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On the origin of a response time underadditivity by means of cross-modal task switching, or the redundancy of operations in the configuration of task sets for cross- modal shifts

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On the Origin of a Response Time Underadditivity by means of Cross-modal Task Switching, or the Redundancy of Operations in the Configuration of Task Sets for Cross-modal Shifts

A dissertation submitted in partial satisfaction of the requirements for the degree of Doctor of Philosophy

in

Psychology

by

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The dissertation of Michael Colin Ard is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2009

DEDICATION:

To Dr. F. Michael Ard, Mary Jo Ard, Cameron Ard, and Jenny Lin,
as well as the many more who I have been fortunate to call my friends,
both in recent years and in times long past.

Your beauty, your love, and your belief in me give me a reason,
and the courage, to challenge myself.

TABLE OF CONTENTS

LIST OF FIGURES AND TABLES

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ABSTRACT OF THE DISSERTATION

On the Origin of a Response Time Underadditivity by means of Cross-modal Task Switching, or the Redundancy of Operations in the Configuration of Task Sets for Cross-modal Shifts

by

Michael Colin Ard

Doctor of Philosophy in Psychology

University of California, San Diego, 2009

Professor Timothy C. Rickard, Chair

Despite a growing body of research into task switching phenomena, little is known of the interactions between the performance decrements that result from shifting task sets and shifting stimulus modalities. Seven experiments are presented which replicate a previously documented underadditivity of task and modality switch costs, and extend the known limits and behavior of this underadditivity by showing that the cost of switching tasks can be reversed under cross-modal stimulus conditions. Cross-modal task switch costs and benefits are also shown to vary widely as a function of experimental protocols, the tasks subjects perform, and the degree of cross-modal overlap in imperative stimuli. Active preparation in response to modality predictive cues was not observed to result in differential reductions in cross-modal task switch costs, contrary to suggestions made by other researchers.

CHAPTER 1

Psychology has long sought a deeper understanding of the manner in which the functioning of the mind gives rise to the vast array of behaviors human beings habitually engage in, from simple to complex tasks, language to arithmetic, and creative problem solving to musical expression. It is only more recently that researchers in the field have taken up en masse the question of whether and to what extent cognitive processing may be affected by a transition from the performance of one task to another. Indeed, a scant 10 years ago anyone interested in undertaking a program of research exploring the phenomena that typically arise when task switches are undertaken would have encountered a literature with perhaps fewer than 10 papers of note dealing explicitly with the subject. In the years since this fledgling domain has become one of the most fertile research areas in cognitive psychology, complete with ongoing discovery of new and instructive phenomena, and continued theoretical development and refinement with potent implications for the understanding of executive control and memory both in and out of the laboratory. The studies presented in this paper come out of this evolving research tradition, and address what has heretofore been a largely overlooked issue in the literature: task-switching under cross-modal stimulus conditions, specifically between the visual and auditory modalities.

Before embarking on a discussion of the particular motivations behind the study of cross-modal task switching (CMTS) performance it will be helpful to undertake a review of the greater bulk of the task switching literature, which has in general concerned itself with the effects of switching tasks in response to purely visual stimuli. The goals of this review will be two-fold. The first will be to identify the core empirical findings that have served to stimulate interest in the field, and which have played an important role in generating and constraining theory. Second, the review will be directed towards providing a clear taxonomy of the theoretical approaches that have been most influential in guiding ongoing research to this point, with the ultimate aim of forging connections between these perspectives – originally intended to account for performance in single-modality variants of the paradigm – and the methodologies and results in the current set of studies. This general review is followed by a more focused consideration of the few published experiments that have explored CMTS, and identifies several critical shortcomings of design and analysis in these studies, as well as some of the important questions that remain unanswered. Further relevant results from other areas of cognitive psychology will also be discussed. Finally, a series of experiments is presented that addresses several of the weaknesses in previous CMTS studies, documents novel and theoretically meaningful empirical phenomena, and explores a range of factors that modulate the effects of concurrent switches of stimulus modality on task-switching performance.

Task switching: The beginnings of the paradigm

While the bulk of what is known or surmised about the intricacies of task-switching phenomena derives from research conducted over the last decade and a half, the paradigm itself is over 80 years old (Jersild, 1927). Subjects in Jersild's experiments worked through lists of stimuli writing down their responses to each item, with list completion time the primary dependent measure. He found that under certain conditions performance was considerably slower, with costs as large as a half a second or more per item, when subjects had to alternate between tasks from one trial to the next as compared to when the task they performed on each trial was the same throughout an entire list. The response time (RT) costs (alternating-list completion time minus pure-list completion time) Jersild observed constitute what is generally regarded as the first demonstration of the cost of switching tasks, though for reasons that will become clear as this discussion proceeds, performance decrements such as these, observed in comparisons between pure and alternating lists, will be referred to as alternation costs rather than as switch costs.

A critical determinant of alternation cost magnitudes in Jersild's (1927) experiments was whether the tasks that subjects switched between on alternating lists shared a common stimulus set. When the stimuli in the lists were associated with more than one valid task in the current experimental context, as for example with numbers that could be added together or multiplied, alternation costs were surprisingly large. On the other hand, Jersild did not observe alternation costs when the stimuli encountered by subjects were each associated with only a single task – for example, when switching between the performance of a mathematical operation and the production of a linguistic antonym – and in one experiment even reported faster completion times in alternating- as compared to pure-list conditions when the tasks subjects performed had disjoint stimulus sets. Stimuli of this latter type are commonly termed univalent, while those of the former type are referred to as bivalent. In large part because of the robustness of the costs incurred by switching between tasks in response to bivalent stimuli, they have typically figured much more prominently in the literature. This does not, of course, imply that observations gleaned from univalent designs may not be theoretically informative, and the performance impairments (or the lack thereof) that obtain in studies employing univalent stimuli will be considered in more detail as the review proceeds.

It wasn't until 50 years after Jersild's pioneering work that any attempts were made to replicate his findings or further develop the paradigm. Spector and Biederman (1976), employing a slightly elaborated version (which in some cases employed random-task, as opposed to strictly alternating-task sequences in mixed lists) of Jersild's original methodology, were able to validate his observations of large RT costs with bivalent stimuli, and a lack of such costs (but not a statistically significant reversal) with univalent stimuli. In addition to this qualitative replication, the authors also reported on two additional interactions of note. First, they found that whereas alternation was faster than repetition in pure blocks with univalent stimuli when the subjects viewed lists that permitted preview of upcoming items (as in Jersild), this monotonic ordering was reversed when the stimuli were presented one at a time. Thus, it appears that subjects in both Jersild's and Spector & Biederman's list conditions were in some cases able to make adaptive use of their opportunities for preview of upcoming stimuli in univalent lists in a way that differentially benefited task alternations. In depth consideration of the precise mechanism of this interaction between preview and task alternation with univalent stimuli is beyond the scope of this paper, although it should be noted that there is some evidence to suggest that the effect is reversed with bivalent stimuli (Fagot, 1994). One attractive explanation for this is that the reduced facilitation from preview in pure lists relative to alternating lists with univalent stimuli reflects interference from adjacent stimuli that afford the same task but a different response (Fagot). Expanding on this insight, one can make the broader claim that the potential for beneficial performance effects from previewed stimuli should be smallest when the potential for confusion is greatest.

A second noteworthy interaction reported by Spector and Biederman (1976) provides some support for this view. Reasoning that the absence of alternation costs with univalent but not bivalent stimuli could be due to the simple fact that the former unambiguously indicate the task that should be performed on each trial in alternating lists, they added an operator symbol ("+" or "–") to numeric stimuli in bivalent lists as a way of indicating when subjects should perform the operations of addition and subtraction, respectively. Consistent with the notion that task alternation with bivalent stimuli may be more difficult because of increased opportunities for confusion, they report that the presence of these task indicators reduced alternation costs by over 200 ms relative to another experiment that used the same tasks without operator symbols. Even after this reduction, however, a 188 ms alternation cost still remained. The considerable slowing that remained clearly demonstrated that the cost of alternation with bivalent stimuli cannot be explained away as resulting from either uncertainty about which task to perform, or a heavier processing load imposed by having to keep the appropriate task sequence active in working memory.

Evolution of the paradigm

The results reviewed thus far conclusively demonstrated that, relative to single-task conditions, performance is considerably worsened in blocks of trials where more than one task is to be performed, provided those tasks share a common stimulus set. Furthermore, the magnitude of this cost of task mixing was shown to be sensitive to, but not eliminated by, the presence of information embedded in bivalent stimuli that unambiguously indicated the identity of the task that was to be performed, and additionally provided a stark contrast with the failure to observe such effects with univalent stimuli, which was arguably the more counterintuitive and surprising finding. The robustness of these core results due to Jersild (1927) and Spector and Biederman (1976) aside, a number of important questions remained concerning the roles of the various components of the experimental methodologies in producing these results, and it is to these that this review now turns. Attention is first given to an important distinction between a pair of factors that jointly account for alternation costs in the comparison of mixed- and pure-task conditions, followed by the introduction of two modern variants of the classic paradigm that are thought to permit isolation of one of these factors.

Estimation and interpretation of the RT cost incurred by task switching depends critically on the manner in which the switch vs. non-switch distinction is operationalized. In the experiments reviewed thus far alternation costs were calculated as the difference between average RT in blocks of trials in which the task alternated (or varied randomly), and average RT in blocks of trials in which only a single task was performed. One weakness of this strategy is that such comparisons necessarily confound two potentially distinct and distinguishable influences on RT. One of these is the cost of switching, as opposed to repeating tasks within a block of trials in which two or more task sets may be invoked. The second is the cost of having to maintain in a relatively active state more than one task set at a time – as is presumably the case in mixed or alternating lists but not in pure lists – which can be indexed as the difference between repeat trial RTs in mixed-task blocks and RTs in pure-task blocks. Fagot (1994) provided the first demonstration that these task-switching (TS) costs, and task-mixing (TM) costs, respectively, contribute independently to the slowing observed in mixed-task conditions. Several lines of evidence speak to the importance of differentiating between these two components of the alternation cost: a) TS and TM costs may not be equivalently affected by the aging process (De Jong, 2001), b) TM costs may be more strongly negatively affected by the presence of bivalent stimuli and increases in the number of tasks subjects must switch between (Rubin & Meiran, 2005), and c) TM but not TS costs may be influenced by the consistency with which bivalent stimuli have been matched to specific tasks (Koch, Prinz & Allport, 2005). There is also some evidence to suggest that TS costs may be more sensitive than TM costs to lengthening of the temporal lag, called the response-stimulus interval (RSI), between the response on the previous trial and the onset of the imperative stimulus on the current trial (Fagot), although this finding has not always replicated (Rubin & Meiran). In line with the first three results enumerated above, it has been suggested that TM costs result from the increased ambiguity inherent to mixed- relative to pure-task blocks, and specifically reflect executive and working memory function (Rubin & Meiran).

Two basic alternatives to the mixed- versus pure-task methodology, the alternating-runs and task-cuing paradigms, have been developed to allow more focused study of the factors that determine TS costs, specifically. In the alternating-runs paradigm performance is measured only in mixed blocks that include both switch and repeat trials, but tasks are predictably sequenced so that subjects are, in theory at least, never in doubt about the identity of the upcoming task. For example, Rogers and Monsell (1995) had subjects switch between letter and digit classification tasks that cycled either every two trials in an AABBAABB sequence, or every four trials in an AAAABBBB sequence. To help subjects keep track of the sequence, stimuli were presented in a 4 x 4 grid, with the location of the stimulus indicating the appropriate task.

The alternating-runs paradigm is arguably the most direct way of isolating the TS component of alternation costs, but shares another weakness in common with alternating- versus pure-task designs, namely the fact that neither methodology permits experimental control over the point at which subjects become aware of the task they are to perform on the upcoming trial. One way of overcoming this is to present cues that indicate unambiguously the identity of the upcoming task. This explicit cuing methodology (*c.f.*, Sudevan & Taylor, 1987), was first adapted for the study of TS costs, per se, by Meiran (1996). In a typical task-cuing experiment subjects switch between tasks in random order, with cues presented in advance of imperative stimuli at variable stimulus onset asynchronies (SOAs). In this way, the RSI is functionally subdivided into two parts: a) the pre-cue interval (PCI), which references the time that elapses between the execution of a response on the previous trial and the presentation of the cue on the current trial, during which time the subject is presumably unaware of which task they will be required to perform, and b) the cue-target interval (CTI), which refers to the SOA between the presentation of the cue and the imperative stimulus, and during which time the subject should in principle have some opportunity to ready themselves to perform the task indicated by the cue. It is generally assumed that independent manipulation of the PCI and CTI permit the researcher to tease apart RSI effects on task switching performance that derive from passive decay of task set or variations in phasic alertness on the one hand, and active and directed reconfiguration of task set on the other. It should be noted that the introduction of explicit cues does not necessarily facilitate a pure isolation of the reconfiguration that may occur with longer RSIs in the alternating runs paradigm. For example, it has been shown that RSI/CTI effects in alternating-runs designs may be more pronounced when explicit cues are also provided (Koch, 2003). Thus, it may be that explicit cues also alter the magnitude and/or type of reconfiguration that takes place during the inter-trial interval (ITI).

Early indications regarding stimulus, response, and task-set contributions to TS costs

The development of the alternating-runs and task-cuing paradigms, along with continued use of the mixed- versus pure-task methodology contributed to a number of empirical discoveries in the middle of the last decade of the 20th century. These findings collectively triggered a proliferation of interest in task-switching phenomena that has carried through to the present day. This section surveys some of these core results, which speak either to the role of stimulus or response characteristics, or to the part played by task sets themselves, in producing and influencing TS costs. The following section will build on this empirical foundation through discussion of a pair of theoretical distinctions that emerged from attempts to explain these results, and which have played a central role in the evolution of the collective scientific understanding of task-switching phenomena.

Two basic considerations that can be addressed relatively easily are the questions of whether TS costs can be accounted for solely in terms of stimulus switches, or alternatively in terms of response switches, from one trial to the next. The answers to these specific questions are a fairly straightforward no, and no (see for example Meiran, 1996; Monsell, Taylor & Murphy, 2001; Rogers & Monsell, 1995); a simple reductionist account of TS costs in terms of switches of these task components is not sufficient to account for the phenomenon. Nevertheless, stimulus and response-related variables in the design have been documented to interact with switch cost magnitudes in informative, and sometimes surprising ways. For example, whereas switching tasks and switching responses both lead to RT slowing relative to trials in which both the task and the required response repeat, it is not uncommon to observe a response repetition cost on task-switch trials (see for example, Rogers & Monsell).

A more striking example of the impact that stimulus factors can have on switch costs is the previously noted failure to observe alternation costs in response to univalent stimuli (Jersild, 1927; Spector & Biederman, 1976). One potential implication of this becomes clear when we recognize that a necessary and sufficient characteristic for a stimulus to be considered univalent is that it is neutral with respect to all but one task in the experimental context. While bivalent stimuli do not offer any direct analogue for this feature, they may nonetheless be divided into two subsets according to whether a given stimulus maps to the same response for two tasks (congruent), or to different responses for each task (incongruent). In so far as S-R mappings for both univalent and congruent stimuli remain constant throughout an experiment, it is reasonable to ask whether switch costs might also be absent on trials in which congruent stimuli are presented. Rogers and Monsell (1995) studied the effects of stimulus congruence on TS costs using the alternating-runs approach. Bivalent stimuli in their experiments consisted of a letter and digit side-by-side, to which subjects could respond either by indicating whether the letter was a consonant or a vowel, or whether the digit was odd or even. The authors observed large TS costs of over 300 ms regardless of whether stimuli were incongruent or congruent. Meiran (1996), replicating and extending these results using the task-cuing paradigm and a pair of spatial-response tasks with overlapping response sets, found smaller but still significant TS costs with both congruent and incongruent stimuli.

While neither Rogers and Monsell (1995), nor Meiran (1996), found that TS costs were influenced by the congruence (or lack thereof) of bivalent stimuli, another result reported by Allport, Styles & Hsieh (1994, Experiment 5), suggests that the effects of incongruence, as contrasted with neutrality (univalence), on RTs for specific tasks may vary as a function of whether subjects are engaged in performance of mixed- or pure-task lists. In a comparison of RTs to neutral and incongruent Stroop stimuli (e.g., colored x's and color words in black versus incongruent color words, respectively), these authors reported large Stroop interference effects (~200 ms) on color-naming trials, as well as modest reverse Stroop interference (~40 ms) on word-reading trials in non-alternating lists. The magnitude of the usual Stroop effect on color-naming trials in alternating lists was comparable to what was observed in pure-task lists. However, the introduction of task-switches served to dramatically inflate the reverse Stroop effect so that the effects of incongruence on word-reading and color-naming trials in alternating lists were nearly identical.

It may at first seem strange that switching tasks in response to congruent stimuli should result in such profound impairments when switching in response to univalent stimuli can in some cases result in facilitation. Some resolution of this apparent discrepancy was provided by several demonstrations showing that earlier failures to observe switch costs with univalent stimuli represented special cases rather than a general rule. In addition to studying the effect of congruence with bivalent stimuli, Rogers and Monsell (1995) also looked at TS costs with univalent stimuli, both in blocks that contained only univalent stimuli and in blocks that also included bivalent stimuli. In contrast to the results of Jersild (1927) and Spector and Biederman (1976), they observed considerable slowing in the neighborhood of 200 ms on switch relative to repeat trials for univalent stimuli, with the magnitude of the cost more pronounced in blocks that featured a mixture of univalent and bivalent trials.

Whether or not switch costs are observed with univalent stimuli appears to depend in large part upon the stimulus-task mappings subjects have previously been exposed to in the experimental context. Allport et al., (1994, Experiment 4), initially had subjects perform two tasks, one each in response to a pair of incongruent stimulus sets consisting of Stroop color words on the one hand, and displays with multiple copies of a single digit number that could in principle be responded to either by indicating the identity or quantity of the presented digits on the other hand. Note, therefore, that while these stimulus sets could each afford at least two tasks, as indeed can all experimental stimuli, they were initially presented to the subject in such a way that they were functionally univalent. Employing a mixed- versus pure-task methodology, Allport et al. observed that alternation costs quickly decayed to zero over the first several lists in the first block of the session. When subjects subsequently performed the remaining two tasks in response to those same stimulus sets, as well as when they switched back to performing the two original tasks, alternation costs again decreased over the early part of each block. However, even after the reduction had reached an apparent asymptote, significant switch costs remained. This was in spite of the fact that within any given block each stimulus was associated with only one currently relevant task set.

