult manipulation of perceptual load, which could have affected the measures of inhibition. However, it is also likely that this lack of correlation could be due to low reliability of the n–2 task repetition cost which has been reported recently (Kowalczyk & Grange, 2017; Chapter 3-4 of this thesis). Therefore, due to the n–2 task repetition cost not being a reliable a measure of individual differences in inhibition, there should be no relationship between other measures of individual differences and the n–2 task repetition cost. This is not to say that day-to-day distractibility tendencies do not influence performance on task-switching, or that these differences do not affect how interference is processed; just that Experiment 8 did not observe this relationship.

Conclusions. Perceptual load manipulation introduced to the BI paradigm worked well, overall n–2 task repetition costs were reduced under HPL compared to LPL. However, obvious episodic retrieval modulatory effect was only present in accuracy performance, and in RT performance it was somewhat much reduced compared to what was expected. Therefore, it is not clear from Experiment 8 whether inhibition—as measured the n–2 task repetition cost controlled for episodic retrieval—is reduced by perceptual load.

The perceptual load manipulation used in Experiment 8—even though it worked well—might have been too difficult and interfered too much with task-switching performance; therefore, Experiment 9 was conducted to test another perceptual load manipulation. The task-irrelevant distractors used in HPL of Experiment 8 looked similar to the target, thereby participants searched for the target ignoring distractors, which made the task difficult—that is, the target did not automatically capture participants' attention—but distractors did not afford any response. As an alternative, different distractors can be used which appear simultaneously with the target, look similar to the target, do not afford a response, make presentation perceptually complex but make the task easier to perform. It was decided that distractors that are salient, not task-relevant but are likely to be processed by visual cortex would be appropriate (Murphy, Dalton, & Spence, 2017; Rees, Frith, & Lavie, 1997; Rorden et al., 2008); as such, small circles around the cue which change colours on each trial were expected to strain attentional resources. As in Experiment 9 task-irrelevant-distractors are meant to be ignored but are expected to strain attentional resources, affecting performance. Therefore, as in Experiment 9 BI paradigm was adapted accordingly to include task-irrelevant distractors; standard and controlled n–2 task repetition costs were measured, as well as day-to-day distractibility using CFQ.

Experiment 9

Method

Participants. Forty participants were recruited from the School of Psychology at Keele in exchange for partial course credits or a one-off monetary reimbursement of £6.50. There were twenty-four women, and the average age was 27.18 (SD 11.16). As in Experiment 8, participants had to be at least 18-years-old, understand spoken and written English, have normal/ corrected to normal vision.

Stopping rule. As in Experiment 8, Sequential Bayes Factors method was used to determine the sample size. Also, the same four probable models were selected for comparison; that is, the full model being the main effect of Response, main effect of Perceptual Load, and interaction between them, as the model of interest. At thirty-six participants, the BF_{10} ascended to below 1/6; however, four more participants were tested as they already booked to take part in the experiment. At forty participants, the BF_{10} was 0.152; this translates to the alternative model (main effect of Response, Model 1; see Table 5.4) being 6.58 times more likely than the full model.

Procedure. As in Experiment 8, participants attended a single session during which they performed two task-switching tests (Low and High Perceptual load) and filled in the CFQ questionnaire. The procedure was the same as in Experiment 8, except that in the HPL, participants saw colored circles around the cue which they had to ignore.

Apparatus and stimuli. Apparatus and stimuli were the same as in Experiment 8, except that in the HPL, one stimulus appeared in one corner of the frame, and the perceptual load was manipulated by introducing small circles around the cue. Small circles around the cue had different colors on each trial, as they were randomly drawn from the color circle (see Figure 5.8). The CFQ questionnaire was also used in Experiment 9. The minimum score on CFQ was 24 and the maximum score was 77.

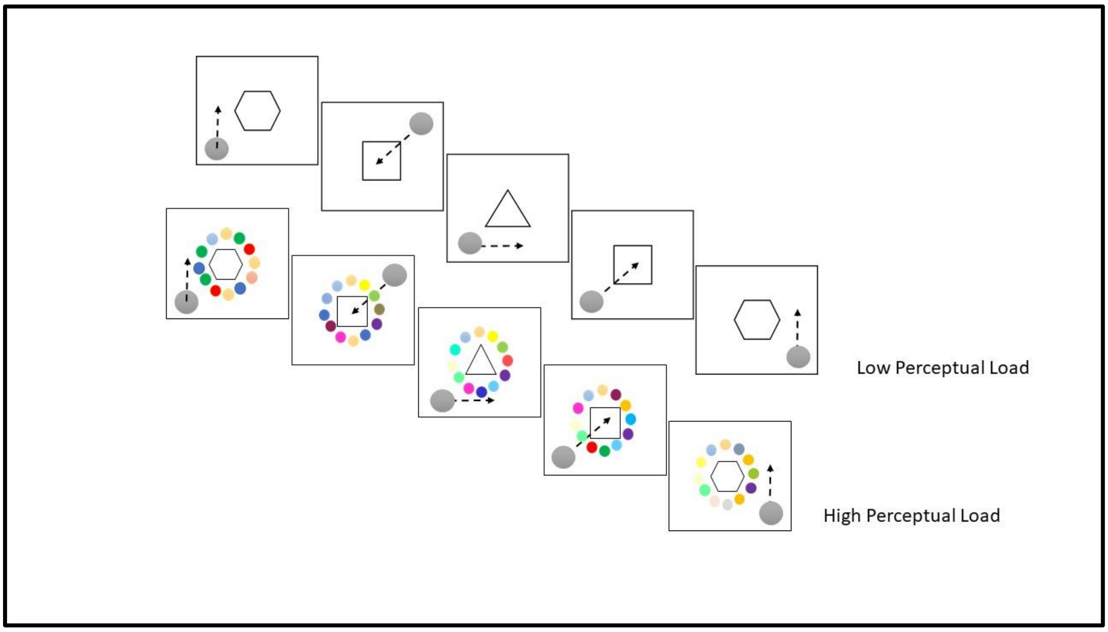


Figure 5.8 A visualisation of the paradigm in Experiment 9. The examples on the top (with one grey circle) represent low perceptual load and the examples at the bottom (with coloured circles around the cue) represent high perceptual load.

Design. As in Study 9, a within-subjects design was utilised to examine RTs (ms) and accuracy (%) as dependent variables, and the Task Sequence (ABA vs. CBA), Response Repetition (Response Repetition vs. Response Switch), and Perceptual Load (High vs. Low) as independent variables. To analyse the relationship between the n–2 task repetition cost (standard and controlled) and CFQ, a frequentist correlation was conducted.

Results

Data Trimming. Data for Experiment 9 were trimmed in the same way as for Experiment 8. All participants had an overall accuracy above 80% for Low and High Perceptual Load conditions.

Reaction time analysis. For descriptive statistics from RTs, see Table 5.3. There was a significant main effect of Task Sequence on RTs, $F(1{,}39) = 45.68, p < .0001, \eta_g^2 = .03$, as participants were on average faster performing CBA (916ms) compared to ABA (1001ms). The main effect of Response was not significant, $F(1{,}39) = 0.31, p = .58, \eta_g^2 < .0001$ (n–2 RR 956ms; n–2 RS 961ms). The main effect of Perceptual Load was non-significant, $F(1{,}39) = 0.09, p = .77, \eta_g^2 = .0005$ (High Load 964ms; Low Load 952ms).

The interaction between Task Sequence and Response was significant, $F(1{,}39) = 10.65, p = .002, \eta_g^2 = .002$. Under n–2 RR performance on ABA (987ms) was significantly different compared to CBA (925ms), $t(79) = 4.2, p < .001$, 95%CI [32.40; 90.63], and for n–2 RS, $t(79) = 12.00, p < .0001$, 95%CI [89.89; 125.46] (ABA 1014ms; CBA 907ms). The n–2 task repetition cost for n–2 RR was 62ms and 107ms for n–2 RS. The interaction between Task Sequence and Perceptual Load was not significant, $F(1{,}39) = 2.00, p = .17, \eta_g^2 = .0003$. The n–2 task repetition cost under HPL was 75ms, and 93ms for LPL. The interaction between Response and Perceptual Load was also non-significant, $F(1{,}39) = 2.68, p = .11, \eta_g^2 = .0008$.

The three-way interaction between Task Sequence, Response, and Perceptual Load was not significant, $F(1{,}39) = 1.31, p = .26, \eta_g^2 = .0002$. For n–2 RR, the n–2 task repetition cost was 45ms under HPL and 78ms under LPL; for n–2 RS, the n–2 task repetition cost was 107ms under HPL and 108ms under LPL (see Figure 5.9). For density for RT n–2 task repetition cost see Figure 5.10.

Table 5.3 Mean response times (RTs) and accuracy (%), with standard error, for ABA and CBA sequences, within n–2 Response Repetitions (n–2 RR) and n–2 Response Switches (n–2 RS), under Low Personal Load (LPL) and High Perceptual Load (HPL), from Experiment 9.

	Task sequence			
	ABA		CBA	
Perceptual Load	RTs (ms)	Accuracy (%)	RTs (ms)	Accuracy (%)
LPL				
n–2 RR	1000 (87)	97.60 (.004)	917 (75)	97.50 (.006)
n–2 RS	1000 (80)	96.70 (.007)	893 (69)	98.00 (.004)
HPL				
n–2 RR	990 (75)	96.71 (.006)	935 (73)	97.10 (.006)
n–2 RS	1027 (84)	96.10 (.007)	917 (72)	97.70 (.005)

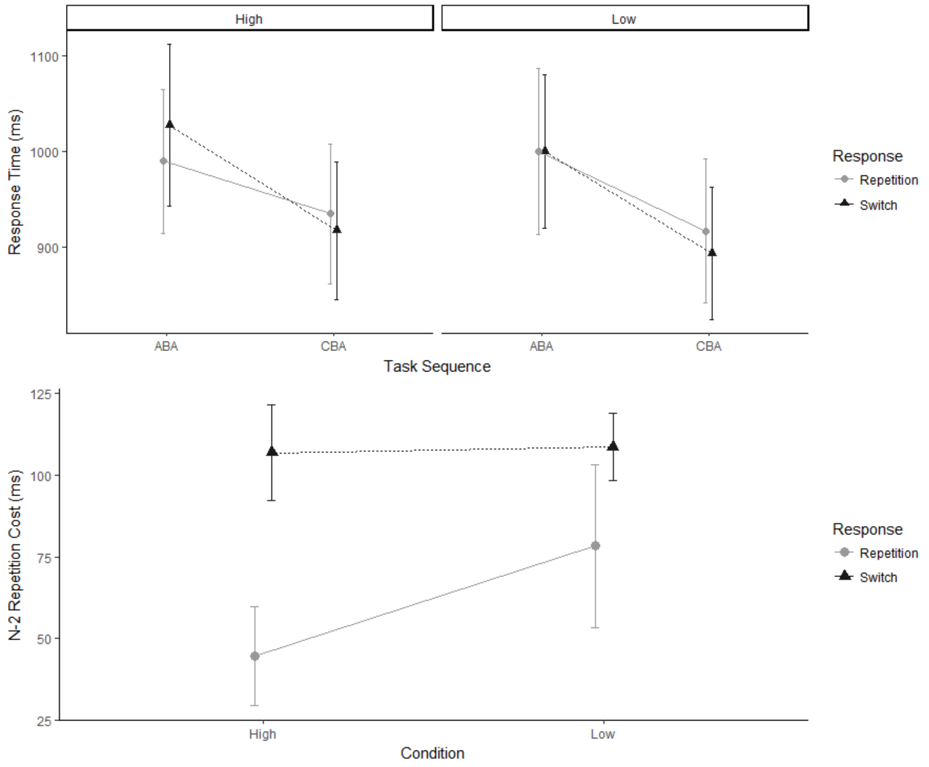


Figure 5.9 At the top, an illustration of a three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and Perceptual Load (High Perceptual Load, HPL vs. Low Perceptual Load, LPL) for RTs. At the bottom, n–2 task repetition costs varying across Response and Perceptual Load. Error bars denote ±SE around the mean.

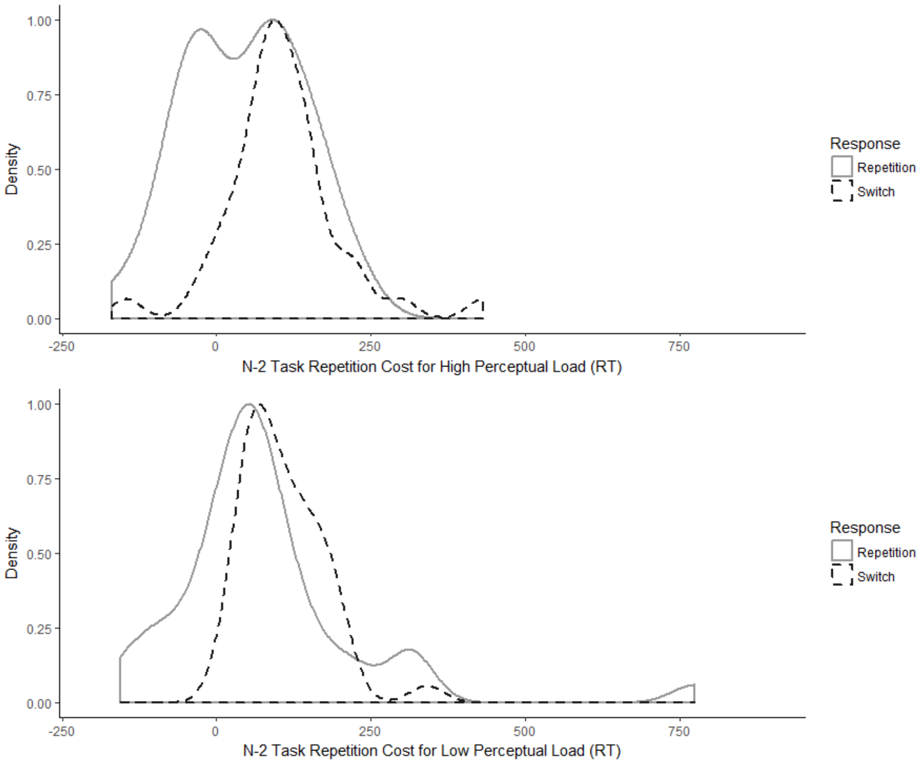


Figure 5.10 Density functions for RT n–2 task repetition costs for High and Low Perceptual Load under n–2 Response Repetitions and n–2 Response Switches

Accuracy analysis. For descriptive statistics from accuracy, see Table 5.3. The main effect of Task Sequence on accuracy was significant, $F(1{,}39) = 7.09, p = .01, \eta_g^2 = .008$; accuracy was higher for CBA (97.60%) compared to ABA (96.90%) sequence. The main effect of Response on the other hand was not significant, $F(1{,}39) = 0.58, p = .45, \eta_g^2 = .0005$ (n–2 RR 97.30%; n–2 RS 97.20%). The main effect of Perceptual Load was significant, $F(1{,}39) = 4.62, p = .04, \eta_g^2 = .005$; this was because the accuracy was higher under LPL (97.50%) compared to HPL (97.00%).

The interaction between Task Sequence and Response was significant, $F(1{,}39) = 6.11, p = .02, \eta_g^2 = .009$. Specifically, there was no significant difference in accuracy between ABA (97.34%) and CBA (97.31%) for n–2 RR, $t(79) = 0.07, p = .90$, 95%CI [-0.007; 0.008]; however, for n–2 RS the accuracy on ABA (96.48%) compared to CBA (97.83%) was significantly different, $t(79) = -4.40, p < .0001$, 95%CI [-0.02; -0.007]. The n–2 task repetition cost was 0.03 for n–2 RR, and -1.35% for n–2 RS. The interaction between Task Sequence and Perceptual Load was not significant, $F(1,39) = 0.52, p = .48, \eta_g^2 = .0004$; neither was the interaction between Response and Perceptual Load, $F(1,39) = 0.01, p = .91, \eta_g^2 < .0001$.

The three-way interaction between Task Sequence, Response, and Perceptual Load, was non-significant, $F(1,39) = 0.18, p = .68, \eta_g^2 = .0002$. Under HPL, for n–2 RR, the n–2 task repetition cost was -0.02 and 0.08 under LPL; for n–2 RS, the n–2 task repetition cost was -1.59 under HPL and -1.11 under LPL (see Figure 5.11). For density functions for accuracy n–2 task repetition cost see Figure 5.12.

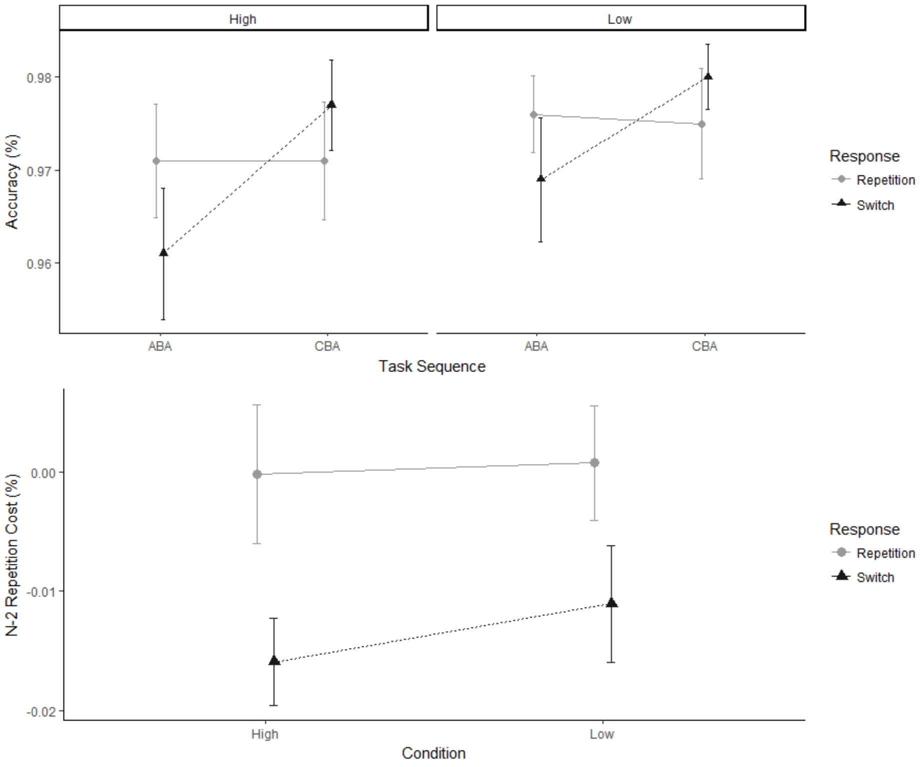


Figure 5.11 At the top, an illustration of a three-way interaction between Task Sequence (ABA vs. CBA), Response (n–2 Response Repetition vs. n–2 Response Switch) and Perceptual Load (High Perceptual Load, HPL vs. Low Perceptual Load, LPL) for accuracy. At the bottom, n–2 task repetition costs varying across Response and Perceptual Load. Error bars de-note ±SE around the mean.

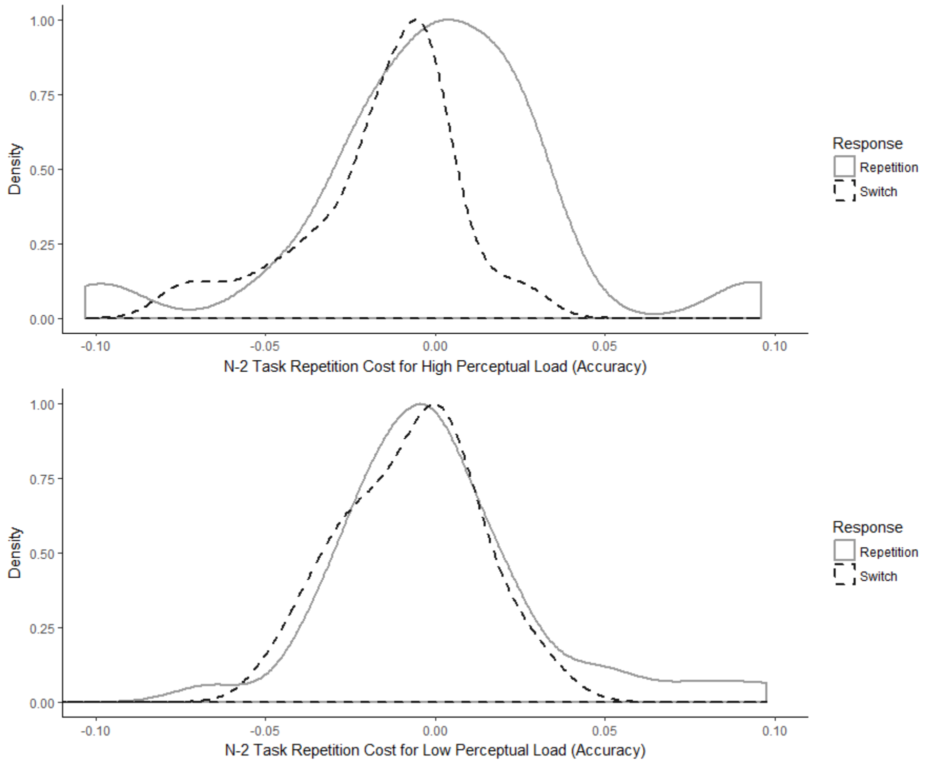


Figure 5.12 Density functions for accuracy n–2 task repetition costs for High and Low Perceptual Load under n–2 Response Repetitions and n–2 Response Switches.

Bayesian factors. The results for BF analysis is in Table 5.4. For RT n–2 task repetition costs, the Bayesian analysis of variance showed that when the full model (Model 4; main effects of Response/ Perceptual Load and an interaction between them) was compared to the best model, which was Model 1 (i.e., main effect of Response), the BF_{10} was 0.152 (4.486/ 29.57). This is considered as substantial evidence in favour of the main effect of Response explaining patterns seen in RT n–2 task repetition cost in data over 6-times more likely (1/0.152) than the full model. There was anecdotal evidence with BF_{10} of 0.4225 (4.486/ 10.85) for the second-best model—that is, Model 3; main effects of Perceptual Load/ Response—being over twice more likely than the full model. Also, there was strong evidence with BF_{10} of 13.09 (4.486/0.3504) for the full model being over 13 times more likely than Model 2 (i.e., main effect of Perceptual Load).

For accuracy, comparing the full model to the best model (i.e., Model 1; main effect of Response), showed that there was strong evidence—with BF_{10} of 0.05027 (0.5274/10.49)—for the main effect of Response being almost 20-times more likely than the full model. The second-best model, which was Model 3 (i.e., main effects of Response/ Perceptual Load), compared to the full model was almost 4-times more likely than the full model, as reflected with a BF_{10} of 0.2502 (0.5274/2.108), which is considered as substantial evidence. And lastly, there was anecdotal evidence for the full model being over twice more likely than Model 2 (i.e., main effect of Perceptual Load), with BF_{10} of 2.6 (0.5274/0.2028).

Table 5.4 Bayes factors for selected models predicting the n–2 task repetition cost as the dependent variable. The reported models were compared to a denominator which was that the n–2 task repetition cost was predicted by a random factor of Participant. "+" indicates an addition of an effect, and "*" indicates an interaction

N–2 task repetition cost	Model	BF
RTs	Response (1)	29.57
	Perceptual Load (2)	0.3504
	R + PL (3)	10.85
	R + PL + R * PL (4)	4.486
Accuracy	Response (1)	10.49
	Perceptual Load (2)	0.2028
	R + P (3)	2.108
	R + PL + R * PL (4)	0.5274

Correlation

Standard n–2 task repetition cost. There was no significant correlation between the RT n–2 task repetition cost and the CFQ for HPL, $r = .06, p = .70, t(38) = 0.34, 95\%CI$ $[-0.26; 0.36]$; or for the LPL, $r = .11, p = .50, t(38) = 0.71, 95\%CI$ $[-0.21; 0.41]$.

For accuracy, the n–2 task repetition cost did not correlate significantly with CFQ under HPL, $r = .04, p = .80, t(38) = 0.23, 95\%CI$ $[-0.28; 0.35]$; and it did not correlate significantly under LPL, $r = .11, p = .2, t(38) = 0.71, 95\%CI$ $[-0.21; 0.41]$.

Controlled n–2 task repetition cost. For HPL, in n–2 RR, the correlation between the n–2 task repetition cost and the CFQ was non-significant, $r = .10, p = .60, t(38) = 0.59, 95\%CI$ $[-0.22; 0.40]$; which was also a case for n–2 RS, $r = .-.006, p = 1.00, t(38) = -0.04, 95\%CI$ $[-0.32; 0.31]$.

For LPL, in n–2 RR condition, the n–2 task repetition cost did not correlate significantly with CFQ, $r = .19, p = .30, t(38) = 1.2, 95\%CI$ $[-0.13; 0.47]$; this correlation was non-significant for n–2 RS too, $r = -.10, p = .50, t(38) = -0.64, 95\%CI$ $[-0.40; 0.22]$.

For accuracy, under HPL, the n–2 task repetition cost from n–2 RR did not correlate significantly with CFQ, $r = -.01, p = 1.0, t(38) = -0.05, 95\%CI$ $[-0.32; 0.30]$; this was also the case for n–2 RS, $r = .09, p = .60, t(38) = 0.56, 95\%CI$ $[-0.23; 0.39]$.

Under PLP, the n–2 task repetition cost from n–2 RR did not correlate significantly with CFQ, $r = -.11, p = .50, t(38) = -0.67, 95\%CI$ $[-0.41; 0.21]$; however, the CFQ correlated significantly with the n–2 task repetition cost for n–2 RS, $r = .35, p = .03, t\ (38) = 2.3, 95\%CI$ $[0.04; 0.60]$. When the p value was corrected for multiple correlations—with corrected *p* was .0125—the latter correlation was no longer significant.

Discussion

Experiment 9 was conducted to investigate further the relationship between the n–2 task repetition cost and perceptual load. Two perceptual load conditions (HPL and LPL) were used, and attentional resources were manipulated by introducing task-irrelevant distractors. As in Experiment 8, in both perceptual load conditions participants made a spatial judgment about the target stimulus; however, in the HPL coloured circles appeared around the cue on each trial, whereas in the LPL there were no such circles around the cue. Participants were instructed to ignore the coloured circles in the HPL. The relationship between day-to-day distractibility tendencies (as measured with CFQ) and n–2 task repetition costs were also looked at. As in Experiment 8, standard and controlled n–2 task repetition costs were measured.

The n–2 task repetition cost was present in RTs as well as accuracy, and it was modulated by episodic retrieval; this is a replication of findings from Chapters 3–4 of the current thesis (Grange et al., 2017). The perceptual load manipulation worked in accuracy but not in RT performance; for accuracy, participants were on average less accurate in HPL compared to LPL, and for RTs, performance was numerically slower under HPL relative to LPL. Contrary to predictions of the current study, the perceptual load manipulation did not influence the standard or controlled n–2 task repetition cost; however, numerically the costs were smaller under HPL, especially for episodic matches. Bayesian analysis, overall, showed that n–2 task repetition costs (standard and controlled for episodic retrieval) were unlikely to be modulated by perceptual load; although, an interaction between perceptual load and episodic retrieval explained results better than perceptual load on its own.

Nevertheless, the predominant support was for episodic retrieval best explaining the patterns seen in RT and accuracy n–2 task repetition costs. Also, as in Experiment 8, the n–2 task repetition cost individual differences were not explained by CFQ.

General Discussion

The current study looked at whether inhibition in task-switching—as measured with the n–2 task repetition cost controlled for episodic retrieval—is modulated by perceptual load, and whether individual differences in this effect can be explained by self-reported day-to-day distractibility. This is the first study of this kind, as there seems to be no study that reports investigation of attentional resources in task-switching using perceptual load. Two experiments were conducted to test for effects of task-irrelevant distractors on n–2 task repetition costs. The study was conducted under the hypothesis that the extent to which inhibition is deployed in cognitive tasks depends on attentional resources—in this case manipulated with perceptual load (Lavie, 1995; Lavie & Fox, 2000)—and that individual differences in n–2 task repetition costs can be accounted for by scores on CFQ, difference which should be diminished under HPL (Forster & Lavie, 2009b).

The results from the two studies are inconclusive, but overall, it seems that n–2 task repetition costs controlled for episodic retrieval are not modulated by perceptual load. However, in Experiment 8 RT n–2 task repetition costs not controlled for episodic retrieval was modulated by perceptual load, with the cost being smaller under HPL compared to LPL. Also, numerically, in both experiments there were trends for n–2 task repetition costs (standard and controlled for episodic retrieval) were smaller under HPL relative to LPL, and within those conditions the costs were smaller for episodic matches compared to episodic mismatches. Moreover, in Experiment 8 Bayesian analysis for RT performance supported evidence for perceptual load modulating the n–2 task repetition cost, followed by episodic retrieval, but in Experiment 9 episodic retrieval more likely explained data. For accuracy for Experiments 8–9, the predominant support from Bayesian analysis was for episodic retrieval modulating n–2 task repetition costs. And in terms of individual differences in n–2 task repetition costs, in both experiments they were not linked to CFQ.

N–2 Repetition Cost

The n–2 task repetition cost was present in RT and log-RT but not in accuracy for Experiment 8, and in both RT and accuracy for Experiment 9; therefore the standard n–2 task repetition cost was replicated, supporting the reports in literature (Grange et al., 2017; Kiesel et al., 2010; Koch et al., 2010; Kowalczyk & Grange, 2017) and current thesis' experiments (Chapters 2–4). In terms of controlling for episodic retrieval (Grange & Kowalczyk, 2017; Mayr, 2002; Neill, 1997; Chapter 3), the n–2 task repetition cost was modulated by it in accuracy only in Experiment 8—with performance being numerically more accurate under episodic matches compared to mismatches—and in both RT and accuracy in Experiment 9, confirming previous reports (Grange & Kowalczyk, 2017; Chapter 3).

Perceptual Load

With it being the first study that looked at perceptual load and inhibition in task-switching, there was no indication on what would be considered a suitable manipulation of perceptual load. As such, rather than creating a new task-switching paradigm, an established BI paradigm (Grange et al., 2017; Mayr, 2007) was adapted for the purpose of the current study.