Allport et al.'s (1994) findings documented a functional transition from univalence to bivalence as a result of experience with conflicting stimulus-task mappings earlier in the experimental session. This result is important for two reasons: a) it serves as a reminder that stimulus valence is a conditional property rather than an absolute one, and b) it indicates that alternation costs are sensitive to the broader experimental context rather than simply to discrete trial-level factors. Further evidence of this was provided by Fagot (1994; Experiments 10, 11 and 12), who compared switching performance with univalent stimuli as a function of the kind of training subjects had received. Across several experiments, subjects were initially exposed to stimuli and tasks either in pure-task lists, strictly alternating lists, random lists, or alternating-runs style lists. In the pure, random, and alternating-runs training conditions RTs were faster on pure than mixed lists during the subsequent testing phase of the session, but this effect was either absent entirely or greatly reduced for subjects in the strictly alternating training condition. Fagot interpreted this result as indicating that subjects who had been trained on strictly alternating lists did not differentiate the two groups of univalent stimuli and their associated response mappings into two distinct task sets, but rather grouped them into an integrated single mapping. Note that in contrast to Allport et al.'s design, the task- and response- mappings afforded by the stimuli in Fagot's experiments remained constant throughout the experiment, making this an even stronger demonstration of the sensitivity of univalent task switching effects to prior experience. A further comment is that Fagot's results seemed to reflect slower RTs on pure-task lists, rather than faster RTs on alternating-task lists, in the strictly alternating training condition. At least two explanations can be offered to account for this finding. The first assumes that subjects in this condition integrated all the experimental stimuli and responses into a single task set. If this occurred, the increased number of S-R mappings subsumed under this larger task set could have led to increases in the RT baseline that manifested in comparisons to other training conditions as a slowing on repeat trials. This account implicitly grants default status to the cognitive processes that characterize repeat trials. Another possibility takes the opposite perspective, namely that the primary function of a task set is to enable more efficient performance in circumstances that permit some degree of perseveration of cognitive action patterns, with the implication being that in the absence of a clearly defined task set cognitive processing defaults to a pattern more characteristic of circumstances requiring a task switch. Accordingly, it may be that strictly alternating training discouraged subjects from adopting a task-set that would facilitate responding on repeat trials.

Two further questions are immediately suggested by Allport et al.'s (1994) and Fagot's (1994) findings documenting persistent learning effects on alternation costs. First, given that prior exposure to conflicting S-R mappings or specific list structures can differentially influence RTs on switch and repeat trials for the duration of an experimental session, is it also the case that the disruptive effects of a task switch can endure across multiple subsequent repeat trials? Rogers and Monsell (1995) addressed this question using bivalent stimuli in an alternating-runs procedure in which the task switched every four trials (AAAABBBB). They observed significant RT slowing on the first trial of each run (i.e., following a switch of task), but found that RTs across the three subsequent repeat trials were identical. In other words, whatever the source of the RT costs that result from a discrete switch of tasks, there was no evidence to suggest that the disruption could not be overcome by a single performance of the new task. On the other hand, Allport et al. (Experiments 6 and 7) examined target detection as a function of number of intervening items between an unpredictable criterion shift and target presentation using Rapid Sequential Visual Presentation (RSVP), and reported impaired detection that persisted for as much as seven stimulus presentations after a shift. The markedly different methodologies that led to these divergent results preclude any strong claims about what the critical variable or variables may have been in this case.

However, two factors seem especially suggestive: a) although Allport et al. found that the number of intervening items rather than time elapsed accounted for the rate of recovery from criterion shifts, stimulus presentation rates in RSVP are fast enough that 7 stimuli can be presented in less time than is required to complete two typical task-switch trials, and b) foreshadowing later results, switches occurred at predictable times in Rogers and Monsell's study, but not in Allport et al.'s.

The second question concerns the effects of practice on task switching in response to bivalent stimuli. Specifically, while it is clear that experience can lead to the appearance of switch costs, can it also serve to overcome them? Indeed, as was discussed in the preceding paragraph, if a single trial is, at least in some cases, sufficient to overcome the deleterious effects of switching tasks, it does not seem unreasonable to expect that enough practice with a pair of tasks could eventually overcome the cost of switching between them altogether. Allport et al. (1994), Rogers and Monsell (1995), and Meiran (1996), using the mixed- versus pure-list, alternating-runs and task-cuing paradigms, respectively, all found evidence that moderate amounts of practice could lead to a reduction in TS costs under certain circumstances, with Meiran also noting that practice had approximately equivalent effects for congruent and incongruent stimuli. However, in none of these cases was practice able to completely overcome the slowing.

The double-edged sword of experience-related factors, which according to the results reviewed thus far appear to be both a cause of, and a partial remedy for, switch costs, receives further sharpening by a surprising finding first reported by Allport et al. (1994, Experiment 5). Among other questions, the authors were interested to see whether alternation costs would differ according to which task was being performed. More specifically, would switch costs be larger when switching to a weaker, more difficult task than when switching to a stronger, relatively easier task? In a comparison of alternation costs for color-naming and word-reading tasks in response to incongruent Stroop stimuli, however, the authors observed the exact opposite pattern, with RTs in pure blocks enjoying a considerable advantage over RTs in alternating lists only when subjects performed the more dominant word-reading task.

Another example of a finding that seems to fly in the face of intuition regarding a prospective relationship between switch costs and difficulty also comes from Allport et al. (1994, Experiment 1). They employed a design that involved four tasks defined by a factorial combination of two stimulus dimensions (local versus global) and two judgment types (odd/even versus parity/magnitude) in response to displays composed of groups of identical digits. Alternating-task blocks were structured so that subjects either switched only dimension, only judgment type, or switched both dimension and judgment type on every trial. The surprising result was that alternation costs were insensitive not only to which components switched, but also to the number of components that switched, and were equivalent for all three block-types.

The final issue to be discussed in this section has arguably been the most important in terms of the quantity of research that has been directed at understanding, and resolving ambiguities relating to, the core result. Recall that Spector and Biederman (1976) reported a reduction in alternation costs that resulted from embedding operator symbols in the imperative stimuli that unambiguously indicated which task subjects were to perform. Because preview of items was possible in their experiment, one interpretation of these findings holds that subjects were able to make use of the information contained in the operator symbols to engage in advance preparation for the upcoming task, and that this preparatory processing served to improve performance more in alternating lists than in pure lists. This hypothesis accords with a number of additional findings that had documented improved performance in mixed-task situations when advance cuing was provided (c.f., Hartley, Kieley & Slabach, 1990; Shaffer, 1965; Shaffer, 1966; Shaffer, 1967; Sudevan & Taylor, 1987), and speaks to the necessity of giving consideration to the nature and limitations of preparatory processing in any remotely comprehensive account of task-set reconfiguration. However, as was noted above, several other plausible explanations can also be offered to account for Spector and Biederman’s result, including the more parsimonious suggestion that the operator symbols facilitated a simple reduction in task confusion.

Fagot (1994) tested the hypothesis that a complete switch of task could be accomplished through preparatory processing by varying the RSI between 0 ms and 1500 ms using an alternating- versus pure-task methodology. He found that longer RSIs were associated with a significant reduction in alternation costs. However, the bulk of the alternation-specific facilitation was evident at RSIs as short as 200 ms, and even at the 1500 ms RSI alternation costs of ~200 ms remained. As was mentioned earlier, a subsequent experiment that used only 0 ms and 400 ms RSIs, and which also included an alternating-runs condition, revealed that TS costs, but not TM costs, were reduced at the longer RSI, though again, TS costs of over 150 ms remained.

There are at least two caveats regarding the interpretation of Fagot's (1994) failure to observe a complete elimination of switch costs. One is that stimulus displays in his experiments did not unambiguously indicate the appropriate task. It may therefore be asked whether subjects necessarily made full use of the RSI for task-set reconfiguration. Rogers and Monsell (1995) addressed this weakness by having stimuli cycle predictably through quadrants in a 4 x 4 grid where the location of the stimulus indicated the appropriate task. Their results indicated that TS costs decreased as RSI increased, but that an asymptotic reduction of a little less than 50% was achieved using a 600 ms RSI, with no further significant switch-specific facilitation evident at the 1200 ms RSI level.

The second consideration of note, which is in fact relevant to both Fagot's (1994), and Rogers and Monsell's (1995) designs, centers on the question of whether and to what extent active, directed preparation contributed to the switch cost reduction over and above factors related to the mere passage of time since the last task was performed. That is, because the point at which subjects become aware of the identity of the upcoming task is not under experimental control in the alternating-runs and list-comparison procedures, it is impossible to tease apart the relative contributions of reconfiguration as opposed to passive decay. The task-cuing paradigm, on the other hand, allows researchers to isolate processes that are aimed specifically at preparing for the upcoming task by varying the PCI and CTI inversely while the RSI is held constant. Using this approach with random task-sequences, Meiran (1996) observed significant, but again only partial reductions in TS cost magnitudes as a function of increasing the CTI, replicating the previous findings and indicating that remoteness from the previous trial was not sufficient to account for

RSI facilitation of task switching. This latter conclusion is buttressed by several instances in other studies in which merely lengthening the ITI was insufficient to bring about a switch cost reduction (Allport et al., 1994; Rogers & Monsell). These preparatory failures are discussed in the next section.

Theoretical perspectives: Preparation, control and carryover of activation/inhibition

The preceding section surveyed a number of instructive, and in several cases, counterintuitive and even contradictory findings. Many of these results played central roles in the development of influential theoretical perspectives on task-set reconfiguration, some of which are now introduced. In particular, the focus will be on two primary issues: a) the decomposition of TS costs into two components that are assumed to be empirically differentiable according to their responsiveness to preparation, and b) questions concerning the extent to which TS costs reflect executive control. Some evidence from subsequent studies that corroborate and build on these theoretical orientations will also be presented.

Of the empirical phenomena reviewed in the preceding section, none have figured more prominently in the task switching literature over the past decade than the so-called preparation effect, whereby switch cost reductions are often observed when subjects are provided with information about the identity of the upcoming task, and time to make use of that information. The fact that preparation leads to switch cost reductions is, by itself, perhaps not all that interesting. What is more surprising is the apparent inability of preparatory processing to overcome the switch cost. Allport et al. (1994), Fagot, (1994), Rogers and Monsell (1995), and Meiran (1996), all explicitly manipulated variables related to preparation using a variety of methodologies, yet in none of their studies was preparation sufficient to eliminate the cost of switching tasks. On the contrary, most of the benefit from preparation was typically achieved in little more than half a second, and RSIs as long as four seconds (*c.f.*, Fagot) failed to bring about a switch cost reduction of much more than 50%. What is to be made of this relative ineffectiveness of preparation intervals that are as much as an order of magnitude larger than the costs they are intended to ameliorate?

To explain these and other findings, Fagot (1994) proposed the *Ready, Set, Go!* (RSG) model of task switching. The full specification of this model involves six properties: 1) the existence of a unique response selection mechanism that can, 2) range along a continuum from being completely ready to perform a given task to not at all ready to perform that task, such that the readiness of the mechanism 3) cannot be changed during the RSI, but, 4) can be changed by performing a given task. Furthermore, 5) the setting of the mechanism, or deciding which task should be performed, is independent of the readiness of the mechanism, and, 6) can be adjusted during the RSI. The RSG model thus accounts for preparatory phenomena in the following way: suppose a subject in a task switching experiment that features two tasks, *A* and *B*, has just performed task *A* on trial $n - 1$. As a result of this, the subject's response selection mechanism should be more or less "ready" to again perform task *A* in trial *n*, and this is not subject to change during the RSI (except perhaps as a result of decay of task set on a relatively long time scale). That is, all else being equal, the subject's RT in trial *n* should be shorter if trial *n* features a repeat of task *A* as opposed to a switch to task *B*. Thus, the carryover of readiness from one trial to the next is sufficient to produce TS costs. Consider next the effects of "setting" the response selection mechanism for one task versus the other. What happens when the RSI is short? If some degree of inertia of the "setting" of the response selection mechanism is assumed, then the subject should not only be "ready", but also "set" to again perform task *A* on trial *n*, and this should likewise favor task repeats over task switches. On the other hand, when the RSI is long enough and switching is cued or predictable, subjects should be able to alter the "setting" of their response selection mechanism during the ITI to favor either a repeat of task *A*, or a switch to task *B*. Thus, in the long RSI case, the "setting" of the mechanism facilitates performance regardless of whether a switch or a repeat occurs, and so does not contribute to switch costs. At the core of Fagot's (1994) proposal, then, is the idea that TS costs with a short RSI, or in the absence of explicit cuing or sequence predictability, index two independent components of cognitive processing. One of these components, which will be referred to as the residual component of the switch cost, is determined entirely by the last task performed, and contributes equivalently to switch costs regardless of whether the subject has prepared to execute a task switch or not. The other, preparatory component of the switch cost can be adjusted by the subject during the RSI and only contributes to switch costs when switching is uncued or unpredictable, or when the RSI is short.

Similar hypotheses have been suggested by a number of other researchers. For example, Rogers and Monsell (1995), distinguished between endogenous (preparatory) and exogenous (residual) factors in the generation of switch costs as a way of explaining their own failure to observe a complete elimination of TS costs with longer preparation intervals. More will be said of their account shortly. Rubenstein, Myer and Evans (2001), proposed a model of switch costs that included goal-shifting and rule-activation stages of task switching, where the former corresponds to the preparatory component and the latter to the residual component. Similarly, Sohn and Anderson (2001), argued for a model of switch costs without foreknowledge that consisted of inadequate preparation and a repetition benefit on repeat trials. A final example comes from Ruthruff, Remington and Johnston (2001). These authors evaluated RTs in response to univalent stimuli with either two or three task sets as a joint function of task recency (number of intervening trials since the currently relevant task was last performed) and task expectancy. The latter factor was evaluated by employing an alternating-runs paradigm with occasional sequence violations. On the basis of an apparent additivity between these two factors, with switch costs tending to increase as recency decreased and as a result of sequence violations (*i.e.*, violations of expectation), they endorsed a *configuration-execution* model of switch costs, with the configuration component corresponding to preparation and the execution component referencing central processing operations directed towards carrying out whichever task set had been prepared.

The consistency of these theoretical positions aside, it should be noted that the methodologies employed in several of the studies mentioned above are not beyond criticism. For example, Rubenstein et al. (2001) operationalized switch costs as the difference between pure-task lists and mixed-task lists, thus leaving open the possibility that TM costs rather than TS costs may have driven their findings. Furthermore, Ruthruff et al. (2001) used only univalent stimuli and manipulated expectation not through RSI or the use of explicit cues, but via intermittent sequence violations, which may or may not have introduced unanticipated but systematic variability into the task-switching equation. It may therefore be asked whether the mere failure to observe TS cost eliminations, which was the primary motivating result behind the theoretical positions suggested Fagot (1994) and Rogers and Monsell (1995), was sufficient to justify the theoretical acceptance of independent components of TS costs. To be sure, the result has proven exceedingly robust, and at least one later study found that even providing extensive feedback and monetary bonuses for good performance was insufficient to eliminate residual costs (Nieuwenhuis & Monsell, 2002). However, preparatory effects have also proven to be fickle on occasion (see for example, Allport et al., 1994), and there remains the possibility that some untested incentive structure could still achieve what those surveyed above could not. A more convincing argument in favor of a theoretical decomposition of TS costs *vis a vis* susceptibility to preparation comes from several demonstrations of considerable overlap between the effects of practice and preparation. Meiran (1996), using the task-cuing paradigm, observed monotonic reductions in switch costs as a function of practice when the CTI was short, but no effect of practice when the CTI was long, a result he interpreted as indicating that practice increased the speed of reconfiguration (see also, Meiran, Chorev & Sapir, 2000, Experiments 2 and 3; Sohn & Anderson, 2001).

The interaction of preparation and practice almost certainly provides an important clue to the manner in which preparation facilitates task switching, and so offers a decidedly non-trivial insight into the nature of task set reconfiguration more generally. The precise interpretation of such clues depends largely on the extent to which it is assumed that switch costs reflect processes of executive control. Intuition would seem to suggest that task-switching paradigms are ideally suited to the exploration of executive function: cognitive control, if such a thing exists at all, surely exists precisely for the sake of enabling goal-directed behavior in situations where the appropriate action must be selected over a range of contextually inappropriate, but otherwise viable, and perhaps even recently endorsed alternatives. However, a key logical distinction must be made on this point. Specifically, while it may be reasonable to assume that executive control is required for successful task switching, particularly with bivalent stimuli, this does not necessarily imply that executive control is differentially involved in task switching as opposed to task repetition in situations where both task repeats and task switches are possible. Stated more directly in the terms of the paradigm, it does not seem an *a priori* untenable hypothesis that the contributions of executive control to task switching performance are indexed most directly by TM costs, and that TS costs reflect inefficiency of a different sort.

To better understand the importance of this distinction, consider the question of whether switch costs vary directly with the number of component operations that must be reconfigured from one trial to the next. If TS costs index control processes, then it is a fairly straightforward prediction that they should increase monotonically with increases in the number of (bivalent) task components that change. Accordingly, it can be argued that a failure to observe this pattern constitutes a falsification of any theory of task switching that is predicated entirely on the notion that differential involvement of control processes on task switch and task repeat trials is the source of the switch cost. The reader may recall, then, that a finding of just this sort was in fact reported by Allport et al. (1994, Experiment 1), who found that alternation costs did not differ as a function of whether subjects switched only between stimulus dimensions, only between judgment types, or between both simultaneously on each trial in alternating-task lists. Further evidence presented by these authors also appeared to run counter to predictions derived from a control hypothesis. These include the failure to observe larger switch costs to a more difficult task relative to an easier one (Experiment 3), the aforementioned observations of a paradoxical asymmetry in alternation costs using color-word Stroop stimuli (Experiment 4), and of the emergence of persisting alternation costs as a function of the experience-driven transition from experiment-wise stimulus univalence to bivalence (Experiment 3), and also a failure of preparation to result in a significant reduction of alternation costs, even though there was some evidence that longer RSIs were sufficient to reduce overall RTs (Experiment 5).

On the basis of these and other results, Allport et al. (1994) proposed a hypothesis they termed *task set inertia* (TSI). According to this proposal switch costs result from a type of proactive interference that acts on task sets themselves, and which stems from competing S-R mappings. Overcoming any meaningful portion of this interference – which is to say successfully disengaging from a previously relevant task set – is not assumed to be possible via control processes (*i.e.*, preparation), but is instead dependent upon either passive decay of interference on a fairly long time scale, or processing of the imperative stimulus on the next trial. Perhaps the strongest support for the TSI hypothesis from among Allport et al.'s results comes from the elegant explanation it provides for the observation that alternation costs in the Stroop paradigm may be larger when switching to the more dominant word-reading task as compared to the color-naming task (for a relevant alternative perspective that hypothesizes a need to suppress highly compatible S-R mappings, see De Jong, 1995). Following Allport et al., the following two assumptions are sufficient: a) non-dominant mappings require a more strongly imposed task set, perhaps involving inhibition of competing mappings, and b) more strongly imposed task sets generate more TSI. Thus, the claim is that TSI generated during performance of the weaker color-naming task on trial $n - 1$ interferes with performance when switching back to the word-reading task on trial n. By comparison, when switching in the opposite direction little TSI is generated by performance of the word-reading task in trial $n - 1$, and hence, performance of the color-naming task on trial n is relatively unaffected. A third assumption, namely that interference resulting from TSI is feature-specific, was also specified by the authors and was primarily motivated by the need to explain why paradoxical asymmetries were not always observed when switching between relatively easy and relatively difficult tasks that did not share a common stimulus set.

While Allport et al.'s (1994) observation that preparation was ineffective in reducing switch costs has not withstood the test of time, the TSI hypothesis is easily salvaged from this particular overextension by restricting its application to the residual component of TS costs. This effort is justified by a number of more recent results that can be, if not necessarily completely accounted for, at the very least accommodated by the TSI hypothesis. In some cases these demonstrations constitute relatively straightforward generalizations and extensions of results originally reported by Allport et al. For example, Meuter & Allport (1999) observed a paradoxical asymmetry in language switching in bilinguals. They had subjects switch between naming digits in two languages and found that switch costs were larger when switching to the more dominant language from the weaker one than vice versa. Furthermore, it was found that the degree of differential proficiency in the two languages predicted the magnitude of the asymmetry, which was most pronounced for those subjects who were relatively lacking in facility with the non-dominant language. Both findings accord well with the notion that TS costs reflect TSI.

An example of a result that was not anticipated in the original specification of the TSI hypothesis, but which nonetheless follows almost directly from it, concerns the effect of stimulus incongruence in trial $n - 1$. Following the account of the paradoxical asymmetry given by Allport et al. (1994), if we assume that responding correctly on trials in which the stimulus is incongruent requires a relatively more strongly imposed task set than responding on trials in which the stimulus is congruent, then the resulting TSI should lead to larger TS costs when the stimulus on the preceding trial was incongruent. Goschke (2000) observed precisely this pattern in a series of experiments in which subjects switched between color and letter judgments. Importantly, the effect was not modulated by RSI (see also Monsell, Sumner & Waters, 2003), consistent with the suggestion from above that it is the residual component of TS costs, specifically, that is impacted by TSI.

The success of the TSI hypothesis in predicting and explaining results such as these has not entirely discouraged the development of task switching theories that speculate about a prominent role for executive function in producing TS costs. An influential example of this arguably more intuitive approach derives largely from the work of Rogers and Monsell (1995), who proposed a specific variant of what is often referred to as an *extra process* account of switch costs. According to the perspective favored by these authors, whereas the preparatory component of switch costs can be overcome by an endogenous act of control, the residual component is not affected by such processes, and can only be overcome through exogenously triggered reconfiguration. Note that this proposal, as well as both Fagot's (1994), and Allport et al.'s (1994), accounts are in basic agreement on this latter point. However, Rogers and Monsell's extra process theory goes one step further and argues that this exogenously triggered reconfiguration is itself a control process, that it is only invoked on task-switch trials (and hence accounts for the RT slowing with preparation on switch relative to repeat trials), and that a single trial is sufficient to achieve complete reconfiguration of task set. The principle lines of evidence they cited in favor of their account are the aforementioned effects of RSI on bringing about a partial reduction in TS costs, as well as the observation that no additional shortening of RTs was evident over the course of several task-repeat trials following a task switch in the alternating-runs paradigm. It has already been noted that former result necessitates a modification of Allport et al.'s original formulation of the TSI hypothesis. Rogers and Monsell argued further that the latter finding also violates the most natural prediction from TSI. Specifically, according to the TSI hypothesis RTs to successive repeat trials should evidence a gradual speedup owing to a similarly gradual dissipation of the inertia that remained after executing the switch of task.

The reader may at this point counter that: a) just as was the case with preparatory processing, the TSI hypothesis would seem easily adjusted to accommodate complete reconfiguration after a single trial, and b) the involvement of control processes in producing residual TS costs is not strictly necessary to explain either of the motivating results specified in the preceding paragraph. Both objections are valid, as the strong endorsement of an extra process account over a modified TSI hypothesis would seem to require demonstrations of, for example, residual TS cost asymmetries that favor strong tasks over weak tasks, or similarly, evidence that reductions of reconfiguration load lead to reductions of TS costs; that is to say, results that directly contradict the paradoxical asymmetry of switch costs, and the non-additivity of simultaneous judgment and dimension shifts first reported by Allport et al. (1994).

One example of an attempt to achieve such results was reported by Rubenstein et al. (2001), who, as noted previously, argued that switch costs include a rule activation component that was conceived of as a control process. In support of this contention they provided evidence that increasing rule complexity of the required tasks increased shift costs. However, these authors employed a list-comparison methodology with uniformly short RSIs. It is therefore not possible to determine whether the effect of the complexity manipulation was realized on the residual component of the TS cost, the preparatory component of the TS cost, or on the TM component of the alternation cost.

A related study that made use of the task-cuing paradigm (Arrington, Altmann & Carr, 2003) asked whether similarity between two tasks – operationalized in terms of the relations between the components that defined them – would lead to a reduction in residual TS costs by reducing the executive load imposed by reconfiguration. Similarity of perceptual dimension (spatial/visual) and response modality (vocal/manual) were both manipulated, and it was observed that TS costs were reduced when the tasks subjects switched between were similar, as predicted by an extra process account. It is important to note, however, that the authors used only a single 500 ms CTI, and hence it would be necessary to take it on faith that residual TS costs were isolated as no comparison of TS costs with and without preparation is available. As will be discussed later, this assumption is almost certainly in error. The evidence from this study is thus likewise equivocal with respect to the question of whether residual TS costs, specifically, reflect control processes.

Several papers have presented critical reexaminations of the circumstances under which a paradoxical asymmetry of switch costs is likely to be observed. Monsell, Yeung and Azuma (2000), provided brief reports on a range of studies that operationalized task dominance in a variety of ways. The authors reported mixed results, with a paradoxical asymmetry evident when pre-experimental experience differed markedly between two tasks and the response sets for the two tasks overlapped. However, no asymmetry was obtained when S-R compatibility was manipulated. Yeung and Monsell (2003a) further detailed a reversal of the paradoxical asymmetry (*i.e.*, smaller TS costs to the "stronger" task) when response sets were segregated between the two tasks, as well as when presentation of the various stimulus dimensions was temporally staggered. In all of the above, however, the authors failed to manipulate the RSI level so that a direct comparison of prepared and unprepared switching is not possible, and hence the relevance of the results to theories of residual costs is not clear.

This repeated failure to isolate residual switch costs was recognized and addressed by Yeung and Monsell (2003b, Experiment 4), in a study examining the effects of recent, intra-experimental practice on TS costs (see also, Monsell et al., 2000). Using alphabet arithmetic and perceptual comparison tasks in a task-cuing procedure, these authors varied the RSI between 200 ms and 1200 ms across triplets of trials and found that under certain specific circumstances, TS costs were larger when responding to the more recently practiced task – a paradoxical asymmetry. Most notably for the present discussion, this pattern was only evident when the RSI was short, whereas TS costs for the more- and less-recently practiced tasks were nearly identical when the RSI was 1200 ms. This finding is important for two reasons: it indicates a) that intra-experimental practice, and not merely pre-experimental experience, is sufficient to lead to a paradoxical asymmetry, and b) that the cost of recent practice on switch trials can be overcome by lengthening the RSI. The latter result, which was interpreted by the authors as indicating that TSI could be reduced by preparation, is particularly interesting if, as suggested previously, TSI is to be conceived of as a phenomenon that is robust to executive processing. Further consideration of this suggestion, however, reveals an important logical qualifier: even if the TSI hypothesis is a wholly accurate and adequate explanation for the paradoxical asymmetry of TS costs observed in situations where task dominance can be differentiated according to pre-experimental experience, this does not imply that every case in which a paradoxical asymmetry is observed is necessarily an example of the influence of TSI. For example, recall that a critical result cited in distinguishing between preparatory and residual components of TS costs was the observation that intra-experimental practice primarily reduced the former but not the latter. Yeung and Monsell's basic result is easily accommodated by the theoretical relationship implied by this finding, since TS costs for the two tasks differed as a function of the recency of intra-experimental practice in the short, but not in the long RSI condition. A further mitigating factor in Yeung and Monsell's results will be discussed in a later section.

A brief summary of the progress that this review has made towards understanding the relationship between executive control and the switch cost may be in order at this point. Working from the assumption, introduced and at least tentatively justified at the beginning of this section, that the TS cost can be viewed as consisting of distinct preparatory and residual components, two basic explanatory classes have been suggested: a) extra process models, which propose that the slowing observed on switch trials reflects the duration of an additional control process or processes inserted in a stage-like architecture, and b) activation carryover models, with TSI as a special case, which assume that the number of executive control stages are equivalent on switch and repeat trials within a mixed block, and that switch costs reflect task-level proactive interference that acts to reduce the efficiency with which response selection typically occurs when the task relevant on trial *n* is defined by S-R mappings that are at least partially incompatible with the mappings that defined the task in trial $n – 1$. Both approaches have had some success in accounting for different, and occasionally contradictory empirical phenomena. However, on the basis of the data reviewed above, it appears that a reasonably viable reconciliation of the predictions made by these two perspectives can be achieved by positing that proactive interference between task sets is a principle contributing factor in residual costs, and that executive processing, or at the very least processing that is susceptible to endogenous control, is a primary cause of preparatory costs.

These conclusions and the arguments on which they are based are by no means conclusive. For example Monsell and colleagues (cf., Monsell et al., 2000), have argued that results consistent with the TSI hypothesis are not necessarily incompatible with extra process accounts, pointing out that a carryover of activation/inhibition on task switch trials could serve as the trigger for an executive control process that is directed at ensuring that interference does not lead to incorrect responding. At the same time, several authors have argued that neither the preparatory nor the residual component of TS costs reflects an additional control process invoked only on switch trials. This section concludes by briefly outlining one such proposal that has been implemented as a parallel distributed processing (PDP) model of task switching in the Stroop paradigm.

Gilbert and Shallice (2002) proposed an interactive activation account of task switching performance in mixed blocks in response to Stroop color-word stimuli by augmenting an existing model of pure-task performance with design features motivated by a carryover of activation account of switch costs. Their model yielded good fits to the basic empirical phenomena associated with both the Stroop and task-switching paradigms, including some that have not yet been introduced in this review. Of principle interest, however, are their findings concerning the persistence of switch costs across multiple repeat trials, and the asymmetry of switch costs between the word and color tasks. Recall that Monsell and colleagues (see for example, Rogers & Monsell, 1995; Monsell et al., 2000) have argued that observations of larger switch costs to the more difficult task, as well as an absence of persisting slowing over multiple repeat trials following a switch of task, both constitute evidence that TS costs reflect the duration of an extra control process on task switch trials. However, the model proposed by Gilbert and Shallice, which included no such extra process component, was able to reproduce both effects in simulations of an alternating runs procedure with run lengths of four (AAAABBBB). While the elimination of switch costs after a single trial with their model does not in any sense undermine the claim that the achievement of asymptotic performance after a single trial is consistent with an extra process account, it does clearly demonstrate that the assumption of an extra process is not in general necessary to explain the effect.

Continuing, in light of the empirical findings regarding switch cost asymmetries reviewed previously, the reader may note that theories of task switching must be capable of accounting for both standard and paradoxical asymmetries, both of which have been documented in the literature. In the initial parameterization of their model, Gilbert and Shallice (2002) found that costs were larger when switching to the dominant word-reading task, consistent with the findings reported by Allport et al. (1994) as well as with standard predictions from carryover accounts. However, subsequent testing with different parameter settings revealed that the model could also generate results consistent with predictions from extra process accounts – that is, larger switch costs to the weaker task – indicating once again that postulation of an additional control process on task switch trials may be unnecessary. Without getting into technical specifics, the performance of their model tended to favor the following conclusion: when the differential efficiency with which the cognitive system is capable of performing a pair of tasks is minimal but the stronger of the two tasks is considerably more likely to be elicited automatically by the experimental stimuli, switch costs should be larger to the more automatic task. By contrast, when the stronger task can be performed with considerably greater efficiency than the weaker task, but is only marginally more likely to be automatically elicited by the experimental stimuli, switch costs should be larger to the less practiced, or more difficult task.

Two important caveats should be noted regarding the performance of Gilbert and Shallice's (2002) model. The first concerns the speed with which asymptotic repeat-trial performance can be reached following a switch of task. The results discussed thus far, including the simulations reported by Gilbert and Shallice, may have conveyed the impression that in standard task switching paradigms subjects obligatorily overcome TS costs in only a single trial, as reported by Rogers and Monsell (1995). A number of other studies, however, have provided evidence that slowing from a task switch can persist across multiple trials (see for example, Meiran et al., 2000; Milan, Sanabria, Tornay & Gonzalez, 2005; Monsell et al., 2003). As alluded to previously, whether slowing from a recent shift of task set in a given switch trial n is observed in repeat trial $n + 1$ or beyond appears to depend primarily on whether task sequencing is random (as in most exemplars of the task-cuing paradigm) or predictable (as in the alternating-runs paradigm). That is, whereas asymptotic performance is generally achieved after only a single trial when the task sequence is predictable, persisting slowing from a task switch has been commonly observed after multiple repeat trials in designs in which tasks do not switch in a predictable fashion. This seems likely to reflect strategic adjustments on the part of subjects, especially when considered alongside other results showing that implicit learning of task sequences was not associated with a reduction in switch costs (Gotler, Meiran & Tzelgov, 2003; Heuer, Schmidtke & Kleinsorge, 2001; Koch, 2001; Koch 2005). For example, it could be that when a task switch can be followed immediately by another task switch with some nontrivial probability, as is usually the case with random switching, the extent of the reconfiguration that has taken place by the end of a switch trial may be reduced in the interest of striking a balance between the goals of responding quickly in the event that a repeat trial follows (for the sake of which complete reconfiguration would be optimal), and minimizing the effort involved in reconfiguring again in the event that an immediate switch back to the just abandoned task set is required (for the sake of which minimal reconfiguration would be optimal). Of note, this suggestion is only tenable if it can explain why RTs continue to decrease across consecutive repeat trials when a task switch is at the very least no less likely to occur after 4 repeats than it is after a single repeat. This obstacle is easily surmounted, however, by assuming that set-specific bias builds up involuntary over successive repeat trials. Consistent with this account, Ruthruff et al. (2001), using a paradigm that incorporated random sequencing elements, reported that switch trial RTs increased as an approximately logarithmic function of the lag (in terms of number of trials) between the current trial and the last trial on which the currently relevant task was performed, indicating not only dynamic adjustments to set-specific bias in the predicted direction, but also a detrimental effect of that bias on subsequent reconfiguration. Unfortunately, it is not clear whether the limited ability of Gilbert and Shallice’s model to simulate persisting shift-generated slowing across a single repeat trial can be extended to the simulation of slowing across four or more repeat trials, as has been reported in the literature, nor is it clear whether the parameter adjustments required to produce such a result would be plausible when compared to the methodological factors that apparently drive these effects.

The second caveat concerns the preparation effect, and more specifically the near ubiquitous failure of preparation to bring about a complete elimination of switch costs. In contrast to typical findings with human subjects, preparation in the model proposed by Gilbert and Shallice (2002) was sufficient to completely eliminate TS costs. As the authors argue, and as will be discussed later, the ability of the model to produce such a result is important, as several studies have provided evidence that residual TS costs may not be as unavoidable a phenomenon as they once seemed to be (see for example, De Jong, 2000). It remains true, however, that in a majority of cases these residual costs are observed, and it is therefore just as important that the model be able to reproduce these failures of preparation. It remains an open question what modifications to the model architecture, or to the functional instantiation of preparation, would be necessary to allow simulation of distinct residual and preparatory TS components, and it is therefore unclear whether the incorporation of such changes would fundamentally alter the behavior of the model in other respects. Given the intimate links between preparation and theoretical perspectives on the role of control in task switching, this ambiguity concerning the performance of Gilbert and Shallice's model is a critical one.

To summarize, this section has focused on two relatively classical theoretical perspectives on task switching, and has suggested a tentative, and hopefully at least reasonable if demonstrably not authoritative synthesis of the two, centering on a distinction between two hypothetical components of the switch cost that may be distinguished according to their susceptibility to preparatory processing during the ITI. It is hoped that the preceding discussion will serve to provide a context and a foundation for the remaining sections of the introduction, which focus on more recent developments in the literature that variously serve to flesh out some of the unsettled details of Rogers and Monsell's (1995) extra process, and Allport et al.'s (1994) TSI accounts, suggest novel perspectives on task switching performance, or directly challenge aspects of the classical task shifting canon.

A closer look at preparation

It was suggested in the previous section that whereas carryover-of-activation theories seem particularly well suited to accounting for a range of phenomena associated with the residual component of switch costs, their ability to contribute to our understanding of the preparatory component may be relatively limited. An alternative class of extra process models was shown to have both intuitive and predictive value when applied to task switching under circumstances in which preparation may not have occurred. However, comparatively little effort has been invested in the specification of the precise nature of these hypothesized extra processes, much less the preparation effects they have been posited to explain. This section is concerned primarily with addressing these two shortcomings from the preceding discussion by considering in more breadth and detail experimental factors that have been shown to facilitate or interfere with preparatory processing. A trio of theoretical propositions that make specific claims about the nature and limitations, or lack thereof, of advance task set reconfiguration are also presented.

The idea that the preparatory component of the TS cost reflects some form of extra process that is susceptible to endogenous control in advance of presentation of the imperative stimulus leads directly to the inference that the observation of a preparatory reduction in switch costs should be contingent on experimental factors that encourage subjects to engage in an almost certainly effortful process of reconfiguration. This claim, although distinct for its focus on a presumably discrete preparation-related component of switch costs, is nonetheless philosophically aligned with the hypothesis outlined toward the close of the last section concerning the finding that asymptotic repeat trial performance is achieved more slowly with unpredictable task sequences than with entirely predictable sequences (*c.f.*, Monsell et al., 2003), and is supported by a number of extant reports in the task switching literature. For example, Rogers and Monsell (1995) reported a failure to observe preparation effects when RSI was varied on a trial-to-trial basis, but not when it was held constant within a block of trials. They offered an analogical explanation for the finding in terms of the costs of initiating a reconfiguration that had no chance of being completed on the intermittent and unpredictable short-RSI trials (see also Monsell & Mizon, 2006; for an alternative explanation of this finding see Altmann, 2004).

It has also been intimated in this review that it may be unwise to assume that residual switch costs have been isolated in experiments employing only a single long preparation interval. This skepticism derives from a literature review and series of experiments reported by Altmann (2004), who found overwhelming evidence that advance task set reconfiguration is unlikely to occur unless subjects are exposed to multiple preparation intervals, and concluded that the mechanism behind preparation effects is, at a minimum, lazy. Meiran et al., (2000), have also provided evidence of the fickleness of preparatory processing by documenting reductions in the magnitude of the preparation effect on switch costs at intermediate SOAs in a task-cuing paradigm as a function of the probability of encountering long versus short CTIs (manipulated between subjects). Specifically, advance reconfiguration with a 432 ms CTI was dramatically reduced when the bulk of the experimental trials featured CTIs of 1000 ms or longer relative to when the majority of trials featured CTIs of less than 432 ms. Note that while this latter result stands somewhat in contradiction with Rogers and Monsell's (1995) results, it is nonetheless in agreement with Altmann's observations, and strongly suggests that unless subjects are, with sufficient regularity, deprived of the opportunity to prepare, they are unlikely to engage in effective preparatory reconfiguration, even when they are afforded the opportunity to do so.

That motivational factors may be potent determinants of the likelihood and/or effectiveness of preparatory reconfiguration was seized upon by De Jong (2000), who proposed that advance reconfiguration reflects a process of *intention activation*, and also took the more radical step of specifying a *failure-to-engage* (FTE) hypothesis that extended his view of the preparatory component to cover residual switch costs as well. Specifically, De Jong argued that, rather than reflecting inevitably incomplete preparation on each switch trial, residual slowing resulted from no preparation having occurred on a subset of those trials. That is, according to the FTE hypothesis the distribution of RTs on switch trials with a long preparation interval is composed of a mixture of two distributions, one comprised of unprepared switches and the other of prepared switches, with the latter assumed to be identical in location, shape and scale to the distribution of repeat trial RTs. The FTE hypothesis therefore views residual switch costs as artifactual, and accordingly attempts to shift the focus from estimation of the magnitude of the preparation effect in terms of RT to estimation of the probability of reconfiguration. These ideas are embodied in the following simple model proposed by De Jong:

$$F_{long/switch}(t) = \alpha * F_{prepared}(t - \delta) + (1 - \alpha) * F_{unprepared}(t),$$

where $F_i(t)$ is the cumulative distribution function (CDF) indicated by i, α is the probability that intention activation, that is task set reconfiguration, will take place during the RSI, and δ is a parameter representing any residual slowing not overcome by preparation, such that in its strongest form the FTE hypothesis predicts that $\delta = 0$. For modeling purposes, De Jong suggested that $F_{prepared}$ could be estimated by the empirical distribution for repeat trial RTs, whereas $F_{unprepared}$ could be estimated by the empirical distribution for switch trial RTs on short-RSI trials.