In Experiment 8 task-irrelevant distractors were used as suitable manipulation to construct conditions for HPL; that is, participants saw four similar looking stimuli out of which one was the target. Three distractors may not be considered enough to exhaust attentional resources (Lavie & Fox, 2000), but the perceptual load manipulation was deemed suitable because the distractors did not afford responses, and searching for the target made the task more difficult. The results showed that this manipulation worked as performance was less accurate and slower under HPL relative to LPL; however, HPL might have been too difficult because participants' accuracy was high but RT performance was considerably slower in HPL compared to LPL. N–2 task repetition costs present in log-RTs were modulated by perceptual load, with the cost being smaller under HPL compared to LPL; also, for RT performance n–2 task repetition costs were smaller under HPL relative to LPL. Moreover, even though n–2 task repetition costs controlled for episodic retrieval were not modulated by perceptual load, numerically the n–2 task repetition cost was smaller for episodic matches under HPL compared to LPL. Since the RTs were so slow, too much time might have passed between trials for episodic retrieval (Grange et al., 2017; Hommel, 1998; Neill, 1997) effects to be present. Bayesian analysis supported perceptual load as likely modulating n–2 task repetition costs (for RT and log-RT performance), followed by an interaction between perceptual load and episodic retrieval being more likely than episodic retrieval on its own.

In Experiment 9 different task-irrelevant distractors were used, making the task easier to perform (as indicated by comparable performance for HPL and LPL). The task-irrelevant distractors were circles in different colours (randomly drawn from the colours circle and change on every trial) placed around the cue. The logic behind this manipulation was that the presentation on each trial was meant to be perceptually rich which should have been enough to strain attentional resources (Rees et al., 1997; Rorden et al., 2008), and increase task-relevant processing. This time, the n–2 task repetition cost was present and was modulated by episodic retrieval, replicating findings from previous chapters of this thesis. The perceptual load manipulation worked well in accuracy but not in RT performance; that is, performance was less accurate under HPL compared to LPL, but it was similar in HPL and LPL in terms of speed. However, n–2 task repetition costs (standard and controlled for episodic retrieval) were not modulated by perceptual load. Numerically, n–2 task repetition costs were smaller under HPL relative to LPL, and within that, they were smaller for episodic matches compared to mismatches (apart from episodic mismatches in RT). However, Bayesian analysis provided more support for episodic retrieval than perceptual load modulating the n–2 task repetition cost. Overall, the evidence from Experiment 9 leans towards suggesting that n–2 task repetition costs are unlikely to be modulated by perceptual load.

The manipulations of perceptual load in the current study can be seen as a limitation, mainly because they are the first of this type used in BI paradigms; as such, the manipulation used in Experiment 8 seemed too difficult and in Experiment 9 might have been too easy. Also, it is uncertain yet how perceptual load interacts with episodic retrieval which makes manipulating perceptual load in task-switching difficult. However, with this study being the first one of this kind, it still offers some insight into whether task-switching is likely to be modulated by attentional resources; based on the current results, it seems that attentional resources may be important in task-switching but it may depend of the type of distractors used to manipulate perceptual load and whether episodic retrieval is controlled for.

Individual Differences

Contrary to what was predicted, individual differences in day-to-day distractibility did not predict inhibition measured with the residual n–2 task repetition costs. Based on previous research (Forster & Lavie, 2008; Kahneman &

Chajczyk, 1981; Lavie & Fox, 2000; Rorden et al., 2008) and assuming that the n–2 task repetition cost controlled for episodic retrieval reflects inhibition, the individual differences seen in the cost were speculated to be explained by day-to-day distractibility (as measured with CFQ), especially under low perceptual load. The source of the lack of the relationship between the n–2 task repetition cost and the CFQ may be manifold.

There is a possibility that perceptual load manipulations utilised in the current study were not optimal and affected adversely measures of inhibition; however, this is unlikely, because n–2 task repetition cost controlled for episodic retrieval did not differ between perceptual load conditions. It is possible also, that this study's findings fit in with results from Chapter 4 (as well as Grange & Houghton, n.d.; Pettigrew & Martin, 2015) that indicated that inhibition, as measured with the n–2 task repetition cost controlled for episodic retrieval, is not resource dependent, or that it does not reflect inhibition. For example, controlling for episodic retrieval in accuracy lead to accuracy n–2 task repetition benefits instead of n–2 task repetition costs; and n–2 task repetition benefits are unlikely to reflect inhibition. Also, another candidate explaining the lack of relationship between n–2 task repetition costs and the CFQ is the lack of the reliability of the n–2 task repetition cost—controlled and not controlled for episodic retrieval—which has been documented recently (Kowalczyk & Grange, 2017; Chapter 2 & 3: Experiment 1). Moreover, the n–2 task repetition cost is a difference score which are difficult to interpret and are often not reliable (Crawford et al., 2008), therefore it is not certain whether inhibition in task-switching is not resource-dependent or whether inhibition is just not measured well with n–2 task repetition costs.

Conclusions

Overall, the results from the current study are mixed. Assuming that n–2 task repetition cost controlled for episodic retrieval reflects inhibition, it seems that it is not influenced by perceptual load. However, n–2 task repetition cost not controlled for episodic retrieval seems affected by perceptual load to some extent. Nevertheless, it is not certain whether in the case of n–2 task repetition costs not controlled for episodic retrieval, perceptual load affected the episodic retrieval or inhibition. Also, individual differences in day-to-day distractibility do not explain n–2 task repetition costs, whether controlled for episodic retrieval or not. This is the first study of this kind, and more refined investigation of perceptual load role in task-switching is needed.

Chapter 6 Episodic Retrieval Modulation of the n–2 task Repetition Cost Examined with Ex-Gaussian and Diffusion Modelling.

Abstract

Data from four studies were examined with ex-Gaussian and diffusion modelling to investigate the n–2 task repetition cost—a behavioural effect seen in task-switching thought to reflect inhibition. The n–2 task repetition cost is considerably modulated by a non-inhibitory mechanism (i.e., episodic retrieval), which questions the extent to which it reflects inhibition. Diffusion modelling results show that the n–2 task repetition cost was predominantly present in drift rate—evidence for the cost to be a carry-over effect (e.g., inhibition)—but the cost was absent or reduced when episodic retrieval was controlled for—evidence for the cost unlikely to be due to carry-over effects. The n–2 task repetition cost was also present in threshold separation (with/ without episodic retrieval controlled for) likely due to more cautious performance on ABA compared to CBA sequences. The results for non-decisional parameter were inconclusive. Ex-Gaussian modelling showed that n–2 task repetition costs were the largest in the tau parameter, but were also observed in mu and sigma parameters; and the costs were reduced when episodic retrieval was controlled for. Overall, the current study questions the validity of the n–2 task repetition cost as a measure of cognitive inhibition.

Introduction

Computerised task-switching paradigms are considered very useful in assessing executive functions (e.g., inhibition, set-shifting), and have been used widely to study cognition in task-switching (for reviews see Kiesel et al., 2010; Monsell, 2003). The main feature of this method is that it involves participants switching between or repeating simple computerised reaction time tasks. What constitutes a task can be determined by instructions given (Dreisbach et al., 2007) but in general a task is characterised as a set of rules and procedures required to perform a task resulting in a motor response (Logan & Gordon, 2001; Rogers & Monsell, 1995). For example, if stimuli are digits presented sequentially on a computer screen (one digit shown at a time), one task may be to make a judgment about whether a digit is lower or higher than number 5, and another task could be to judge whether a digit is an odd or an even number. Typically, a cue is presented before each stimulus so participants know which task they should be performing. Response choices are often binary (i.e., Correct and Incorrect) and are made via pressing relevant keyboard keys (e.g., Z-key press for an odd number, N-key press for an even number). Efficient task-switching requires participants to learn as well as to manipulate sets of rules and procedures (i.e., task sets) required to perform individual tasks, which typically consists of having to remember what each cue represents, which task's rule (e.g., parity or magnitude judgment) to apply to the presented stimulus, and then which response to make. Task-switching performance—as measured via reaction times and accuracy—is typically examined with analysis of variance (i.e., ANOVA); and the differences in performance between experimental conditions, groups, as well as individuals are used to make inferences about cognitive processes that might be at play.

N–2 Task Repetition Cost

One of the consistent findings from the task-switching research is the observation of an effect known as the n–2 task repetition cost (Mayr & Keele, 2000), which is seen in task-switching paradigms utilising three tasks, a paradigm designed to test a hypothesis about inhibition being an important factor for successful task-switching. In a series of experiments Mayr and Keele have shown that data produced by their paradigm were characterised with a pattern consistent with the theory that predicts inhibition aiding disengagement from a task—which later was supported by many other studies (for a review see Koch, Gade, Schuch, & Philipp, 2010). In general, when participants switch between three tasks, the n–2 task repetition cost is identified by an overall slower and less accurate performance when returning to a task that was completed on n–2 trial (e.g., ABA sequence), compared to performance on a task that was not performed on n–2 trial (e.g., CBA sequence). The letters in sequences ABA and CBA are arbitrary labels for three tasks (e.g., A-parity judgment, B-magnitude judgment, C-form judgment); the labels reflect whether in a sequence of presented tasks, the task on a current trial repeats or not from the n–2 trial. Typically, tasks are presented to participants in a random order, and a computer code compares a task on a current trial to the one on the n–2 trial and records the sequence as ABA or CBA.

The n–2 task repetition cost is thought to reflect cognitive inhibition in task-switching, and the reason for it is supported by the following logic. To perform rapid task-switching efficiently, participants have to shift back and forth between three tasks; that is, on a current trial they have to bring to mind the relevant task set selected among all possible tasks. Since task-switching is rapid, once a given task is completed, its activation is thought not to stop instantly; instead, when attention is shifted away from the just performed task, its activation is thought to decay gradually whilst a new task set is being activated (Houghton & Tipper, 1994; Mayr & Keele, 2000). The continued activation of an abandoned task is thought to interfere with the activation of the next task; and inhibition is a process believed to temporarily supress the interfering activation of the not relevant task to allow for the next task to be activated. However, if a task that was temporarily inhibited is required again, its reactivation will be impaired; that is, a task's reactivation/ execution is delayed until previously applied inhibition decays or is overcome. Mayr and Keele (2000) speculated, and consequently found, that a behavioural cost (i.e., slower RTs, lower accuracy) is incurred when returning to a task from n–2 trial (i.e., as seen in ABA compared to CBA sequences), because that task is less accessible being under the influence of inhibition. Since the n–2 task repetition cost was observed back in year 2000, it has been replicated many times, and until recently it has been resistant to non-inhibitory explanations, making it a popular measure of cognitive inhibition in task-switching (Kiesel et al., 2010; Koch et al., 2010).

However, new evidence emerged which questions the usefulness of the n–2 task repetition cost as a measure of inhibitory control (Kowalczyk & Grange, 2017; Chapter 2), and the extent to which the n–2 task repetition cost reflects cognitive inhibition (Grange, Kowalczyk, & Loughlin, 2017; Chapter 3). Specifically, the n–2 task repetition cost recently has been shown not to be reliable as a measure of individual differences in cognitive inhibition, and has been demonstrated to be modulated considerably by an effect known as episodic retrieval (Grange et al., 2017; Neill, 1997)—a memory related process; as a consequence, the n–2 task repetition cost has been deemed a confounded measure of cognitive control. The evidence of episodic retrieval modulating the n–2 task repetition cost is of interest to the current study, but before explaining how and why, it is useful to look at how performance priming effects influence day-to-day life, and why it is relevant to task-switching research.

Priming in Task-Switching

Human behaviour is shaped by learning through positive as well as negative experiences, and practice. With enough exposure to new environmental factors and their pairing with action, behaviours can become automatic (Logan, 1985, 1988). This is because it is assumed that the associations between environmental factors and action are stored in memory and retrieved when necessary, making our actions more efficient (Hommel, 2000, 2004, Logan, 1985, 1988). For example, it is through learnt associations between stimuli and actions that we learn how and when to use objects (e.g., a pen, fork), keep away from dangerous stimuli (e.g., a hot stove), put letters into words and sentences, ride a bicycle, or drive a car. Priming, which is a subconscious memory process, can facilitate or impair our behaviour through automating activation of declarative and procedural memories upon seeing a given stimulus (e.g., keeping a safe distance from a hot stove). This unconscious memory effect is an important factor in human behaviour because in general, it can make us more efficient responding to and interacting with our environment as well as other humans (e.g., Bargh, Chen, &

Burrows, 1996; Doyen, Klein, Pichon, & Cleeremans, 2012). Positive priming is when behaviour is facilitated through previous exposure to stimuli and action associated (e.g., reading), whereas negative priming is a slower behaviour due to previous exposure to stimuli (e.g., slower reaction to previously ignored stimuli) (Tipper, 1985; Tulving & Schacter, 1990).

Task-switching performance can be sensitive to priming effects because it involves repetitive exposure to pairing of stimuli and responses leading to positive as well as negative priming (e.g., tasks' shared perceptual features and/ or responses may become inhibited if not relevant in a given moment) (Allport & Wylie, 1999, 2000; Altmann, 2011; Grange & Houghton, 2010a; Hommel, 1998, 2004; Horoufchin et al., 2011; Koch & Allport, 2006; Logan & Bundesen, 2003; Neill, 2007; Schmidt & Liefooghe, 2016; D. W. Schneider & Logan, 2005; Waszak et al., 2003; Woodward & Meier, 2003). For instance, there is evidence (Waszak et al., 2003) that if a presented stimulus was previously relevant for another task, task-switching performance is less efficient—as measured by a larger switch cost, an effect of slower RTs switching compared to repeating a task—relative to when a stimulus was not previously relevant for another task. This was taken as evidence that processing of stimuli associated with other tasks leads to interference due to competing stimuli' associations, which has support in other studies (Druey, 2014; Schmidt & Liefooghe, 2016). Responses in task-switching also can be primed (Gade & Koch, 2007b; Schmidt & Liefooghe, 2016); for example, evidence suggests that if response keys overlap across different tasks (i.e., in bivalent tasks, where a task can afford more than one response category), n–2 task repetition costs are larger compared to when responses do not overlap (i.e., in univalent tasks, where a task can afford one response category). Moreover, cues used in task-switching can also be influenced by priming effects (Logan & Schneider, 2006b; Mayr & Kliegl, 2003); for instance, if two cues per task are used where some cue-pairs are semantically associated within a given task (e.g., parity task, cues: king-queen; magnitude task, cues: salt-pepper), associated between tasks (e.g., parity task, cue: king-salt; magnitude task, cues: queen-pepper), or are semantically unassociated (e.g., parity task, cues: king-salt; magnitude task, cue: night-verb), performance is the most efficient for task-repetitions under semantically or associatively primed cues. In terms of whether a cue or a task repeats or alternates, performance was the fastest for cues repetitions, and the slowest for cues alternations. Together these findings were taken as supporting evidence for the view that positive and negative priming effects—specifically, cue-encoding priming—play an important role in successful task-switching.

Episodic retrieval. Findings from research into priming effects in general, are very relevant to task-switching as shown in previous paragraph; further evidence for this comes from research investigating a memory mechanism known as episodic retrieval—thought to be behind negative priming (Neill, 1997)—which became particularly important for the interpretation of the n–2 task repetition cost as it will be demonstrated in the next paragraphs.

Episodic retrieval can be described as an operation by which the most recent exposure to a given stimulus (i.e., declarative and procedural memories; for example, task's parameters such as stimulus' features, distractors, response made) is retrieved automatically upon seeing that stimulus again, either facilitating or impairing performance. It has been speculated and supported experimentally (Hommel, 1998, 2004, 2009), that performing a task results in this task's parameters being integrated in the form of a memory trace (also known as an event-file), which can be retrieved as triggered by being exposed to the same stimulus again (i.e., a retrieval of the most recent episode). If upon seeing a given stimulus a retrieved response does not match the currently required action, by the episodic retrieval account, it should lead to impaired performance because a new response will have to be generated against the one that was initiated automatically through episodic retrieval; however, if the retrieved task's parameters match the demands of the current task, performance should be facilitated (Hommel, 1998, 2000, 2004).

In task-switching research in general, of interest is whether across trials tasks repeat or not, and typically, what constitutes a task repetition is whether the same task and the same instructions are presented. What has not been considered an issue in task-switching is whether, when a task repeats all its parameters repeat too, which by episodic retrieval account should be relevant. If one of the tasks is to make a parity judgment (i.e., is presented number odd or even?), the first time participant performs this task, they may see a digit "4" for which they have to press "Z" key, but when that task repeats, they may see digit "7" for which they have to press "M" key. In this scenario, the task and instructions that go with it repeat across trials, but the outcome is different. This was problematic for interpreting of the n–2 task repetition cost because, when in ABA sequences the current task A repeats from n–2 trial (e.g., parity-magnitude-parity) but the response on the current trial is different than on n–2 trial (e.g., odd-smaller-even), we no longer can tell if the observation of slower RTs and lower accuracy on ABA compared to CBA sequence, is due to performing a previously inhibited task, or due to mismatches of retrieved responses for the repeated task.

Some of the priming effects seen in task-switching have been already linked to episodic retrieval mechanisms (Altmann, 2011; Grange & Houghton, 2010a; Horoufchin et al., 2011; Logan & Schneider, 2006b; Mayr & Kliegl, 2003; Schmidt & Liefooghe, 2016; Waszak et al., 2003). In relation to the n–2 task repetition cost, unlike in negative priming—another inhibitory mechanism which has been shown to be well explained by episodic retrieval (Neill, 1997; Tipper, 2001)—initial investigation of episodic retrieval effects on the n–2 task repetition cost showed that it could not be attributed to, or modulated by episodic retrieval account (Mayr, 2002). In a specially prepared paradigm (Figure 6.1), in which tasks' features can be easily manipulated, Mayr showed that when episodic retrieval was controlled for, there was a numerical reduction in n–2 task repetition costs for episodic matches compared to episodic mismatches; however, episodic retrieval account did not explain the n–2 task repetition cost, as statistically the n–2 task repetition cost was not different for n–2 Response Repetitions and n–2 Response Switches. This was taken as evidence that the n–2 task repetition cost is a result of persisting inhibition rather than episodic retrieval mismatches. However, since Mayr's work was published, further studies have been reported which do not support Mayr's findings (Grange et al., 2017; Chapter 3). In a series of three experiments, Grange and colleagues showed that n–2 task repetition costs were considerably reduced under n–2 Response Repetitions (i.e., episodic matches) compared to n–2 task repetition costs under n–2 Response Switch (i.e., episodic mismatches). As such, Grange and colleagues illustrated that the n–2 task repetition cost—typically thought to be a measure of cognitive inhibition—is inflated by episodic retrieval mismatches, which has been further supported by experiments conducted in Chapters 4-6 in of the current thesis. The current investigation will explore the episodic retrieval modulation of the n–2 task repetition cost further using alternative to standard analysis methods used in task-switching research.

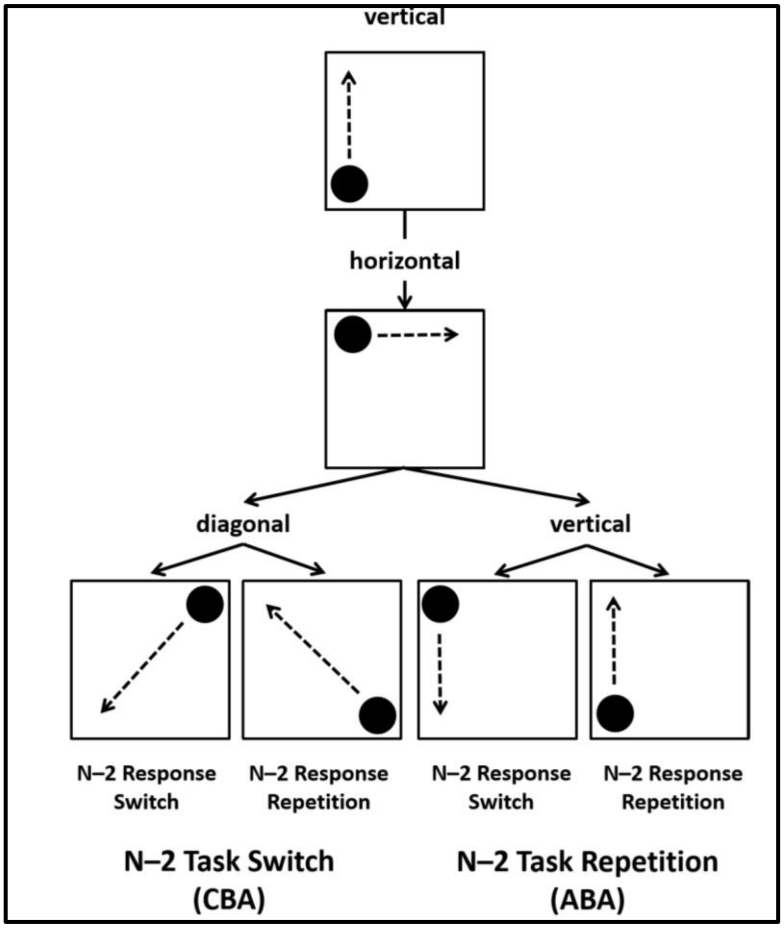


Figure 6.1 Schematic overview of Mayr's (2002) task-switching paradigm. The arrows indicate correct answers and they were used only for instruction purposes. Tasks are spatial transformations (i.e., horizontal, vertical, diagonal) of a circle.

Reaction Time Analysis

When looking at RT data—including in task-switching research—it is very common to use measures of central tendencies (e.g., mean RTs and accuracy) and a dispersion parameter (e.g., standard deviation) as a reflection of performance on cognitive tasks; that is, central tendencies measures are used as a summarised reflection of performance. However, using central tendencies as an analysis input can be problematic or at least not optimally informative (Whelan, 2008). For example, data are often not normally distributed (i.e., they are positively skewed and need to be trimmed or transformed (or both) before they can be analysed; therefore, potentially informative data points are often removed which can make mean outputs misleading and obscuring valuable information about data (e.g., two conditions with the same mean performance may have different distributions which can be meaningful) (Balota & Yap, 2011; Heathcote et al., 1991; Hervey et al., 2006). Moreover, mean RT performance can also overestimate experimental effects (Verbruggen, Chambers, & Logan, 2013).

Ex-Gaussian. Mental chronometry—a study of the processing of nervous system via, for example RT and accuracy—is a very popular method used in experimental psychology to explore mental processes (Jensen, 2006; Meyer, Osman, Irwin, & Yantis, 1988); however, the full exploratory potential of these measures is not used. As mentioned previously, mean RTs and accuracy are commonly chosen as a suitable reflection of data; however, instead, the right tail of the positively skewed RT distribution can be more informative. For example, it has been noted that in people with diminished intelligence the right tail of the distribution is much flatter compared to individuals with an average intelligence, which was speculated to be due to neuronal oscillations, attentional or motivational differences (Baumeister, 1998). According to the ‘worst performance’ theory—that the worst performance trials (i.e., trials at the right tail of distribution) are more predictive of general intelligence (*g*) than best performance trials (for a review see Coyle, 2003)—smaller number of worst performance trials seen in people with higher general intelligence (hence shorter distribution right tail compared to people with lower intelligence), are indicative of less working memory (WM) lapses, and efficient cognitive control.

As an alternative to central tendencies analyses, whole RT distributions can be investigated incorporating the positive skew of the distribution using an ex-Gaussian—a convolution of normal and exponential distribution (Luce, 1986)—modelling of RTs (Ratcliff, 1993). The ex-Gaussian analysis is more informative than looking at central tendencies in data because rather than producing outcomes of average performance, ex-Gaussian analysis characterises RTs distributions with three parameters: μ (*mu*-mean of the normal distribution), δ (*sigma*-standard deviation of normal distribution), and τ (*tau*-the mean of exponential distribution, the long right tail of the distribution) (Burbeck & Luce, 1982). The three ex-Gaussian parameters are estimated from raw data to establish whether and which parameters differ significantly between experimental conditions. Then, the differences in parameters among experimental conditions and groups can be linked to different cognitive processes (Hervey et al., 2006; Schmiedek et al., 2007), although some would argue that ex-Gaussian parameters’ interpretation has no strong theoretical grounds (Matzke & Wagenmakers, 2009). Among the three ex-Gaussian parameters, the one that is being depicted as most meaningful is the tau parameter—

which characterises the long right tail of distributions, where worse performance trials are seen—and it has been associated with cognitive inhibition (Grange & Houghton, 2011; Schuch & Konrad, 2017; Spieler, Balota, & Faust, 1996), tasks-conflict processing (Shahar & Meiran, 2014; Steinhauser & Hübner, 2009), WM (Schmiedek et al., 2007; Shahar, Teodorescu, Usher, Pereg, & Meiran, 2014; Tse, Balota, Yap, Duchek, & McCabe, 2010), intelligence (Baumeister, 1998), and attentional lapses (Hervey et al., 2006). In terms of the Gaussian parameters, larger mu values have been linked to response criterion (Spieler, 2001) and response conflicts (Steinhauser & Hübner, 2009); that is, in general response related processes.

Regardless of the view that individual ex-Gaussian parameters cannot be confidently mapped on to different cognitive processes (Matzke & Wagenmakers, 2009), ex-Gaussian analyses can be useful as a descriptive method for examining experimental effects associated with higher cognition. For example, assuming that tau can reflect WM lapses and task conflicts, in task-switching it would mean that this parameter can be sensitive to processing conflicts due to inhibition and episodic retrieval, which both can. Also, ex-Gaussian modelling can be extended by theoretically stronger methods such as using computational models of how—rather than whether—performance differs between conditions, and what give rise to these differences (Schmiedek et al., 2007; Spieler, 2001; Voss, Nagler, & Lerche, 2013).

Computational modelling. Assuming that cognition can be studied through mental chronometry and that we understand basics of what cognitive and perceptual processes generate RTs (Jensen, 2006; Medina, Wong, Diaz, & Colonius, 2015; Posner, 2005; Simmons, Wass, Thomas, & Riley, 2002), models of cognitive mechanisms giving rise to observed data can be designed and tested. Specifically, with the use of what is known as computational modelling, theories about cognitive processes can be simplified and translated into mathematical equations or computational algorithms—based on abstract assumptions made about cognition—and parameters describing performance can be analysed and put into simulation to re-create observed data. These models then can be used to test or generate hypotheses, rather than just describe data.

Of interest to the current study is one particular computational method, known as diffusion modelling (Ratcliff, 1978; Voss, Nagler, et al., 2013). This computational model assumes that in two-choice reaction time tasks (e.g., Odd vs Even), upon seeing a stimulus, the made decision is a result of a noisy information accumulation process. Specifically, the following is assumed: The presented stimulus is encoded and compared in parallel fashion with information held in memory; information for this comparison accumulates gradually in what is referred to as a random walk process (i.e., diffusion process, which is assumed to resemble firing neuronal populations), towards either of the responses. The response that is executed is the one for which accumulated evidence reaches decision criterion first (e.g., which response' threshold is reached faster) (Figure 6.2).

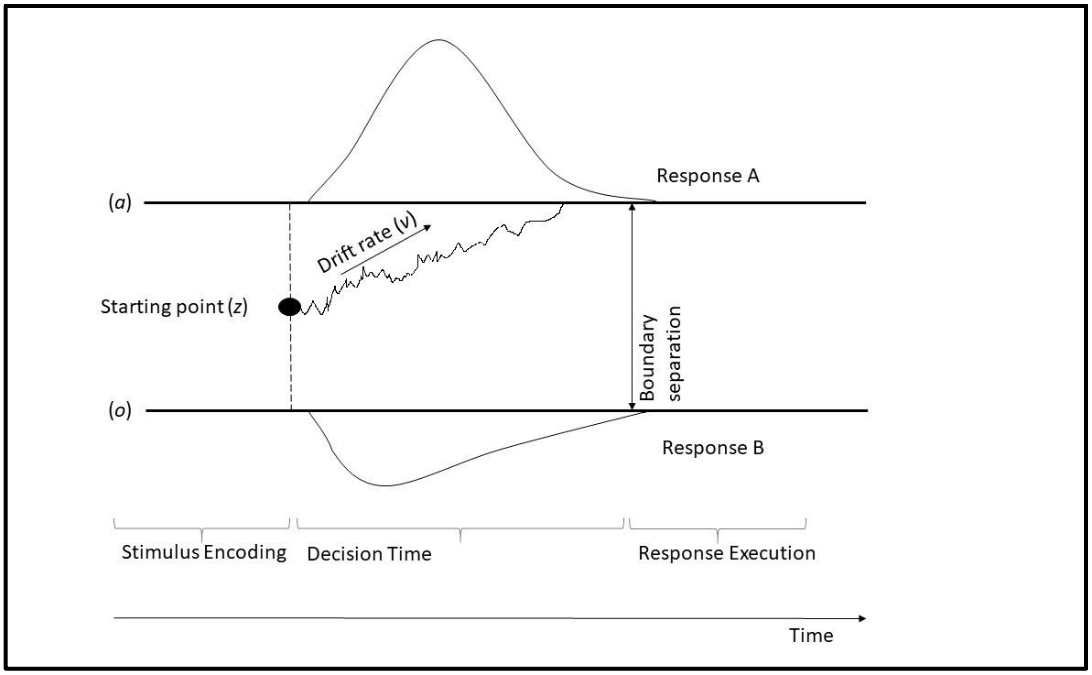


Figure 6.2 Schematic overview of Ratliff's diffusion model. At point z information accumulation process starts; slope v reflects the drift rate, when an upper (a) or lower (0) threshold are hit, the decision is made. Outcomes can vary from trial to trial because they result from a random walk process. Examples of decision-time distributions are depicted outside the decision time (top, and bottom of the graph).

In diffusion modelling, correct as well as incorrect RTs—unlike other methods that only use correct responses (e.g., ex-Gaussian), or look at RT and accuracy separately—are modelled for each participant and condition, by estimating diffusion modelling parameters (Ratcliff & McKoon, 2008; Voss, Nagler, et al., 2013; Voss & Voss, 2007). Each parameter reflects distinct processing mechanism and the basic diffusion modelling parameters are, drift rate (*v*), threshold separation (*a*), starting point (*z*), and non-decisional time (t_0).