De Jong (2000) tested the predictions of the intention activation and FTE hypotheses in a pair of studies in which block length was the primary methodological variable of interest. He reasoned that consistency of intentionality was likely to be suboptimal in long runs of trials, and therefore anticipated reduced residual switch costs, and consequently higher estimated values of α, in short relative to long blocks. The results conformed strongly to expectations, as longer RSIs were associated with a reduction in RT switch costs, and residual switch costs were reduced in shorter blocks. In terms of model parameters, estimates of δ hovered closely around zero regardless of block length, and estimates of α were as high as .8 in short blocks, but were in the .5 to .6 range in long blocks, as predicted by the intention-activation and FTE hypotheses. In the more concrete terms of the empirical distributions themselves, the consistent pattern was one in which variability was markedly increased for long-RSI switch trials such that the fastest RTs in this condition were just as fast as the fastest repeat trial RTs, with the difference between the two distributions becoming progressively more pronounced in the higher deciles as though the switch trial distribution had been stretched by pulling on the upper tail while the lower tail remained anchored. By way of contrast, the short-RSI switch trial distribution exhibited a scale and shape much closer to that of the repeat trial distribution such that the slowing reflected in average RT switch costs with a short RSI appeared to be a considerably truer representation of what for the most part amounted to a rightward distributional shift.

Much as was the case with Gilbert & Shallice's (2002) PDP model, De Jong's (2000) FTE hypothesis offers an attractively parsimonious account of task switching in terms of a single cognitive phenomenon, and in so doing presents a strong challenge to some prominent existing theories. Taken at face value it threatens to invalidate any explanatory perspective on switch costs that derives from the assumption of separable preparatory and residual components thereof. The proposal is not without its critics, however. Two general points are that: a) it is not clear how the model could both maintain its simplicity, and at the same time account for the range of manipulations that variously interact, or combine additively, with preparation in terms of their impact on TS costs (*c.f.*, Meiran & Chorev, 2005), and b) the fact that De Jong's mixture model is capable of providing reasonably good fits to data does not in and of itself provide a strong justification for tacit acceptance thereof. Nieuwenhuis and Monsell (2002) have also questioned why it should be the case that direct and extensive motivational manipulations still failed to push estimates of α above .7 in their own experiment if ostensibly avoidable failures of intentionality are indeed the sole cause of switch costs. A more mathematically principled criticism was recently levied by Brown, Lehmann and Poboka (2006), who pointed out that binary mixture models such as De Jong's necessarily predict that crossover points in the contributing distributions should be invariant across different mixture probabilities (that is, irrespective of the value of α), and demonstrated a statistically significant deviation from this prediction in data obtained using an alternating-runs design. They argued that while this finding constituted an unambiguous falsification of the simplest variant of the FTE hypothesis, it could nonetheless be accommodated with relatively minor tweaks to the theory, and outlined as an example a *shift-FTE* model, in which changes in RSI produced shifts in the RT distribution in addition to changes in α.

Several other researchers have proposed reinterpretations of the distributional patterns that motivated the FTE model. Lien, Ruthruff, Remington and Johnston (2005), reasoned that even if preparation could under certain circumstances achieve a complete reconfiguration of task set, and even if the switch/long-RSI distribution was composed of a simple binary mixture of prepared and unprepared trials, it need not be the case that the probability of preparation was the same for all S-R mappings comprising the task set. Instead, they argued that the beneficial effects of reconfiguration might depend on the activation of particular S-R mappings, and accordingly that subjects might rather tend to selectively prepare for only a subset of these mappings on long-RSI trials. Evidence in support of their conjecture was claimed on the basis of a series of experiments in which spatial positioning of S-R pairs appeared to determine which mappings evinced complete advance reconfiguration (*i.e.*, which mappings exhibited switch costs that were not significantly different from zero) and which did not (*i.e.*, which mappings exhibited robust residual switch costs). Monsell and Mizon (2006) subsequently reported a partially successful replication of this result for RT, but not error data. As was briefly hinted at by these authors, however, that the spatial position of S-R mappings should serve as the organizational structure around which advance reconfiguration is enacted is perhaps an unreliable convenience which, even if Lien et al.'s *partial mapping* (PM) hypothesis were correct, does not necessarily imply that the mapping-specific effects should always be so readily observable. In particular, there seems no reason to expect that the subset of S-R pairs that undergo preparatory reconfiguration should necessarily be consistent throughout the experimental session, much less equivalent across subjects. In so far as the generation and evaluation of testable predictions from the PM hypothesis depends to some extent on the ability to distinguish S-R pairs that undergo preparation from those that do not on *a priori* grounds, this represents a potentially serious obstacle to the implementation of a productive research program built around the model tenets.

Meiran (2000a, 2000b) has proposed a model to account for performance in the spatial judgement tasks he had employed in his pioneering work with the task-cuing paradigm (Meiran, 1996). Subjects in these experiments signaled either the vertical or horizontal position of stimuli within a 2 x 2 grid as instructed by arrow cues that explicitly and unambiguously indicated the appropriate task. While the model was designed with those specific tasks in mind, it is nonetheless worthy of consideration in the broader context of the present discussion for several reasons, including but not limited to the perspective it offers on De Jong's (2000) distributional observations and the alternative account favored by Lien et al., (2005).

Meiran's proposal is expressly componential, embodying a philosophical antithesis with De Jong's (2000) minimalist FTE hypothesis, and shares several features with the perspective that has been gradually taking shape in this review (for the full model specification see Meiran, 2000a). Structurally, TS costs are distinguished from TM costs, and are further subdivided into preparatory and residual components. The latter is assumed to reflect the duration of an obligatory response selection stage, whereas the former is thought to reflect a process termed stimulus-set (S-set) biasing. The S-set – the mental representation of the grid and imperative stimulus – which can presumably be optimized for judgments of either verticality or laterality, comprises one of three task sets that may be relevant on any given trial. The others are two response sets (R-sets), which can be dynamically distinguished according to whether they were relevant on the preceding trial (Prev. R-set) or not (Alt. R-set). It is assumed that the S-set can be adjusted during the preparation interval so that S-set biasing constitutes an extra process that affects performance only on short-CTI, or unprepared, switch trials. By comparison, the dominance of the now-irrelevant Prev. R-set on switch trials cannot be overcome in advance of actual performance of the task. A final important specification concerns a process called R-set adjustment, which occurs sometime *after* response selection and achieves a stronger biasing of the system in favor of what will become the Prev. R-set on the next trial. This is, of course, counterproductive on switch trials, and is in fact conceived of as the root cause of residual TS costs.

The hypothesized process of R-set adjustment provides the mechanism by which Meiran (2000a) argues that his model is capable of accommodating the suggestion that long-CTI switch trial distributions may comprise a mixture of fully prepared and relatively unprepared responding. Specifically, if R-set adjustment were to occur on only a proportion of trials, then the duration of the response selection stage on switch trials that immediately followed these instances of non-adjustment would be expected to nearly equal the duration of the response selection stage on task-repeat trials. In combination with completed preparatory S-set biasing on long-CTI trials we would predict from this that some percentage of prepared switch trial RTs would be sampled from a distribution that was largely indistinguishable from the repeat-trial RT distribution.

A direct application of the logic employed in the preceding paragraph to the claims made by Lien et al. (2005) regarding the spatial determination of which mappings are and are not consistently reconfigured is largely untenable since Meiran's (2000a) explanation of the ostensible distributional mixing focuses on the probabilistic absence of a particular type of post-response processing that would not be expected to lead to effects that were specific to particular S-R pairs. Taking some artistic liberties with Meiran's proposal, however, a slightly different perspective is suggested by considering his arguments regarding the unique necessitude of response selection for reconfiguration of the R-sets. Specifically, assume for the moment that, given that preparatory S-set biasing has occurred on a long-CTI switch trial, subjects are able to work through the process of identifying and selecting a response to an internally generated stimulus image without actually producing a response. If this process were sufficient to achieve repeat-trial levels of bias in favor of the response selection pathways for the imagined S-R pair, the result would be a subset of trials on which subjects effectively guess right (*i.e.*, on which the imagined S-R pair is concordant enough with the actual S-R pair that the full spectrum of preparation serves to facilitate responding), and are therefore able to perform at a level commensurate with what would be expected on repeat trials. Of some relevance to this hypothesis is a finding reported by Hubner, Kluwe, Luna-Rodriguez and Peters (2004), who observed larger effects of CTI length on RTs to tasks that featured fewer S-R links.

Meiran's *componential* model was also used to derive a pair of novel predictions concerning the impact of stimulus and response valence manipulations on the preparatory and residual components of the TS cost, respectively (2000b). With regard to the former, the componential model assumes that the sole purpose of S-set biasing is to overcome the effects of stimulus bivalence. Accordingly, Meiran argued that when the previous stimulus, the current stimulus or both are univalent, S-set biasing should be largely unnecessary, with the implication that short-CTI/switch RTs, absent the contributions of the extra process of S-set biasing, should themselves reflect primarily the residual component of the TS cost. This prediction was strongly supported. Switch costs under these conditions were found to be reduced relative to when both the previous and current stimulus were bivalent, and preparation accomplished little in terms of further reducing this cost. It was similarly claimed that the lengthening of response selection stemming from R-set competition should only be observed when response sets are bivalent. To test this, Meiran varied the S-R mappings according to task so that subjects responded to one task with one hand and to the other task with the opposite hand. He reasoned that this arrangement should serve to isolate the preparatory component of the TS cost as the only contributor to slowing on short-CTI/switch trials. Consistent with expectations, results indicated that preparation was sufficient to bring about a virtually complete elimination of switch costs.

The componential model (Meiran, 2000a) has proven to have enduring explanatory power in light of more recent findings regarding preparation effects. One example comes from Rubin and Koch (2006), who employed a design in which they manipulated the correlational patterns between stimulus color and task identity across the experimental session using tasks derived from Meiran's. A performance cost was observed when the task implied by the color of the imperative stimulus was invalid. Critically, the impairment was disproportionately evident on switch trials following a short CTI, but reduced following a long CTI. This result can be interpreted in line with Meiran's componential model by assuming that subjects were able to engage in preparatory S-set biasing on long-CTI/switch trials that largely prevented the occasionally spurious stimulus coloring from playing too large a role in response selection processes.

A final result that suggests the need for some restriction of the assumed generality of the componential model was first reported by Hunt and Klein (2002), and concerns the responsiveness of TS costs to preparation when switching between pro- and antisaccades (saccades directed towards, or away from, a target onset, respectively). In so far as the R-sets for the saccade tasks are bivalent, it may be suspected that robust residual costs should be observed when switching from one task to the other. On the contrary, whereas TS costs were found when the CTI was short, lengthening of the preparation interval was sufficient to completely eliminate residual costs. Meiran (2000a), himself had permitted that the hypothesized resistance of R-sets to preparation might not apply to designs that involved simple mapping reversals. However, Hunt and Klein also found that preparation was not sufficient to overcome residual switch costs in an analogous manual response task, a finding that they subsequently replicated in an experiment that sought, yet failed, to bring manual-task performance in line with saccade-task performance through the maximization of S-R compatibility for one of the tasks (Hunt, Ishigami and Klein, 2006). Thus, it appears that saccade tasks may constitute a special case with respect to the advance reconfiguration of R-sets.

Meiran's (2000a) suggestion that switch costs may be determined in part by post-response selection processes has an often overlooked, but potentially important implication for interpretation of the effects of manipulations centered on the preparation interval. Specifically, unless such processing has naturally terminated by the time the response is executed, then a portion of what we have thus far tacitly assumed is a unified process of advance reconfiguration that occurs during the RSI will of necessity be concerned not with preparing for the upcoming trial, but rather with bringing to a close the cognitive cascade triggered by the onset of the imperative stimulus on the preceding trial. Put more simply, determining when a given trial has truly ended may be a considerably more difficult task than determining when the next trial has begun. An obvious conclusion from this is that PCI duration may play a critical role in determining whether and to what extent post-response selection processes affect RTs. Indeed, although evidence indicates that the effects of PCI manipulations can in some cases be accounted for simply by assuming a process of passive decay that leads to marginal reductions in switch costs (Meiran et al., 2000), Altmann (2005) has found, just as with the CTI, that these reductions may only be observed when PCI is manipulated within subjects. This is at a minimum consistent with strategic (although not necessarily conscious) modulation of post-response processing since the identity of the upcoming task remained in doubt until the cue was presented.

A poignant illustration of the importance of these issues relating to processing during the PCI centers on an effect first reported by Mayr and Keele (2000). At issue in their design was the question of whether subjects would evince selective impairments when switching back to a recently abandoned task set as opposed to one that was less recently performed. It may at first seem counterintuitive that having recently performed a task would not have a beneficial effect on performance. However, these authors reasoned that *backwards inhibition* (BI) of task sets might occur when they are switched away from, and that the gradual decay of this inhibition over time would mean that immediately switching back to that task, as opposed to performing another task that was not as recently inhibited, would be relatively difficult. They tested this prediction by defining three task sets, *A*, *B* and *C*, and presenting them in sequences of two types: *ABA* and *CBA*. In the *ABA* triplet the second occurrence of task *A* corresponds to switching back to the most recently abandoned task set, whereas in the *CBA* triplet task *A* represents the task that was less recently performed, and *ex hypothesi*, should be relatively weakly inhibited and easier to perform, leading to smaller RTs in this case. The predicted *lag-2 cost* is then operationalized by subtracting the average RT to task *A* in *CBA* triplets from the average RT to the second occurrence of task *A* in *ABA* triplets, with the expectation that the observed difference should be positive.

These predictions were confirmed in a series of experiments that documented moderate (typically 15-50 ms), but reliable lag-2 costs (Mayr & Keele, 2000). The authors also demonstrated that the effect could not be accounted for by negative priming of repeated stimulus attributes, was only elicited when top-down task set selection occurred, and obtained even when subjects had complete advance knowledge of the sequence they were to perform. Most importantly for the present discussion, lag-2 costs were not reduced with the longer CTI, and in fact appeared, if anything, to be somewhat larger for the moderate- and long-, as compared to short-CTI conditions when RSI was held constant. A failure of CTI to modulate the contribution of lag-2 costs to TS costs, specifically (as opposed to TS RTs, as in the other experiments), was also reported, in this case with the PCI held constant. Together, these results strongly imply that BI, if it is indeed the causative factor of lag-2 costs, selectively affects the residual component of TS costs. This claim is further strengthened by evidence that lag-2 costs can be elicited by shifts of response modality, *i.e.*, by R-set switches (Phillip & Koch, 2005), and may be reduced or reversed when response selection processing is curtailed or eliminated in trial $n - 1$ because of a stop-signal at target onset or the interposition of a simple-RT task in place of the usual choice-RT task (Schuch & Koch, 2003; but see also Koch, Gade & Phillip, 2004).

Returning to the issue of RSI manipulations and their affect on the lag-2 cost, it is interesting to note that while none of these studies produced direct evidence that a longer CTI could overcome the impairments, some evidence indicated that BI might be reduced when the PCI was extended (Koch et al., 2004; Phillip & Koch, 2005). Indeed, Mayr and Keele's (2000) observation that lag-2 costs tended to be larger with a longer CTI can just as easily be rephrased to state that lag-2 costs were reduced with a longer PCI since the two varied inversely in the contrasts of interest. It is largely beyond imagining how or why a long PCI would permit subjects to overcome BI when a long CTI seems utterly incapable of producing a similar result. Deliverance from this intellectual quandary comes with the realization that overcoming BI is wholly unnecessary when it is not elicited in the first place, and in the studies of the lag-2 cost reviewed thus far a long PCI between trial *n* – 1 and trial *n* has implied a long PCI between trial *n* – 2 and trial *n* – 1. This suggests the hypothesis that a long PCI before trial *n* – 1 may allow task *A* from trial *n* – 2 to settle so that the need for brute force inhibition thereof is reduced when switching to task *B* on trial *n* – 1. Accordingly, we would expect that response selection processing for task *A*, when it is again performed in trial *n*, would be subject to less interference relative to when the PCI to trial *n* – 1 was short. Gade and Koch (2005) provided a test of this possibility by manipulating PCI to trial *n* – 1 and trial *n* independently of one another. Consistent with the prediction outlined above, lag-2 costs were markedly reduced when the PCI to trial *n* – 1 was lengthened. No consistent effects of the PCI manipulations on trial *n* were observed. These results provide a clear demonstration that RSI manipulations that have no discernable impact on the extent or effectiveness of preparation for the task required on a given trial *n* can nonetheless significantly impact factors that contribute to TS costs by interacting with post-response selection processing from earlier trials.

The final matter that will be pursued before concluding this section is a theoretical specification, in relatively concrete terms, of the nature of the cognitive processes that support advance reconfiguration of task set. A number of different proposals have been touched on to this point: Fagot (1994) attributed preparatory effects to a ballistic process of setting a response selection mechanism for one or the other task, Rubenstein et al. (2001) referenced a process of goal setting, De Jong (2000) hypothesized that preparation involved intention activation, and Meiran (2000) argued that advance reconfiguration in his spatial judgment task involved the adoption of an optimized perceptual stance. While these suggestions arguably run the gamut from overly vague to lacking in generality, one all-too obvious commonality among them is that the work of advance reconfiguration in each is understood to depend critically on the ability of subjects to retrieve task rules from long-term memory (LTM). This realization also accords with results showing that practice and preparation can have overlapping effects, as reviewed earlier in this chapter, and further suggests that intra-experimental practice specifically may have its beneficial effects on task switching largely through a facilitation of the LTM retrieval of task rules.

Mayr and Kliegl (2000) explored the role that LTM retrieval plays in task switching more generally, and preparation effects more specifically, in a series of experiments in which they had subjects make either semantic judgments about words (size or living/nonliving), or episodic judgments relating to a previous learning phase of the experiment (location or color of earlier word presentation). Using an alternating runs approach with AAAABBBB sequences, they found that switch costs, which were extremely large to begin with (in several cases more than 1000 ms), were considerably larger when the current trial involved an episodically mediated judgment. A less pronounced exacerbation of TS costs, which was additive with the slowing associated with performance of an episodic task on the current trial, was also observed when the previous task was episodic. While the latter result seems to fit best with carryover of activation accounts (although the authors note that it is at odds with the paradoxical asymmetry predicted by TSI), the former clearly indicates that TS costs were sensitive to the LTM retrieval demands of the task on the current trial. More critical for the current discussion are Mayr and Kliegl’s findings regarding preparation and the arbitrariness of explicit cues. In a final experiment they utilized a between subjects cuing manipulation in which some subjects were presented spatial cues that lacked any prepotent links to the tasks they signaled, and others were presented cues that effectively depicted the appropriate task as well as the associated S-R mapping rules. The makeup of the RSI was also manipulated to permit estimation of the impact of lengthening the PCI and the CTI independently. Results for the spatial cues indicated that TS costs were again larger for the episodic task, but that lengthening of the CTI was not only sufficient to reduce TS costs overall, but also to largely eliminate the asymmetry between the episodic and semantic trials. By comparison, TS costs were in general smaller, and differed only minimally between the episodic and semantic tasks regardless of the length of the CTI, for the group that saw nonarbitrary cues. Furthermore, preparatory reductions in TS costs for these subjects appeared to depend on the length of the RSI, rather than specifically on the length of the CTI. Thus, preparation during the CTI given spatial cues, and the presentation of cues that expressly indicated the appropriate S-R rules for the upcoming task, appeared to overlap almost completely in terms of their ability to overcome asymmetries in the performance of the episodic and semantic tasks, and additionally appeared to have similar overall effects on switch cost magnitudes. This strongly suggests that nonarbitrary cues and advance reconfiguration given an extended CTI both serve to facilitate the LTM retrieval of task rules and/or the integration of those task rules into real-time working memory processes.