The drift rate is meant to reflect the rate (i.e., strength and quality) at which information is accumulated, it has been linked to task difficulty (e.g., difficult tasks being characterised with smaller drift rates), and speed of processing (e.g., higher drift rates linked to faster processing, Schmiedek et al., 2007). The smaller the drift rate, the longer is the information accumulation, and the slower the RT; also, under smaller drift rates the probabilities of either of the two responses being made are similar, leading to more errors. The larger the drift rate, the faster the information accumulation, and the faster the RTs are; that is, the larger the drift rates the higher the chance that the correct answer is reached sooner. Also, drift rates are sensitive to carry-over effects (e.g., interference), and as such, slower information accumulation can be speculated to be due to, for example inhibition.

The threshold separation parameter represents decision criterion boundaries (i.e., the width between decisions' thresholds); that is, the amount of information accumulated needed to make a decision. Larger threshold separation parameters are linked to a conservative decision style—more information is accumulated before a response is selected (i.e., slow but accurate performance)—and smaller threshold separation parameters are linked to liberal decision style (i.e., less information is accumulated before decision is made; fast but less accurate performance). This parameter has been shown to be sensitive to age-related slowing, instructions given to participants, and speed-accuracy trade-off (Voss et al., 2004).

The starting point is a parameter that is relative to separation thresholds (i.e., threshold boundary to which the starting point is closer to is the more likely decision to be reached) and is meant to reflect where between decision boundaries, information accumulation begins; this parameter can be adjusted to introduce prior biases (e.g., proportion of responses: 40:60 Left vs Right key presses). The non-decisional parameter reflects the cognitive processes related to information encoding, working memory configuration, and response execution.

The diffusion modelling has become an important method to bring together investigation of underlying components of processing and RT data. A good model should predict mean performance (i.e., RTs, accuracy), RTs distributions, and changes in dependent variables across experimental conditions (Ratcliff & McKoon, 2008). Diffusion modelling of two-choice reaction tasks is relatively easy to implement, as ready software packages are available. Also, the diffusion model’s parameters can be mapped on to different cognitive processes more confidently compared to ex-Gaussian method, because each diffusion parameter links to a specific cognitive mechanism (Matzke & Wagenmakers, 2009). Moreover, specific theories can be tested because differences in performance across experimental conditions can be speculated to be due to different stages of processing such as, information accumulation, response criterions, or delayed motoric response. Therefore, diffusion modelling parameters are considered less noisy—that is, they are framed around specific cognitive mechanisms—and are more informative than central tendencies measures or ex-Gaussian analysis (Voss, Nagler, et al., 2013; Voss, Rothermund, Gast, & Wentura, 2013; White, Ratcliff, Vasey, & McKoon, 2010).

Task-Switching and Computational Modelling

Despite the advantages of diffusion modelling (and ex-Gaussian analysis) over central tendencies analyses, not many studies have adopted this method for investigation of task-switching (Schmitz & Voss, 2012, 2014; Weeda et al., 2014); however, within this limited literature some findings are consistent.

In task-switching research, the non-decisional parameter is associated with stimulus encoding, preparation, and response execution; also, it seems to be task-specific (as it does not change during cue-switches vs. cue-repetitions), and it tends to be larger for task-switches compared to task-repetitions, as well as for informative cues compared to non-informative cues (Schmitz & Voss, 2012, 2014). Moreover, in children the non-decisional parameter decreases with age showing that children' encoding and response-related processes become more efficient over time (Weeda et al., 2014).

The drift rate parameter seems to be sensitive to carry-over effects and tends to be larger for task-repetitions compared to task-switches, as well as for short cue-stimulus-interval (CSI) compared to long CSI. Also, drift rates seem to be influenced by task predictability; that is, higher drift rates are observed for predictable compared to unpredictable task-switches (Schmitz & Voss, 2012, 2014); that is, under unpredictable task sequences, information accumulation is less efficient compared to predictable sequences where information is accumulated faster. In children, drift rates increase with age, which suggests that as children get older the information accumulation becomes more efficient; moreover, for task-repetitions only, drift rates have been shown to be smaller during initial task-repetitions, and to increase with more repetitions (Weeda et al., 2014). These observations are taken as evidence that higher drift rates (i.e., reflecting more efficient information accumulation) indicate task readiness, with higher rates characterising more prepared responses relative to lower drift rates associated with unprepared tasks.

In terms of threshold separation, it has been shown to reflect response cautiousness which can change on a trial-to-trial basis depending on task predictability; specifically, threshold separation tends to be larger for task-switches compared to task-repetitions, indicating that for the former one responses are more cautious (Schmitz & Voss, 2012, 2014).

The n–2 task repetition cost. As the current thesis was being conducted, a couple of studies utilising diffusion modelling to investigate the n–2 task repetition cost were reported. The most recent study (Schuch & Konrad, 2017) investigated whether and how inhibition (as measured by n–2 task repetition cost) differs between children (9-11 years old) and adults (21-30 years old). The main behavioural prediction of this study was that children would show smaller n–2 task repetition costs than adults, because cognitive inhibition is thought not to be developed in children as well compared to adults. In terms of computational modelling, it was speculated that inhibition, as measured with the n–2 task repetition cost, would be reflected in the drift rate, which was predicted to be smaller in ABA than CBA sequences. This is because drift rates are sensitive to carry-over effects and as such, out of the all diffusion modelling parameters, the drift rate is most likely to be affected by interference from previous trials, caused for example by inhibition.

The results showed that the behavioural n–2 task repetition cost was similar in size in children and adults; however, the diffusion modelling showed that adults and children differed in the magnitude of drift rates. Specifically, the n–2 task repetition cost was present in drift rates for adults but not for children, who instead showed the n–2 task repetition cost in the non-decisional, and to some extent in threshold separation parameters. The main conclusion taken from this study was that despite no differences in behavioural data between children and adults found, diffusion modelling can distinguish contrasts in how information was processed in these two age groups (e.g., differences seen in information accumulation). It seems that inhibition, as measured with the n–2 task repetition cost, was reflected in drift rates for adults and in the non-decisional parameter for children, indicating that adults' performance was affected by carry-over effects (e.g., inhibition) and children's performance was likely to depend on preparatory processes (i.e., they could not prepare tasks quickly enough, a skill which in another study has been shown to improve with age, Weeda *et al.*, 2014).

Schuch and Konrad (2017) used also ex-Gaussian modelling to examine RTs distributions; the main finding from that analysis was that n–2 task repetition costs were present in tau for adults and mu in children. The former finding fits in with the observation that n–2 task repetition costs are most pronounced at slow RTs (Grange & Houghton, 2011), and in tau (Grange & Juvina, 2015; Koch, Frings, & Schuch, 2017). The reports of larger n–2 task repetition costs at slower RTs compared to smaller n–2 task repetition costs at fast RTs, have been interpreted as evidence that fast RTs represent prepared responses (hence smaller n–2 task repetition costs) whereas slow RTs reflect unprepared responses (hence larger n–2 task repetition costs), which overall has been taken as proof that inhibition can be overcome with preparation (DeJong, 2000a; Grange & Houghton, 2011). In general, Schuch and Konrad study’s findings were consistent with the literature (Schmiedek et al., 2007; Spieler, 2001; Steinhauser & Hübner, 2009).

Another study (Schuch, 2016) investigated inhibitory mechanisms between older (64-79 years old) and younger adults (18-26 years old), looking at task and response inhibition. Task inhibition was measured with the n–2 task repetition cost, and response inhibition was assessed with the n–1 response repetition cost. Responses repeated from n–1 trial are in general slower compared to n–1 responses switch, an effect which is believed to be due to response inhibition (Grzyb & Hübner, 2012, 2013; Koch, Schuch, Vu, & Proctor, 2011; Verbruggen et al., 2005); that is, upon selectin a given response in the context of one task, that action is inhibited hence less accessible if needed for the subsequent task. The standard analysis of variance of mean performance (i.e., RTs and accuracy) showed that overall, older adults were slower and more accurate than younger adults, and n–2 task repetition costs were present in younger and older adults, but they did not differ significantly between those groups. The diffusion modelling analysis revealed that slower and more accurate performance in older adults was due to larger non-decision time, variability of non-decision, and threshold separation; importantly, the n–2 task repetition cost was present in drift rates but they did not differ significantly between the two age groups. The results for response inhibition analysis showed no age differences (for the standard analysis of variance and diffusion modelling), and the response inhibition cost was reflected in non-decisional parameter for both age groups; specifically, the non-decisional parameter was larger for response repetitions compared to response switches. Together, these results were taken as evidence that in older adults, inhibitory processing is intact, and that older adults may engage in more task preparation compared to younger adults.

The Current Study

To summarise, task-switching research shows a lot of behavioural evidence suggesting that the n–2 task repetition cost reflects inhibition. When task-switching adult data are examined with ex-Gaussian analysis, the n–2 task repetition cost is consistently reflected in the tau parameter, and in diffusion modelling this cost is present in drift rates. Both, tau and drift rate parameters are associated with the exponential tail of the RT distribution; therefore, assuming that in task-switching slower RT are due to inhibition, the following can be speculated. The presence of the n–2 task repetition cost in drift rates is evidence that this cost is likely to be due to cognitive inhibition, because slower information accumulation on ABA compared to CBA sequences can reflect persisting inhibition in ABA sequence. When it comes to ex-Gaussian results, the n–2 task repetition cost in tau—assuming that it reflects unprepared RTs, lapses in WM—can be interpreted as evidence that the RT are less ready at slower RTs because of persisting inhibition in ABA compared to CBA sequences. Also, in children who are assumed to have underdeveloped cognitive inhibition—hence inhibition would not be expected to be seen as a carry-over effect—n–2 task repetition costs are present in non-decisional and mu parameters. Together, these findings support the view that the n–2 task repetition cost represents cognitive control processes, which in children develop at later age. However, in the view of new evidence suggesting that the n–2 task repetition cost is inflated by episodic retrieval mismatches, it became necessary to investigate whether the n–2 task repetition cost—when controlled for episodic retrieval mismatches—continues to be reflected in tau in ex-Gaussian analysis and drift rate in diffusion modelling.

Since ex-Gaussian modelling is an effective method to describe data, and diffusion modelling offers more insight into specific cognitive processes (e.g., information accumulation, response criterions, or delayed motoric response related mechanisms), the current study used ex-Gaussian and diffusion modelling to re-investigate the n–2 task repetition cost considering modulating effects of episodic retrieval. Data from three already-conducted studies, and one study run specifically for this investigation were analysed. There are no reports of such analyses being conducted for the n–2 task repetition cost which is controlled for episodic retrieval; therefore, this will be the first investigation of this type.

Main Predictions

For the standard n–2 task repetition cost (i.e., not controlling for episodic retrieval), it is predicted that in all data sets used in the current investigation, n–2 task repetition costs will be present in tau for ex-Gaussian analysis, and in drift rate for diffusion modelling, as it has been shown already in existing literature. The tau parameter is expected to be larger for ABA compared to CBA sequence. In diffusion modelling, the n–2 task repetition cost is expected to be present in drift rate; smaller in ABA than CBA, with smaller drift rates indicating slower information accumulation over time, in this case speculated to be due to persisting inhibition. For the n–2 task repetition cost which has been controlled for episodic mismatches, predictions are different than for the standard n–2 task repetition cost. There does not seem to be much literature to guide hypotheses on whether and how episodic retrieval matches compared to mismatches can manifest in diffusion modelling; therefore, related predictions are less clear.

Diffusion modelling. Existing behavioural evidence suggests that the standard n–2 task repetition costs are due to inhibition, as reflected in the drift rate parameter which is sensitive to carry-over effects and interference. However, under the episodic retrieval hypothesis, n–2 task repetition costs are largely due to episodic mismatches; therefore, it is uncertain whether and how episodic retrieval affects information accumulation. With the reports that lower drift rates are linked to negative, competitor and associative priming, task-switches, random task sequence, and short cue-stimulus (CSI) interval (Schmitz & Voss, 2012; Voss, Rothermund, et al., 2013; Weeda et al., 2014), it can be speculated that under episodic matches, the n–2 task repetition cost should not be present in the drift rate parameter. This is because, episodic matches, unlike episodic mismatches, do not lead to interference, which typically is reflected in the drift rate parameter, but rather facilitate performance leading to faster information accumulation. Moreover, the drift rate has been shown to increase with task subsequent repetitions and practice (Weeda et al., 2014; Weigard & Huang-Pollock, 2014); this means that as tasks are practiced and repeated, the information accumulation improves over time. Together, these observations can be interpreted as an indication that the drift rate is likely to reflect relative task readiness, with lower drift rates reflecting uncompleted preparation or response selection difficulty; therefore, the n–2 task repetition cost under episodic mismatches should be characterised with lower drift rates compared to the n–2 task repetition cost under episodic matches which should be characterised by higher drift rates.

The non-decisional parameter has been linked to task-set reconfiguration, cue encoding, categorical priming, longer motor response, and longer retrieval (Voss, Rothermund, et al., 2013; Weeda et al., 2014). The n–2 task repetition cost is not expected to be present in the non-decisional parameter, because the current study is not looking at age differences, and encoding is assumed to be similar across tasks used; however, if under episodic mismatches information is retrieved less efficiently (e.g., due to longer retrieval) or if motor response is slower, compared to episodic matches, effects in the non-decisional parameter may be present.

The threshold separation tends to be larger for task-switches compared to task-repetitions, and this is thought to be because large boundary reflects more information needed before decision is made, hence more cautious decision (Schmitz & Voss, 2012; Weeda et al., 2014); so, for example, if participants cannot predict the upcoming task, they will be more cautious with their responses compared to if task-switches are predictable. Also, this parameter does not seem to change with practice (Weigard & Huang-Pollock, 2014). Also, participants in all four studies from which data were used in the current investigation, were told that both, accuracy and speed, are equally important. Moreover, n–2 task repetition costs tend to be present in accuracy as well as speed. As such, the n–2 task repetition cost is not expected to be seen in threshold separation; unless, participants are more cautious performing ABA compared to CBA sequences, or more cautious under episodic mismatches compared to episodic matches.

Overall, the main hypothesis for the current investigation is that, if the n–2 task repetition cost is present in the drift rate—after controlling for episodic retrieval—it will be evidence that this cost is driven by unprepared responses due to carry-over effects (e.g., cognitive inhibition), or difficulty in decision selection (i.e., interference). However, if the n–2 task repetition cost is no longer present in the drift rate—for episodic matches compared to episodic mismatches—it will be evidence that the n–2 task repetition cost is not driven by carry-over effects, such as inhibition. Also, if the n–2 task repetition cost is present in non-decisional parameter (and/ or it differs between episodic mismatches compared to episodic matches), it can be interpreted as evidence that there is a difference in stimuli encoding, and/or longer retrieval. There is no expectation that the n–2 task repetition cost will be present in threshold separation, unless participants are particularly more cautious in some conditions over others.

Ex-Gaussian modelling. For the ex-Gaussian analysis, typically the n–2 task repetition cost is predominantly present in tau; therefore, in the current study it was expected to be the case for the standard n–2 task repetition cost too. In previous studies, such observation was interpreted as evidence that the n–2 task repetition cost is due to inhibition (Grange & Houghton, 2011; Schuch & Konrad, 2017; Spieler et al., 1996); moreover, the tau parameter has been also linked to tasks-conflict processing (Shahar & Meiran, 2014; Steinhauser & Hübner, 2009), WM (Schmiedek et al., 2007; Shahar et al., 2014; Tse et al., 2010). With the tau parameter in general being associated with executive functions, and episodic retrieval being an automatic mechanism, tau should not be affected by episodic retrieval as such. Instead, the Gaussian parameters (i.e., mu and sigma) are more likely to be affected by episodic retrieval, as these parameters are linked to response conflict (Steinhauser & Hübner, 2009), which can be generated by mismatching response being retrieved. Schuch and Konrad's (2017) study reported some differences between ABA and CBA in mu and sigma parameters, results which were deemed limited to make conclusions; however, larger mu parameter values for ABA compared to CBA can be linked to non-decision time from diffusion modelling. If episodic retrieval affects response criterion (Spieler & Balota, 1996)—that is, whether participants are conservative or liberal with how much information is needed before response is made—tau could be affected by episodic retrieval. Specifically, episodic mismatches may lead to more cautious responses compared to episodic matches; in which case, tau would be larger for ABA than CBA under episodic mismatches compared to episodic matches. Assuming that episodic matches facilitate performance as a form of priming mechanism, mu and sigma parameters should be smaller for episodic matches compared to episodic mismatches, which would reflect less noisy and more efficient response selection. That is, episodic matches, unlike episodic mismatches, lead to more consistent performance, because there is less interference and conflict between retrieved and required responses.

Modelling Parameters and WM Correlations. The WM study (Chapter 4; Experiment 2)—one of the studies from which data were used in the current investigations—looked at individual differences in WM capacity (WMC) as a predictor of inhibition, as measured by the n–2 task repetition cost. Specifically, it was predicted that people with higher WMC would have larger n–2 task repetition costs; this is because, the higher the WMC, the more resources are available for deployment of inhibition; this is assuming that inhibition is resource dependent (Conway & Engle, 1994; Conway et al., 1999). The WM study's results showed that n–2 task repetition costs (i.e., standard and controlled for episodic retrieval) and WMC were not related. These results were interpreted as evidence that inhibition—as measured with n–2 task repetition cost—is not resource dependent. Having measures of WMC for every participant from WM study, it can be explored whether ex-Gaussian and diffusion modelling parameters are associated with WMC. For example there is evidence that the ex-Gaussian tau and diffusion modelling drift rate are linked strongly to WM (Schmiedek et al., 2007). Specifically, tau was negatively correlated with WM, and drift rate was positively correlated with WM; that is, in the latent factors analysis of ex-Gaussian and diffusion modelling parameters, higher tau values were linked to lower WM, and higher drift rates were linked to higher WM. For the relationship between tau and WM is further supported by other studies (Heathcote et al., 1991; Shahar et al., 2014).

Since tau and drift rate are linked to WM, the current study set out—as an additional check—whether in the WM study data, the mentioned parameters are also associated with WM. Individual ex-Gaussian and diffusion modelling parameters', as well as n–2 task repetition costs' for those parameters relationship with WM will be looked at. Based on existing literature, it was predicted that tau would be linked negatively and the drift rate positively to WM.

Method

Simulation Studies: Data Information

Four data sets were used to examine the n–2 task repetition cost with ex-Gaussian and diffusion modelling. Data were obtained from Chapter 4 (Experiment 5) for Stimulation Study 1, Chapter 3 (Experiment 2) for Simulation Study 2, data collected in the lab for another study (Grange & Kowalczyk, 2017) for Simulation Study 3, and data collected especially for Simulation Study 4. For details on individual studies see relevant chapters and journal papers; apart from Simulation Study 4, for which details are included in this report. In experiments from which data were used in Simulation Studies, participants performed 480 trials (Simulation Studies 1–3) and 960 trials (Simulation Study 4).

Participants. Data of forty-two participants were used for Simulation Study 1 (mean age 19.17; SD 1.55; thirty-six females), seventy-six for Simulation Study 2 (mean age 21.63; SD 6.43; fifty-eight females), twenty-nine for Simulation Study 3 (age range 18–25), and forty-four for Simulation 4 (mean age of 19.52; SD 3.59; thirty-five women).

Data trimming: standard ANOVA. For ANOVA analyses (RT and accuracy), all four data sets were trimmed in the same way. For accuracy analysis, null trials were removed (i.e., trials that could not be classified as ABA or CBA); two trials at the beginning of each experimental block, and two trials following each error. Also, participants had to have an overall accuracy at 90% to be included in further analyses.

For RT analysis, for each participant's data, and each experimental design cell (i.e., ABA, CBA), RTs faster than 150ms or slower than 2.5 standard deviations above participant's mean were removed. The DVs (i.e., dependent variables) were accuracy and RTs, and IVs (i.e., independent variables) were Task Sequence (ABA vs CBA sequence) and Response (n–2 Response Repetition vs. n–2 Response Switch; for the purpose of results reporting these will be referred to as n–2 RR vs n–2 RS).

Data trimming: ex-Gaussian. For ex-Gaussian modelling correct RTs distributions from all four data sets were used. Data used for ex-Gaussian modelling were free from null trials (i.e., first two trials of each experimental block and two trials following errors) as well as error trials; however, RTs were not trimmed for slow and fast responses. It was important to not trim RT data and instead to use the whole RT distribution because the exponential tail of the distribution—characterised by tau in the ex-Gaussian modelling—is thought to reflect extreme but meaningful scores rather than just outliers (Heathcote et al., 1991). Three ex-Gaussian parameters (i.e., mu, sigma, tau) were estimated, used for modelling of RT data for the standard as well as the controlled n–2 task repetition costs (i.e., controlled for episodic retrieval), and analysed via 2-way ANOVAs.

Data trimming: diffusion modelling. For diffusion modelling purposes, data were trimmed differently than for the standard analysis and ex-Gaussian modelling; that is, the null trials were removed (i.e., two first trials from each block, two trials following an error), but also RTs faster than 200ms and slower than 4 standard deviations from the participant's mean, per cell of the experimental design, were removed. Outliers can negatively influence diffusion modelling parameter estimation (Luce, 1986; Voss, Nagler, et al., 2013; Voss, Rothermund, et al., 2013; Voss, Voss, & Lerche, 2015; Whelan, 2008); responses faster than 200ms are considered to be most problematic because they are thought to be guesses or a result of processes difficult to identify, therefore are not included in the modelling. Slow RTs—though less problematic for diffusion modelling than fast RTs—also have to be trimmed. In general, slow RTs are informative, reflecting slower processing (i.e., decision making) and response execution; however, typically, RT slower than 5sec are considered outliers (Voss, Nagler, et al., 2013). Nevertheless, the trimming criteria can differ between studies (Schmiedek et al., 2007; Schuch, 2016; Schuch & Konrad, 2017; Weeda et al., 2014; Weigard & Huang-Pollock, 2014). With the evidence that removing outliers does not necessarily lead to a better parameter estimation (Lerche, Voss, & Nagler, 2016), and since the parameters estimation method used in the current study (i.e., KS, Kolmogorov-Smirnov method) has been shown to be robust against outliers (Voss & Voss, 2008), the chosen trimming criteria for the current study were considered as suitable. Error trials were kept in the data set; this is because in diffusion modelling correct as well as error RTs are used to determine the cognitive processes giving rise to particular patterns in data (Ratcliff & McKoon, 2008; Voss et al., 2004; Wagenmakers, van der Maas, & Grasman, 2007).

Simulation Studies: Modelling Parameters Estimation

The modelling began with estimating modelling parameters for ex-Gaussian and diffusion modelling; see details below. Next the estimated parameters were used to simulate new data sets, of which distributions are graphed and analyse to show how well the parameters can reflect the observed data.

Ex-Gaussian. The ‘timefit’ function of the ‘retimes’ package (Massidda, 2015) in R software (R Developement Core Team, 2018) was used to estimate ex-Gaussian parameters, and the ‘rexgauss’ function (Massidda, 2015) was used to simulate new data.

The ex-Gaussian data models’ fitness was examined with density and cumulative distribution functions (i.e., CDFs) graphs. CDFs were used as an additional check for how accurately modelled data can be fitted to the observed data, because CDFs offer detailed insight into patterns in data through examining performance across the whole RT distribution at various points. Moreover, in task-switching literature CDFs have been used to inform us about prepared and unprepared trials under the *failure to prepare* theory (DeJong, 2000a; Grange & Houghton, 2011); where reduction in n–2 task repetition costs at fast RTs is thought to reflect well prepared responses, whereas larger n–2 task repetition costs at slow RTs are believed to reflect unprepared responses. This reduction of n–2 task repetition costs at fast RTs is seen as evidence that inhibition (as measured with the n–2 task repetition cost) can be reduced with preparation.

To create CDFs graphs, for each data set RTs were sorted from the fastest to the slowest, and then CDFs were obtained by calculating RT quantiles—at 0.1, 0.3, 0.5, 0.7, 0.9 points—for each participant and condition (i.e., ABA and CBA). For modelled data, RTs were sorted from the fastest to the slowest, and RT quantiles were calculated for

ABA and CBA sequences. Next, mean quantiles RTs were calculated for observed data as well as modelled data and graphed.

Diffusion modelling. Diffusion modelling parameters—for the standard n–2 task repetition cost and the n–2 task repetition cost controlled for episodic retrieval—were estimated using fast-dm programme (Voss & Voss, 2007); within that programme, parameters were optimised with KS criterion. The KS criterion is a statistic which informs a researcher on how well modelled data fit the observed data; the KS statistic is a *p* value, and the higher the KS value is the better the model fit is (KS < 0.05 denoting a poor model fit). Using estimated diffusion modelling parameters' (a, v, t_0) mean values for every condition, new data were simulated using fast-dm 'construct-samples' function.

Results

The results will start with reporting how well estimated parameters reproduced the observed data in ex-Gaussian and in diffusion modelling.

Ex-Gaussian: Model Fit

Standard n–2 task repetition cost. Overall, ex-Gaussian parameters estimated for RTs on ABA and CBA sequences reflected observed data well, as seen through RT simulated data based on ex-Gaussian parameters (Figure 6.3); apart from Young Adults data for which the model fit was not optimal.

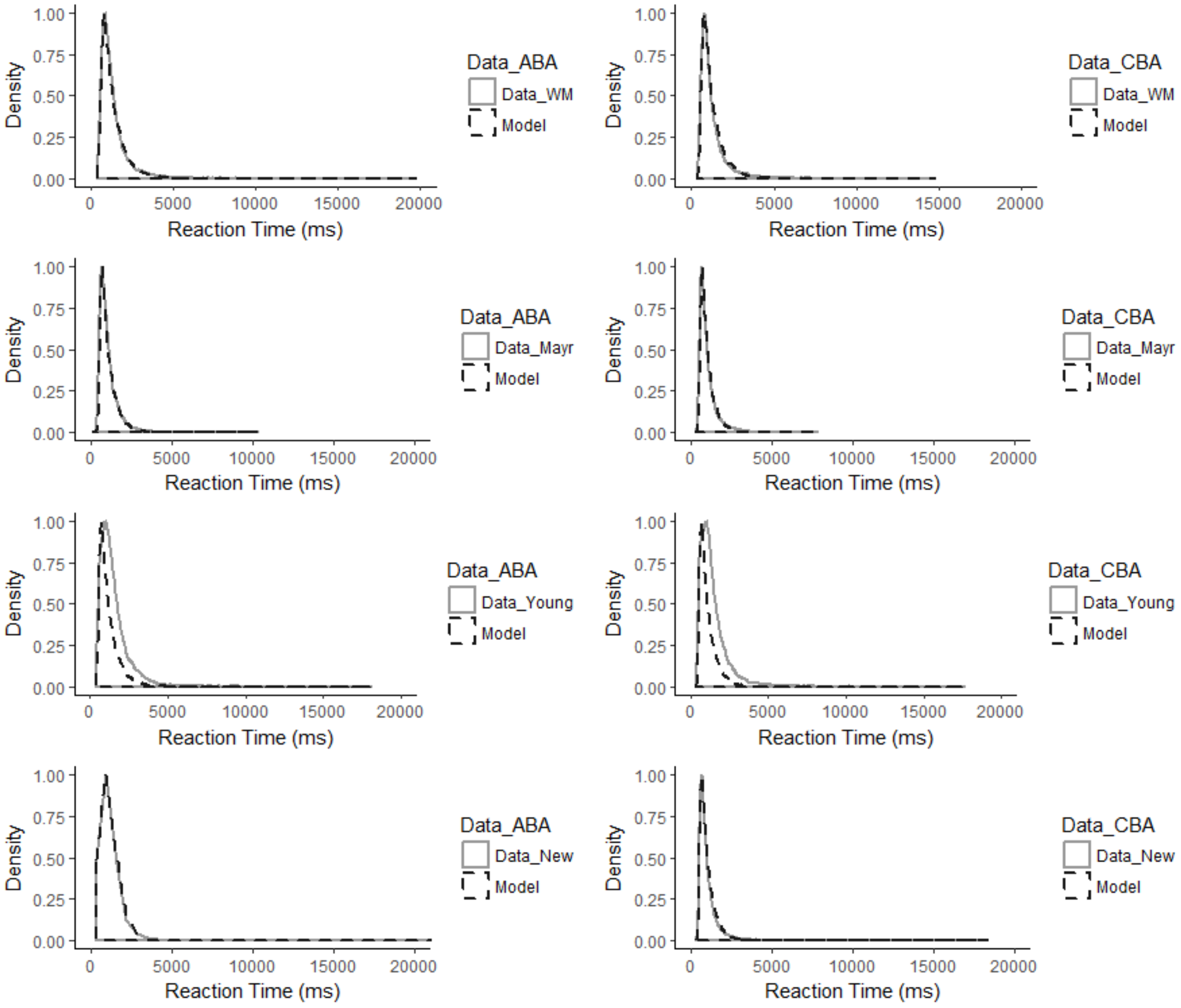


Figure 6.3 Density functions graphs for observed and ex-Gaussian modelled RTs (i.e., Reaction Times) for ABA (on the left) and CBA (on the right) sequences, for Stimulation 1 with WM data, Simulation 2 with Mayr Replication data, Simulation 3 with Young people data, and Simulation 4 with New Data set.

As another check for how well the estimated parameters reflected observed data CDF graphs were produced; Figure 6.4 shows that overall, the modelled data were similar to the observed data, as reflected with overlapping average RTs at quantile points. However, for Young Adults and New Data the fit was not ideal.

It was also of interest whether the n–2 task repetition cost increased with slower RTs (Figure 6.4); to investigate that, ANOVAs were performed on observed data for quantile RTs means as DVs, and Quantiles (i.e., quantile points) and Task Sequence (ABA vs. CBA) as IVs. For WM data, the interaction between Task Sequence and Quantiles was significant, *F* (1, 40) = 4.62, *p* = .04, η_g^2 = .005; that is, the magnitude of the n–2 task repetition cost increased significantly across quantile points. Specifically, the n–2 task repetition cost was the smallest at 0.1 quantile (38ms) and gradually increased with slower RTs reaching 106ms at 0.9 quantile, which was confirmed by the linear trend characterising Task Sequence and Quantiles interaction, *F* (1, 3) = 26.9, *p* = .01. For Mayr Replication data the interaction between Task Sequence and Quantiles was also significant, *F* (1, 75) = 29.39, *p* < .0001, η_g^2 =.01. The magnitude of the n–2 task repetition cost increased gradually with slower RTs, with the smallest cost at 0.1 quantile (28ms), and the largest at 0.9 quantile (111ms), as confirmed by the linear trend for Task Sequence and Quantiles interaction which was significant, *F* (1, 3) = 34.4, *p* = .01. For Young Adults data Task Sequence and Quantiles interacted significantly too, *F*(1,28) = 13.47, *p* < .01, η_g^2 =.006. The n–2 task repetition cost increased with slower RTs from 25ms at 0.1 quantile, to 137ms at 0.9 quantile, which was confirmed by a significant linear trend, *F* (1, 3) = 25.2, *p* = .02. And lastly, for New Data the n–2 task repetition cost increased with slower RTs, as Task Sequence and Quantiles interacted significantly, *F*(1,43) = 27.49, *p* < .001, η_g^2 =.02, which was confirmed by the linear trend of this relationship, *F*(1, 3) = 29.6, *p* = .01.

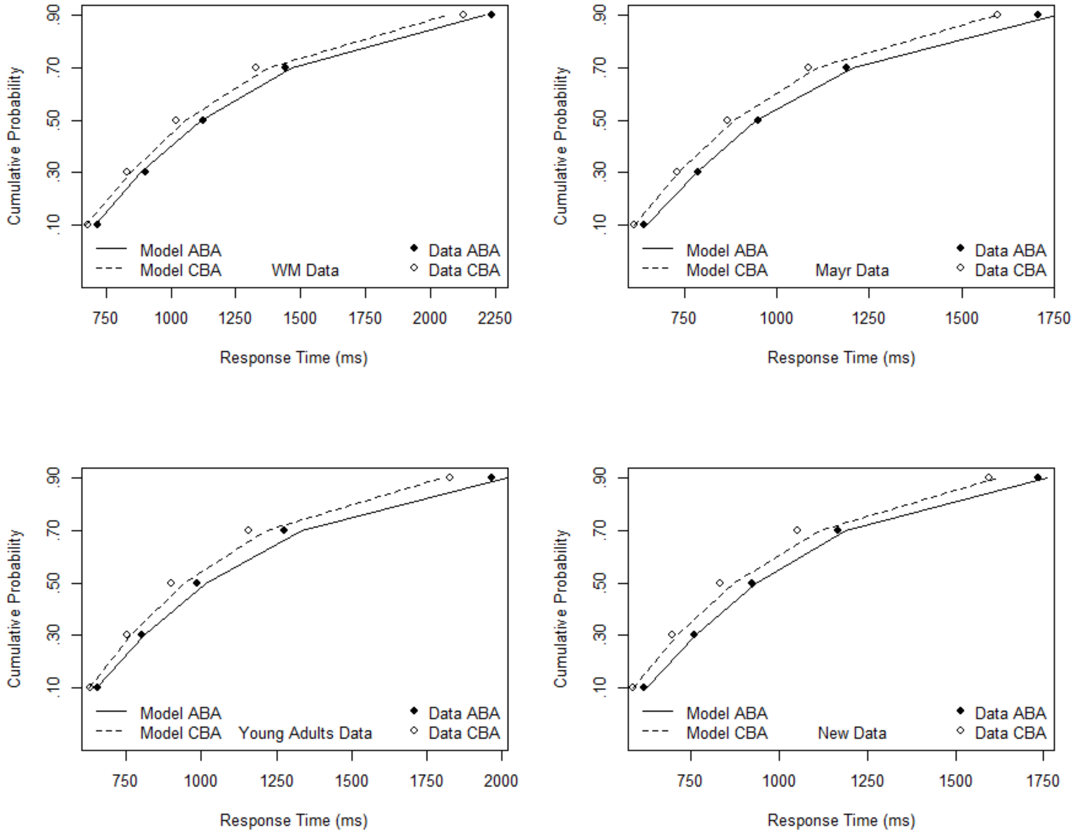


Figure 6.4 Cumulative distribution functions for observed and ex-Gaussian simulated data from Simulation Study 1 (WM Data), Simulation Study 2 (Mayr Replication Data), Simulation Study 3 (Young Adults Data), and Simulation Study 4 (New Data). Data depicted in the graphs are for cumulative functions for ABA and CBA sequences; the lines represent modelled data (dashed: model for ABA; solid: model for CBA) and the dots represent observed data (black dots: ABA; white circles: CBA sequences). Correct RTs only included. Correct RTs only included.

Controlled n–2 task repetition cost. Ex-Gaussian parameters were estimated for the n–2 task repetition cost controlling for episodic retrieval; that is, ex-Gaussian parameters were estimated for ABA and CBA sequences under episodic matches (n–2 RR) and episodic mismatches (n–2 RS). Next using estimated parameters new data were simulated for every data set, and examined for how well simulated data fit the observed data. As it can be seen in Figures 6.5–6.6, all simulated data fitted the observed data well. The CDFs for four simulated and observed data sets show (Figure 6.7) that ex-Gaussian estimated parameters for ABA/ CBA under n–2 RR/ n–2 RS reflect observed data well.

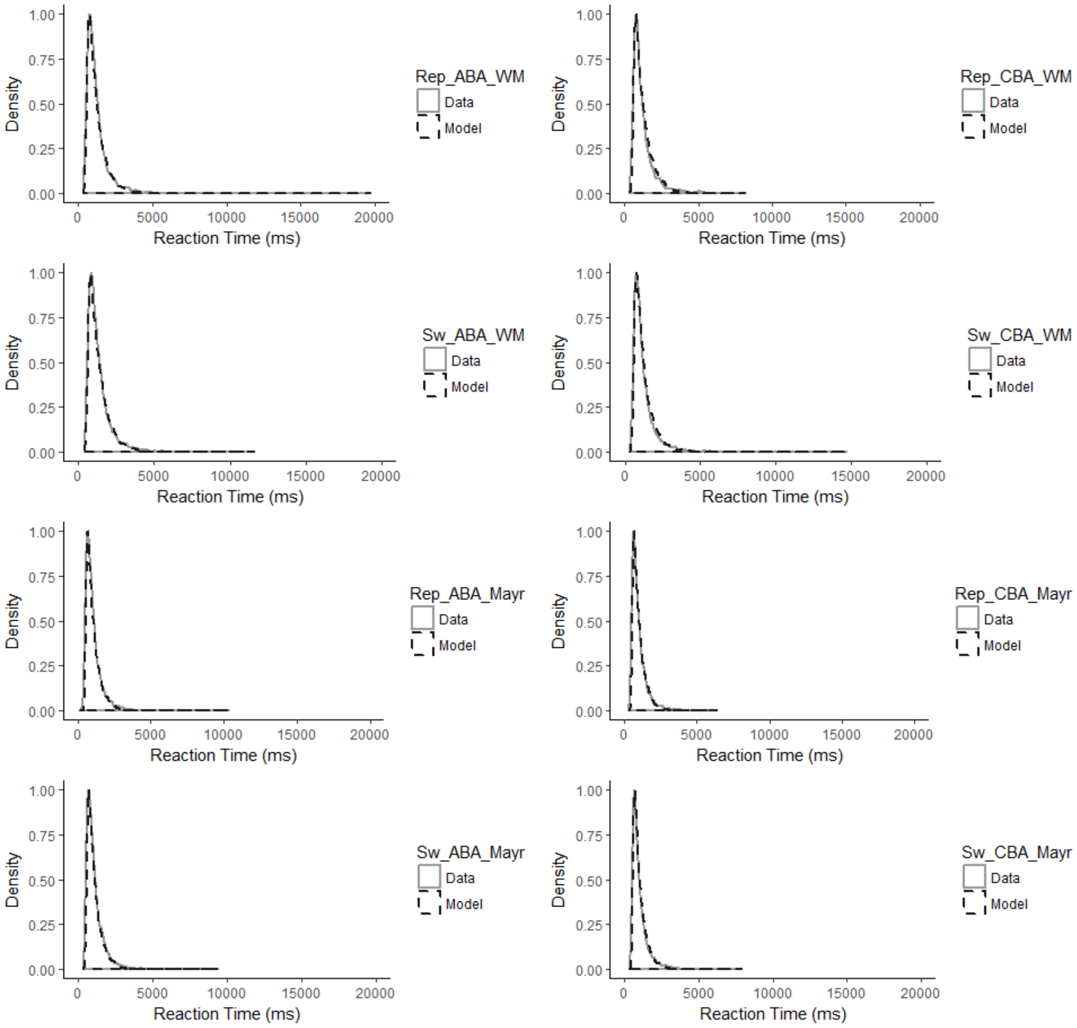


Figure 6.5 Density functions graphs for observed and ex-Gaussian modelled data points (i.e., Reaction Times) for ABA (on the left) and CBA (on the right) sequences, under n–2 Response Repetitions (Rep) and n–2 Response Switch (Sw) for Stimulation 1 with WM data and Simulation 2 with Mayr Replication data.

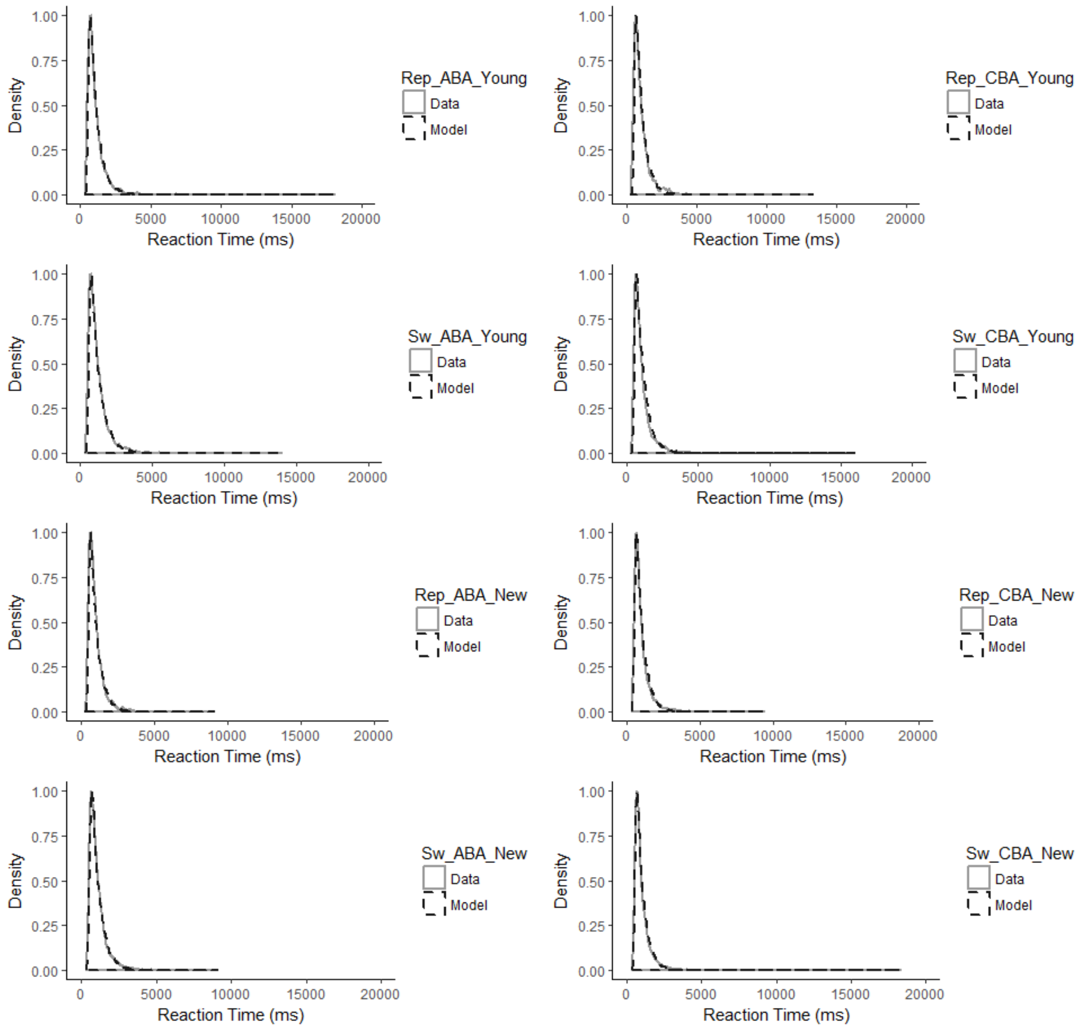
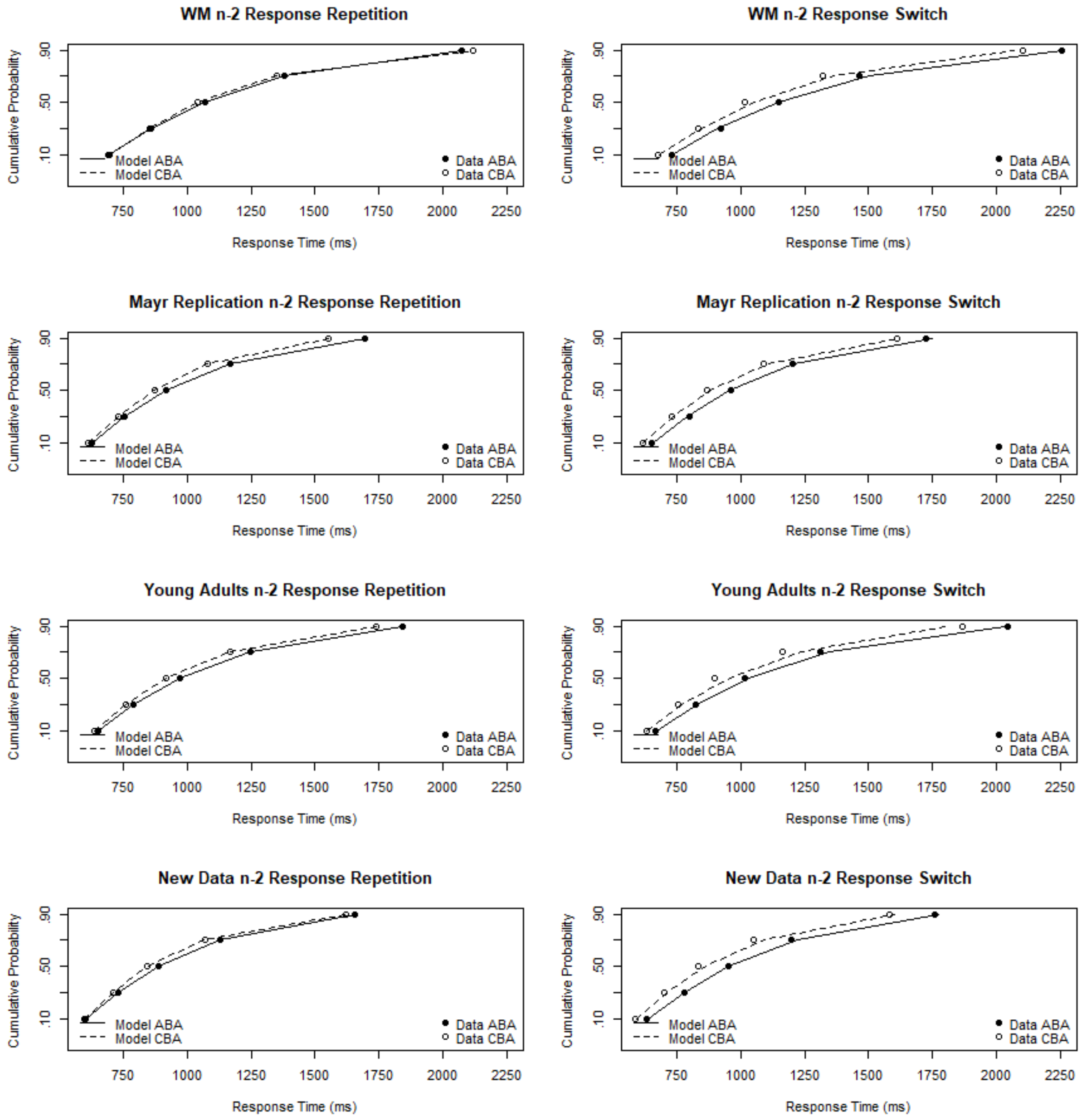


Figure 6.6 Density functions graphs for observed and ex-Gaussian modelled data points (i.e., Reaction Times) for ABA (on the left) and CBA (on the right) sequences, under n–2 Response Repetitions (Rep) and n–2 Response Switch (Sw) for Stimulation 3 with Young Adults data and Simulation 4 with New Data.

Looking at whether the n–2 task repetition cost increased with slower RTs for n–2 RR and n–2 RS in WM data, it was found that Task Sequence did not interact significantly with Quantiles, F (4, 160) = 1.53, p = .20, η_g^2 =.001; that is, the n–2 task repetition cost did not increase with slower responses, which was the case for n–2 RR and n–RS, as evidenced by non-significant interaction between Task Sequence, Response and Quantiles, *F* (4, 160) = 1.89, *p* = .11, η^2_g =.001. For Mayr Replication data, Task Sequence interacted significantly with Quantiles, *F* (4, 300) = 12.57, *p* = .001, η^2_g =.003; the linear trend for the n–2 task repetition cost to increase with slower RTs was significant, *F* (1, 3) = 33.50, *p* = .01. However, the interaction between Task Sequence, Response and Quantiles was not significant, *F* (4, 300) = 0.85, *p* = .49, η^2_g =.001.

In Young Adults data Task Sequence did not interact significantly with Quantiles, *F* (4, 112) = 2.29, *p* = .06, η^2_g =.003. The interaction between Task Sequence, Response and Quantiles was not significant, *F* (4, 112) = 0.08, *p* = .08, η^2_g =.001. This means that for Young Adults' data n–2 task repetition cost did not increase with slower RTs.

In New Data the interaction between Task Sequence and Quantiles was significant, *F* (4,172) = 8.10, *p* < .0001, η^2_g =.002 (ABA, CBA); showing the n–2 task repetition cost increased with slower RTs, *F* (1, 3) = 29.20, *p* = .01. The three-way interaction between Task Sequence, Response, and Quantiles was also significant, *F* (4, 172) = 5.70, *p* < .0001, η^2_g < .001. This was followed by two separate ANOVAs; for n–2 RR, the interaction between Task Sequence and Quantiles was not significant, *F* (1, 43) = 0.51, *p* = .92, η^2_g < .001; but for n–2 RS Task Sequence and Quantiles was significant, *F* (1, 43) = 24.01, *p* < .001, η^2_g = .02; the linear trend for n–2 task repetition cost to increase with slow RTs was significant, *F* (1, 43) = 31.70, *p* = .01


WM n-2 Response Repetition

WM n-2 Response Switch

Mayr Replication n-2 Response Repetition

Mayr Replication n-2 Response Switch

Young Adults n-2 Response Repetition

Young Adults n-2 Response Switch

New Data n-2 Response Repetition

New Data n-2 Response Switch

Figure 6.7 Cumulative distribution functions for observed and modelled mean RTs from Simulation Studies 1–4, for ABA and CBA sequences under n–2 Response Repetitions (on the left) and n–2 Response Switches (on the right); the lines represent modelled data (dashed: model for ABA; solid: model for CBA) and the dots represent observed data (black dots: ABA; white circles: CBA sequences). Correct RTs only included.

Diffusion Modelling: Model Fit

Standard n–2 task repetition cost. Diffusion modelling parameters based data fitted well observed data in all four data sets (Figure 6.8). The KS criterion for WM data for diffusion modelling parameters ranged .57–1; for Mayr Replication data .15–1; and for Young Adults data .08–.50. For New Data the KS criterion ranged from .0002–.99; that is, for four out of 44 participants parameters were not estimated optimally, and these participants' data were removed from further analysis.

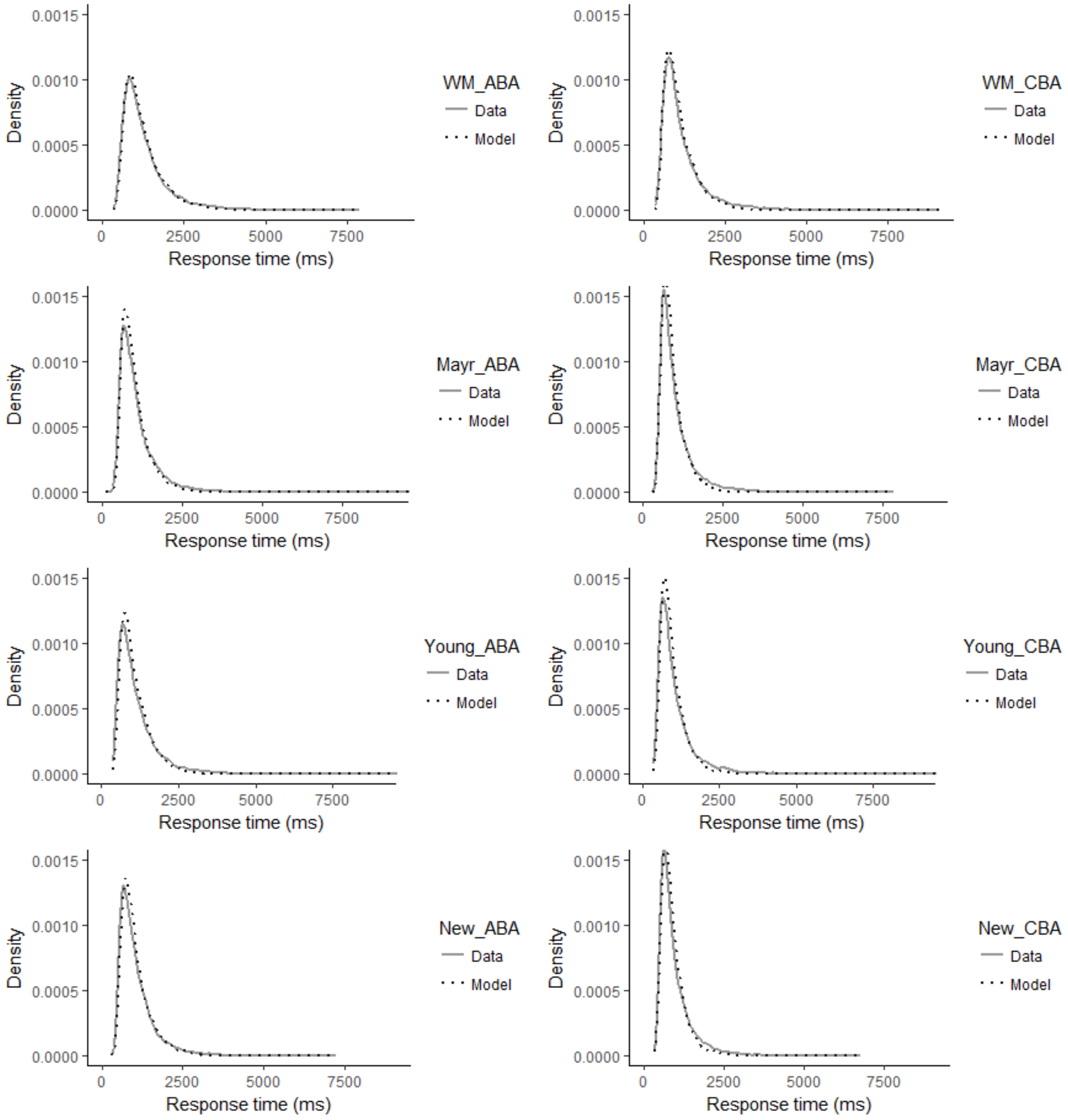


Figure 6.8 Correct RTs distributions densities for four data sets (i.e., Data)—for WM, Mayr Replication, Young Adults, New Data—and diffusion modelling data (i.e., Model); ABA (the left panel) and CBA (the right panel).

Controlled n–2 task repetition cost. The *p* value for KS statistic—for all participants across all conditions in WM data (i.e., ABA and CBA under n–2 RR and n–2 RS)—ranged .11–1. For Mayr Replication data estimated diffusion parameters the KS statistic ranged .35–1, and for Young Adults it ranged .05–.99. For New Data the KS statistics ranged from .0005 to 1; for two out of 44 participants the *p* value KS statistic was significant, which means that parameters for these models were not well estimated. Two participants for whom *p* values for KS statistic were significant were removed from further analysis. Those two participants were the same two (out of four) whose data were not used in diffusion modelling analysis for the standard–2 task repetition cost. Overall, data simulated on estimated diffusion modelling parameters fitted observed data well (Figures 6.9–6.10).

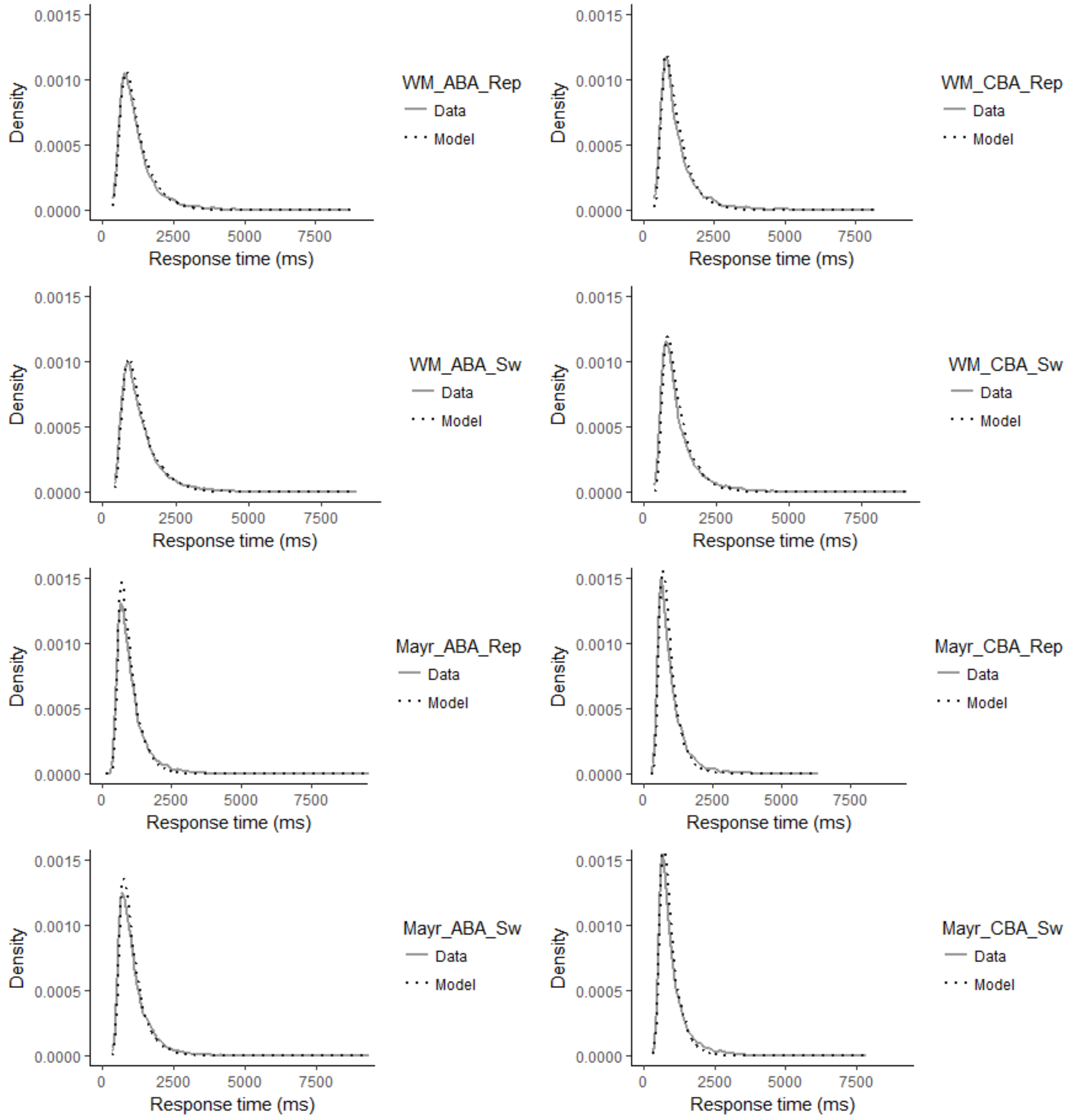


Figure 6.9 Correct RTs distributions densities for two data sets (i.e., Data)—for WM and Mayr Replication—and diffusion modelling data (i.e., Model); ABA (the left panel) and CBA (the right panel) under n–2 Response Repetition (Rep) and n–2 Response Switch (Sw).