As a final comment on Mayr and Kliegl's (2000) findings, it is interesting to note that the most conspicuous example of non-overlap between preparation and nonarbitrary cues centered on the behavior of TS costs as a function of whether the previous task had been episodic or semantic. The pattern for the nonarbitrary cues was broadly comparable to what was observed when TS costs were analyzed as a function of retrieval type on the current trial, but whereas preparation with spatial cues was sufficient to reduce TS costs regardless of whether the previous task had been episodic or semantic, it was not sufficient to overcome the previously noted asymmetry in TS costs between these two conditions. Taken together, these results suggest: a) that the effects of previous retrieval type affect the residual component of TS costs, strengthening the contention that they reflect carryover of activation, which in turn implies, b) that explicit inclusion of the S-R mapping rules in the nonarbitrary cues may have permitted subjects to overcome some proportion of the residual component of the switch cost. This latter point is consistent with, the synthesis of De Jong's (2000) FTE model, Lien et al.'s (2005) PM hypothesis and Meiran's (2000a) componential model outlined earlier in this section.

A number of other studies have produced results that lend credence to the LTM retrieval hypothesis of Mayr and Kleigl (2000). For example, Arbuthnott and Woodward (2002) replicated Mayr and Kliegl's findings concerning cue-task relations by observing reduced TS costs with prepotent verbal cues relative to arbitrary spatial and symbolic cues given a constant CTI. Other evidence has also suggested that the ability to maintain an active linguistic representation of the task identity in working memory may in some cases be a necessary component of the LTM retrieval processes that occur during the preparation interval. An early demonstration of this was provided by Goschke (2000), who found that preparatory reductions in TS costs could be blocked by having subjects produce an irrelevant word during the RSI, but remained evident when subjects named the task that they were to perform on the next trial instead. Miyake, Emerson, Padilla and Ahn (2004; see also Logan, Schneider & Bundesen, 2007; for a counterexample given a heavier working memory load, see Kleinsorge, Schmidtke, Gajewski & Heuer, 2003), subsequently replicated this basic result, and also reported that whether or not articulatory suppression interfered with preparation depended on the manner in which tasks were cued. Specifically, when the cues were the names of the tasks themselves (SHAPE and LETTER) switch costs given articulatory suppression were comparable to those observed with a long and unfilled CTI. On the other hand, when the cues were the letters S and L switch costs with articulatory suppression were comparable to those observed with a short CTI.

A further demonstration of the role of LTM retrieval of task rules in producing TS costs was provided by Dreisbach, Goschke and Haider (2006a; 2006b) in a pair of studies that produced results reminiscent of previously reported findings regarding the effects of stimulus valence (*e.g.*, Allport et al., 1994; Fagot, 1994). These authors employed a constant RSI without any explicit cues, and had subjects learn S-R mappings for eight words. Two task sets were defined, each uniquely applicable to only four of the words (*i.e.*, the stimuli were univalent), which were themselves presented in either green or red according to the task that they were relevant to. The critical manipulation centered on if and when subjects were provided with information about the underlying task rules, which were not necessary for correct responding. Findings (2006b) indicated that performance was better overall, and TS costs were absent, when subjects were not informed of the task rules. On the other hand, explicitly informing subjects about the underlying task rules led to either marked (if information was given early in the session) or relatively moderate (if information was given later in the session) slowing primarily on TS trials, consistent with the notion that performance was negatively affected by the retrieval of LTM rules on TS trials in these cases. Furthermore, the slowing on TS trials appeared to be only temporary, and was largely overcome by the end of the session, consistent with the suggestion above that practice and LTM retrieval act primarily on the same (preparatory) component of TS costs (although note that because stimuli were gradually introduced at a rate of two per block other explanations of this pattern are possible). Interestingly, it appeared to be the case that *explicit* knowledge of the underlying task rules was necessary to produce this spike in TS costs. (2006a). Specifically, results with transfer stimuli that were variously congruent or incongruent with the task rules (owing to color-task mismatches), indicated that even among subjects who had been given no information about the task rules, and who were not slowed on TS trials, implicit knowledge of the color-task relationships from earlier in the session led to slower responding on incongruent transfer stimuli.

A final relevant set of results concerns the effect of transitioning between sequences of tasks, as distinct from switching between task sets, per se. This was first explored by Lien and Ruthruff (2004), who manipulated temporal and spatial factors in the design of their experiments in an effort to encourage subjects to form cognitive hierarchies around pairs (*AB*), or triplets (*ABA* vs. *AAB*), of trials. The question they asked was whether transitioning from one sequence type to another (*BA* – *AB*), as opposed to performing a sequence repetition (*AB* – *AB*), would lead to impairments in performance. Results indicated the presence of marked slowing on the first trial after a sequence transition in spite of the fact that, as is evident in the examples above, these sequence transitions were accompanied by task repeats. That is, the usual task repetition benefit was in some cases nonexistent or even reversed – dependent upon the extent to which experimental factors indicated the existence of the intended hierarchy – when the task repeat occurred across a sequence transition. Schneider and Logan (2006b; for a similar example with intra-trial judgment sequences see Luria, Meiran & Dekel-Cohen, 2006) later replicated and extended this basic result under conditions in which subjects repeated, or switched between, learned sequences (*e.g.*, *AABB* versus *ABBA*). These authors also reported that first-trial slowing could dominate the usual task-repetition benefit even when the sequence repeated, with sequence complexity (more slowing for sequences that featured more task switches) playing a crucial role in determining the extent to which this effect was observed. Perhaps most instructive, first-trial slowing in this latter case was shown to be differentially sensitive to manipulation of the between-sequence RSI, indicating that at least some portion of the impairment observed for task repetitions may have reflected processing related to the preparatory component of task set reconfiguration.

Lien and Ruthruff (2004) initially proposed a *dual-route model of executive task control* to explain their results, arguing that a conditional processing route specialized for task set switches was invoked at the start of each sequence. Relating this suggestion to the broader context, it seems likely that the LTM retrieval of task rules at sequence transitions may play a critical (but not unique) role in producing first-trial slowing given a task repeat, which would explain the partial susceptibility of these sequence transition costs to lengthening of the inter-sequence RSI. It is also interesting to consider this proposed role for LTM retrieval in connection with Schneider and Logan's (2006b) observation that sequence complexity contributed to sequence transition costs. Although the requisite methodology for determining whether the first-trial slowing from sequence complexity could be endogenously overcome was not employed, their results leave open the possibility that the LTM retrieval processes that have been proposed to underlie the preparatory component of TS costs may be concerned not merely with task rules, but also with contingencies regarding the likelihood of future reconfiguration.

It should be noted that the phenomena currently under discussion presumably overlap to a considerable extent with an effect known as the restart cost, which references a tendency for RTs on the first trial of a run to be selectively slowed, even when the task repeats, as a function of prior experience with a competing task set, and was originally interpreted as reflecting the retrieval of S-R bindings (Allport & Wylie, 2000). A somewhat different account of restart costs more in keeping with the LTM retrieval hypothesis has recently been suggested by Bryck and Mayr (2008). They had subjects perform pairs of tasks with marked asymmetries in strength/S-R compatibility (ipsilateral and contralateral manual responses or Stroop tasks) and utilized a longer temporal delay between pairs of trials (500 or 5000 ms) than within pairs of trials (50 or 500 ms) to elicit restart costs. At issue were two questions: a) whether a paradoxical asymmetry would be evident in restart costs (larger costs for the stronger task) when the task repeated between pairs as well as when the task switched between pairs, and b) whether the magnitude of the asymmetry would be sensitive to the between-pairs RSI. Paradoxical asymmetries were observed for restart costs on both TS and TR trials, and both the restart costs themselves and the asymmetry were most pronounced when the RSI between runs was 5000 ms. These findings were accounted for in line with the LTM retrieval hypothesis by assuming: a) that restart costs on repeat trials resulted from a loss of set during the longer RSI, and the resulting necessity of re-retrieving task rules from LTM, and b) that greater attentional involvement/control when performing the weaker task resulted in memory traces for the performance of that task accumulating more quickly – perhaps owing to a higher probability of encoding – than memory traces for the performance of the dominant task, with the result that these traces for the weaker task interfered with LTM retrieval of task rules for the dominant task to produce the paradoxical asymmetry on the first trial of a run, whether or not that first trial involved a task switch. The authors also argued that the paradoxical asymmetry in restart costs given a task repeat cannot be explained by carryover accounts such as Gilbert and Shallice's (2002) PDP model, although it should be noted that this does not necessarily imply that carryover of activation is not involved in producing paradoxical asymmetries on switch trial RTs within runs. Indeed, even in Bryck and Mayr's data the restart cost asymmetry appeared to be more pronounced on TS as compared to TR trials. It is not possible to ascertain how these restart costs are impacted by preparation given existing data.

To conclude, this section has reviewed a range of phenomena and theories that are in various ways relevant to our understanding of the nature of cognitive processing during the RSI. On the basis of this review the following points seem worthy of restatement: a) advance reconfiguration is an inherently unreliable phenomenon, and as such it seems unwise to assume that preparation for a task switch has occurred unless direct evidence of such an effect is provided, b) in spite of this, it may under some circumstances be possible to complete task set reconfiguration during the preparation interval on at least some proportion of trials through the activation of response selection pathways, c) post-response selection processes – which may include R-set biasing as well as BI – may play a role in determining how performance will be affected by manipulations centered on the RSI, and d) the bulk of the preparation effect owes to an ability on the part of subjects to retrieve (but not necessarily enact) task rules from LTM in response to either explicit cuing or predictable and repetitive sequencing of tasks. Consideration of the role of memory function in task switching will continue to play a prominent role in the research discussed in the final two sections of this introduction.

Cue-driven effects in task switching

In the latter parts of the preceding section considerable attention was given to a proposed role for LTM retrieval in the development of an empirically grounded conceptualization of the nature of the preparatory component of TS costs. This section will continue to consider the way in which memory (not just LTM) contributes to TS costs, but with a focus specifically on the task-cuing paradigm. The starting point for the discussion that follows is the simple, yet important and thus far overlooked realization that when task switches and repeats are indicated through the use of explicit cues with only a single cue defined for each task, task switches will necessarily be confounded with cue switches. That is, cue repeats always signal task repeats, and cue switches always signal task switches. Obviously, this presents a problem since, in so far as cue switches can be assumed to lead to some slowing irrespective of whether the task also switches or remains the same, TS costs calculated as the difference between task-repeat/cue-repeat and task-switch/cue-switch RTs will tend to overestimate TS cost magnitudes. Beyond merely interfering with efforts to quantify specifically TS-dependent slowing, an additional difficulty stemming from this concerns the impossibility of determining the extent to which preparation effects observed in studies that use the task-cuing paradigm with 1:1 cue-task mappings reflect processes that overcome the cost of switching tasks, per se, as opposed to the cost of switching cues.

Logan and Bundesen (2003) and Mayr and Kliegl (2003) independently developed a novel task-switching methodology, the cue-switching paradigm, to address these issues (see also Gotler et al., 2003). The strategy adopted in both cases involved a doubling of the number of cues used to signal each task, resulting in a 2:1 mapping of cues to tasks that theoretically permitted the independent estimation of cue-switch (CS) costs – defined as the difference between task-repeat/cue-switch (CS) RTs and task-repeat/cue-repeat (TR) RTs – on the one hand, and pure TS costs – defined as the difference between task-switch/cue-switch (TS) RTs and CS RTs – on the other. Logan and Bundesen found that pure TS costs were practically nonexistent, indicating that cue switching might be able to account for virtually all of the cost of switching tasks as traditionally defined. Furthermore, although preparation was effective in reducing TS costs relative to the TR baseline, the bulk of the effect was again borne by CS costs, rather than by pure TS costs. Mayr & Kliegl reported similar results, although these authors observed significant pure TS costs over and above CS costs. They also documented a double dissociation regarding the sensitivity of CS costs and pure TS costs to other experimental factors. Specifically, whereas data indicated that it was primarily the former that was reduced by practice and preparation, the latter was impacted by lag-2 task repetitions provided the cue also switched between trial $n - 2$ and trial n (the authors hypothesized that countervailing repetition priming effects may have been responsible for the specificity of this result), and also by response repetitions/switches.

Logan and colleagues have advanced a fairly aggressive reductionist agenda through theoretical and modeling approaches to account for this basic pattern of results. Logan and Bundesen (2003, 2004) proposed, tested, and endorsed a model developed on the assumption that in the task-cuing paradigm subjects may be able to execute a *compound-stimulus strategy* (CSS hypothesis) wherein the cue and imperative stimulus are integrated into a single representation from which the correct response can be uniquely deduced. From this they argued that when subjects are presented with the cue they identify it by engaging in a matching process that can access both short-term memory (STM) and LTM stores in parallel. On CS and TS trials, when the cue is presumably not represented in STM, only LTM matching is possible. However, on TR trials the cue from the previous trial may still be present in STM, leading to a standard race model prediction that RTs should be faster on these TR trials owing to the dual routes through which identification of the cue can occur. Note that according to this version of the CSS hypothesis (an alternate version augmented with a task-switching component was also specified and received some support) there is no prescribed role for task-set reconfiguration, and hence this account makes the strong claim that TS costs in the task-cuing paradigm can be accounted for completely as a byproduct of cue switching. Arrington and Logan (2004) subsequently sought to qualify whether the matching process specified by the CSS hypothesis should be conceived of as being episodic or semantic in nature by varying the number of imperative stimuli subjects encountered, and thus also the frequency with which they encountered them. The size of the stimulus set, which ranged between 8 and 640 exemplars, was not observed to impact the results, consistent with the idea that the CSS did not rely on episodic retrieval from LTM.

As noted above, the simplest formulation of the CSS hypothesis explains TS costs in standard task-cuing paradigms as being entirely due to cue-switching effects. Accordingly, the model in which the hypothesis was originally implemented predicted that pure TS costs should be absent entirely. Faced with evidence to the contrary (*e.g.*, Mayr & Kliegl, 2003), Schneider and Logan (2005) devised a revision of the CSS hypothesis that incorporated a priming mechanism, dependent on response categories rather than perceptual representations, through which both cues for a given task could be activated in STM when either one of the cues for that task was presented. For example, if one of the tasks involved an odd-even classification and could be cued by either of the words “odd” or “even”, flashing “odd” to the subjects would prime “even” as well. Given that some portion of the direct and crossover activation that resulted from cue presentation on a given trial $n - 1$ is carried over into trial n, this reformulation of the CSS model is capable of accounting for both CS costs and pure TS costs. The former in this case reflects stronger initial activation of the presented cue from trial $n - 1$ than of that cue’s response-category associate. By comparison, the latter reflects a relative lack of STM activation of the cues for the task not performed on trial *n* – 1 when trial *n* features a task switch, relative to the residual activation of the response-category associate of the cue presented on trial *n* – 1 when trial *n* features a task repeat with a cue switch. Note further that because pure TS and CS costs reflect the same set of processes according to this version of the CSS account, preparation effects should be evident for both.

Schneider and Logan (2005, Experiment 1) confirmed the ability of the revised CSS model to approximately capture patterns in CS and pure TS costs using tasks that required subjects to make odd-even and high-low judgments in response to digits, with the words “odd”, “even”, “high” and “low” used as cues. Two further issues raised in their report are also noteworthy. First, on the basis of a failure to observe an advantage for dual-word cues (*e.g*., “odd-even”) over congruent single-word cues (Experiments 2 and 3), the authors argued that theories of task switching that postulated a prominent role for the retrieval of S-R mapping rules were not viable. It is important to point out, however, that retrieval of task rules from LTM need not imply retrieval of rules that specifically relate decisions about stimuli to particular (and in this case, arbitrary) responses (*c.f.*, Meiran, 2000a, 2000b), and accordingly this conclusion should not be accepted without some reservations. Finally, in an attempt to generalize their model beyond the task-cuing paradigm, Schneider and Logan suggest that their model could be applied to other task-switching paradigms by assuming a similar set of processes acting on internally generated cues.

To an even greater extent than was the case with De Jong’s (2000) FTE model, the proposals made by Logan and colleagues regarding cue switching represent a strong challenge to established doctrine in the task switching literature as they threaten to reduce not merely one component of the TS cost, but rather the entirety thereof to the status of mere epiphenomenality. It should therefore not be surprising that their proposals have met with something less than tacit acceptance. Altmann (2006) levied several criticisms in arguing that TS costs could not be accounted for entirely by cue switches. Most notable among these, he found that RTs overall were markedly slowed by the use of 2:1 as compared to 1:1 cue-to-task mappings, and argued form this that the cue-switching paradigm fundamentally alters the task switching landscape and as such does not provide for a pure decomposition of TS costs as obtained in the task-cuing paradigm into CS and pure TS components. Altmann also reported that RTs on obligatory uncued repeat trials (the second in a two-trial pairing in which the task on the first trial was explicitly cued) were slowed by 24 ms when the preceding trial had featured a task switch relative to when it had featured a cue repeat, but not when it had featured a cue switch, furthering the argument that a simple reduction of TS costs to CS costs is at a minimum likely to be something of an overstatement.

Welcome progress towards a resolution of the cue-switching debate came from a group of recent studies that manipulated the probabilities of individual transition types (TR, CS, and TS) in the cue-switching paradigm. To set the stage, first consider an experiment conducted by Schneider and Logan (2006a). Relative transition frequencies of TR, CS and TS trials in their design were varied across sessions so that in a given session one of the three types would occur with a probability = .7 and the other two would occur with a probability = .15 each. Results indicated that depending on the frequency of the various transition types, both CS and TS costs could be evident, and both could be reduced by preparation. Of greater importance, the relative size of CS and pure TS costs were impacted in what would seem to be a fairly intuitive manner by the relative frequency manipulations. That is, CS costs were relatively inflated when TR trials were most frequent, pure TS costs were largest when CS trials were most frequent, and pure TS costs were smallest when TS trials were most frequent. Put simply, as the probability of a particular transition type increased, the speed of responding when that transition occurred tended to increase relative to when the other transitions occurred. The “relative to” qualifier is important in this case because the slowest RTs to TS trials (as well as TR and CS trials) were actually observed when TS transitions were most frequent (for corroborating evidence that frequent TS trials can reduce TS costs primarily by slowing responding on non-TS trials, see Dreisbach & Haider, 2006). Relying heavily on model fitting results, Schneider and Logan argued that the sensitivity of RTs to the probabilities associated with the various transition types reflected memory-based facilitation for specific cue-cue transitions that occurred more frequently.

Monsell and Mizon (2006) also conducted a series of experiments examining the impact of different transition probabilities on CS and pure TS costs in the cue-switching paradigm after identifying this variable as a possible contributor to divergent results across studies conducted in their own and in other laboratories. In one of their designs (Experiment 6) employing odd/even and high/low classification tasks comparable to those used by Logan and colleagues (*c.f.*, Logan & Bundesen, 2003) they manipulated transition frequencies between subjects so that for one group CS trials occurred with probability = .5 and TR and TS trials each occurred with probability = .25. By contrast a second group encountered TS trials most frequently (probability = .5) and TR and CS trials with reduced but equal frequencies (probability = .25 for each). Results were generally comparable to those reported by Schneider and Logan (2006a) in that preparation reduced both CS and pure TS costs (particularly when TS trials were less frequent in the latter case). Furthermore, TS costs were larger when CS trials were more frequent, and smaller when TS trials were more frequent. An alternate methodological strategy was employed in a pair of experiments in which TR trials never occurred and the probability of a task switch was expressly controlled (Experiments 4 and 5). Similar to the results described above, they found that pure TS costs for short CTI trials were largest when the ratio of TS to CS trials was 1:3, and smallest when the ratio was 3:1. On the other hand, pure TS costs with a long CTI exhibited almost no sensitivity to the frequency manipulations.