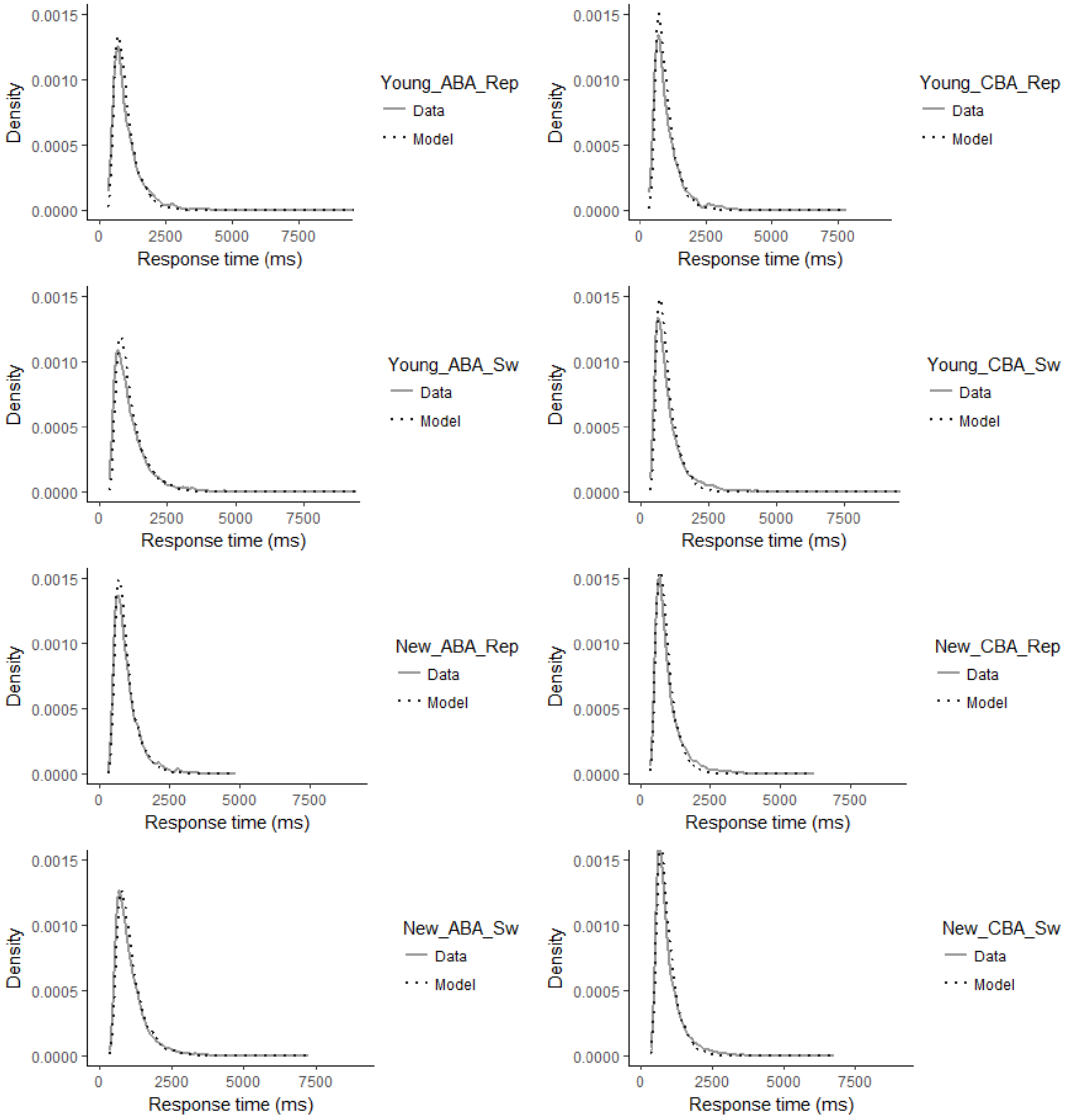


Figure 6.10 Correct RTs distributions densities for two data sets (i.e., Data)—for Young Adults and New Data—and diffusion modelling data (i.e., Model); ABA (the left panel) and CBA (the right panel) under n–2 Response Repetition (Rep) and n–2 Response Switch (Sw).

ANOVAs

Having ensured that ex-Gaussian and diffusion modelling parameters reflect observed data well, the parameters were put through a series of ANOVAs to examine if and which parameters are marked with n–2 task repetition costs and whether these costs are modulated by episodic retrieval. Before modelling parameters' ANOVAs are reported, ANOVAs on RT and accuracy from all four data sets are summarised.

RT performance. All ANOVAs results are provided in Table 6.1. The n–2 task repetition cost was present in RT performance in all four data sets; that is, the main effect of Task Sequence was significant (i.e., difference between ABA vs. CBA sequence). The n–2 task repetition cost was modulated by episodic retrieval (Figure 6.11); that is, Task Sequence interacted with Response, as the n–2 task repetition cost was consistently present and larger under episodic mismatches (i.e., n–2 RS) compared to episodic matches (i.e., n–2 RR) where the cost was present only in Mayr Replication data. Specifically, in WM data the n–2 task repetition cost was not significant for n–2 RR, *t* (40) = 0.03, *p* = 1.0, 95%CI [-51.09, 53.67], however, it was significant for n–2 RS, *t* (40) = 8.6, *p* < .001, 95%CI [87.44, 141.95]. In Mayr Replication data, the n–2 task repetition cost was significant for n–2 RR, *t* (75) = 4.20 *p* < .0001, 95%CI [25.21, 70.37], as well as for n–2 RS, *t* (75) = 13.00, *p* < .0001, 95%CI [72.94, 98.56]. In Young Adults data, Individual t–tests revealed that, the difference between ABA (1365ms) and CBA (1329ms) was not significant for n–2 RR, t (28) = 0.99, p = 0.3, 95%CI [-32.00, 91.97], however it was significant for n–2 RS, t (28) = 9.0, p < .0001, 95%CI [89.2, 141.7]. And in New Data the n–2 task repetition cost was not significant for n–2 RR, *t*

(43) = 0.98, *p* = 0.30, 95%CI [-14.79, 42.68], however it was significant for n–2 RS, *t* (43) = 11.0, *p* < .0001, 95%CI [88.11, 127.10].

Table 6.1 Results for ANOVAS on RT and accuracy performance for WM (1), Mayr Replication (2), Young Adults (3), and New Data (4), with Task Sequence and Response as IVs.

		RT and accuracy ANOVAs								
		Main effect of Task Sequence			Main effect of Response			Interaction: Task Sequence x Response		
		F(1,40)	*p*	η_g^2	*F*(1,40)	*p*	η_g^2	*F*(1,40)	*p*	η_g^2
1	RT	12.30	<.01	.01	13.17	<.001	.004	21.66	<.001	.01
	Error Rates	4.04	.051	.01	5.97	.02	.02	5.90	.02	.02
2	RT	94.14	<.001	.02	18.22	<.001	.004	9.60	<.01	.002
	Error Rates	3.36	.07	.006	12.17	<.001	.02	1.32	.25	.003
3	RT	15.67	<.001	.01	6.33	<.02	.0008	9.07	<.01	.005
	Error Rates	1.11	.30	.006	0.87	.36	.003	20.07	<.001	.09
4	RT	74.89	<.0001	.02	20.33	<.0001	.007	22.18	<.0001	.01
	Error Rates	19.30	<.001	.07	8.49	<.006	.02	24.63	<.001	.04

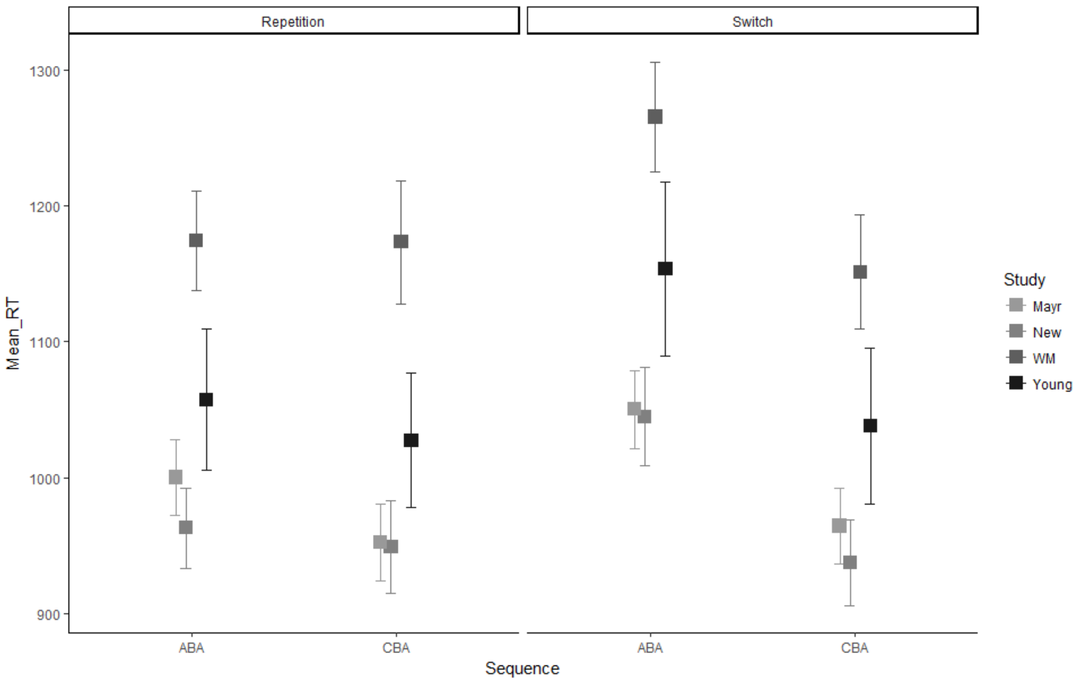


Figure 6.11 An illustration of a two-way interactions in RTs between Task Sequence (ABA vs. CBA) with Response (n–2 Response Repetition vs. n–2 Response Switch) for WM, Mayr Replication, Young Adults, and New Data. Error bars denote ±SE around the mean.

Accuracy performance. For statistical results from accuracy ANOVAs see Table 6.1. In accuracy, the n–2 task repetition cost was only present in New Data set; however, Task Sequence interacted significantly with Response in 3/4 data sets (Figure 6.12). Specifically, in WM data under n–2 RR there was no n–2 task repetition cost, *t* (40) = 0.15, *p* = .90, 95%CI [-0.007, 0.008] but the cost was present under n–2 RS, *t* (40) = -3.3, *p* = .002, 95%CI [-0.018, -0.004]. In Mayr Replication data, Task Sequence did not interact with Response. In Young Adults data set, under n–2 RR an n–2 task repetition benefit was present (ABA 97.14%, CBA 95.61%) and significant, *t* (28) = 2.2, p = .03, 95%CI [0.001, 0.03], and for n–2 RS an n–2 task repetition cost was present

(ABA, 94.76%; CBA, 97.25%) *t* (28) = -4.2, p < .001, 95%CI [-0.04, -0.01]. And in New Data set, for n–2 RR there was no significant n–2 task repetition cost, *t* (43) = -0.89, *p* = .40, 95%CI [-0.01, 0.004]; whereas for n–2 RS the cost was present, *t* (43) = -7.4, *p* < .0001, 95%CI [-0.02, -0.01].

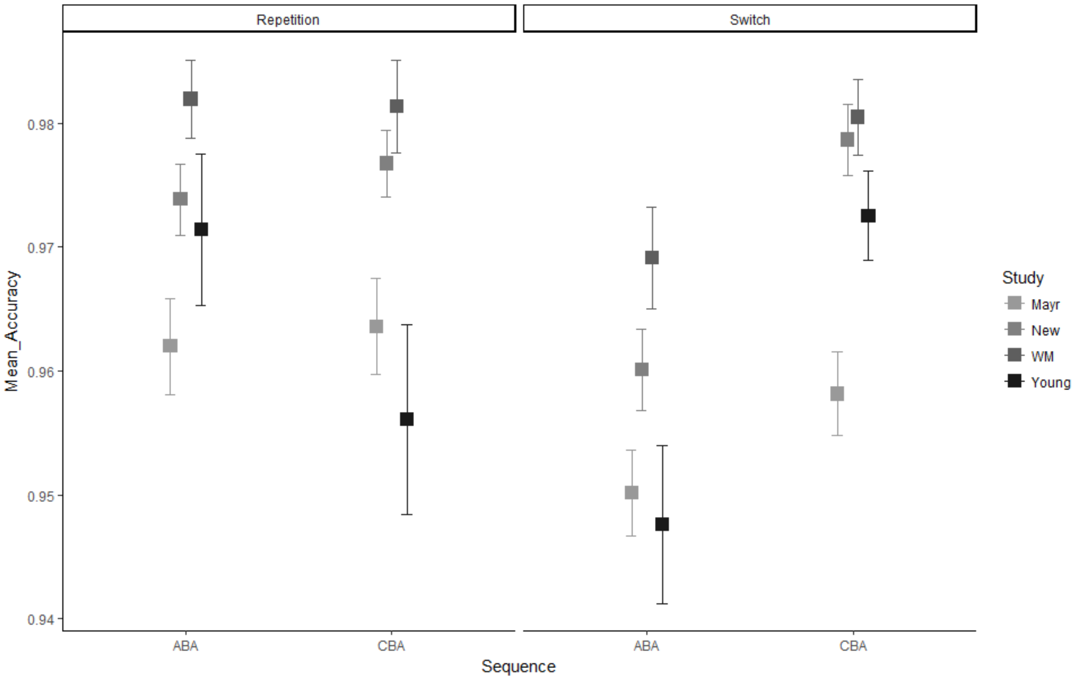


Figure 6.12 An illustration of a two-way interactions in accuracy between Task Sequence (ABA vs. CBA) with Response (n–2 Response Repetition vs. n–2 Response Switch) for WM, Mayr Replication, Young Adults, and New Data. Error bars denote ±SE around the mean.

Ex-Gaussian parameters. The standard n–2 task repetition cost was consistently present in mu, sigma, and tau parameters (apart from sigma parameter in the Young Adults data set; see Table 6.2). All three parameters were larger for ABA compared to CBA sequences (Figure 6.13).

Controlling for episodic retrieval analysing ex-Gaussian parameters produced somewhat mixed results. Overall, the n–2 task repetition cost remained in the mu and tau parameters, but for sigma parameter was present in 2/4 data sets (Table 6.2). In terms of the effect of Response on ex-Gaussian parameters, the results were very mixed and are difficult to interpret (Table 6.2; Figure 6.13).

For the mu parameter, there was a significant interaction between Task Sequence and Response for 3/4 data sets (Table 6.2); specifically, in WM data set, the n–2 task repetition cost was not present under n–2 RR, *t* (40) = 0.35, p = .70, 95%CI [-23.77, 33.79], but was present under n–2 RS,), *t* (40) = 5.1, p < .001, 95%CI [25.10, 58.45]. The results for Mayr Replication data were the same, with the n–2 task repetition cost under n–2 RS, *t* (75) = 8.0, p < .0001, 95%CI [31.28, 51.94], but absent under n–2 RR, *t* (75) = 1.1, p = .30, 95%CI [-6.00, 21.11]. For Young Adults data, there was no interaction between Task Sequence and Response. And in the New Data set, the n–2 task repetition cost was absent under n–2 RR, , *t* (43) = 1.4, p = .20, 95%CI [-3.91, 23.63], but present under n–2 RS, *t* (43) = 6.2, p < .0001, 95%CI [31.08, 61.04].

For the sigma parameter, in none of the data sets interaction between Task Sequence and Response was significant; and for the tau parameter in 2/4 data sets this interaction was significant. In the WM data set the n–2 task repetition cost was absent under n–2 RR, *t* (40) = -0.14, p = .9, 95%CI [-70.22, 61.35], but was present under n–2 RS, *t* (40) = 4.3, p < .001, 95%CI [35.50, 97.61]. In the Mayr Replication and Young Adults data sets, the interaction between Task Sequence and Response was not significant. In the New Data set, the n–2 task repetition cost was absent under n–2 RR, *t* (43) = -0.29, p = .80, 95%CI [-37.70, 28.29], but it was present under n–2 RS, *t* (43) = -5.5, p < .001, 95%CI [38.87, 84.53].

Table 6.2 Results for ANOVAS for ex-Gaussian parameters-mu, sigma, and tau—for WM (1), Mayr Replication (2), Young Adults (3), and New Data sets (4), with Task Sequence and Response as IVs.

		Ex-gaussian parameters ANOVAs								
		Main effect of Task Sequence			Main effect of Response			Interaction: Task Sequence x Response		
		F(1,40)	*p*	η_g^2	*F*(1,40)	*p*	η_g^2	*F*(1,40)	*p*	η_g^2
	Standard n–2 task repetition cost									
1										
	mu	25.14	<.001	.03						
	sigma	6.61	.01	.05						
	tau	9.39	.004	.008						
	Controlled n–2 task repetition cost									
	mu	7.42	<.01	.01	3.22	.08	.004	5.48	.02	.006
	sigma	2.06	.16	.02	4.73	.04	.02	0.06	.80	.001
	tau	2.61	.10	.003	0.86	.36	.0006	4.53	.04	.004
	Standard n–2 task repetition cost									
2										
	mu	48.64	<.0001	.02						
	sigma	17.39	<.001	.05						
	tau	34.65	<.0001	.01						
	Controlled n–2 task repetition cost									
	mu	30.72	<.001	.01	11.25	<.01	.007	17.12	<.001	.007
	sigma	8.75	<.01	.02	5.37	.02	.02	1.45	.23	.004
	tau	24.24	<.0001	.01	1.93	.17	.0009	0.01	.90	.0001
	Standard n–2 task repetition cost									
3	mu	6.23	<.02	.02						
	sigma	0.46	.50	.004						
	tau	21.31	<.001	.01						
	Controlled n–2 task repetition cost									
	mu	9.34	<.01	.01	0.04	.83	.004	3.35	.08	.004
	sigma	0.27	.61	.002	0.66	.42	.004	2.36	.14	.01
	tau	10.71	<.003	.008	6.03	.02	.007	2.02	.15	.001
	Standard n–2 task repetition cost									
4	mu	27.85	<.0001	.03						
	sigma	16.88	<.0001	.08						
	tau	35.16	<.0001	.02						
	Controlled n–2 task repetition cost									
	mu	20.43	<.0001	.02	9.66	<.01	.003	25.91	<.001	.01
	sigma	11.95	.001	.05	3.18	.08	.007	1.84	.18	.005
	tau	9.28	.004	.005	9.80	.003	.005	9.98	.003	<.007

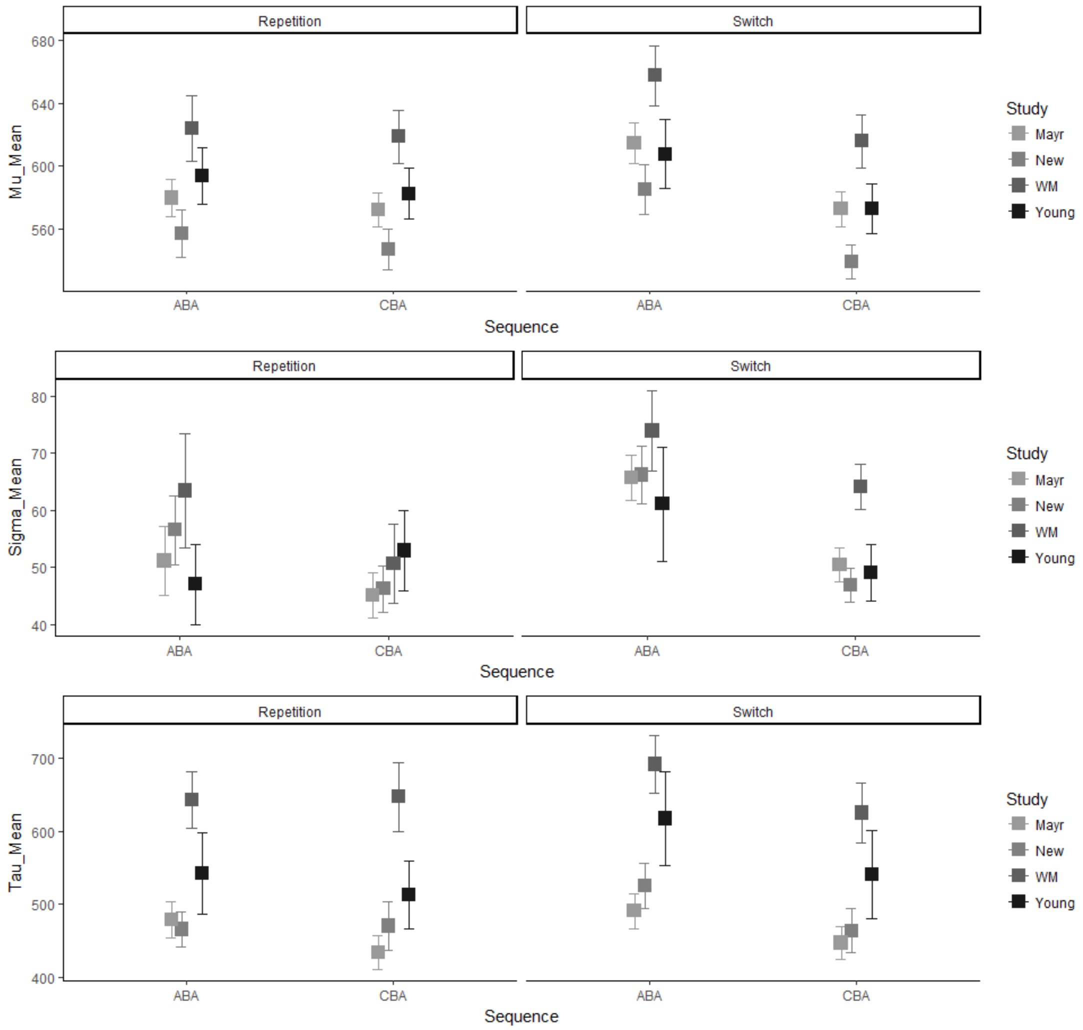


Figure 6.13 An illustration of two-way interactions between Task Sequence (ABA vs. CBA) and Response (n–2 Response Repetition vs. n–2 Response Switch) for ex-Gaussian parameters—mu, sigma, and tau—in WM, Mayr Replication, Young Adults, and New Data. Error bars denote ±SE around the mean.

Diffusion modelling parameters. All the statistical results for diffusion modelling parameters ANOVAs are contained in Table 6.3 and visualised in Figure 6.14. The n–2 task repetition cost was present in threshold separation in majority of data sets, with the threshold separation parameter being consistently higher in ABA compared to CBA sequence. Also, the n–2 task repetition cost was consistently present in the drift rate parameter with smaller drift rates under ABA relative to CBA sequence. However, the cost was absent from non-decisional parameters across all data sets.

For the parameters estimated when controlling for episodic retrieval, the n–2 task repetition cost continued to be present in drift rate and threshold parameters, and absent from non-decisional parameter. The diffusion modelling parameters were not affected by the effect of Response; apart from the drift rate in New Data set with the drift rate higher under n–2 RR compare to n–2 RS.

In terms of the modulatory effect of episodic retrieval on the n–2 task repetition cost, the results are mixed; in two of the data sets the cost was modulated (Young Adults and New Data sets) in the drift rate and non-decisional parameters. For the drift rate, the n–2 task repetition cost was absent under n–2 RR in Young Adults, *t*(28) = 0.39, p = .07, 95%CI [-0.14, 0.21], and in New Data set, *t*(41) = -0.96, p = .40., 95%CI [-0.23, 0.08], compared to n–2 RS where the n–2 task repetition cost was present in Young Adults data, *t*(28) = -6.0, p < .001, 95%CI [-0.57, -0.28], and in New Data, *t*(41) = -6.4, p < .001, 95%CI [-0.53, -0.28].

For the non-decisional parameter there was no n–2 task repetition cost under n–2 RR in Young Adults, *t* (28) = -1.5, p = .10, 95%CI [-0.08, 0.01], and under n–2 RS, *t* (28) = 1.8, p = .09, 95%CI [-0.006, 0.08]; however, in New Data, the n–2 task repetition cost was present (just about) under n–2 RR, *t* (41) = -2.1, p = .04, 95%CI [-0.05, 0.001], and absent under n–2 RS, *t* (41) = 1.9, p = .06, 95%CI [-0.001, 0.05].

Table 6.3 Results for ANOVAS for diffusion modelling—a, v, and t0—for WM (1), Mayr Replication (2), Young Adults (3), and New Data sets (4), with Task Sequence and Response as IVs.

		Diffusion modelling parameters ANOVAs								
		Main effect of Task Sequence			Main effect of Response			Interaction: Task Sequence x Response		
		F(1,40)	*p*	η_g^2	*F*(1,40)	*p*	η_g^2	*F*(1,40)	*p*	η_g^2
1	Standard n–2 task repetition cost									
	a	10.62	<.01	.06						
	v	4.11	.0492	.009						
	t0	1.70	.20	.04						
	Controlled n–2 task repetition cost									
	a	8.27	<.01	.01	.039	.53	.001	.32	.57	.001
	v	0.72	.40	.002	.84	.36	.003	1.38	.25	.003
	t0	3.20	.08	.01	1.17	.29	.003	0.82	.37	.003
2	Standard n–2 task repetition cost									
	a	10.20	<.01	.02						
	v	41.63	<.0001	.03						
	t0	0.29	.59	.0007						
	Controlled n–2 task repetition cost									
	a	6.95	.01	.01	0.03	.80	.00005	0.79	.40	.001
	v	24.10	<.0001	.02	2.16	.15	.001	1.22	.27	.0009
	t0	0.12	.73	.0002	0.87	.35	.001	0.89	.34	.002
3	Standard n–2 task repetition cost									
	a	5.64	.02	.02						
	v	17.18	<.001	.05						
	t0	0.55	.46	.002						
	Controlled n–2 task repetition cost									
	a	5.32	.03	.02	0.001	.98	.0001	2.34	.14	.01
	v	10.75	.003	.03	3.22	.08	.007	20.93	<.001	.03
	t0	0.01	.92	.0001	0.34	.56	.002	4.64	.04	.02
4	Standard n–2 task repetition cost									
	a	0.48	.49	.002						
	v	59.18	<.0001	.16						
	t0	3.62	.07	.02						
	Controlled n–2 task repetition cost									
	a	8.79	.005	.02	0.19	.66	.0006	1.27	.27	.002
	v	28.61	.001	.06	13.60	<.001	.02	23.61	<.001	.04
	t0	0.01	.92	<.0001	3.06	.09	.01	8.07	<.01	.02

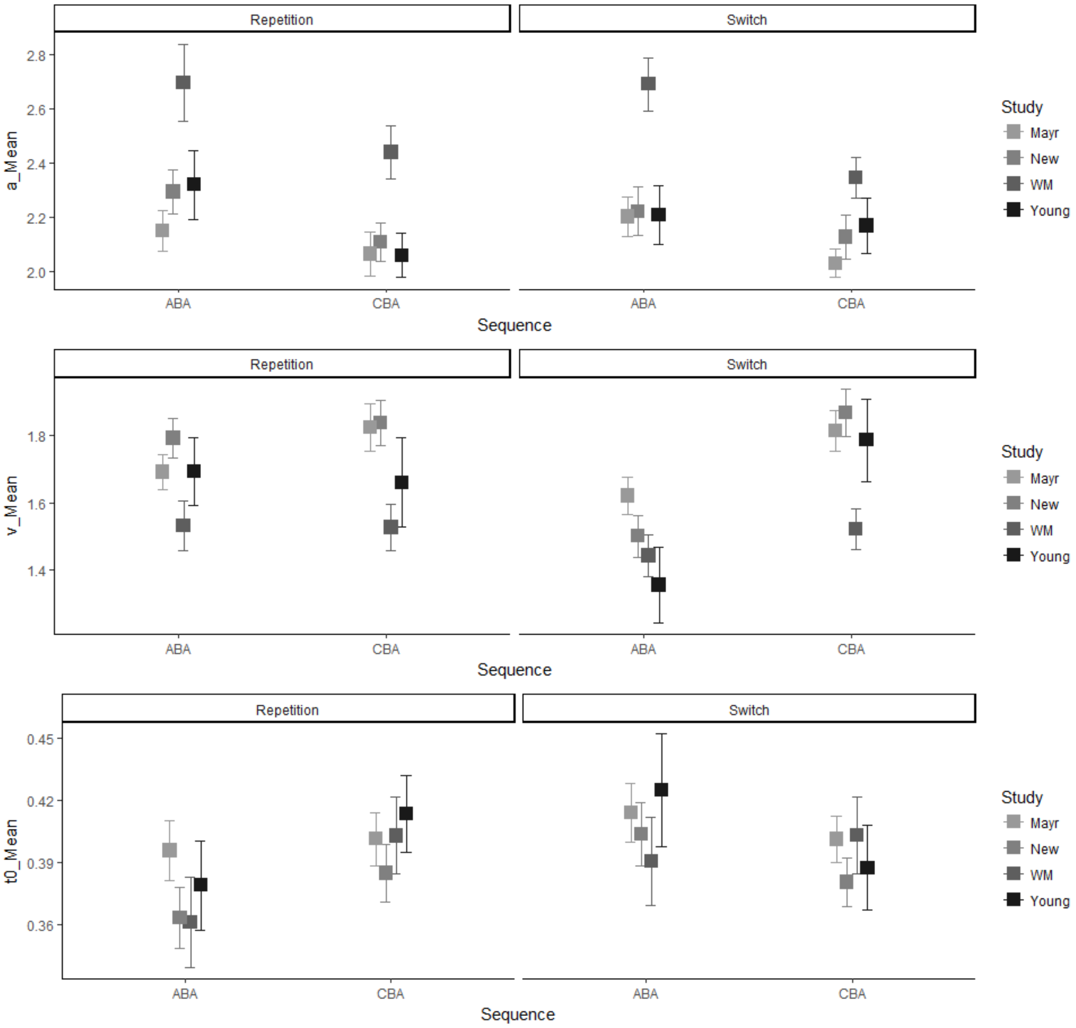


Figure 6.14 An illustration of two-way interactions between Task Sequence (ABA vs. CBA) and Response (n–2 Response Repetition vs. n–2 Response Switch) for diffusion modelling parameters—a, v, and t0—in WM, Mayr Replication, Young Adults, and New Data. Error bars denote ±SE around the mean.

Correlation

In Chapter 4 of the current thesis (WM data, Experiment 5) correlations for the standard and controlled n–2 task repetition costs (for RTs and accuracy) with WM capacity (WMC) are reported. That study found that the RT standard n–2 task repetition cost did not correlate significantly with WMC, $r = .23, n = 41, p = .14$; this was the same for the n–2 task repetition cost under n–2 RR, $r = .09, n = 41, p = .57$. For accuracy, the standard n–2 task repetition cost correlated significantly with WMC, $r = .34, n = 41, p = .03$; however, this correlation did not remian significant after adjustment for multiple testing. The n–2 task repetition cost under n–2 RR did not correlate significantly with WMC, $r = .01, n = 41, p = .95$. These correlations were revisited using modelling parameters.

Ex-Gaussian. Looking at individual ex-Gaussian parameters'—for ABA and CBA sequences—correlations with WMC, it was found that none of the parameters for ABA sequences were significantly correlated with WMC (Table 6.4). For CBA sequences—under not controlled episodic retrieval—sigma correlated significantly with WMC, $r = .31, n = 39, p = .05$; and so did tau, $r = -.30, n = 39, p = .05$. The tau parameter also, correlated significantly with WMC for CBA sequences under n–RS also, $r = -.33, n = 39, p = .04$. However, these correlations did not remain after controlling to multiple testing.

Table 6.4 Correlations coefficients for relationships between WMC and ex-Gaussian parameters for ABA and CBA sequences under not controlled episodic retrieval (i.e., Mixed), and controlled episodic retrieval (i.e., n–2 RR and n–2 RS).

		ABA	CBA
mu	Mixed	-.004	.05
	n–2 RR	-.04	.08
	n–2 RS	.09	.007
sigma	Mixed	-.04	.31*
	n–2 RR	-.17	.17
	n–2 RS	.18	.27
tau	Mixed	-.21	-.30*
	n–2 RR	-.12	-.21
	n–2 RS	-.27*	-.33*

In terms on the parameter's n–2 task repetition costs with WMC correlation, the results are following. The mu n–2 task repetition cost did not correlate significantly with WMC, $r = -.11, n = 39, p = .50$, which was also a case for the sigma, $r = -.26, n = 39, p = .10$. However, the tau n–2 task repetition cost correlated significantly with WMC, $r = .33, n = 39, p = .04$.

For ABA and CBA sequences parameters controlling for episodic retrieval, none of the correlations between parameters' n–2 task repetition costs were significant. For the n–2 RR, the correlation between mu n–2 task repetition cost and WMC was $r = -.16, n = 39, p = .30$; for the sigma it was $r = -.23, n = 39, p = .20$; and for tau it was $r = .15, n = 39, p = .30$. For the n–2 RS, the correlation between mu n–2 task repetition cost and WMC it was $r = .19, n = 39, p = .20$; for the sigma it was $r = .02, n = 39, p = .90$; and for tau it was $r = .18, n = 39, p = .20$.

Diffusion modelling. For individual diffusion modelling parameters for ABA and CBA sequences, majority of correlations between parameters and WMC were not significant (Table 6.5). The two correlations that were significant were for CBA sequences parameter with episodic retrieval not controlled for: for drift rate, $r = .31, n = 39, p = .05$; and, for the non-decisional parameter, $r = -.30, n = 39, p = .05$. However, these correlations did not remain after controlling to multiple testing.

Table 6.5 Correlations coefficients for relationships between WMC and diffusion modelling parameters for ABA and CBA sequences under not controlled episodic retrieval (i.e., Mixed), and controlled episodic retrieval (i.e., n–2 RR and n–2 RS).

		ABA	CBA
a	Mixed	.30	.05
	n–2 RR	.07	.03
	n–2 RS	-.19	-.11
v	Mixed	-.04	.31*
	n–2 RR	.16	.16
	n–2 RS	-.02	.16
t0	Mixed	-.21	-.30*
	n–2 RR	-.14	-.16
	n–2 RS	.10	.05

Correlating diffusion modelling parameters' n–2 task repetition costs means with WMC yielded the following results. The threshold separation n–2 task repetition cost did not correlate significantly with WMC, $r = .004, n = 39, p = 1$; the drift rate n–2 task repetition cost also did not correlate significantly with WMC, $r = -.09, n =$

$39, p = .60$; and the non-decisional parameter did not correlate with WMC too, $r = .006, n = 39, p = 1$.

For parameters for ABA and CBA when episodic retrieval was controlled for, the following was found. For n–2 RR, there was no significant correlation between the threshold separation n–2 task repetition cost and WMC, $r = .05, n = 39, p = .80$; the drift rate n–2 task repetition cost also did not correlate significantly with WMC. $r = .01, n = 39, p = 1$; and, the non-decisional parameter n–2 task repetition cost did not correlate significantly with WMC too, $r = -.01, n = 39, p = 1$. For the n–2 RS, the correlation between the threshold separation n–2 task repetition cost and WMC was not significant, $r = -.18, n = 39, p = .30$; so was the correlation between the drift rate n–2 task repetition cost and WMC, $r = -.19, n = 39, p = .20$; and between the non-decisional n–2 task repetition cost and WMC, $r = .25, n = 39, p = .10$.

Discussion

The n–2 task repetition cost, argued by many to reflect cognitive inhibition, is a behavioural effect seen in task-switching research. Recently the n–2 task repetition cost has been shown to be modulated considerably by non-inhibitory processes (i.e., episodic retrieval); specifically, a large proportion of the n–2 task repetition cost was evidenced to be due to an automatic memory retrieval rather than control mechanism, questioning the validity of the n–2 task repetition as a suitable measure of cognitive inhibition. Typically, in task-switching research performance is analysed by looking at central RT and accuracy tendencies; the current study offers insight into the cognitive processing underlying the n–2 task repetition cost by adopting alternative analyses. The standard procedure of analysing task-switching performance was complemented with ex-Gaussian analysis, CDFs, and diffusion modelling to decompose the n–2 task repetition cost. Because of these analyses, a better understanding of the n–2 task repetition cost, and how episodic retrieval affects it was obtained.

First, it is important to report that consistently with existing literature, the standard n–2 task repetition cost (when episodic retrieval is not controlled for) was present in behavioural results in all four data sets. When diffusion modelling was applied, expectedly the standard n–2 task repetition cost was present in drift rate; this supports the view that this cost is due to carry-over or interference effects inhibition (Schmitz & Voss, 2012, 2014), with the likely candidate being inhibition. Specifically, the drift rate was smaller for ABA sequence compared to CBA sequence, and smaller drift rates are linked with less efficient and slower information accumulation (Schmiedek et al., 2007). However, the novel finding that the current study provides is that when episodic retrieval is controlled for, the overall trend is that the n–2 task repetition cost is no longer observed in drift rate under episodic matches (n–2 RR) compared to episodic mismatches (n–2 RS). The main implication of this finding is that the n–2 task repetition cost is unlikely a reflection of cognitive control, pointing to automatic memory retrieval processes as being the source of the n–2 task repetition cost. Further discussion on results and their implications are presented below.

Standard n–2 Task Repetition Cost

Ex-Gaussian. The ex-Gaussian modelling offers a detailed description of data through estimating ex-Gaussian parameters characterising RTs distributions. In terms of the standard n–2 task repetition cost, the existing literature (Grange & Juvina, 2015; Schuch & Konrad, 2017) shows that this cost is predominantly seen in the ex-Gaussian tau parameter, which was replicated for all four data sets used in the current investigation. This observation has been interpreted as evidence that the standard n–2 task repetition cost is driven by the exponential tail of distribution which in task-switching literature is linked to slower RTs and less prepared responses (DeJong, 2000a), and in general is associated with executive functions (Schmiedek et al., 2007; Shahar & Meiran, 2014). However, the rest of the ex-Gaussian modelling results showed that n–2 task repetition costs were also observed in Gaussian parameters mu and sigma (apart from the Simulation Study 3 where the n–2 task repetition cost was not present in sigma), which is somewhat inconsistent with evidence that n–2 task repetition costs are predominantly associated with tau (Grange & Juvina, 2015). As such, this inconsistency makes reported results harder to interpret; however, similar results have been noted previously (Schuch & Konrad, 2017) and have been seen as noisy. Still, having looked at four separate data sets in the current study, it seems unlikely that all four simulations produced noisy results, but it is possible if ex-Gaussian modelling does not capture effects which lead to n–2 task repetition costs adequately. Importantly, values for tau n–2 task repetition cost were the largest among the three parameters (see Appendix A), which is more consistent with literature; hence, overall, the results for the ex-Gaussian modelling of the standard n–2 task repetition cost can be taken as a confirmation that this cost is largely driven by higher cognitive mechanisms (e.g., inhibition).

Also, despite some unusual results from the ex-Gaussian modelling for the standard n–2 task repetition cost, further consistent patterns were observed; namely, for ABA sequences, parameters mu, sigma, and tau, were higher than for CBA sequences parameters. Overall, performance on ABA sequences was characterised by slower RTs as reflected by longer and flatter right tail of distribution, compared to CBA sequences, indicating differences due to higher cognitive processing (i.e., executive functions) between those two conditions. This is because, larger tau and longer right tail of distribution are associated with attentional/ WM lapses (Hervey et al., 2006; Schmiedek et al., 2007), inhibition (Grange & Houghton, 2011; Schuch & Konrad, 2017; Spieler et al., 1996), tasks-conflict processing (Shahar & Meiran, 2014; Steinhauser & Hübner, 2009), WM (Schmiedek et al., 2007; Shahar et al., 2014; Tse et al., 2010), and lower intelligence (Baumeister, 1998); all of which can be linked to cognitive control. As such, current results can be interpreted as evidence that participants find ABA sequences, compared to CBA sequences, harder to perform to their best of abilities; that ABA sequences put a cognitive strain on processing of information, leading to overall slower performance compared to CBA sequences.

In terms of the patterns seen in Gaussian parameters, larger mu parameters have been linked to response related processes, such as response criterion (Spieler, 2001) and response conflicts (Steinhauser & Hübner, 2009). For example, more conservative response criterion is linked to slower performance, and more liberal response criterion leads to faster performance. Taking this into account, it is possible that on ABA sequences participants find it harder to select responses (due to, for example episodic mismatches), and as a consequence they may adopt a more cautious response criterion, compared to CBA sequence, assertion which current results would support. In terms of the sigma parameter, literature does not offer a clear indication on whether, how and why it would differ between ABA and CBA sequences; however, sigma as well as mu have been linked to response conflict (Steinhauser & Hübner, 2009).

Diffusion modelling. For the diffusion modelling analyses, it was predicted that the standard n–2 task repetition cost would be present in drift rate. This is because this parameter has been linked to carry-over effects and interference with cognitive inhibition being the likely mechanisms leading to the n–2 task repetition cost (Schmitz & Voss, 2012, 2014). Consistent with existing literature, the current study revealed that the standard n–2 task repetition cost was present in drift rate for all four data sets; specifically, the drift rate was smaller for ABA compared to CBA sequence. Smaller drift rates are linked with less efficient and slower information accumulation (Schmiedek et al., 2007), which fits in with how inhibition is believed to be affecting performance on task-switching. That is, if inhibition from n–2 trial impairs performance on the current trial, we would expect to see slower information accumulation due to the current task being less accessible. Another observation for the current study was that n–2 task repetition costs were also present in the threshold separation parameter for the three out of four data sets. Specifically, the threshold separation was larger for ABA sequences compared to CBA sequences, which can be interpreted as evidence that, for the ABA sequence responses were more conservative compared to the CBA sequences in those two conditions; larger threshold separation typically indicates slower but more accurate responses (in the New Data for which there was no n–2 task repetition cost seen in threshold separation, numerically, this parameter was also larger for ABA compared to CBA). In terms of the non-decisional parameter, there were no significant differences between ABA and CBA across all four data sets, which can be interpreted as evidence that in all four data sets information was equally well encoded and responses were equally efficiently executed.

Overall, these results support the view that the n–2 task repetition cost is likely to be due to carry-over effect with inhibition being the probable candidate. This is consistent with results from existing studies (Schuch, 2016; Schuch & Konrad, 2017). With drift rates being consistently smaller for ABA than CBA sequences—where smaller rates indicate slower information accumulation—the average n–2 task repetition cost for drift rates resulted in negative values (Appendix A). This taken together with the observation that threshold separation was higher for ABA compared to CBA sequences—with higher rates indicating more conservative responses—can be taken as evidence that less efficient information accumulation for ABA sequences was driving participants response criterion to be higher (Schuch, 2016). Also, despite ex-Gaussian modelling producing, what can be interpreted as, noisier results compared to diffusion modelling, results from both point to the n–2 task repetition cost being due to higher cognition conflicts (as reflected in tau being higher and drift rate being smaller for ABA compared to CBA sequences) as well as response related effects (as reflected in mu/ sigma and threshold separation being higher for ABA compared to CBA sequences).

Controlled n–2 Task Repetition Cost

Ex-Gaussian. The results from ex-Gaussian and diffusion modelling analyses for controlled n–2 task repetition cost are less clear than for the standard n–2 task repetition cost, but some patterns can be distinguished.

In terms of the tau parameter, the n–2 task repetition cost was present as expected (for three out of four data sets); however, for two data sets this cost was modulated by episodic retrieval, whereby for the episodic matches (i.e., RR) there was no n–2 task repetition cost but it was present for episodic mismatches (i.e., RS). This was the general pattern for three out of the four data sets. Assuming that tau is associated with executive functions (Grange & Houghton, 2011; Hervey et al., 2006; Schmiedek et al., 2007; Schuch & Konrad, 2017; Shahar & Meiran, 2014; Shahar et al., 2014; Spieler & Balota, 1996; Steinhauser & Hübner, 2009; Tse et al., 2010), and that episodic retrieval is an automatic mechanism, these findings can be interpreted as evidence that, either tau can reflect automatic processes as well as executive functions or that episodic retrieval influences how information is processed within executive functions.

When it comes to the mu parameter, generally the n–2 task repetition cost was also modulated by episodic retrieval; this was the case for three out of four data sets. Specifically, for episodic matches (i.e., RR) mu for ABA and CBA were similar but they differed considerably for episodic mismatches (i.e., RS). Consistently, n–2 task repetition costs in mu were substantially smaller for episodic matches compared to episodic mismatches (see Appendix A). Keeping in mind that Gaussian parameters are associated with response mechanisms (Spieler et al., 1996), these results can be taken as evidence that episodic retrieval mismatches drive the n–2 task repetition cost in mu by creating a response conflict.

With regard to the sigma parameter, it was not straight forward to predict results from ex-Gaussian analysis; however, in general, Gaussian parameters are associated with response related mechanisms (Spieler, 2001; Steinhauser & Hübner, 2009). There were no substantial differences for n–2 task repetition costs in sigma for episodic matches and episodic mismatches for Task Sequence and Response, and they did not interact. As a general observation, it was noted that for episodic matches the variance of sigma was smaller than for episodic mismatches (see Appendix A). Assuming that episodic retrieval, as an automatic mechanism, leads to more consistent responses under episodic matches compared to episodic mismatches, this would be expected; namely, episodic mismatches generate response conflict, which may be solved by various strategies and depend on individual differences, compared to episodic matches which automatically prime responses to be more efficient and consistent.

Diffusion modelling. When the standard n–2 task repetition cost is explored with diffusion modelling, the typical finding is that this cost is present predominantly in drift rate, which is sensitive to carry-over effects and interference (with inhibition being the likely candidate driving the n–2 task repetition cost). In light with new reports that the n–2 task repetition cost is modulated by episodic retrieval, whereby the large proportion of this cost is explained by episodic retrieval, it was of interest to investigate whether under episodic matches the n–2 task repetition cost would continue to be observed in drift rate. Specifically, if episodic retrieval can affect information accumulation needed to make a decision, it was speculated that under episodic matches the n–2 task repetition cost would not be present compared to episodic mismatches. However, if the n–2 task repetition cost was still observed under episodic matches, it would mean that episodic retrieval does not affect information accumulation.

The general finding for the drift rate was that under episodic matches, the n–2 task repetition cost was reduced or absent, compared to episodic mismatches where this cost remained. That is, information accumulation was considerably less efficient and slower for episodic mismatches compared to episodic matches (see Appendix A). This fits in with prediction that the n–2 task repetition cost is unlikely to reflect cognitive inhibition and instead is more likely to be a result of non-inhibitory mechanism, specifically episodic mismatches. Larger drift rates for episodic matches compared to episodic mismatches, suggest that under the former one information accumulation was more efficient. This fits in with reports of smaller drift rates being linked to interference and carry-over effects (Schmitz & Voss, 2012; Voss, Rothermund, et al., 2013; Weeda et al., 2014). Episodic matches, unlike episodic mismatches, do not lead to interference, but rather facilitate performance leading to faster information accumulation and higher drift rates. Together, these observations can be interpreted as an indication that drift rates reflect relative task readiness, with lower drift rates reflecting uncompleted preparation or response selection difficulty, likely due to a conflict caused by episodic mismatches; and higher drift rates reflecting completed preparation, with episodic matches likely facilitating information accumulation, and relative task readiness.

The threshold separation was not expected to differ between ABA and CBA, mainly because this parameter reflects response criterion; that is, how much information is accumulated before a decision is made. Typically, threshold separation is sensitive to speed-accuracy trade-off, and since participants for all four studies from which data were taken for the current study were instructed that both, accuracy and speed, are equally important, it was expected that the n–2 task repetition cost should not differ between ABA and CBA sequences, or episodic matches compared to episodic mismatches, unless participants were more cautious under some conditions (e.g., under ABA vs CBA, or episodic mismatches vs episodic matches). Also, the n–2 task repetition cost is known to be present in both, RTs and accuracy; which is another reason why no n–2 task repetition cost would be expected in threshold separation. The results from the current study are rather inconclusive; that is, the standard n–2 task repetition cost was present in threshold separation for two out of the four data sets (WM and Mayr Replication), with the ABA characterised by larger threshold separation compared to CBA. This means that for ABA sequences, threshold separation for responses was more conservative, compared to CBA; it is uncertain though, whether participants naturally responded more cautiously for ABA compared to CBA sequences, or whether inefficient information accumulation led to more cautious responses. It seems that episodic retrieval could not affect threshold separation, because n–2 task repetition costs did not seem to differ between episodic matches compared to episodic mismatches. This somewhat makes sense if it is assumed that episodic retrieval is automatic, therefore should not lead to bias in threshold separation. Moreover, threshold separation parameter's n–2 task repetition cost was not modulated by episodic retrieval, further suggesting that this parameter is not affected by automatic retrieval processing.

In relation to the non-decisional parameters, the n–2 task repetition cost was not expected to be present in that parameter, which for all four data sets was the case. However, for two out of the four data sets there was a non-significant numerical trend for the parameter to be smaller for ABA compared to CBA under episodic matches, but for episodic mismatches, the non-decisional parameter was smaller for CBA compared to ABA sequences (i.e., there was an interaction between Task Sequence and Response; see Appendix A). The non-decisional parameter is associated with information encoding, motor response, preparation, and WM configuration (Schmitz & Voss, 2012, 2014; Weeda et al., 2014). For example, the smaller the non-decisional parameter is, the more efficient encoding and responses are; also, larger non-decisional parameters are linked with task-switches compared to task-repetitions, unpredictable task-switches compared to predictable task-switches, additional preparation, and task reconfiguration. As such, the results for the non-decisional parameter in the current study can be interpreted in the following way.

First of all, it can be assumed that information encoding for all task-switches in the four data sets were the same because the same cues, stimuli, and response mappings were used; moreover, the execution of response, once is selected should be the same for ABA and CBA sequences regardless episodic retrieval. This leaves WM reconfiguration and preparation as possible processes affected by episodic retrieval. Assuming that retrieved information is manipulated in WM, under the episodic retrieval account, for episodic matches, there should be less of a conflict when it comes to response selection because retrieved response matches the task demands; under the episodic mismatches however, retrieved response conflicts with required response which leads to a conflict in

WM. As such, the former one would be expected to lead to smaller non-decisional parameter and faster performance on ABA compared to CBA sequences; whereas the latter one would be associated with larger non-decisional parameter and slower performance on ABA compared to CBA sequences, a general pattern seen in the current study. Also, in terms of preparation, absence of it is associated with completed preparation, whereas increased non-decisional parameter has been linked to additional preparation. Therefore, despite the same paradigms being used in four studies from which data sets for the current investigation were used, it could be that the results of no effect of Task Sequence or interaction between Task Sequence and Response, could be an indication of these particular participants' samples being able to prepare their responses better than in data sets where differences in the non-decisional parameter were observed. However, since in all four studies' paradigms, the preparatory interval (i.e., the cue-stimulus-interval) was identical, the possibility of additional preparatory processes taking place in some but not in the other data sets seems unlikely.

WM and Modelling Parameters

For one of the data sets used for modelling in the current study—the WM study from Chapter 4 of the this thesis, measures of WMC were also collected; it was found that WM was not associated with n–2 task repetition cost whether episodic retrieval was controlled for or not. This was interpreted as evidence that inhibition—as measured with n–2 task repetition cost–was not dependent on WM resources, contrary to some theories (Conway & Engle, 1994; Conway et al., 1999), Since, there is evidence that WM is linked to ex-Gaussian tau and diffusion modelling drift rate parameters (Heathcote et al., 1991; Schmiedek et al., 2007; Shahar & Meiran, 2014), it was of interest to investigate whether modelling parameters from WM study relate to WM scores. To ascertain that, individual modelling parameters as well as n–2 task repetition costs for parameters were correlated with WMC measure. Evidence of a relationship between parameters and WMC is very limited; however, some patterns can be identified though.

The main observation was that, none of the ex-Gaussian and diffusion modelling parameters for ABA sequences were linked to WMC. For CBA sequences, it was sigma and tau parameters from ex-Gaussian modelling, as well as drift rate and non-decisional parameters from diffusion modelling, that were associated with WMC; although, those relationships did not remain so after controlling for multiple testing, some insight can be gained from those associations.

Consistently with existing evidence (Schmiedek et al., 2007), higher tau values were linked with lower WM in the current study. Assuming that ABA sequences (compared to CBA sequences) are linked with inhibition, and inhibition has been shown to be WM dependant (Conway & Engle, 1994; Conway et al., 1999), it is somewhat unexpected that correlations were predominantly present for the CBA and not ABA sequences. However, keeping in mind that tau parameter has been argued to reflect tasks-conflict processing (Shahar & Meiran, 2014; Steinhauser & Hübner, 2009), intelligence (Baumeister, 1998), and attentional lapses (Hervey et al., 2006); whereby, people with higher WM and intelligence show smaller tau values, and task-switches as well as more attentional lapses as characterised by larger tau values, the following can be speculated. It is possible that, WM seems to be linked more to CBA sequences because under these conditions—due to what can be considered as substantial task as well as response conflicts—WM is more needed to resolve these conflicts. It would appear that WM was particularly important for CBA sequences when episodic retrieval was not controlled for (as seen in sigma and tau) and under episodic mismatches (seen in tau); this would imply, that WM may be especially important for resolving response conflicts.

In terms of diffusion modelling parameters, the results pattern was that WM associated with drift rate and non-decisional parameters for CBA under not controlled episodic retrieval, where drift rate was correlated positively and the non-decisional parameter negatively with WM. Higher drift rates are in general linked to faster and more efficient information accumulation; therefore, it is possible that people with higher WMC can resolve task/ response conflict more efficiently than people with lower WMC. This fits in with findings from ANOVAs analyses in the current study where it was observed that drift rates were considerably smaller for episodic matches compared to episodic mismatches; that, in general, episodic matches are associated with more efficient information accumulation, which can depend on WMC, as illustrated by results from correlational analyses.

The non-decisional parameter—for CBA sequences not controlled for episodic retrieval—was linked with WMC too, which can be interpreted in the following way. It has been observed by other researchers that non-decisional parameter is higher for task-switches compared to task-repetitions (Schmitz & Voss, 2012, 2014), and task reconfiguration (Voss et al., 2004), and is linked to more preparation (Schuch, 2016). As such, it is possible that, the negative correlation between the non-decisional parameter and WMC from the current study is an indication of some individual differences in how

WMC affects non-decisional parameter. It is uncertain at this point though, whether people with higher WMC are more efficient at preparing their responses, or reconfiguring tasks between switches, compared to people with lower WMC. Since it was the non-decisional parameter for CBA sequences under episodic not controlled for that correlated with WM, it is unlikely that people with higher WMC simply executed their responses faster.

CDFs

To further explore the n–2 task repetition cost, CDFs were obtained for the standard and controlled n–2 task repetition cost. There is evidence (Grange & Houghton, 2011) to suggest that the n–2 task repetition cost is smaller for fast RTs compared to slower RTs; this pattern was replicated for all four data sets in the current investigation for the standard n–2 task repetition cost. When episodic retrieval was controlled for, the linear trend for the n–2 task repetition cost did not seem to differ between episodic matches and mismatches; apart from New Data where for episodic matches the n–2 task repetition cost did not increase with slower RTs, but for episodic mismatches it did increase.

Assuming that fast RTs reflect prepared and slow RTs not fully prepared responses (DeJong, 2000a), the linear trend of the n–2 task repetition cost to increase with slower RTs, has been interpreted as evidence that inhibition (as measured with the n–2 task repetition cost) can be overcome with preparation (Grange & Houghton, 2011). If faster RTs reflect better prepared responses, it would be expected that episodic retrieval mismatches would impair that preparation; as such, it would mean that for episodic matches there should be no linear trend of n–2 task repetition cost increasing with slower RTs, contrary to episodic mismatches which should be characterised by that trend. Among four data sets used in the current study, only one data set fit that prediction, as shown with CDFs analyses, the New Data; and looking at CDF graphs, there was a tendency for that pattern in other data sets too (although not statistically significant). Also, from diffusion modelling, for two out of the four data sets (including New Data), the n–2 task repetition cost in non-decisional parameter was affected by episodic retrieval, where by it was larger for episodic mismatches compared to episodic matches. The non-decisional parameter has been linked to preparation with larger parameters indicating additional preparation (Schmitz & Voss, 2012, 2014; Weeda et al., 2014), which would fit it with prediction that n–2 task repetition cost should remain small across fast and slow RTs. Nevertheless, findings from CDFs analyses can be considered as limited evidence; therefore, for now it is unclear whether episodic retrieval truly does not affect the linear trend for the n–2 task repetition cost to increase with slower RTs (as majority of the results from the current investigation would suggest), or whether to tap into these effects more data are needed (as seen in Simulation 4).

Conclusions

Findings from Chapter 6 question the validity of the n–2 task repetition cost as a measure of cognitive inhibition. Consistently with previous literature the n–2 task repetition cost was predominantly present in the drift rate; however, when episodic retrieval was controlled for, the cost in drift rate reduced numerically in half of the simulations and was removed in the other half. This means that the n–2 task repetition cost in drift rate—typically interpreted as evidence of inhibitory processes—is likely to be driven by non-inhibitory, automatic mechanisms, in this case episodic retrieval. The trend for the n–2 task repetition cost presence in threshold parameter—which was not expected—was interpreted as evidence that participants were more cautious on ABA compared to CBA sequences. Also, n–2 task repetition costs seen in threshold parameter were not affected by episodic retrieval, which means that automatic retrieval does not bias the decision criterion. And as expected, non-decisional parameter was not marked by the n–2 task repetition cost. In terms of ex-Gaussian modelling, despite some results being consistent with existing literature (e.g., n–2 task repetition costs seen in the tau parameter), overall, conclusions are difficult to draw due to no specific pattern of episodic retrieval on the tau parameter, and n–2 task repetition costs seen in mu and sigma parameters.

Chapter 7 General Discussion

General Discussion

The current thesis aimed to further our understanding of the n–2 task repetition cost (Mayr & Keele, 2000) which is a behavioural effect seen in task-switching, specifically in the Backward Inhibition (BI) paradigm. In that paradigm participants switch back and forth between three tasks, and it is a very common observation that when they repeat a task from an n–2 trial (referred to as an ABA sequence) they are slower and less accurate compared to when the task they perform is a switch from the n–2 trial task (referred to as a CBA sequence); and the difference between those two conditions is known at the n–2 task repetition cost. This effect has been replicated many times and used extensively as a measure of cognitive inhibition (for reviews see Kiesel et al., 2010; Koch, Gade, Schuch, & Philipp, 2010). Successful and efficient task-switching is argued to be attributable to inhibition (Goschke, 2000; Mayr & Keele, 2000), and as such the n–2 task repetition cost has become an important effect reflecting inhibitory processing.

The predominant explanation of the source of the n–2 task repetition cost is the inhibitory theory; specifically, when participants switch from one task to another, as a given task is abandoned it becomes inhibited so its activation does not interfere or interferes less with the next task (Mayr & Keele, 2000). When performing a task repeated from the n–2 trial (i.e., ABA sequence) that task is under the influence of recently applied inhibition, making it less accessible, which leads to slower reaction times (RTs) and more errors, compared to performing a task that is different from the one on the n–2 trial. In the current thesis, across four experimental chapters (Chapters 2–5; Experiments 1–9) and one computational modelling chapter (Chapter 6; Simulation

Studies 1–4), the n–2 task repetition cost was examined with regards to its reliability, factors that may modulate it, and individual differences that may predict it. Together, the results show that the n–2 task repetition cost is unlikely to reflect cognitive inhibition and should be avoided as a measure of individual differences in inhibitory control. The general discussion will summarise the overall findings, focusing on the key messages, which will be followed by conclusions.

Reliability

Chapter 2 looked at the reliability of the n–2 task repetition cost using three different BI paradigms known to produce this effect (Gade & Koch, 2008; Houghton et al., 2009; Schuch & Koch, 2003), and explored potential predictors of the individual differences seen in the n–2 task repetition cost (processing speed: Brown, Brockmole, Gow, & Deary, 2012; Naber, Vedder, Brown, & Nieuwenhuis, 2016; Stawski, Sliwinski, & Hofer, 2013; depressive rumination: Whitmer & Banich, 2007). It was important to investigate these factors in relation to the n–2 task repetition cost because this effect has been used by many as a measure of inhibitory control in group (Fales et al., 2006; Foti et al., 2015; Lawo et al., 2012; Mayr, 2001; Mayr et al., 2006; Moritz et al., 2004; Prior, 2012; Yiu-kwan, 2008) to individual differences research (Pettigrew & Martin, 2015; Whitmer & Banich, 2007, 2012). However, the reliability of the n–2 task repetition cost was not known; as much as it was not a problem for group differences studies, for individual differences research it was an area of concern. Many robust behavioural effects used widely for assessment of healthy and impaired cognition are proven to be reliable (stop-signal task: Congdon et al., 2012; go/no-go task: Leue, Klein, Lange, & Beauducel, 2013; Stroop test: Strauss, Allen, Jorgensen, & Cramer, 2005), but some have been shown to have low reliability (e.g., the negative priming effect; Bestgen & Dupont, 2000), which indicates that we should not mistake robustness for reliability of the effect. Chapter 2 of this thesis shows that the n–2 task repetition cost is not reliable despite its robustness.