Although their results to some extent empirically corroborated Schneider and Logan's (2006a) findings, Monsell and Mizon (2006) offered a considerably different theoretical account. In line with Altmann (2006), these authors rejected the claim that TS costs could be explained entirely as a byproduct of cue switching, and further argued that the sensitivity of pure TS costs to relative frequency manipulations resulted not from the frequency with which individual cue-cue transitions occurred, but rather from strategic variables affecting processing at the level of the task set. Specifically, they proposed that when faced with a situation in which TS trials were relatively common, subjects might tend to initiate reconfiguration, or adopt a neutral task set, in advance of the cue being presented. Additionally (or alternatively), if cue switches were predictive of task switches, subjects might also start to reconfigure as soon as a cue switch had been detected. Thus, the pure TS costs and preparation effects observed when task switches were improbable, both in general and conditional upon cue switches, were argued to result from subjects refraining from advance reconfiguration until the cue had been fully identified and processed. By comparison, the relative lack of pure TS costs or preparatory reductions thereof when TS trials were more frequent was assumed to reflect frequent anticipatory reconfiguration in advance of cue identification that both facilitated performance when the task switched, and impaired performance when the task repeated.

Although Logan et al. (2007) have questioned the validity of some of the assumptions made by Monsell and Mizon (2006), in particular their suggestions that subjects could adopt neutral task sets and detect a cue change faster than they could identify the cue, a direct test of the predictions made by Schneider and Logan's (2006a) variant of the CSS model and a *task-level adaptation account* similar to Monsell and Mizon's proposal overwhelmingly favored the latter. Mayr (2006) subdivided the possible cue-cue transitions on TS trials into two subsets. The frequency with which transitions in the first subset (TS-1 transitions) occurred was equated to the frequency with which TR and CS transitions each occurred. The second subset (TS-2 transitions) could then either occur with high frequency or low frequency in a between subjects manipulation. Thus, the probability of a task switch could be varied (via TS-2 frequency) without affecting the relative frequencies of the cue-cue transitions for TR, CS and TS-1 trials to permit evaluation of the claim made by the task-level adaptation account that trial types, as opposed to specific cue-cue pairings, are impacted by the frequency manipulations. At the same time, the RTs for more and less frequently encountered cue-cue transitions (TS-1 and TS-2 RTs, respectively, when TS-2 frequency was low; vice versa when TS-2 frequency was high), could be directly compared to determine whether, as predicted by Schneider and Logan's CSS hypothesis, frequent cue-cue transitions are associated with faster responding. With regard to the first prediction, TS-2 frequency had opposite effects on CS and pure TS costs, such that when TS trials were more likely CS costs were increased and pure TS costs were reduced relative to when TS trials were less likely. This finding mirrored those discussed above from Schneider and Logan, and Monsell and Mizon, but in controlling for cue-cue transition frequency indicated clearly for the first time a result that directly contradicted the CSS model. A similar failure of Schneider and Logan's account was indicated in the test of the second prediction, where the data provided no support for the hypothesis that frequent cue-cue transitions are associated with faster RTs. On the basis of these findings Mayr argued, in keeping with Monsell and Mizon, that a high conditional probability of a task switch given a cue switch can lead subjects to anticipatorily abandon the previous task set when a cue switch is detected.

To summarize, the results reviewed above clearly indicate that cue switching is associated with RT costs, a result that will be of considerable importance in interpreting the existing literature dealing with the CMTS paradigm in the next chapter. It has also been shown that these CS costs are sensitive to practice and preparation, indicating that processing of cues is likely to comprise at least some portion of the preparatory component of the TS cost in the task-cuing paradigm. Although in some cases CS costs have been observed to account for the entirety of the slowing on TS trials in the cue-switching paradigm, particularly when TS transitions are highly probable, in general it does not appear to be the case that task switching in the task-cuing paradigm can be reduced to cue switching. Indeed, when TS trials occur more rarely in the cue-switching paradigm, pure TS costs can be of a considerable magnitude, and can themselves evidence sensitivity to preparation, although the precise nature of the advance reconfiguration that takes place in this case remains an open question. For example, it could simply be that subjects are reluctant to initiate reconfiguration until they have achieved a highly stable internal representation of the cue when task switches are, either in absolute or conditional terms, improbable, resulting in selectively slower RTs on TS trials with a short preparation interval.

Stimulus specificity

Although not expressly discussed in these terms, at the most basic level the cue-switching results reviewed in the previous section provide a clear demonstration that although TS costs may obtain across stimulus repetitions and switches, they nonetheless apparently display some degree of stimulus specificity, even if the stimuli in question are not the imperative stimuli to which subjects must actually generate a response. The final section of this chapter briefly considers evidence suggesting that imperative stimuli can also exert a memory-dependent influence on the magnitude of switch costs beyond the extent to which set-level bivalence has been established.

Several studies have produced indications that some effects that have been previously discussed may be sensitive to the context in which the imperative stimulus in a given trial was previously encountered. For example, Wylie and Allport (2000) reported evidence suggesting that a gradual increase in TS costs for a word reading task within a block featuring fully bivalent Stroop stimulus sets following exposure to a block of trials in which stimuli for the competing color task had all been univalent appeared to be contingent on a buildup of negative priming effects at the level of individual stimuli through the Stroop block. Allport and Wylie (2000, Experiment 5), also reported that restart costs on a word-reading task were more pronounced for Stroop stimuli that had been negatively primed as a result of having previously been responded to in line with a competing color task.

As another example, Yeung and Monsell's (2003b) observation that paradoxical asymmetries could result from recent intra-experimental practice was in fact contingent on both the preceding and the current stimulus having been previously encountered. Citing correspondence with Gordon Logan, the authors suggested the specificity of the effect might reflect a phenomenon they termed *process-level inertia*. According to this account, recent practice performing a given task *A* in response to a particular stimulus *X* increases the efficiency and likelihood of memory-based, as opposed to algorithm-based, response selection in line with task *A* occurring when *X* is again encountered. Consequently, when a less recently practiced task *B* is required in response to *X*, subjects may have to inhibit the memory-based response selection strategy that favors the now inappropriate task *A*. If this process-level suppression is then carried over into a subsequent task switch trial, performance of task *A* in response to a trained stimulus should be slowed accordingly. Note that this account could also be rephrased to emphasize a facilitative effect on repeat trials featuring trained stimuli and a recently practiced task when memory-based response selection was utilized on the preceding trial, *i.e.*, process-level repetition priming. This perspective is perhaps to be preferred given the fact that in Yeung and Monsell's data, repeat trial RTs to the more recently practiced task were only faster than repeat trial RTs to the less recently practiced task when the stimulus on trial $n - 1$ had been encountered earlier in the experimental context, whereas the monotonic ordering was reversed when the previous stimulus was novel. This interpretation of their results carries with it the implication that part of what subjects were able to accomplish through preparation in Yeung and Monsell's Experiment 5 (when the CTI/RSI was varied within subjects) was the prioritization of a memory-based response selection strategy on TS trials.

Moving on to highlight a more novel set of results, stimulus-specific priming effects on TS costs were also purportedly studied in a series of studies by Waszak, Hommel and Allport (2003, 2004, 2005), though it should be noted at the outset of a discussion of their findings that the phenomenon they investigated may more properly be identified as the restart cost, or alternatively the sequence transition cost. The reason for this is that they held the RSI constant in the range of 500 ms within runs when the task repeated, but increased it to 2000 ms or more between runs when the task switched. Rather than ensuring preparation as assumed by the authors, this strategy could have induced regular losses of set between runs (*c.f.*, Mayr, 2006), and thus it may be more appropriate to view their data as reflecting performance under conditions of minimal preparation. These qualifiers aside, their results are nonetheless instructive. For example, they observed exaggerated first-trial slowing on a word-reading task in response to bivalent stimuli that had been previously presented on trials requiring a picture-naming response relative to bivalent stimuli that had not been encountered on picture-naming trials (2003). Furthermore, the effect was more pronounced for stimuli that had been previously presented multiple times as opposed to only once on picture-naming trials, and was also found to be extremely long lasting (evident at lags as long as 100 trials). Curiously, the corresponding stimulus specific effects on picture naming trials seemed to be consistent across position in run. The authors interpreted this discrepancy as resulting from overlearning of the word-reading task. The item-specific effects on word-reading trials were also subsequently observed to generalize to semantic associates (2004).

It was also pointed out that two different types of priming could be responsible for the effects outlined above: a) competitor priming occurring when the currently irrelevant stimulus dimension was last presented on a trial in which the currently irrelevant task set was called for, and b) negative priming occurring when the currently relevant stimulus dimension was last presented on a trial in which the currently irrelevant task was called for. Waszak et al. (2005) investigated the contributions of these of these two types of priming by measuring first trial slowing according to whether stimuli were novel, competitor primed, negatively primed, or both competitor and negatively primed. For the word-reading task, when the stimulus set was relatively large competitor priming was observed to contribute to first-trial slowing without impacting repeat/second-trial RTs, whereas no effects of negative priming were observed. On the other hand, when the stimulus set was smaller both competitor priming and negative priming selectively interfered with first-trial performance, and additionally had additive effects on RTs. The authors argued from this that stimulus-specific priming effects are most likely to come into play when task competition is high.

The additivity of the competitor priming and negative priming effects reported by Waszak et al. (2005) are interesting to consider in the context of the preparatory/residual distinction in switch costs. If, as was argued above, and in contradiction of the authors' assumptions, first-trial costs in their design consisted of both preparatory and residual components, then it seems plausible that competitor priming may have affected one component and negative priming the other. In particular, in so far as competitor priming results from a bias towards selecting a response that is appropriate for the irrelevant stimulus dimension, it stands to reason that it should be possible to overcome the effects of this form of priming by biasing the perceptual representation of the imperative stimulus in such a way that processing of the irrelevant dimension is lessened, as with S-set biasing in Meiran's (2000a) S-R model. On the other hand, as S-set biasing would presumably not be sufficient to counteract negative priming of the currently relevant stimulus attribute, it is harder to see how preparation would be effective in reducing these effects. These considerations suggest the hypothesis that competitor priming should tend to affect the preparatory component of TS costs, and negative priming should tend to affect the residual component.

It is unfortunate that the existing literature on stimulus specific effects in task switching has generally failed to effectively manipulate (or analyze, *e.g.*, Hubner et al., 2004)) preparation to permit a more decisive treatment of these issues. One further result is noteworthy, however, for having at least employed variations in the duration and structuring of the RSI, albeit between subjects. Koch and Allport (2006) investigated the effects of stimulus specificity on TS costs by employing a consistent mapping of stimuli to tasks in training blocks and then reversing those mappings so that, for example, a stimulus that had previously only been presented on trials requiring an odd/even judgment would now be presented only on trials requiring a magnitude judgment. The question of interest was whether and to what extent this mapping reversal would result in impaired performance, and furthermore whether the extent of the stimulus-specific interference would be affected by the RSI manipulations. Results revealed that mapping reversals led to considerable slowing on both congruent and incongruent trials, indicating that the reversal had its effects at the level of the task-set. The magnitude of the reversal effect was considerably more pronounced for the short-CTI groups than for the long-CTI group, however, a pattern that was interpreted by the authors as indicating that stimulus-based priming acts on the preparatory component of TS costs. While this conclusion is to some extent consistent with the suggestion above that competitor priming, specifically, may act on the preparatory component of TS costs, it should be noted that lengthening of the CTI between groups was not observed to lead to a reduction in TS costs when RSI was held constant, consistent with expectations developed in this review given the fact that preparation was varied between subjects. In light of this, at least one alternative account seems deserving of mention. Specifically, because preparation was manipulated between subjects, it varied between groups not only after the mapping reversal, but also during the training blocks. It may therefore have been the case that post-mapping reversal differences between the groups had their origins in the training portion of the session.

To summarize, these results collectively provide clear evidence that TS costs can be influenced by stimulus-specific priming, even if it is less clear whether these effects should be conceived of as reflecting interference on the preparatory or residual component of TS costs, or both. One fact that is important to bear in mind is that in general these effects have been observed in designs in which the probabilities of particular stimuli occurring, or of them being paired with specific tasks, have differed markedly from chance levels. It does not therefore follow that these results necessarily document effects that are likely to be particularly pronounced or pervasive in designs that equate the probabilities associated with particular stimuli, or the mappings of those stimuli to specific tasks.

CHAPTER 2

Building on the foundations established in Chapter 1, we can now take up the more specific paradigmatic variant with which the experiments comprising this thesis are principally concerned: cross-modal task switching (CMTS). The aims of the current chapter are threefold. First, assuming a naïve perspective, some of the motivations underlying the development of the CMTS paradigm will be outlined, and a simple taxonomy of prospective outcomes and the reasons they might be expected to obtain will be discussed. In doing so we will rely on precedents of both outcome and interpretation established in: a) studies of task switching under unimodal stimulus conditions, as well as b) other cognitive subdomains that have compared performance under unimodal and crossmodal conditions. Second, two published studies of CMTS will be reviewed. In addition to a summary of the results they present, a critical consideration of several factors relating to methodology and theoretical interpretation will be undertaken. Finally, Experiment 1 will be presented, and its implications for understanding CMTS phenomena will be discussed.

Motivations and empirical expectations

Human cognition is not restricted to operating solely on information obtained through visual perception, but is of course concerned with making adaptive use of the other sensory modalities as well. More than that, much of what is most basic to the way people behave and think depends critically on the ability to coordinate function across, and transition between, multiple perceptual domains. The task-switching paradigm, however – perhaps owing to the inceptive belief shared by a number of early researchers that the phenomena they were exploring were centralized in the processing architecture – has only recently begun to expand its focus to explicitly document and interpret the effects of cross-modal stimulus conditions on TS costs. The elaboration of these efforts is justified not only for the sake of ecological applicability and generalizability, but also in light of TS theories that emphasize the role of stimulus factors and internal representations of stimulus dimensions in determining switch cost magnitudes.

Before investing ourselves in a discussion of how modality switching is likely to affect task-switching performance, it would seem worthwhile to consider the more basic question of how single task performance is affected by modality switching. As is the case with transitioning from one task set to another, shifts of stimulus modality are known to lead to performance decrements, a phenomenon known as the modality shift effect (MSE). The MSE has typically been studied with the primary aim of exploring differences between individuals suffering from schizophrenia and other populations distinguishable on the basis of alternative pathologies or a lack thereof, with results generally showing that individuals suffering from schizophrenia evince greater impairments in responding when the stimulus modality has changed from the previous trial, in particular when the current stimulus is auditory. The MSE was first reported by Sutton, Hakarem, Zubin, and Portnoy (1961), who interpreted their results as indicating, “that attention, or maximal readiness to respond (set), is not equally available to all sensory inputs at any given moment in time.” Subsequent research has indicated that the MSE may depend to some extent on whether the preceding stimulus has been oriented or responded to, as the slowing that results from having responded to a contramodal imperative stimulus in trial *n* – 1 has proven robust to the introduction of a modality non-predictive cue presented in advance of the imperative stimulus on trial *n* (Rist & Thurm, 1984; but see also Turatto, Benso, Galfano & Umilta, 2002). Furthermore, there is some reason to believe that a similar shift cost can be elicited by processing of concepts whose meanings are modality-specific in the absence of an external modality switch. Pecher, Zeelenberg and Barsalou (2003) had subjects make true-false judgments concerning the correspondence between prime and probe words that referenced concepts and modality-specific properties, respectively (*e.g.*, leaves – rustling), and gauged performance as a function of whether the probes on trial *n* – 1 and trial *n* referred to the same modality. Consistent with results from the standard MSE paradigm, RTs were slowed when the processing modality indicated by the probe changed from trial *n* – 1 to trial *n*.

The observation that both task and modality switching can lead to RT slowing (TS and MS costs, respectively, from this point forward) suggests that a reasonable preliminary question to ask is whether these two will combine additively or interact when both are switched concurrently, and if it is the latter, then will the interaction take the form of an overadditivity, an underadditivity, or perhaps even a nonmonotonicity (*i.e.*, a combined switch advantage)? Consider first the case of a simple additivity of TS and MS costs in determining the magnitude of the slowing incurred when task and modality switch concurrently. Several experiments in the task switching literature have manipulated the ease with which visual stimuli can be identified and measured the effects of these manipulations on TS costs (Mayr and Kliegl, 2000; Rubenstein et al., 2001). Most notable among these, and representative of the typical outcome, are a pair of experiments conducted by Oriet and Jolicoeur (2003, Experiments 1 and 2). Using an alternating runs paradigm with parity/magnitude tasks and both short and long RSIs, they varied the contrast level of imperative stimuli to produce RT slowing on low contrast trials, and found that this effect combined additively with the switch cost at both short and long RSIs, the latter of which were sufficient to bring about a preparatory reduction in TS costs. Working from the assumption of a serial architecture with (admittedly) vaguely specified early, central, and post-central stages, and employing the slack logic endemic to theorizing in the psychological refractory period paradigm, the authors concluded that task set reconfiguration (both the preparatory and residual components thereof) imposed a hard bottleneck on the system that prevented even early perceptual processing from taking place.

The veracity of Oriet and Jolicoeur's (2003) conclusions was arguably overstated. For example, Gilbert (2005) has shown that with some moderate tweaks his PDP model (Gilbert & Shallice, 2002), which assumes that reconfiguration and stimulus processing occur in parallel, is capable of reproducing an additivity of stimulus contrast and TS costs. Alternatively, it could be that the arrow of causation points in the reverse direction and, faced with a reduced contrast imperative stimulus, subjects delay reconfiguration until a stable and reliable perceptual representation of that stimulus had been constructed. More pertinent to the current discussion than the correctness of the hypothesis offered to account for these results, however, is the basic observation that a manipulation – reduced stimulus contrast – that presumably impacted processing in early perceptual circuitry produced RT costs that combined additively with the TS cost. To the extent that modality shifting reflects relatively inefficient processing in centers that precede response selection, as seems reasonable, MS costs might be expected to combine additively with TS costs in much the same way.

That said, coordinating task performance across two modalities might present a challenge to cognitive function that is not entailed by simple manipulations of the size, orientation, contrast level, etc. of stimuli presented only in the visual modality. If this were the case, then the additivity prediction derived above might represent a lower bound on combined-switch RTs, with some degree of overadditivity being the more likely outcome. That cross-modal stimulus situations could have such an effect has been suggested by Turatto et al., (2002), who proposed that central processing resources necessary for making complex decisions about stimulus properties could be differentially taxed by shifting between modalities. Brand D'Abrescia and Lavie (2008) made a similar suggestion on the basis of a demonstration that compatibility effects in a visual flanker task could be inflated when the flanker task was performed immediately after having performed an auditory discrimination task, but not after having performed a visual discrimination task.

On the other hand, a number of researchers have made arguments in favor of, or presented evidence consistent with the idea that no such interference should occur, which by extension implies that additivity should perhaps be viewed as a likely upper, rather than a lower bound prediction. To cite a pair of recent examples, Hanewinkel and Ferstl (1996) found that the MSE, but not response hand switching costs, served to distinguish between patients with schizophrenia and normal controls, suggesting that the two types of switches might act on distinct facets of the cognitive system. To the extent that task switching is comparable to the latter, this diagnostic dissociation might be expected to manifest as an absence of interference between task and modality switching in the CMTS paradigm. In line with this perspective, Gondan, Lange, Rosler and Roder (2004) have argued that the MSE reflects inefficiencies in unimodal processing centers. Further evidence comes from an experiment employing a cross-modal variant of the AB paradigm (Duncan, Martens & Ward, 1997). The authors in this study had subjects monitor two concurrent streams of rapidly presented stimuli either in the same or different sensory modalities for randomly occurring targets, and measured accuracy in detection of the second target as a function of the SOA between the first and second targets. Whereas interference in second target identification time-locked to presentation of the first target was evident for streams presented in the same modality, no such deficit was evident when the streams were segregated between the visual and auditory modalities.