Across three BI paradigms used in Experiment 1 it was systematically shown that the reliability of the n–2 task repetition cost is low at an individual level (Kowalczyk & Grange, 2017; Chapter 2). The reliability (r_c, coefficient corrected for the split-half reliability) of the reaction times (RTs) n–2 task repetition cost ranged .37–.60, and for accuracy n–2 task repetition cost it ranged .13–.32. For a test to be considered suitable for individual differences research, it should have a reliability coefficient of minimum .70 (e.g., Cronbach, 1951; Nunnally, 1967; Picardi & Masick, 2013; Revelle & Condon, 2014; Streiner, 2003). This is because a cognitive test used for individual differences research should provide reproducible results, and be sensitive to fluctuations occurring in performance (Drost, 2011; Onwuegbuzie & Daniel, 2002). Reliability values similar to the ones reported in Chapter 2 were interpreted in other studies as “moderate” (Leue, Klein, & Lange, 2013), “poor”–good” (Condon et al., 2012), and “quite low” (Pettigrew & Martin, 2015). None of these are necessarily “correct”; however, there seems to be an overall consensus in the wider literature that reliability within the range reported for the n–2 task repetition cost in this thesis is not satisfactory.

Individual differences. Experiment 1 also measured individual differences that were likely to predict the n–2 task repetition cost, as it was planned that they would be controlled for in the reliability analysis. It was demonstrated that the n–2 task repetition cost was not linked to processing speed (van der Elst et al., 2006) or depressive rumination (contrary to Whitmer & Banich, 2007), and therefore, they were not used in further analysis. The lack of the relationship between the n–2 task repetition cost and individual differences examined in Experiment 1 can be speculated to be due to at least a couple of reasons. Processing speed and depressive rumination may simply not be linked to the n–2 task repetition cost, or the n–2 task repetition cost is not a reliable measure. Low reliability of the n–2 task repetition cost can be due to the cost not measuring inhibition exclusively (e.g., measuring inhibitory and non-inhibitory processing), or due to its nature—that is, the cost being a difference score—which will be discussed next.

Difference scores. The n–2 task repetition cost is a difference score—that is, a score obtained from subtracting performance in one condition from another—and these can be very difficult to interpret (e.g., negative priming, Bestgen & Dunpont, 2000), and are known to have low reliability (Crawford et al., 2008; Cronbach & Furby, 1970; Dunlap et al., 1989; Kopp, 2011). Specifically, a reliability of difference scores is “a function of the average reliability of its two components and of the correlation between them” (Kopp, p. 562; Crawford et al.). If a difference score is made up of two related components, the correlation between those components will be high (or even approach the reliabilities of the two components); as such, the variance of the difference scores is likely to reflect mainly error variance. Since the n–2 task repetition cost is a difference score, low reliability of the n–2 task repetition cost is likely to be due to the nature of the cost, which means that the lack of the relationship between individual differences and the n–2 task repetition cost is difficult to interpret, and we cannot say for certain that processing speed and/ depressive rumination do not influence cognitive inhibition. For now, it can be said that the n–2 task repetition cost as we know it, may not be as meaningful as originally thought (Mayr & Keele, 2000; Whitmer & Banich, 2007), therefore should be used with caution in individual differences research.

There is evidence that other cognitive effects which are difference scores—the negative priming (Bestgen & Dupont, 2000; Frings, Schneider, & Fox, 2015) and Stroop effects (Strauss et al., 2005)—also can have low reliability, but not always. For example, in terms of the Stroop effect there is evidence that this effect can be reliable (Strauss, Allen, Jorgensen, & Cramer, 2005); this means that low reliability of difference scores cannot be assumed, and researchers should always examine the reliability of their difference scores effects when used in individual differences research. Also, it indicates that there may be factors which influence difference scores' reliability which are not fully understood yet. And, with regard to the negative priming effect, there are at least a couple of issues which may affect its reliability; that is, it is a difference score (like the Stroop effect and the n–2 task repetition cost), and it has been shown to reflect more than one cognitive process, episodic retrieval as well as inhibition (Tipper, 2001). Considering these issues together, some suggest that this is evidence against the negative priming effect being used as a measure of inhibitory processes (Frings et al., 2015). Therefore, when it comes to the n–2 task repetition cost, we should exercise caution interpreting this effect because it is also a difference score, and as it is proposed in discussion of Chapter 2 and later shown in Chapter 3, the n–2 task repetition cost also reflects a mixture of inhibitory and non-inhibitory mechanisms (which is discussed further in the later parts of this discussion). This means that, as the negative priming effect, the n–2 task repetition cost is most likely not a good measure of individual differences in inhibitory control, and should be used with caution if at all.

The issues around the reliability of difference scores and whether/ how well a given measure captures the processes of interests, highlight a bigger problem, which is that cognitive effects are not examined enough in terms of their reliability, and that the usefulness of those is often based on how replicable they are which seems to be associated with robustness. The point is that, robustness of an effect—how *replicable* it is—should not be mistaken for how reliable it is, and as a good practice, reliability of cognitive effects should be reported in individual differences research.

There are alternatives to difference scores, for example some suggest that instead, rate residual scores can be used (Hughes et al., 2014), which take into account reaction times and accuracy, and as such are considered as more accurate measure of cognitive processes (Rush et al., 2006). This method involves calculating a rate of correct responses per second for the conditions of interest, and these rates are put into regression (for a detailed description of this method see Hughes et al., 2014); however, one study (Pettigrew & Martin, 2015) showed that using rate residual scores did not improve the reliability of the n–2 task repetition cost. Pettigrew and Martin also showed that both the standard n–2 task repetition cost and rate residual scores when used to reflect the performance in the BI paradigm did not correlate with working memory (WM) capacity. Therefore, it terms of n–2 task repetition cost it seems that the cost's reliability and the extent to which it can be explained by individual differences, is not improved with the use of rate residual scores, which further adds to the uncertainty over the n–2 task repetition cost.

It is important to continue to explore methods alternative to difference scores to examine cognitive effects, as well as issues around difference scores' reliability and meaningfulness, but for now based on results from Chapter 2, it seems that the n–2 task repetition cost is not reliable or correlated with processing speed and depressive rumination. Therefore, the n–2 task repetition cost should be avoided as a measure of individual differences in cognitive control, but research on task-switching should continue in order to gain a better understanding on the source of the differences between ABA and CBA sequences; and if the n–2 task repetition cost is used in individual differences research, as a good practice, its reliability should be reported.

Episodic Retrieval

The n–2 task repetition cost is thought to reflect inhibitory processes, and this is the predominant interpretation of this effect (Kiesel et al., 2010; Koch et al., 2010; Mayr & Keele, 2000). However, in light with the evidence that the n–2 task repetition cost has low reliability at an individual level (Kowalczyk & Grange, 2017), task-switching performance in general being sensitive to priming effects (Allport & Wylie, 1999; Altmann, 2011; Goschke, 2000; Horoufchin et al., 2011; Logan & Bundesen, 2003; Schmidt & Liefooghe, 2016), and other effects ascribed to cognitive inhibition shown to be highly modulated by priming effects (Tipper, 2001), it was important to explore to what extent the cost measured cognitive inhibition. Therefore, based on Mayr's (2002) work three studies investigated the extent to which the n–2 task repetition cost was modulated by episodic retrieval (Hommel, 1998, 2004, 2009; Neill, 1997).

Experiments 2–4 from Chapter 3 show that the n–2 task repetition cost is largely due to automatic and non-inhibitory mechanism, namely episodic retrieval (Hommel, 1998, 2004, 2009; Neill, 1997), as evidenced with much smaller n–2 task repetition costs under episodic matches compared to episodic mismatches. Specifically, when within n–2 task repetitions we ensure that the n–2 response repeats too, the n–2 task repetition cost reduces compared to n–2 task repetitions with n–2 response switches. This is thought to be because if upon seeing a given stimulus a retrieved response does not match the currently required action (as seen in n–2 task repetition costs under n–2 response switches), by the episodic retrieval account, it leads to an impaired performance because a new response will have to be generated against the one that was initiated automatically through episodic retrieval; however, if the retrieved task's parameters match the demands of the current task (as seen in n–2 task repetition costs under n–2 response repetitions), performance is facilitated (Hommel, 1998, 2000, 2004). This shows that the n–2 task repetition cost, as we know it, does not exclusively reflect inhibition, but instead captures processing of episodic mismatches and inhibition.

Moreover, apart from experiments 2–4, the rest of the experiments in this thesis (5–9) and computational modelling study also controlled episodic retrieval within the BI paradigm. Together, in 7/10 data sets reaction time (RT) n–2 task repetition costs and 8/10 data sets accuracy n–2 task repetition costs were modulated by episodic retrieval. Taking into account only data sets in which the n–2 task repetition cost was modulated by episodic retrieval, for RT performance in 4/7 cases and for accuracy performance in

5/8 cases the n–2 task repetition costs were removed; that is, controlling for episodic retrieval led to performance to be similar between ABA and CBA sequences, compared to when episodic retrieval was controlled for. Furthermore, for accuracy there was a general trend for n–2 task repetition benefits under episodic matches; that is, for 4/5 data sets where there were no significant differences between ABA and CBA sequences, accuracy for ABA was higher than for CBA; and for 3/3 data sets where those differences were significant, accuracy for ABA was higher than for CBA. These are the first results to show that RT n–2 task repetition cost can be removed when episodic retrieval is controlled for, and that it can lead to n–2 task repetition benefits in accuracy which is contrary to the original research conducted in this area (Mayr, 2002). These results are important because they question the nature of the n–2 task repetition cost and its usefulness as a measure of cognitive control, and they provide some evidence the n–2 task repetition cost can be explained with episodic retrieval.

It is not to say that after controlling for episodic retrieval, we can be certain that the remaining/ small residual costs (if present) are attributable to inhibition, instead, at this stage it is uncertain if they do, or what type of inhibition they may reflect. For example, it seems that residual n–2 task repetition costs are unlikely to be due to response conflicts/ inhibition because these are controlled by episodic matching of responses. Also, from the current thesis it cannot be ascertained whether the n–2 task repetition cost controlled for episodic retrieval is not an underestimated reflection of inhibition; that is, episodic matches may lead to a better performance, but it is not to say that inhibition is not applied during task-switching under episodic mismatches. For instance, episodic mismatches may lead to interference due to retrieved information not matching demands of a task at hand, which may require inhibition of irrelevant responses; which means that the standard n–2 task repetition cost may be a reflection of inhibition and episodic mismatches processing. Whereas when episodic retrieval is controlled for, there is less of a chance for interference due to retrieved information not matching the demands of a task at hand, hence less inhibition would be present. For now, it is concluded that episodic mismatches contribute largely to the n–2 task repetition cost increasing its magnitude, and that controlling for episodic retrieval can reduce as well as remove the n–2 task repetition cost; however, it is uncertain under what conditions the n–2 task repetition cost is entirely removed and when it is just reduced.

Residual n–2 task repetition cost's reliability. Experiment 2 from Chapter 3 also showed that when the n–2 task repetition cost is controlled for episodic retrieval, the reliability of the cost does not improve, which supports the recent reports about the standard n–2 task repetition cost's low reliability (Kowalczyk & Grange, 2017; Chapter 2). This means, that the n–2 task repetition cost not being a pure measure of cognitive inhibition is not the reason for the cost's low reliability as it was suggested in Chapter 2. There is still an issue that the n–2 task repetition cost's low reliability may be due to it being a difference score, as it was discussed earlier. Moreover, there are no studies reported that explored the controlled n–2 task repetition cost's reliability using rate residual scores or any other methods alternative to difference scores; therefore, for now it is uncertain if when the n–2 task repetition cost is controlled for episodic retrieval, the rate residual scores' reliability would be improved compared to the n–2 task repetition cost. For now, results from Experiment 2 further question the n–2 task repetition cost as a measure of individual differences in inhibitory control.

Cue-transparency. In Experiment 3 of Chapter 3, transparent and non-transparent cues were used to investigate whether the cue-transparency (Grange & Houghton, 2010b; Houghton et al., 2009) influences the n–2 task repetition cost or episodic retrieval. This is because there is evidence that the lower the cue-transparency is, the larger the n–2 task repetition cost is (Grange & Houghton, 2010a, 2010b; Houghton et al., 2009), which is taken as evidence that the non-transparent cues, unlike transparent cues, require more translation in WM leading to interference which is resolved by inhibition, as evidenced by larger n–2 task repetition costs (Grange & Houghton, 2010a, 2010b; Houghton et al., 2009). However, in light with evidence that the n–2 task repetition cost is highly modulated by episodic retrieval (Kowalczyk & Grange, 2017; Chapter 3), it was not certain whether and to what extent the episodic retrieval modulatory effect on the n–2 task repetition cost extended to processing of cues, which is an important aspect in task-switching and one that is likely to be sensitive to priming effects (Jost et al., 2013; Mayr & Kliegl, 2003).

The results from Experiment 3 showed that when comparing the n–2 task repetition cost controlled for episodic retrieval across the Cue-Types (i.e., transparent, Arrows vs. non-transparent, Shapes), the n–2 task repetition cost was absent under episodic matches regardless of the Cue-Type, but was present under episodic mismatches for both Cue-Types, with the n–2 task repetition cost being larger under non-transparent relative to transparent cues. This means that, when the n–2 task repetition cost is controlled for episodic retrieval, there seems to be no interference from non-transparent cues processing (i.e., no evidence of a conflict in WM), which implies that episodic retrieval is the likely source of the cost in the first place. When episodic retrieval is not controlled for, non-transparent cues lead to more interference than transparent cues, as shown previously by other researchers (Grange & Houghton, 2010b; Houghton et al., 2009), with the n–2 task repetition cost being larger under non-transparent cues relative to transparent cues. What that means is that episodic retrieval seems to drive the n–2 task repetition cost more than cue-transparency, because under episodic matches in non-transparent cues condition the n–2 task repetition cost was removed, which implies that cues, thanks to efficient retrieval, were likely not lead to interference, hence less if any inhibition was required/ present.

It is not to say that cue-transparency is not important for the n–2 task repetition cost; clearly it is, but its role seems different to the one originally suggested (Houghton et al., 2009). Specifically, when episodic retrieval is not controlled for, it appears that non-transparent cues make the retrieval less efficient leading to larger n–2 task repetition cost compared to transparent cues, which is likely to be due to weak cue-task associations leading to inaccurate information being retrieved, and/ or due to a failure of episodic retrieval which leads to information being processed in an “algorithmic” manner (Logan, 1988). That is, rather than performance being facilitated via an automatic retrieval of task’s parameters (i.e., via automatisation), an effortful process of identifying a cue and manipulating of a task-set in WM must take place, which over time can be replaced by automatic mechanisms given enough practice (Logan). Therefore, it is important for the future research to establish whether practice effects

(Grange & Juvina, 2015) can lead to the reduction of the n–2 task repetition cost under non-transparent cues when episodic retrieval is not controlled for.

Stimuli low-level features. Furthermore, Experiment 4 of Chapter 3 was interested whether low-level stimuli features affect the n–2 task repetition cost and/ or episodic retrieval. This is because, there is evidence from WM (Baddeley, 2000), episodic retrieval (Hommel, 2004), procedural learning (Pashler & Baylis, 1991), and task-switching research (Allport & Wylie, 2000; Koch & Allport, 2006; Schmidt & Liefooghe, 2016; Waszak et al., 2003) that perceptual features of stimuli can become bounded with other task's parameters (e.g., cue, response); as such, stimulus' features may be important for manipulating and retrieving information during task-switching. The results from Experiment 4 showed that the modulatory effect of episodic retrieval on the n–2 task repetition cost does not seem to be influenced by low-level stimulus features, because the costs were similar across the two conditions which manipulated stimulus matching (i.e., n–2 stimuli match, letter A presented on each trial vs. n–2 stimuli mismatch, different letters of alphabet presented on each trial).

However, the interpretation of the results from Experiment 4 is limited due to its design. For example, the stimuli-manipulation used varied stimuli features which were not task-relevant, that is, they were not necessary for task-switching performance. Also, letters used in Experiment 4 either always repeated or never repeated, therefore a comparison of n–2 stimulus switch compared to an n–2 stimulus repetition was not possible. This means that it is uncertain from this experiment whether if stimuli features became task-relevant, the stimuli-mismatching/ matching across ABA/ CBA sequences would affect the performance on task-switching; but based on research on episodic retrieval theory and priming in general (Neill, 1997; Schmidt & Liefooghe, 2016; Spapé & Hommel, 2008; Woodward & Meier, 2003) manipulation of task-relevant features would be expected to influence performance. That is, controlling for episodic retrieval of stimuli with task-relevant features should facilitate performance on task-switching. In terms of the n–2 task repetition cost, assuming that the cost is in general affected by episodic retrieval, it would be expected that, repeating task-relevant stimuli-features would facilitate performance and reduce the n–2 task repetition cost, compared to stimuli-switching.

Implications. With regards to practical implications of the modulatory effect of episodic retrieval on the n–2 task repetition cost (Grange et al., 2017), an important one is that interpretations of findings from many studies that used the cost as a measure of inhibitory control (Dreher et al., 2001; Fales et al., 2006; Lawo et al., 2012; Mayr et al., 2006; Moritz et al., 2004; Prior, 2012; Whitmer & Banich, 2007) are now likely to be incomplete or even inaccurate. That is, group differences thought to be due to inhibition—for example, larger n–2 task repetition costs in Parkinson's patients compared to control group interpreted as evidence for impaired inhibition in patients (Fales et al.)—are less clear now and should be revisited to establish to what extent the differences in n–2 task repetition costs can be ascribed to inhibition and/ or episodic retrieval. The same applies to individual differences research using the n–2 task repetition cost as a measure of inhibitory control. For instance, in one study higher rates of depressive rumination predicted smaller n–2 task repetition costs, which was interpreted by Whitmer and Banich as evidence for ruminative trait to be linked to impaired inhibition; however, since the n–2 task repetition cost is largely due to episodic retrieval mismatches, this interpretation is likely to be lacking.

There is another important implication of the modulatory effect of episodic retrieval on the n–2 task repetition cost. In the current thesis it was demonstrated that when episodic retrieval is controlled for, a reduction in an average n–2 task repetition cost, its removal, and/or n–2 task repetition benefits were present. This was also noted at an individual level as many participants showed n–2 task repetition benefits instead of n–2 task repetition costs, or showed no cost at all, which is something that has been observed by others in their research (e.g., Grange & Juvina, 2015). Also, it seems that the number of n–2 task repetition benefits increases when episodic retrieval is controlled for. This means that any model that attempts to explain n–2 task repetition cost should account for the role of episodic retrieval in task-switching facilitating/ impairing performance—as well as episodic retrieval leading to n–2 task repetition benefits in accuracy—and that not all participants show the costs associated with task-switching.

Existing models lead to different predictions; for example, some previous work (Grange et al., 2013) shows that activation-only model leads to n–2 task repetition benefits (which at the time was deemed a not suitable model of the cost), and a model which accounts for small amounts of inhibition can produce null n–2 task repetition cost (which showed that behavioral absence of inhibition does not mean that it was not present); but now it seems more plausible that a combination of both is more likely to explain the n–2 task repetition cost. Moreover, existing models of control in task-switching based on activation-decay (Altmann & Gray, 2008) and inhibition-decay

(Altmann, 2007; Mayr & Keele, 2000) will have to be reevaluated; for example, how is episodic retrieval in task-switching affected by activation-decay, and does residual n–2 task repetition cost benefit from extended response-cue intervals (Horoufchin et al., 2011)?

For now we know that the n–2 task repetition cost is greatly modulated by episodic retrieval, but it remains uncertain if and to what extent inhibition affects performance on task-switching, especially that there is evidence that episodic retrieval and inhibition are not mutually exclusive (Spapé & Hommel, 2008; Tipper, 2001). But contrary to the view that smaller n–2 task repetition costs reflect weaker/ impaired cognitive inhibition (e.g., Fales et al., 2006; Moritz et al., 2004; Whitmer & Banich, 2007; Whitmer & Gotlib, 2012), it is concluded from this thesis that since participants (which represented a healthy university population) performed task-switching efficiently and at high accuracy regardless of whether they showed n–2 task repetition costs or benefits, they had no impairment of cognitive control. Instead, it is likely that a behavioral lack of n–2 task repetition costs does not mean that inhibition was not deployed (Grange et al., 2013), and that it is more likely that n–2 task repetition cost can reflect a combination of inhibitory and non-inhibitory effects which can manifests as overall costs or benefits.

Working Memory

Chapter 4 examined whether individual differences in the n–2 task repetition cost can be explained with WM capacity (WMC) or be modulated by WL load (WML). Inhibition has been shown to depend on WM resources (Conway & Engle, 1994; Engle, 1996), and with the n–2 task repetition cost assumed to reflect inhibition, one of the aims of the current thesis was to examine if and to what extent this cost depends on WM. With a considerable amount of research supporting the importance of WM in task-switching (Goschke, 2000; Grange & Juvina, 2015; Grange et al., 2013; Houghton et al., 2009; Logan, 1988; Mayr & Kliegl, 2000, 2003; Meiran & Kessler, 2008; Miyake et al., 2004; Saeki & Saito, 2004b, 2004a), it was likely that the n–2 task repetition cost would be resource-dependent. At the point of this thesis being conducted there was one unpublished study that reports investigation between the n–2 task repetition cost and WM (Grange & Houghton, n.d.), and another study was published (Pettigrew & Martin, 2015) after this thesis began. Both of those studies provided evidence against the view that the n–2 task repetition cost is resource-dependent; however, they both had limitations which were addressed by the current thesis. The n–2 task repetition cost's relationship with WM was investigated by assessing WMC with three measures (Foster et al., 2014), using the n–2 task repetition cost controlled for episodic retrieval (Grange et al., 2017)—rather than the standard n–2 task repetition cost used by previous studies—and using two different WML manipulations. Together, Experiments 5–7 showed consistently that the n–2 task repetition cost—whether controlled for episodic retrieval or not—is not dependent on WM resources; that is, n–2 task repetition costs were not linked to individual differences in WMC, or modulated by WML.

Taking into account only statistically significant results, the findings from Chapter 4 are consistent with other studies that researched the relationship between the n–2 task repetition cost and WM (Grange & Houghton, n.d.; Pettigrew & Martin, 2015), which is that they are not related. That is, WMC does not predict the n–2 task repetition cost (whether controlled for episodic retrieval or not) and the n–2 task repetition cost is not modulated by WML.

WMC. In terms of individual differences, at this stage it is uncertain whether the n–2 task repetition cost controlled for episodic retrieval does not reflect cognitive inhibition, or if it does but it is not resource-dependent. Also, in terms of individual differences, there is also the issue of the n–2 task repetition cost being a difference score—argued to be not very reliable (Crawford et al., 2008; Kopp, 2011)—which means that the lack of the relationship between WMC and the cost could be attributable to the cost being a difference scores. Also, assuming that residual n–2 task repetition costs reflect inhibition, the results and existing literature point to another likely alternative explanation on why the residual n–2 task repetition cost and WM are not linked; specifically, the residual cost being due to automatic (i.e., lateral) rather than a top-down inhibition (Gade & Koch, 2005, 2007; Houghton et al., 2009; Schneider & Verbruggen, 2008; Schuch & Koch, 2003), or arising as a result of self-inhibition (Grange et al., 2013; Koch et al., 2010).

WML. In terms of WML and the n–2 task repetition cost, it is important to add that manipulating WML seemed to have affected the modulatory effect of episodic retrieval on the n–2 task repetition cost (i.e., the cost was not modulated by it); that is, manipulating WML might have interfered with how efficient episodic retrieval was. As such, knowing that in previous experiments in this thesis the n–2 task repetition cost is modulated by episodic retrieval, interpreting the lack of effect of WML on n–2 task repetition costs is difficult. This is because the results show that we may have to understand how WML interacts with episodic retrieval in task-switching first, before we attempt to reinvestigate the relationship between the n–2 task repetition cost controlled for episodic retrieval and WM.

Despite no statistical evidence for WM being influencing the n–2 task repetition cost, some observations based on numerical trends can be made. If the n–2 task repetition cost controlled for episodic retrieval reflects inhibition, and that inhibition depends on WM, we would expect the cost under episodic matches to be more affected by WML than under episodic mismatches, a pattern which is observed in Experiments 6–7, although not statistically significant. That is, it appears that under episodic mismatches the n–2 task repetition cost is comparable for high and low WML for RT and accuracy, but more importantly, there was a numerical trend for the smallest n–2 task repetition costs to be observed under high WML under episodic matches. This is as predicted by the theory that inhibition is resource dependent. On the other hand, if WM was important for episodic retrieval more than inhibition—because retrieved task-set information can be assumed to be manipulated in WM—we would expect n–2 task repetition costs not controlled for episodic retrieval to be more dependent on WM than the cost controlled for episodic retrieval. There is some evidence from the current thesis to support that hypothesis. In Experiment 5 accuracy of the n–2 task repetition cost not controlled for episodic retrieval was significantly negatively correlated with WMC, with higher WMC scores linked to smaller accuracy n–2 task repetition cost. This can be interpreted as evidence that individuals with higher WMC were more accurate retrieving and manipulating task's parameters and matching them to the current task's demands compared to those who had smaller WMC. However, this correlation did not remain significant after adjusting for multiple testing, therefore these results must be interpreted with caution. Nevertheless, as mentioned earlier, n–2 task repetition costs not controlled for episodic retrieval were in general similar under both high and low WML (for RT and accuracy performance), which suggests performance under episodic mismatches was not affected by manipulation of WML; therefore, overall, the results seem inconsistent.

Overall, it can be concluded that based on Experiments 5–7 the n–2 task repetition cost is not dependent on WM, and further investigation on how manipulation of WML affects episodic retrieval and/ or inhibition in task-switching should be carried out to help to ascertain whether it is the episodic retrieval and/ or inhibition that are influenced by WM.

Perceptual Load

Chapter 5 looked at the n–2 task repetition cost, attentional capacity/ resources (i.e., perceptual load, Lavie, 1995; Lavie & Fox, 2000) and distractibility traits (Broadbent et al., 1982; Forster & Lavie, 2009b). With the n–2 task repetition cost not being linked to WM, attentional capacity was identified as another potential cognitive resource hypothesised to influence inhibition. Attentional capacity (Broadbent, 1958; Lavie, 1995) is important in selective attention, specifically it plays a role in how task-relevant information is processed against task-irrelevant information. Perceptual processing happens automatically but how much is perceived and processed depends on attentional capacity, which as WM, is limited (Lavie, 1995; Lavie & Tsal, 1994).

Attentional resources are typically manipulated via varying perceptual load with the use of distractors (Cave & Chen, 2016; Forster & Lavie, 2009b; Rorden et al., 2008; Wilson et al., 2011), which can be present without researcher's intention in the form of task-unrelated-thoughts (TUTs, Forster & Lavie, 2009a). In the current thesis, in Experiments 8–9 perceptual load was manipulated within the BI paradigm by incorporating distractors to create HPL (high perceptual load) condition, and the BI paradigm without distractors was used as the LPL (low perceptual load) condition. In Experiment 8 participants searched for the target among distractors that looked similar to the target, and in Experiment 9 distractors in the form of small coloured circles presented around the cue were used.

The important aspect of perceptual load research that is relevant to the n–2 task repetition cost is that distractors are processed differently under LPL relative to HPL; under LPL distractors have been shown to be processed and may interfere with the task at hand, whereas under HPL due to attentional resources being exhausted, task-relevant stimuli are predominantly processed and distractors less if at all (Lavie & Tsal, 1994). Importantly for the current thesis, under HPL relative to LPL, perceptual load have been shown to reduce cognitive control effects such as negative priming, the Stroop effect, and response competitor (Forster & Lavie, 2008a; Kahneman & Chajczyk, 1981; Lavie & Fox, 2000; Rorden et al., 2008). Therefore, it was speculated that if the n–2 task repetition cost (controlled for episodic retrieval) reflects inhibition, it was likely to be modulated by perceptual load. Moreover, since the n–2 task repetition cost has been shown to be modulated by episodic retrieval (Grange et al., 2017; Chapter 3), it was of interest to examine whether perceptual load would affect episodic retrieval and/ or inhibition; that is, it was of interest whether distractors under different perceptual loads would become part of memory traces and be retrieved interfering with performance (Lavie, 2000; p.1050). There seems to be no reports in literature on the relationship between the n–2 task repetition cost and attentional resources manipulated with perceptual load, therefore this is the first investigation of this kind.

Overall, the results were rather inconsistent. That is, the perceptual load manipulations were in general successful; that is, in Experiment 8 performance was less accurate under HPL compared to LPL, and in Experiment 9 performance was less accurate and slower under HPL relative to LPL. Since the n–2 task repetition cost was used as a measure of inhibition, it is unclear whether inhibition was modulated by perceptual load. This is because in accuracy, episodic retrieval modulatory effect led to n–2 task repetition benefits under both HPL and LPL; that is, n–2 task repetition benefits were not affected by perceptual load. In RT performance, the episodic retrieval modulatory effect on the n–2 task repetition cost was present only in Experiment 9, and the residual n–2 task repetition cost was not affected by perceptual load; that is, the residual cost was similar under HPL and LPL.

However, in Experiment 8, statistically RT n–2 task repetition cost was not modulated by episodic retrieval, but the log-RT the n–2 task repetition cost was, as it was smaller under HPL compared to LPL. Also, there was a numerical trend across Experiments 8–9 for the n–2 task repetition costs not controlled for episodic retrieval to be smaller under HPL vs. LPL. Therefore, for the n–2 task repetition cost not controlled for episodic retrieval, it is not clear whether it was episodic retrieval or inhibition that was being influenced by perceptual load. Despite the limited support in the form of statistical evidence, the overall observations from Experiments 8–9 is that we cannot rule out perceptual load as a factor that may influence n–2 task repetition costs. It seems that when episodic matches are ensured, perceptual load does not influence n–2 task repetition costs/ benefits, but n–2 task repetition costs confounded by episodic mismatches seem to be affected by it. That is, n–2 task repetition costs confounded by episodic mismatches were smaller under HPL compared to LPL; this suggests that, HPL created conditions under which less interference was caused by episodic mismatches or less inhibition was deployed because of those mismatches, compared to LPL.