The experiment reported by Duncan et al., (1997), would in fact seem consistent with the hypothesis that TS and MS costs might combine underadditively on combined-switch trials, with modality switches serving to bypass some portion of the interference that typically results from having performed a different task on the preceding trial. Similar results have also been reported by Potter, Chun, Banks and Muckenhoupt (1998), who observed that second target identification was, if anything, facilitated when the modality of the second target differed from that of the first. Interestingly, these authors found that whether time-locked errors in second target identification were observed at all in cross-modal trials appeared to depend on whether the task sets defining the first and second targets, respectively, were the same or different. Specifically, SOA-sensitive impairments were evident only when the task set switched, suggesting that even if TS and MS costs do combine underadditively, CMTS costs (defined as the difference between combined-switch RTs and MS RTs) should still be expected to obtain. Although favoring a contrary interpretation of the cross-modal AB, Arnell and Jenkins (2004) nonetheless offered a theoretical perspective on cross-modal interference effects in the AB paradigm that would seem at a minimum consistent with the observation of an underadditivty of TS and MS costs. Specifically, they argued that target detection under unimodal conditions is likely to be subject to both central and modality-specific sources of interference, whereas performance under cross-modal conditions would suffer only from the former.

Much of the evidence surveyed above comes from studies exhibiting marked methodological departures from convention in the task-switching paradigm, and as such there is reason to doubt whether the indicated conclusions should necessarily be directly applicable. It may therefore be asked whether there is any compelling precedent for underadditivity in the visual task-switching literature to counter the additivity prediction ostensibly favored by Oriet &

Jolicoeur's (2003) arguments. One intriguing and relevant theory derives from the work of Kleinsorge and collegues, who proposed a *hierarchical model* to explain a systematic pattern of interactions among dimensional switching costs. The empirical profile in question was first documented in an experiment in which subjects made either numerical or spatial judgments in response to peripherally presented digits (Kleinsorge & Heuer, 1999). In addition to switching between these two judgment types, not only the responses, but also the S-R mappings for the tasks could switch between compatible and incompatible arrangements (a simple reversal). Precues were not provided, and the spatial position and color of the imperative stimuli indicated the appropriate judgment and mapping, respectively. The authors hypothesized that this design would lead subjects to adopt a hierarchical dimensional organization of the task space with judgment type at the highest level, response at the lowest level, and mapping at an intermediate level. Supplemental to this hierarchical structure, the authors assumed that task switching might proceed in two stages: a) reconfiguration at the highest dimension represented in the hierarchy for which a shift is required, triggering a concurrent reconfiguration at all lower levels as well, and b) subsequent adjustment of these lower levels to reflect the specific demands of the current trial.

To illustrate, suppose that on trial $n - 1$ a numerical judgment had been required, and that the relevant S-R mappings were incompatible, necessitating a left hand response. Consider, then, a trial n in which a spatial judgment was required. According to the hierarchical model, reconfiguration of the cognitive system to perform the spatial task would lead to a biasing in favor of the compatible mappings and a right hand response, since these dimensions reside at a lower level in the hierarchy. If these biases were appropriate on task n, no further reconfiguration would be required and a right hand response would be generated with an RT reflecting slowing from stage (a), above. On the other hand, if either the mapping and/or the response remained the same as on trial $n - 1$ it would be necessary to adjust these biases in stage (b), leading to relatively slower responding. For the sake of completeness, if trial n required a repeat of the numerical judgment but a shift to the compatible mapping, this mapping shift would lead to a reconfiguration at the response level as well, but would not be expected to exert any (hierarchically) bottom-up effects on the judgment dimension. The predictions of the hierarchical model were generally confirmed by the empirical results of Kleinsorge and Heuer's (1999) study. In particular, switching responses was associated with RT slowing when the judgment and mapping remained the same, but led to faster RTs if the judgment, the mapping, or both also switched. Furthermore, isolated judgment or mapping switches each led to RT costs, with greater slowing evident for the former relative to the latter, but RTs for combined judgment and mapping switches were in fact faster than RTs for isolated judgment switches. That is, switch costs were not merely underadditive; they were nonmonotonic.

One issue with applying this result and theory to the study of CMTS is that in Kleinsorge and Heuer's (1999) study the dimensions in the hierarchy were defined not by stimulus characteristics, but by response, and response selection factors. Kleinsorge (2004) provided a more directly relevant demonstration of nonmonotonicity of switch costs in a dimensionally organized task space. Following Allport et al. (1994), Kleinsorge had subjects switch between judgments of parity and magnitude of numerical displays, where the judgments could be made on the basis of either the identity or the quantity of the numerical stimuli presented in the display. Unlike Allport et al., who used blocked switching conditions, in Kleinsorge's study all four trial types (judgment switch, stimulus dimension switch, both switch, and both repeat) could occur at random within a single block. No precuing was provided, as in Kleinsorge and Heuer. Results for this experiment also indicated that switch costs were nonmonotonic, such that isolated judgment and stimulus dimension switches both led to RT slowing but the cost of combined switches was reduced when compared to isolated judgment switches. Of note, switching judgment type was associated with a cost both when the stimulus dimension switched and when it did not. Viewed through the lens of the hierarchical model, these findings can be interpreted as indicating that judgment again occupied the upper most level in the dimensional hierarchy, so that a stage (a) reconfiguration of the stimulus dimension set was triggered whenever a judgment switch occurred.

The precise structure of the theorized hierarchy in this case most likely does not provide an entirely appropriate analog for the CMTS paradigm, at least under some conditions.

Specifically, in both the existing studies of task switching between modalities, to be discussed next, as well as in the novel results reported in this thesis, imperative stimuli appeared only in a single modality on each trial. Indeed, it would be patently absurd to expect such a methodology to support a hierarchical switching mechanism that would promote the shifting of attention away from the modality in which the imperative stimulus has just been presented merely because a task switch has been required. An inverted hierarchy with stimulus modality exerting some influence on task set would seem more reasonable.

To briefly recap the preceding discussion, a range of empirical results and theoretical perspectives with varying degrees of applicability to the CMTS paradigm have been reviewed. Although no clear consensus has emerged regarding the manner in which TS and MS costs should be expected to combine and/or interact on trials in which both types of switches occur, the greater weight of the empirical evidence surveyed to this point appears generally consistent with the hypothesis that TS and MS costs should be, at most, additive. As will be seen in the next section, this contention has received support in a pair of published studies documenting an underadditivity of switch costs in the CMTS paradigm.

Previous studies of CMTS

The first such demonstration was provided by Hunt and Kingstone (2004). The design they employed required subjects to make parity and magnitude judgments in response to digits that could be presented either visually or auditorily. An alternating-runs task sequence was employed with spatial position of the imperative stimuli serving to indicate the appropriate task (*i.e.*, no precues were provided), whereas stimulus modality varied randomly from trial to trial. An additional, and somewhat unusual feature of the methodology was that the rate at which stimuli were presented was held constant at 3500 ms, irrespective of how quickly (or slowly) subjects responded. Results indicated that both TS (78 ms) and MS (103 ms) costs were present, but that these costs combined underadditively when both task and modality switched, such that CMTS costs were only 36 ms, on average; roughly half of what was observed for modality-repeat conditions. Hunt and Kingstone, working from a perspective in which switch costs were conceived of as an index of executive function, interpreted their results as being inconsistent with theories that proposed the existence of a single bottleneck (as in, for example, Oriet and Jolicoeur, 2003), arguing instead that the most plausible explanation was that there were multiple bottlenecks – one for task switching and one for modality switching – but that these bottlenecks were linked through shared functions such as stimulus representation and attentional selection; the multiplicity of the bottlenecks accounting for the underadditivity, and the links between them accounting for the fact that CMTS cost were not entirely absent.

One issue that has not yet been discussed in the context of cross-modal task switching is the question of how the profile of switch costs (TS, MS and CMTS) will be affected by preparation. Even ignoring the indispensable role studies of the preparation effect have played in advancing theories of task switching in general, examination of the limits of advance reconfiguration would seem to be especially worthwhile for the CMTS paradigm. To see this, consider the following. As was shown in Chapter 1, there is good reason to believe that the preparatory component of the TS cost largely reflects processing related to the retrieval of task rules from LTM and the establishment of adaptive and flexible biases in the representation of stimulus sets. Foregoing for the moment any strong theoretical claims either in agreement with or contrary to Hunt and Kingstone's (2004) account of the switch cost underadditivity, we may ask to what extent the hypothetical components of reconfiguration noted above might affect task and modality switching processes, respectively. As one example, if switching between tasks primarily involves operations on amodal internal stimulus representations (as seems reasonable with Hunt and Kingstone's parity/magnitude tasks) then the aspects of preparation that are most critical to reconfiguration of task sets might be expected to have a relatively trivial effect on MS costs, and accordingly it would not be surprising to observe a preparatory dissociation between TS and MS trials. At least three different outcomes would then seem possible for CMTS RTs: a) to the extent that such amodal preparatory processing exhausts the scope of advance reconfiguration, owing either to systemic properties or restrictions in the amount of advance information provided to subjects (*e.g.*, when upcoming modality is not cued), CMTS RTs should react to preparatory processing in the same manner as TS RTs, b) if reconfiguration also includes a modality-specific component that is for any reason optimized for the modality encountered on the previous trial this would be expected to result in larger effects of preparation on TS as compared to CMTS trials, or c) if the modality specific component of reconfiguration were capable of optimization for either visual or auditory stimuli and modality were predictable or cued, CMTS RTs might benefit from reconfiguration to a greater extent than either TS or MS RTs, leading to a more pronounced underadditivity for prepared relative to unprepared switching.

Unfortunately, the methodology employed by Hunt and Kingstone (2004) provides very little in the way of answers to these and other speculations regarding preparation in the CMTS paradigm. Two aspects of their design, both previously stated, are critical in this respect. First and most important is the fact that the interval between successive stimuli was held constant at 3500 ms. Because of this it was not possible to analyze performance as a function of RSI, and as such it is an open question whether and to what extent their data reflect prepared as opposed to unprepared task switching. Going one step further, some proportion of the data may have been contaminated by restart costs, given that on trials when RTs were close to or faster than the mean RT (~ 1 sec) – almost certainly more than half the sample given the positively skewed character of RT distributions – the RSI would have been at least 2500 ms, which could be sufficient to induce occasional, or even frequent losses of set. Second, whereas the task sequencing was at least predictable, the modality sequencing was random. It is not known whether predictability effects are parallel and/or comparable in size on TS and MS costs, and hence it is equally unclear how this might have impacted the results.

These issues have also been noted by Murray, De Santis, Thut and Wylie (2009), who recently reported on a pair of experiments intended to more directly explore the effects of preparation in the CMTS paradigm. The design used by Murray et al. differed from the one used by Hunt and Kingstone (2004) in several respects. First, speculating that the extent of the functional overlap between the tasks subjects performed could influence the magnitude of switch costs, they used one spatial judgment, and one object classification task, with the intention of segregating processing for the two tasks between the “what” and “where” pathways to some extent. Second, they provided subjects with explicit cues. In Experiment 1 these cues were bimodal and signaled only the identity of the upcoming task. In Experiment 2, the stimuli were unimodal and indicated both the appropriate task and the modality in which the imperative stimulus would be presented (with visual cuing visual and auditory cuing auditory). A constant PCI and CTI of 500 ms and 650 ms, respectively, were used in both cases.

Beginning with Experiment 1, Murray et al. (2009) hypothesized that the use of bimodal cues specifying only the upcoming task would result in a profile of switch costs that was comparable to what was reported by Hunt and Kignstone (2004). Consistent with this expectation, an underadditivity of TS and MS costs on CMTS trials was observed, with CMTS costs (~ 90 ms) nonetheless clearly present and statistically significant. By comparison, the authors speculated that the use of unimodal cues in Experiment 2 would permit subjects to engage in modality-specific preparation for the upcoming task, thereby bypassing some proportion of the interference between task sets and resulting in reduced CMTS costs. This prediction appeared to receive mixed support. For the auditory modality, no discrepancy between the results of Experiments 1 and 2 was observed. For the visual modality, however, a numerically more pronounced underadditivity was reported for Experiment 2, and visual CMTS costs were a nonsignificant 22 ms, leading the authors to conclude that cuing for the upcoming modality in addition to the upcoming task had enabled subjects to switch tasks without incurring any cost; ostensibly a reflection of the independence of the control processes mediating task and modality switching, respectively.

In the analysis offered earlier in the prospective discussion of the effects of preparation on TS, MS and CMTS costs and the possible dissociations among them it was suggested that preparation could lead to a selective advantage for CMTS relative to MS trials (*i.e.*, to reduced CMTS costs) if either: a) the bulk of the preparation effect was concentrated in amodal processing centers, in which case preparation would have little impact on MS trials but would speed RTs on TS and CMTS trials, b) preparation was directed primarily, but not entirely towards the preceding modality so that CMTS trials would benefit more than MS trials, but not as much as TS trials or c) if relatively complete preparatory reconfiguration also involved a forward-looking modality-specific component and modality were predictable, since CMTS trials would then benefit from both amodal and modality-specific preparatory components whereas MS trials would benefit only from the latter. As Murray et al.'s (2009) results were argued to provide evidence of a comparative advantage for CMTS performance relative to MS performance, it may be asked whether any of these conditions may have been met; that is, whether the perspective on CMTS performance that motivated the specification of (a), (b) and (c), above, is tenable.

In fact, without some modification none of these three possibilities leads directly to the prediction that preparatory reductions in CMTS costs should result from the transition from modality-nonpredictive to modality-predictive cues. In particular, since in each case a preparatory advantage for CMTS relative to MS trials would be assumed to reflect amodal reconfiguration, to account for Murray et al.'s (2009) results it would be necessary to assume that subjects failed to take advantage of the task-specific information in bimodal cues, but made effective use of that information in unimodal cues. There are at least two potential reasons for this explanatory shortcoming. First, as arguments (a) through (c) were predicated on the decidedly amodal parity and magnitude judgments used by Hunt and Kingstone (2004), adjustment of the criteria to accommodate Murray et al.'s use of a spatial task, and of modality-specific (albeit associated) stimuli for the discrimination task, might be justified. For example, it could be that unimodal cues permitted greater task-specific reconfiguration because the tasks themselves were defined for stimulus sets that were to some extent modality-specific. Second, and of greater theoretical interest, it may reflect an important philosophical distinction between the control-laden perspective favored especially by both Hunt and Kingstone, and the perspective implicit to options (a) through (c), above. To wit, whereas Hunt and Kingstone, and Murray et al. both assume that functional overlap between task and modality switching will be indicated by slowing due to overload at processing bottlenecks, the alternative view suggested in this paper assumes that functional overlap will be indicated by a more pronounced underadditivity reflecting a consolidation of switching operations. In the spirit of this latter perspective, let us now consider in more detail the experiments reported by Murray et al. to ascertain whether their conclusions are justified by the data they report and the methodologies used in the collection of that data.

There are several aspects of the design and analysis employed by Murray et al. (2009) that cast considerable doubt on their interpretations of the results. For example, although the stated goal of their research was to explore preparatory effects in the CMTS paradigm, they utilized only a single, constant CTI. As previously discussed, it is possible, and perhaps even likely, that their data may reflect unprepared rather than prepared switching. It might be possible to counter this criticism if it could be shown that switch costs were larger in Experiment 1 (bimodal cues) relative to Experiment 2 (unimodal cues), however, with the exception of the nonsignificant visual CMTS costs in Experiment 2, no such effect was apparent. In fact, the most conspicuous difference between the results of Experiments 1 and 2 was a numerical (but unanalyzed) increase in visual MS RTs (and consequently in visual MS costs) in Experiment 2, as evident in the graphs presented by the authors. That is, whereas visual MS costs with bimodal cues appeared to be 100 ms or less, with unimodal cues they were at least twice as large. Importantly, it appeared that this increase in visual MS RTs across experiments, rather than a reduction in visual CMTS RTs (which was not in evidence), was sufficient to account for the apparent reduction in visual CMTS costs in Experiment 2. This observation is obviously difficult to reconcile with their implicit claim that unimodal cues served primarily to facilitate performance when the modality of the imperative stimulus switched.

Although seemingly counterintuitive at first glance, the finding that unimodal cues may have interfered with modality switching can be accommodated quite easily by reference to the cue-switching paradigm, as discussed in Chapter 1. Specifically, only one bimodal cue was used for each task in Murray et al.'s (2009) Experiment 1. Thus, MS trials were always preceded by a cue repeat. In Experiment 2, on the other hand, each task-modality combination was assigned a unique cue, so that MS trials would now be associated with a cue switch. It should therefore not be surprising that MS costs would be inflated by CS costs in the former case, relative to the latter. It should be pointed out, however, that auditory MS costs were, if anything, smaller in Experiment 2.

It is not clear what should be made of this difference, although one possibility might be that the balancing of the costs and benefits associated with the two cuing approaches differed between the two modalities, such that attentional orienting benefits associated with unimodal cues were larger when the upcoming stimulus was auditory, whereas priming-related cue switch costs could have been more pronounced when the upcoming trial was visual.

Before continuing, one further issue with the primary RT analyses presented by Murray et al. (2009) should be noted. In general, to the extent that TS costs have proven to be an exceedingly robust phenomenon, it may be argued that researchers who claim to have eliminated these costs face a somewhat more stringent burden of proof than might otherwise be the case for acceptance of a statistical null hypothesis – in this case, that CMTS and MS RTs do not differ. Although the authors did not report directly on the precision of the 22 ms cost they observed, it is possible to construct a confidence interval from the details they report regarding the nonsignificant t test of the difference between CMTS and MS RTs on visual trials. Taking Murray et al.'s reported $t = 0.67$, and $df = 13$, and solving for the standard error, $s = 32.8$, yields a 95% CI = [-49, 93] ms. Put simply, this range of likely values is far too wide to serve as adequate justification for the conclusion that CMTS costs are eliminated by preparation with unimodal cues, rendering any debate over the true underlying cause all but moot at this stage.

Before concluding this section, one further set of results from Murray et al.'s (2009) experiments is of interest. In addition to a traditional analysis of switch cost magnitudes, the authors also reported on a subset of the correlational patterns in their data sets. Again reasoning that switching modalities results in a bypassing of task set reconfiguration demands, they speculated that whereas within-modality TS costs are likely to be positively correlated between the two modalities (*i.e.*, high visual TS costs and high auditory TS costs tending to co-occur), owing to a common set of processes underlying task switching performance, a weaker relationship might entail for CMTS costs if competition between tasks is reduced on cross-modal trials. In fact, while they observed significant between-modality correlations of $r(14) = .522$ and $r(12) = .698$ for TS costs in Experiments 1 and 2, respectively, for CMTS costs the between-modality correlations of *r*(14) = -.109 and *r*(12) = -.072, did not approach significance. Although generally consistent with Murray et al.'s stated expectations, there are at least two reasons to exercise caution in interpreting this finding. First, as was the case with the visual CMTS cost magnitude in Experiment 2, the precision of estimation for Pearson's *r* is poor in general for small sample sizes (Experiments 1 and 2 collected data from 16 and 14 subjects, respectively), and this weakness is magnified when the absolute value of *r* is small. Second, in so far as statistically significant CMTS costs on the order of 90 ms were obtained for at least one of the sensory modalities in each of their experiments, performance still clearly suffered when subjects had to switch tasks on MS trials. Thus, there seems little justification for concluding that the lack of a clear association between visual and auditory CMTS costs indicates that task-set interference was bypassed in a literal sense. The less adventurous claim that the interference indexed by CMTS costs in the visual modality was to some extent distinct from the interference indexed by CMTS costs in the auditory modality is more defensible. Note that in principle this is likely to say just as much about the MS conditions for the two modalities – the baselines against which CMTS costs are calculated – as it says about the CMTS conditions. Other factors such as the specific tasks subjects perform, the manner in which tasks are sequenced, and the overlap of the stimulus sets for the two modalities might also figure importantly.