In terms of individual differences in inhibition (as measured with n–2 task repetition costs controlled for episodic retrieval), it was predicted that in LPL condition people highly distractible day-to-day—as measured with Cognitive Failures Questionnaire (CFQ; Broadbent et al., 1982)—would have stronger inhibition compared to people who are not very distractible in day-to-day life, and that these differences would be diminished under HPL (Forster & Lavie, 2009b). The results showed that n–2 task repetition costs (controlled for episodic retrieval and not controlled) were not predicted by day-to-day distractibility trait.

However, at this stage it is not certain whether residual n–2 task repetition cost does not reflect inhibition or whether inhibition in task-switching in not linked to the day-to-day distractibility trait. That is, for n–2 task repetition modulated by episodic retrieval, RT n–2 task repetition costs did not differ between perceptual load conditions, and in accuracy n–2 task repetition benefits were present. That is, in accuracy performance no residual n–2 task repetition costs—thought to measure inhibition—were present, and in RT performance, even though the residual n–2 task repetition cost was present, it did not differ between perceptual load conditions. Therefore, it can be speculated that n–2 task repetition benefits are unlikely to reflect inhibition hence were not linked to the distractibility trait; and in terms of the residual n–2 task repetition cost, since it did not differ between perceptual loads, is also unlikely to reflect inhibition, because by perceptual load theory, inhibitory effects should differ between perceptual load conditions (Forster & Lavie, 2008; Kahneman & Chajczyk, 1981; Lavie & Fox, 2000; Rorden et al., 2008).

Also, another candidate explaining the lack of relationship between n–2 task repetition costs and the CFQ is the lack of the reliability of the n–2 task repetition cost—controlled and not controlled for episodic retrieval—which has been documented recently (Kowalczyk & Grange, 2017; Chapter 2 & 3: Experiment 1). Moreover, the n–2 task repetition cost is a difference score which are difficult to interpret and are often not reliable (Crawford et al., 2008), therefore it is not certain whether inhibition in task-switching is not resource-dependent or whether inhibition is just not measured well with n–2 task repetition costs.

Computational Modelling

Chapter 6 adopted alternative methods to analyse the n–2 task repetition cost to explore to what extent this effect is due to inhibition, and to what extent it can be attributed to non-inhibitory factors (i.e., episodic retrieval). Central tendencies measures (e.g., mean RTs and mean accuracy) and dispersion parameter (e.g., standard deviation), which are used in task-switching research, are not optimally informative due to RT trimming and output averaging (Balota & Yap, 2011; Heathcote et al., 1991; Hervey et al., 2006; Whelan, 2008). Therefore, ex-Gaussian (Heathcote et al., 1991) and diffusion modelling (Ratcliff & McKoon, 2008) were used on three pre-collected data sets and one data set collected specifically for Chapter 6 to gain a better understanding of the n–2 task repetition cost, and how episodic retrieval affects it.

Consistently with the literature (Grange & Juvina, 2015; Schuch & Konrad, 2017), ex-Gaussian modelling showed a tendency for n–2 task repetition costs to be the largest in the tau parameter, typically interpreted as evidence for the costs linked to executive functions (Schmiedek et al., 2007; Shahar & Meiran, 2014), and to slower RTs and less prepared responses (DeJong, 2000a). However, n–2 task repetition costs were also present in the mu and sigma parameters, which is more difficult to interpret but has been noted before (Schuch & Konrad, 2017). When episodic retrieval was controlled for, the tau and mu parameters were modulated to some extent (i.e., not in all data sets). In terms of the tau parameter, n–2 task repetition costs were absent from episodic matches relative to episodic mismatches, and assuming that effects in the tau parameters reflect executive functions processes (Grange & Houghton, 2011; Hervey et al., 2006; Schmiedek et al., 2007; Schuch & Konrad, 2017; Shahar & Meiran, 2014; Shahar et al., 2014; Spieler & Balota, 1996; Steinhauser & Hübner, 2009; Tse et al., 2010) the results can be interpreted as evidence that either tau can reflects automatic processes as well as executive functions or that episodic retrieval influences how information is processed within executive functions. For the mu parameter, n–2 task repetition costs were much smaller under episodic matches compared to episodic mismatches, and since the mu parameter has been linked to response mechanisms (Spieler et al., 1996), these results can be taken as evidence that episodic retrieval mismatches drive the n–2 task repetition cost in mu by creating a response conflict. And in terms of the sigma parameter, there were no substantial differences for n–2 task repetition costs in sigma for episodic matches and episodic mismatches.

In terms of diffusion modelling results, as it was predicted, the standard n–2 task repetition cost was predominantly present in drift rate, which supports the view that this cost is due to carry-over or interference effects, with the likely candidate being inhibition (Schmitz & Voss, 2012, 2014); specifically, the drift rate was smaller for ABA sequence compared to CBA sequence, and smaller drift rates are linked with less efficient and slower information accumulation (Schmiedek et al., 2007). The n–2 task repetition cost was also present in the threshold separation parameter for the three out of four data sets, with larger threshold separation parameter for ABA compared to CBA sequences; this is likely to be due to more conservative responses in ABA relative to the CBA sequences, as larger threshold separation parameter typically indicates slower but more accurate responses. In terms of the non-decisional parameter, there were no significant differences between ABA and CBA across all four data sets, which indicates that in all four data sets information was equally well encoded and responses were equally efficiently executed.

However, when episodic retrieval was controlled for, the n–2 task repetition cost was reduced or absent from the drift parameter. This finding is taken as evidence that the n–2 task repetition cost in the drift rate is unlikely to reflect cognitive inhibition and instead is more likely to be a result of non-inhibitory mechanism, specifically episodic retrieval mismatches. That is, the observation of larger drift rates for episodic matches compared to episodic mismatches, imply that under the former one information accumulation was more efficient, probably because episodic matches—unlike episodic mismatches—do not lead to interference, but rather facilitate performance leading to faster information accumulation, which improves the relative task readiness (Weeda et al., 2014).

In terms of the threshold parameter, episodic retrieval did not seem to affect it because n–2 task repetition costs in the threshold parameter did not differ between episodic matches compared to episodic mismatches; this is consistent with the assumption that episodic retrieval is automatic and it should not lead to bias in threshold separation. And, in relation to non-decisional parameter, for two out of the four data sets there was a trend for this parameter to be smaller for ABA compared to CBA under episodic matches, but for episodic mismatches, the non-decisional parameter was smaller for CBA compared to ABA sequences. The non-decisional parameter is associated with information encoding, motor response, preparation, and WM configuration (Schmitz & Voss, 2012, 2014; Weeda et al., 2014). In the current investigation, information encoding and motor responses are assumed to not differ between ABA and CBA sequences across the four data sets, which leaves preparation and WM reconfiguration as possible sources of smaller non-decisional parameter in ABA compared to CBA sequences. It is unlikely that there were preparation differences across task-sequences because identical preparatory interval (i.e., the cue-stimulus-interval) was used across the four studies, which leaves WM reconfiguration as a likely source of differences observed in non-decisional parameter. That is, assuming that retrieved information is manipulated in WW, episodic matches do not lead to conflicts during response selection because retrieved response matches the task demands (smaller non-decisional parameter and faster performance on ABA compared to CBA sequences), but episodic mismatches result in conflicts in WM because required response is different to the one retrieved (larger non-decisional parameter and slower performance on ABA compared to CBA sequences).

The main implication of findings from Chapter 6 is that its evidence further questions the validity of the n–2 task repetition cost as a measure of cognitive inhibition. That is, consistently with previous literature (Schuch, 2016; Schuch & Konrad, 2017) the n–2 task repetition cost was predominantly present in the drift rate, however in half of the Simulation Studies the cost was modulated by episodic retrieval (a pattern observed in other data sets too). This means that the n–2 task repetition cost in drift rate—previously interpreted as evidence of inhibitory processes (Schuch, 2016; Schuch & Konrad, 2017)—is likely to be driven by non-inhibitory, automatic mechanisms, in this case episodic retrieval mismatches. Moreover, effects seen in non-decisional parameter would imply further that the n–2 task repetition cost is largely driven by non-inhibitory and automatic processes. In terms of ex-Gaussian modelling, despite some results being consistent with existing literature (e.g., n–2 task repetition costs seen in the tau parameter), overall, conclusions are difficult to draw due to no specific pattern of episodic retrieval on the tau parameter, and n–2 task repetition costs seen in mu and sigma parameters.

Conclusions

The current thesis provides novel insights into the n–2 task repetition cost which have theoretical and practical implications on what the cost is thought to represent. It was established that the n–2 task repetition cost is modulated by a non-inhibitory mechanism, specifically episodic retrieval; that is, controlling for episodic retrieval considerably reduces the n–2 task repetition cost, and in some instances removes the cost or leads to n–2 task repetition benefits. Together with computational modelling results, this is taken as evidence that it is unlikely that the n–2 task repetition cost is a measure of inhibition; instead, it seems that the cost reflects processing of mismatches in episodic retrieval. Moreover, the n–2 task repetition cost has been shown to have low reliability at an individual level, and as such should be avoided as a measure of individual differences in inhibitory control. In terms of individual differences, the n–2 task repetition cost is not predicted by WMC, processing speed, depressive rumination, or day-to-day distractibility. It is not to say that these individual differences are not linked to inhibition, instead it is more likely that the n–2 task repetition cost is not a good measure of inhibition. And lastly, the n–2 task repetition cost does not seem to depend on WM resources but as it stands can be influenced by attentional resources to some extent. Altogether, based on results from the current thesis, it is concluded that the n–2 task repetition cost is unlikely to reflect cognitive inhibition, and if used as such should be interpreted with caution.

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Appendices

Appendix A

A.1 Digit-Symbol Substitution Task

1	2	3	4	5	6	7	8	9
S	D	B	P	E	C	Q	T	G

5	8	2	9	6	3	8	2	8	4	7	2	6	3	3	6	7	4	5	2

8	9	9	7	1	9	7	8	5	6	4	2	7	5	3	7	8	6	3	5

4	7	5	7	2	7	9	2	2	6	9	8	1	8	2	7	1	7	6	6

4	8	4	3	8	4	3	9	2	2	2	3	5	5	3	9	1	7	3	6

7	4	6	1	9	6	8	1	7	4	9	1	2	1	1	3	6	2	5	6

5	9	4	4	8	9	8	3	5	3	4	8	3	9	4	1	3	1	7	6

3	2	1	8	5	1	9	4	5	1	8	1	6	5	2	9	5	4	6	9

A.2 Ruminative Response Scale

Please read each statement carefully. I would like to stress that no assumptions of existing depression (or other such problems) are made. Therefore, some statements in this questionnaire may be more relevant to you than others, or they may not be relevant at all. After reading each statement decide how often you do things described in each statement:

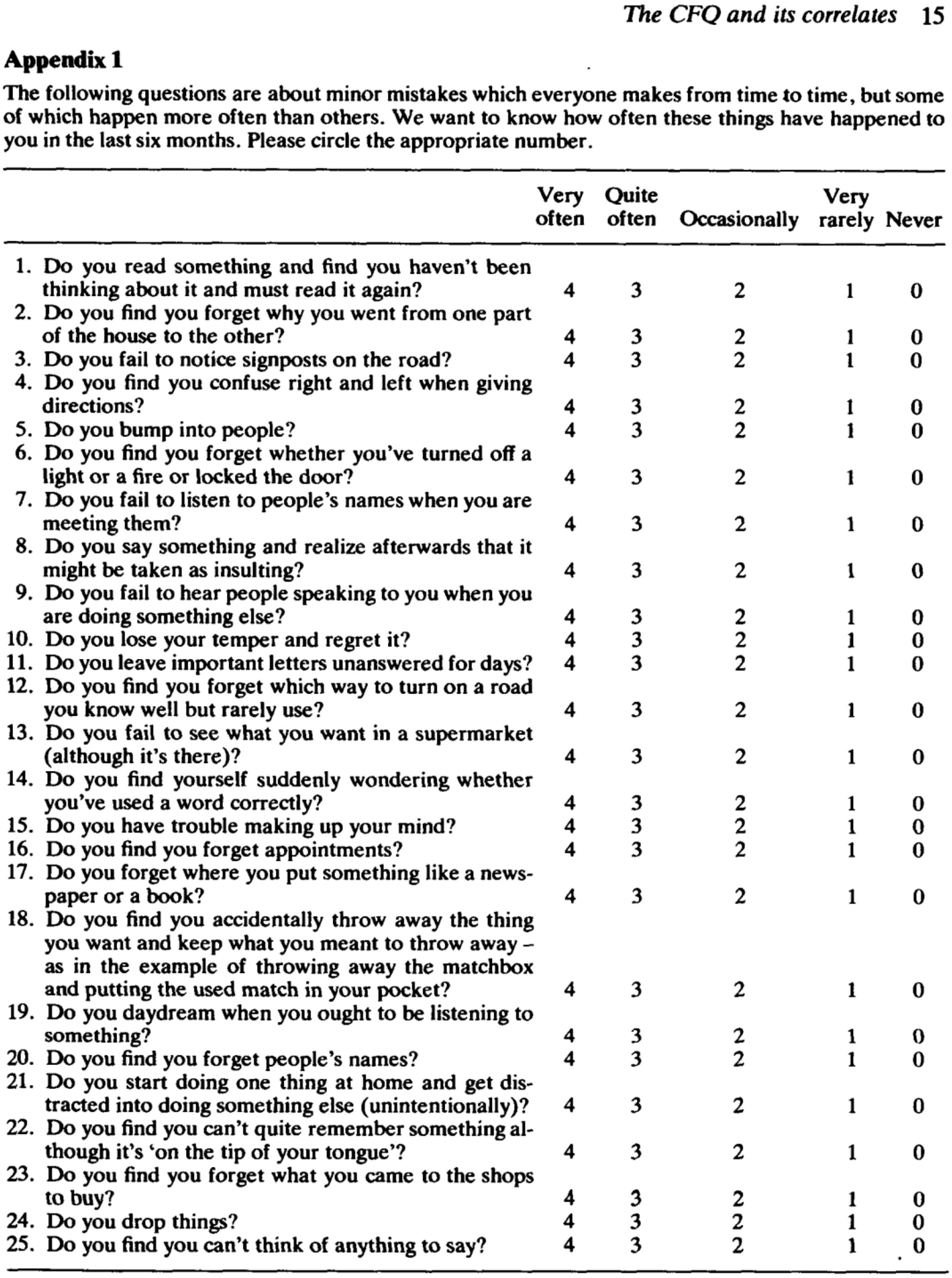
1. almost never 2.sometimes 3.often 4.almost always

Circle the answer that best reflects your answer.

This questionnaire is used for non-diagnostics purpose, which means that when questionnaire is scored, no diagnosis will be given, and you will not be contacted regarding your score. If you are concerned about how you answer questions on this measure, please consult your GP or services which details are provided in the De-brief Form. Your data will be kept anonymous and confidential.

1	Think “What am I doing to deserve this?”	1	2	3	4
2	Analyze recent events to try to understand why you are depressed	1	2	3	4
3	Think “Why do I always react this way?”	1	2	3	4
4	Go away by yourself and think about why you feel this way	1	2	3	4
5	Write down what you are thinking and analyse it	1	2	3	4
6	Think about a recent situation, wishing it had gone better	1	2	3	4
7	Think “Why do I have problems other people don’t have?”	1	2	3	4
8	Think “Why can’t I handle things better?”	1	2	3	4
9	Analyse your personality to try to understand why you are depressed	1	2	3	4
10	Go someplace alone to think about your feelings	1	2	3	4

A.3 Cognitive Failures Questionnaire



Appendix 1

The following questions are about minor mistakes which everyone makes from time to time, but some of which happen more often than others. We want to know how often these things have happened to you in the last six months. Please circle the appropriate number.

	Very often	Quite often	Occasionally	Very rarely	Never
1. Do you read something and find you haven't been thinking about it and must read it again?	4	3	2	1	0
2. Do you find you forget why you went from one part of the house to the other?	4	3	2	1	0
3. Do you fail to notice signposts on the road?	4	3	2	1	0
4. Do you find you confuse right and left when giving directions?	4	3	2	1	0
5. Do you bump into people?	4	3	2	1	0
6. Do you find you forget whether you've turned off a light or a fire or locked the door?	4	3	2	1	0
7. Do you fail to listen to people's names when you are meeting them?	4	3	2	1	0
8. Do you say something and realize afterwards that it might be taken as insulting?	4	3	2	1	0
9. Do you fail to hear people speaking to you when you are doing something else?	4	3	2	1	0
10. Do you lose your temper and regret it?	4	3	2	1	0
11. Do you leave important letters unanswered for days?	4	3	2	1	0
12. Do you find you forget which way to turn on a road you know well but rarely use?	4	3	2	1	0
13. Do you fail to see what you want in a supermarket (although it's there)?	4	3	2	1	0
14. Do you find yourself suddenly wondering whether you've used a word correctly?	4	3	2	1	0
15. Do you have trouble making up your mind?	4	3	2	1	0
16. Do you find you forget appointments?	4	3	2	1	0
17. Do you forget where you put something like a newspaper or a book?	4	3	2	1	0
18. Do you find you accidentally throw away the thing you want and keep what you meant to throw away – as in the example of throwing away the matchbox and putting the used match in your pocket?	4	3	2	1	0
19. Do you daydream when you ought to be listening to something?	4	3	2	1	0
20. Do you find you forget people's names?	4	3	2	1	0
21. Do you start doing one thing at home and get distracted into doing something else (unintentionally)?	4	3	2	1	0
22. Do you find you can't quite remember something although it's 'on the tip of your tongue'?	4	3	2	1	0
23. Do you find you forget what you came to the shops to buy?	4	3	2	1	0
24. Do you drop things?	4	3	2	1	0
25. Do you find you can't think of anything to say?	4	3	2	1	0

Appendix B

B.1 Ex-Gaussian parameters n–2 task repetition costs graph

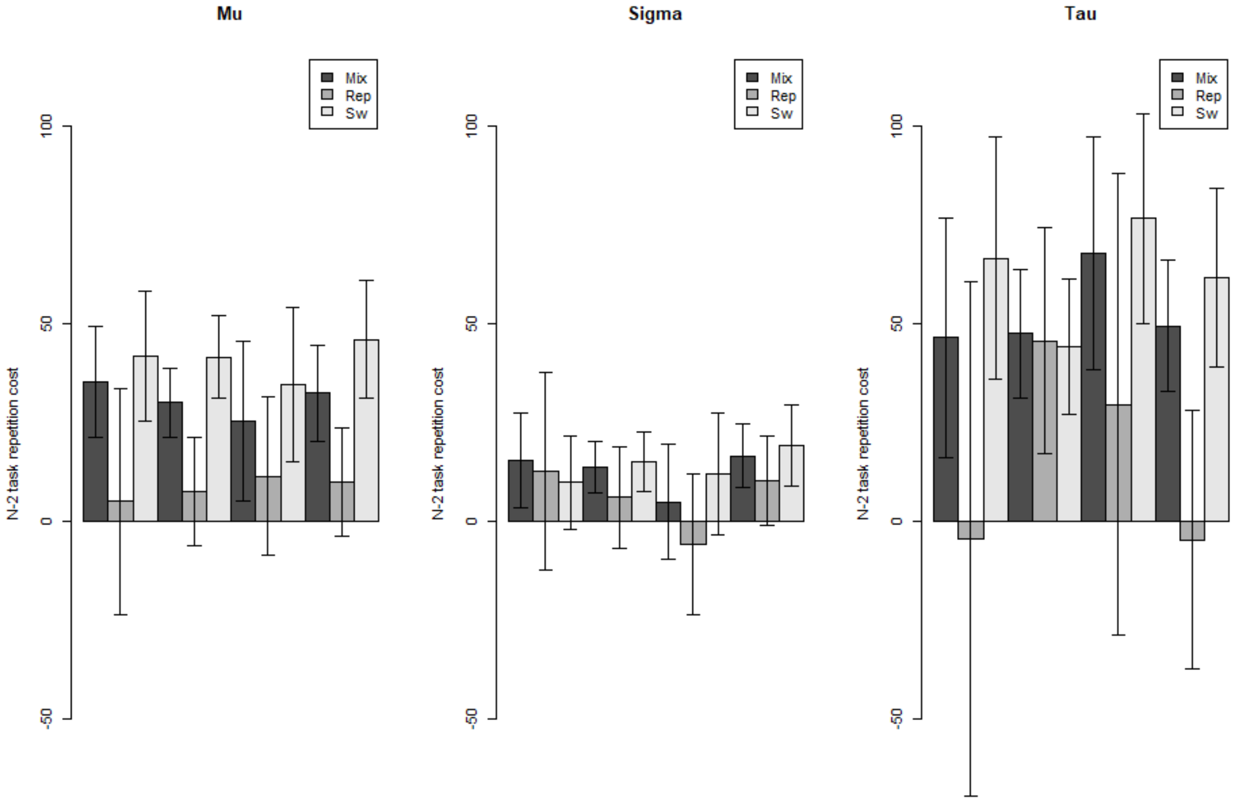


Figure A.1 A visualisation of the n–2 task repetition cost for ex-Gaussian parameters for the standard n–2 task repetition cost (Mix), under episodic matches (Rep), and under episodic mismatches (Sw).

B.2 Diffusion modelling parameters n–2 task repetition costs

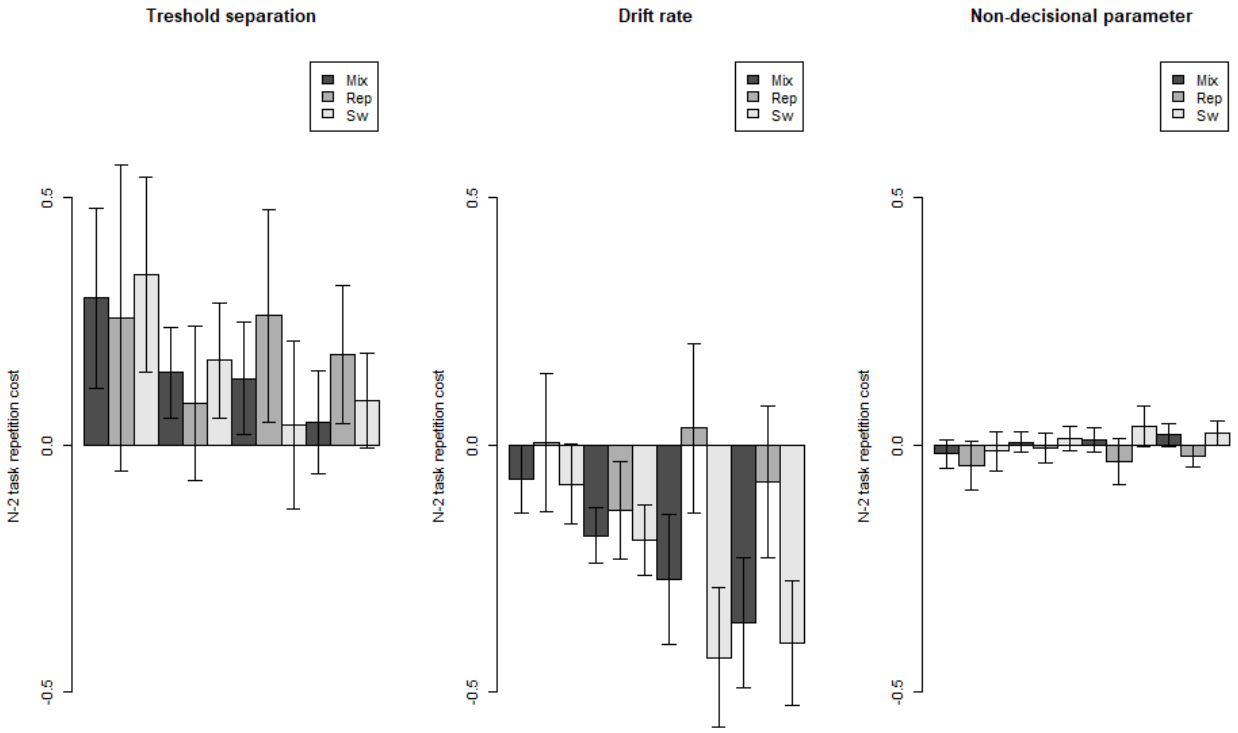


Figure A.2 A visualisation of the n–2 task repetition cost for diffusion modelling parameters for the standard n–2 task repetition cost (Mix), under episodic matches (Rep), and under episodic mismatches (Sw).

Appendix C

Ref: ERP347
24th September 2015
Agnieszka Kowalczyk
Research Institute of Social Sciences Room CBA
2.018
Chancellor's Building
Keele University

Dear Agnieszka

Re: Individual Differences in Inhibitory Control during Task Switching

Thank you for submitting your revised application for review. I am pleased to inform you that your application has been approved by the Ethics Review Panel. The panel would like to commend you for your careful attention to their requests.

The following documents have been reviewed and approved by the panel as follows:

Document	Version	Date
Summary Document	2	10/07/2015
Study Poster	1	10/07/2015
Information Sheet	3	20/08/2015
Consent Form	2	10/07/2015
Debrief Form	2	10/07/2015
Tasks 1, 2 & 3	1	27/05/2015
Ruminative Research Scale	2	28/08/2015
Digital Symbol Substitution Task	1	27/08/2015

If the fieldwork goes beyond the date stated in your application 31st August 2016, you must notify the Ethical Review Panel via the ERP administrator at uso.erps@keele.ac.uk stating **ERP3** in the subject line of the e-mail.

If there are any other amendments to your study you must submit an 'application to amend study' form to the ERP administrator stating **ERP3** in the subject line of the e-mail. This form is available via http://www.keele.ac.uk/researchsupport/researchethics/

If you have any queries, please do not hesitate to contact me via the ERP administrator on uso.erps@keele.ac.uk stating **ERP3** in the subject line of the e-mail.

Yours sincerely

pp C H Bennerman

Dr Helena Priest
Chair – Ethical Review Panel
CC RI Manager
Supervisor

Directorate of Engagement & Partnerships
T: +44(0)1782 734467

Keele University, Staffordshire ST5 5BG, UK
www.keele.ac.uk +44 (0)1782 732000

Ref: ERP2322

15th February 2017

Agnieszka Kowalczyk
School of Psychology
Dorothy Hodgkin Building
Keele University

Dear Agnieszka,

Re: Task Switching and Perceptual Load

Thank you for submitting your revised application for review. I am pleased to inform you that your application has been approved by the Ethics Review Panel.

The following documents have been reviewed and approved by the panel as follows:

Document(s)	**Version Number**	**Date**
Information Sheet	2	13-02-2017
Consent Form	1	17-01-2017
De-brief Form	2	10-02-2017
CFQ	1	11-01-2017
Task Switching Paradigm	1	14-01-2017

If the fieldwork goes beyond the date stated in your application, **31st August 2017**, or there are any other amendments to your study you must submit an ‘application to amend study’ form to the ERP administrator at research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail. This form is available via http://www.keele.ac.uk/researchsupport/researchethics/

If you have any queries, please do not hesitate to contact me via the ERP administrator on research.governance@keele.ac.uk, stating **ERP2** in the subject line of the e-mail.

Yours sincerely

pp C H Bannerman

Dr Colin Rigby
Chair – Ethical Review Panel

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Keele
University

Ref: ERP2322

9th March 2017

Agnieszka Kowalczyk
School of Psychology
Dorothy Hodgkin Building
Keele University

Dear Agnieszka,

Re: Task Switching and Perceptual Load

Thank you for submitting your application to amend study, informing us that you will be using an alternative form of reimbursement for participants. I am pleased to inform you that your application has been approved by the Ethical Review Panel.

The following document have been reviewed and approved by the Panel as follows:-

Document	Version	Date
Participant Information sheet	3	07-03-2017
Receipt Form	1	07-03-2017

Just to remind you, if the fieldwork goes beyond the **31st August 2017**, or there are any other amendments to your study you must submit an 'application to amend study' form to the ERP administrator at research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail. This form is available via http://www.keele.ac.uk/researchsupport/researchethics/

If you have any queries, please do not hesitate to contact me via the ERP administrator on research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail.

Yours sincerely

pp C H Bannerman

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Keele
University

Ref: ERP2322

25th May 2017

Agnieszka Kowalczyk
School of Psychology
Dorothy Hodgkin Building
Keele University

Dear Agnieszka,

Re: Task Switching and Perceptual Load

Thank you for submitting your application to amend study, informing us that you have amended your application to include all Keele University students. I am pleased to inform you that your application has been approved by the Ethical Review Panel.

The following document have been reviewed and approved by the Panel as follows:-

Document	Version	Date
Participant Information sheet	4	17-05-2017
Poster	1	17-05-2017

Just to remind you, if the fieldwork goes beyond the **31st August 2017**, or there are any other amendments to your study you must submit an 'application to amend study' form to the ERP administrator at research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail. This form is available via http://www.keele.ac.uk/researchsupport/researchethics/

If you have any queries, please do not hesitate to contact me via the ERP administrator on research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail.

Yours sincerely

pp C H Bannerman

Dr Colin Rigby
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Keele
University

Ref: ERP2322

30th August 2017

Agnieszka Kowalczyk
School of Psychology
Dorothy Hodgkin Building
Keele University

Dear Agnieszka,

Re: Task Switching and Perceptual Load

Thank you for submitting your application to amend study (3rd request), informing us of an extension to your project end date from the 31st August 2017 to the **31st October 2017** and various other changes. I am pleased to inform you that your application has been approved by the Ethical Review Panel.

The following document has been reviewed and approved by the Panel as follows:-

Document	Version	Date
Participant Information sheet	5	30-07-2017

Just to remind you, if the fieldwork goes beyond the **31st October 2017**, or there are any other amendments to your study you must submit an 'application to amend study' form to the ERP administrator at research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail. This form is available via http://www.keele.ac.uk/researchsupport/researchethics/

If you have any queries, please do not hesitate to contact me via the ERP administrator on research.governance@keele.ac.uk stating **ERP2** in the subject line of the e-mail.

Yours sincerely

pp C H Bannerman

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