Given the surprising results reported by Murray et al. (2009) regarding the absence of a detectable relationship between CMTS costs in the two modalities, correlational patterns were also examined for the experiments presented in this thesis. Of note, correlations involving CMTS RTs, partialed with respect to MS RTs, are reported in lieu of correlations between RT difference scores (*i.e.*, CMTS costs). The reasoning behind this decision, quite simply, is that whereas difference scores are entirely natural for the estimation of the magnitude of interference effects from switching tasks, correlations lend themselves most directly to questions concerning shared and unique sources of variance. As these can be addressed using partial correlations, the construct of "switch costs" was deemed unnecessary for this component of the analyses.

Experiment 1

Although planned and carried out in advance of the publication of either Hunt and Kingstone's (2004) or Murray et al.'s (2009) results, Experiment 1, nonetheless addresses several of the outstanding issues related to preparation effects and cuing methods in the CMTS paradigm that were outlined in the preceding section. The essentials of the approach adopted are as follows. The tasks involved simple judgments of spatial position of visual and auditory stimuli, where task switching was operationalized through a complete reversal of the S-R mappings. Explicit cues were used to indicate the appropriate task and modality on each trial. A single cue was assigned to each Task-Modality combination, ensuring that all three switch-trial types (TS, MS and CMTS) were preceded by cue switches and were thus comparable. Finally, RSI was manipulated within subjects in such a way as to permit evaluation of the effects of changes in the durations of both the PCI (potentially affecting post-response processing and/or passive decay) and the CTI (presumably influencing the likelihood of active preparation taking place). It was expected that, as in previous studies of CMTS, the profile of switch costs would evidence an underadditivity. Of principle interest was the question of whether the Switch Type profile would be sensitive to the RSI manipulations, and if so, in what way.

Experiment 1 Methods

Participants

Twenty-four undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

Subjects were tested on IBM-compatible personal computers with 14" color monitors running E-Prime software, and a set of stereo headphones that covered each ear entirely. Visual cues consisted of a color change across the full vertical and horizontal extents of the screen from the default black to either red or blue. Visual choice-stimuli consisted of a filled in, white circle displayed on either the right or left side of the monitor. Auditory cues consisted of high-pitch (D7) and low-pitch (D3) tones presented simultaneously to both ears, whereas white noise presented either to the left or the right ear served as the imperative stimulus for the auditory modality. All auditory stimuli were presented at what was judged by the experimenter to be approximately conversation-level intensity. Assignment of cues to tasks was counterbalanced across subjects. No fixation stimulus was provided.

Procedure

Subjects were seated approximately 65 cm in front of the monitor and were tested individually, with headphones worn throughout the experiment. Training consisted of a series of displays containing written instructions on how to perform the tasks, and included a single exposure to each of the four cues accompanied by an explanation of what the cues indicated. These instructions were read to the subjects by the experimenter. Both tasks required subjects to respond to the spatial location of the target stimulus, and differed only in the response mappings they employed. One task, introduced to subjects as “Presence” in the instructions, required an ipsilateral response to the stimulus, while the other task, labeled as “Absence”, required a contralateral response. Subjects were instructed to perform the tasks as quickly as possible while still maintaining high accuracy and responded by making single key-presses with either their left or right-hand index finger. The “S” key was used for left hand responses, and the “K” key was used for right hand responses. There were no differences between the target stimuli or the response-sets for the two tasks.

Three timing conditions were used. The first condition, 27/200, featured a PCI of 27 ms, followed by a CTI of 200 ms. The second condition, 27/653, featured a PCI of 27 ms, followed by a CTI of 653 ms. The third condition, 480/200, featured a PCI of 480 ms followed by a CTI of 200 ms. The three RSI’s varied between blocks, cycling completely through once every three blocks. Ordering of the RSI’s was counterbalanced between subjects.

Subjects completed a total of 54 randomly ordered blocks, each consisting of 66 trials. One subject completed a version of the experiment that contained 60 blocks. Data from the last 6 blocks for this subject were discarded prior to analysis. The first two trials in each block were filler trials, and the remaining 64 trials were sequenced so that each block contained exactly one observation of each possible condition defined by the factorial structure: task, previous task, modality, previous modality, response, and previous response (or, equivalently, stimulus identity and previous stimulus identity). Thus, taking into account the three RSI levels used, subjects were exposed to each possible two-trial sequence at least once every three blocks. At the end of each block subjects were presented with a screen reading: "Press any button to continue". Incorrect responses were followed by a screen informing the subject that they had made an error. This screen remained present for 1.5 seconds before trials resumed without requiring any further action on the subject's part.

Experiment 1 Results

Analysis of the RT Switch Cost Profile

Data were cleaned prior to analysis by first excluding from further consideration trials on which computer timing errors occurred, trials on which subjects responded incorrectly, and trials following incorrect responses. The first nine blocks completed by each subject, and the first two trials in each block were also excluded. In the former case this was done to minimize the influence of changing skill level through the experimental session, and inspection of the data revealed that RTs had largely stabilized by this point. Exclusion of the first two trials in each block was done to minimize the influence of warm-up effects. The final 6 blocks for the subject who completed 60 blocks were also excluded, as noted above. Finally, data from any remaining block in which the proportion of errors was greater than .3 were eliminated from further consideration under the assumption that data from these blocks was unlikely to reflect the cognitive processes of interest. Data points for the analyses were then determined by: a) calculating the median for each cell in the factorial structure defined by the variables Subject, RSI, Task, Modality, Task Switch, Modality Switch, and Response Switch, and then b) averaging over Response Switch, which was not analyzed. These values served as the cell means for tests of the highest-order interactions. Marginal means for calculation of lower-order effects were determined by collapsing over ignorable variables. For convenience, in addition to the TS, MS and CMTS designations, from this point forward trials on which both task and modality repeat will be referenced as RR trials. These four conditions resulting from the factorial crossing of the variables Task Switch and Modality

Switch are often discussed as though they comprise a single factor, Switch Type. Similarly, the four conditions resulting from the factorial crossing of the variables Task and Modality were treated as a single factor, Trial Type, in the analyses. The statistical approach adopted in this experiment and those that follow is detailed in Appendix A, which the reader may now wish to consult.

Means corresponding to the interactions between Switch Type and RSI, and Switch Type and Trial Type, are depicted in Figures 1 and 2, respectively. The results of the primary analyses, excepting the interactions involving both RSI and Trial Type, are detailed in Table 1. The findings (with significant effects from the higher interactions and *post hoc* comparisons noted where appropriate) can be summarized as follows. Lengthening the PCI given a short CTI, and the CTI ignoring PCI, both led to a reduction in RR RTs. It was also observed that these effects were more pronounced for Auditory than for Visual trials, $F(1, 23) = 37.58$, $p < .001$, and $F(1, 23) = 135.26$, $p < .001$, respectively. Visual RR RTs were faster than Auditory RR RTs, and the same was true for the Ipsilateral relative to the Contralateral Task. These latter two effects interacted in a manner consistent with scaling, such that the task differential was largest on Auditory trials. The Ipsilateral advantage was likewise inflated for the long CTI relative to the two short CTI conditions, $F(1, 23) = 13.13$, $p < .002$. Marginal TS and MS costs were observed, and both were larger for the Auditory relative to the Visual modality. A paradoxical asymmetry was also apparent for TS costs, which were larger for the Ipsilateral task. Both TS costs, and to an even greater extent, MS costs, were reduced in the 27/653 condition. Although falling just short of established significance levels, there was some indication that the paradoxical asymmetry in TS costs may have been reduced by lengthening the CTI, $F(1, 23) = 9.78$, $p < .005$, 95% CI = [9, 45]. Finally, a marginal CMTS advantage was observed, and was detected as being more pronounced in the 27/200 condition than in the 480/200 condition. Visual inspection of the data indicated that the differences between CMTS and MS RTs in the 480/200 and 27/653 conditions were comparable, but of a reduced magnitude in comparison to the 27/200 condition. A *post hoc* contrast supported the conclusion that the CMTS advantage was most pronounced in the short RSI condition, $F_S(2, 23) = 10.46$, $p_S < .014$. 95% confidence intervals for CMTS cost magnitudes for each RSI (negative values indicate CMTS advantage) yielded CI = [-55, -26] ms for the 27/200 condition, CI = [-26, 2] ms for the 27/653 condition, and CI = [-30, 5] ms for the 480/200 condition.

Analysis of the Error Switch Cost Profile

Data preparation for analysis of errors was accomplished largely in the same manner as that outlined for RTs, with the obvious exceptions that trials on which errors occurred were not excluded from the analysis, and that the percentage of valid trials on which errors occurred served as the per-subject, per-cell dependent variable. Because the experiments were not designed to measure accuracy, analysis of errors was undertaken primarily to ensure that none of the principal findings from the RT analyses could be attributed to a speed-accuracy tradeoff. Median error percentages for the four Switch Types at each RSI level, and for each Task/Modality combination, are presented in the bottom panels of Figures 1 and 2, respectively. The data speak for themselves in clearly indicating that neither the RT switch cost underadditivity, nor the CMTS advantage reflected such a tradeoff. Two secondary observations that may indicate the presence of speed-accuracy tradeoffs are the numerically larger error percentages in the 480/200 RSI condition, in particular as compared to the 27/200 condition, and also the tendency for accuracy to be better on the Contralateral relative to the Ipsilateral task.

RT Correlations

Correlations were calculated on data prepared in the same manner as described for RT analysis of the switch cost profile. The correlation between Visual and Auditory CMTS RTs partialed with respect to Visual and Auditory MS RTs yielded $r(20) = .03$, $p < .89$, replicating Murray et al.'s (2009) findings. By comparison, the unadjusted correlation between Visual and Auditory CMTS RTs was $r(22) = .66$, $p < .001$. Further context for these results is provided by the relationship between CMTS RTs and MS RTs within each modality. For both Visual and Auditory trials the value obtained was $r(22) = .98$, $p < .001$.

Discussion

Experiment 1 was conducted first and foremost to evaluate the effects of preparation on the Switch Type profile. At issue was the question of whether CMTS costs would evidence any sensitivity to a prolongation of the CTI when explicit cues that indicated both the upcoming task and modality were presented. With respect to existing CMTS literature, a finding of reduced CMTS costs would provide support for Murray et al.'s (2009) claims regarding the ostensibly reduced CMTS costs for the visual modality in their Experiment 2. The results of Experiment 1 violated such expectations in at least two ways. First and most strikingly, CMTS costs were entirely absent in the present results, and a significant CMTS advantage was in fact observed in the 27/200 RSI condition. Given the lack of precision already noted in Murray et al.'s results, this represents the first convincing demonstration that CMTS costs may under some circumstances be entirely absent, and goes even further in demonstrating that switching tasks cross-modally can facilitate performance relative to when the same task is performed in a new modality. Second, against expectations derived from Murray et al.'s interpretation of their own results, there was no evidence to suggest that lengthening of the CTI led to a reduction in CMTS relative to MS RTs. On the contrary, it was the combination of a short CTI and a short PCI that produced the largest difference.

It is important to point out that the ineffectiveness of preparation with respect to the relative standing of CMTS and MS RTs in the long RSI conditions was not reflective of an overall failure of the longer CTI to bring about improvements in switching performance. In contrast to previous studies of CMTS, Experiment 1 systematically manipulated the RSI level within subjects, permitting a direct test of the assumption that preparation occurred. Strong confirmation of this assumption was evident, as TS costs, which were not affected by the length of the PCI, were clearly reduced by the introduction of a long CTI, and MS RTs were even more strongly influenced by the length of the CTI than were TS RTs. Thus, the invariance of the CMTS advantage across CTI conditions given a long RSI does not reflect a failure of preparatory processing on CMTS trials, but rather gives the impression that when the RSI was held constant preparation was equally effective in reducing RTs on trials in which the modality of the stimulus switched, irrespective of whether the task also switched.

Carrying this perspective a bit further, one possible interpretation of the larger CMTS advantage observed for the 27/200 condition as compared to the 27/653 condition is that lengthening the RSI by way of the CTI produced greater preparatory facilitation on MS trials than on CMTS trials. Of course, since CTI and RSI are confounded for this particular comparison, a second possibility that must be considered is that extending the CTI in particular was not so critical as prolonging the RSI in general, and that passive decay rather than active reconfiguration mediated this effect. The relative merits of these two perspectives will be given further consideration below. To foreshadow, although the choice between them given the present data must be made on inductive grounds, the former account is favored.

The divergent CMTS advantage magnitudes between the two short CTI conditions, on the other hand, are less likely to reflect advance reconfiguration. For one thing, while lengthening the PCI did lead to a reduction in RR RTs when the CTI was short, and while the general trend evident in the data was for RTs overall to be reduced in the 480/200 relative to the 27/200 condition, neither TS costs, nor the relative magnitudes of TS and MS RTs, were observed to differ between the two short CTI conditions. On contextual grounds, then, the differences between the magnitudes of the CMTS advantages in the 27/200 and 480/200 conditions are probably not due to preparation effects according to the common meaning of the term in the task switching literature. Furthermore, given the observation that error percentages were numerically larger in the 480/200 condition than in the two short PCI conditions, the RT advantage enjoyed by the former relative to the 27/200 condition may have resulted in part from a speed-accuracy tradeoff. Whereas in the case of the longer CTI it makes considerable sense to hypothesize that the MS/CMTS imbalance present in the 27/200 condition was overcome by preparation, for the long PCI it seems at least as likely as not that the imbalance was never present, or was present only to a reduced extent, in the first place.

The primary challenges in interpretation of the switch cost profile and the manner in which it was affected by the RSI manipulations would thus appear to be threefold. Specifically, explanations are required for: a) the absence of CMTS costs, where previously only an underadditivity had been observed, b) the presence of a CMTS advantage that was possibly unique to, and/or more pronounced for, the shortest RSI condition, and c) the observation that an increased opportunity for preparation in the form of a longer CTI appeared to be, if anything, most effective for MS trials.

Considered in isolation, the absence of CMTS costs is entirely consistent with the hypothesis that switching modalities permits subjects to bypass task-set interference. The account's attractive simplicity quickly becomes an encumbrance, however, when we attempt to devise from it a reasoning by which a CMTS advantage and a disproportionately large preparatory effect on MS trials should be expected to obtain. More problematic still is the fact that considerable slowing was evident on MS RTs. While this monotonic ordering is not unprecedented in the CMTS literature (*c.f.*, Hunt and Kingstone, 2004), the MS costs obtained in Experiment 1 (and in previous CMTS studies) were nonetheless considerably larger than what is typically found in investigations of the MSE (*c.f.*, Rist & Thurm, 1984).

This latter realization in fact strongly suggests that MS trials in the CMTS paradigm do not represent an analog for a task-repeat baseline condition, but are themselves subject to the deleterious effects of task-set interference or uncertainty. This being the case, an alternative account of the absence of CMTS costs is seen to be worthy of consideration, namely that switching tasks cross-modally in Experiment 1 did not produce any evidence of further slowing relative to MS trials because MS RTs already reflected slowing from task set (re)configuration requirements. An immediate dividend of this interpretive angle is the explanation it provides for the relatively pronounced reduction in MS RTs brought about by lengthening the CTI. Recall from earlier the suggestion that greater preparation effects on CMTS trials relative to TS trials might be expected if the former benefited from both preparatory attentional orienting and modality-specific task-set reconfiguration. This effect was not anticipated for MS trials under the assumption that subjects would already be prepared for the upcoming task when only the stimulus modality switched. If, however, as argued above, unprepared MS RTs reflect suboptimal task readiness, then the arguments concerning CMTS RTs can be extended in straightforward fashion to account for the larger preparatory reductions observed for MS RTs. That CMTS and MS RTs were similarly affected by lengthening of the CTI when the RSI was held constant is likewise easily accommodated so long as the extent of task unpreparedness for each switch type in the 480/200 condition is assumed to have been equivalent. It should be noted that other explanations of this effect beyond a partial additivity of modality- and task-directed preparation effects are possible, however. For example, following De Jong (2000), it could be that modality switches made reconfiguration more likely by reducing failures to engage on MS and CMTS, relative to TS trials. Alternatively, it could be that there was simply more room for preparatory reductions in MS RTs than in TS RTs. Consistent with this latter suggestion, the switch cost profile was very nearly flat for Visual trials with a long CTI, perhaps indicating that the RT floor had been reached. Note, however, that these possibilities are not mutually exclusive, and the effect could just as easily have resulted from a combination of any of the above.

The possibility that CMTS costs were absent because of a functional overlap between the processing operations required on MS and CMTS trials also provides a framework for understanding the results of the correlational analyses. Replicating the results reported by Murray et al. (2009), the partial correlation between Visual and Auditory CMTS RTs eliminating both Visual and Auditory MS RTs did not approach significance. While in this author’s view the virtual absence of any association between two such similar performance measures should have been largely unthinkable on *a priori* grounds, it has now been observed across different combinations of tasks and explicit cuing paradigms and it is therefore important to consider its context more explicitly to better understand its origins. To this end, in Experiment 1 of this thesis correlations between Visual and Auditory CMTS RTs, as well as between MS and CMTS RTs in the same modality were both examined. A reasonably strong positive relationship between Visual and Auditory CMTS RTs was apparent before partialing, indicating that the absence of an effect in the analogous second-order partial correlation did not reflect a more fundamental performance dissociation. More striking, however, were the extremely strong within-modality correlations between MS and CMTS RTs. The picture painted by this collection of results is one in which the nil correlation between CMTS RTs eliminating MS RTs seems likely to reflect the fact that the overlap between MS and CMTS RTs within each modality was so pronounced that once performance on MS trials had been taken into account the remaining sources of theoretically meaningful variance in CMTS RTs were overwhelmed by random error or other variables that were not systematically or consistently related to cross-modal task set reconfiguration. As with the failure to observe CMTS costs, while this could be argued to reflect a bypassing of task set interference, the slowing evident on MS trials again argues that the presence, rather than the absence of interference is the more likely culprit.

Returning to the consideration of the switch cost profile, while the hypothesis that MS RTs may have been inflated because subjects tended to reconfigure task set on these trials does not, in and of itself, directly indicate a solution to the problem of why a CMTS advantage would have obtained in the short RSI condition, it does lend itself to a relatively simple explanation of this phenomenon when married to the hierarchical model proposed by Kleinsorge and Heuer (1999). Indeed, adopting the perspective of the hierarchical model provides some further justification for the claim that MS RTs were victimized by incompletely or incorrectly specified task sets as well. Specifically, if it is assumed that the switching hierarchy is arranged in such a way that stimulus modality (perhaps more accurately, perceptual attentional orientation) is superordinate to task set, then the hierarchical model predicts that the detection of a shift in stimulus modality should trigger a shift in the task set subjects have adopted, leading to relatively poor performance on MS RTs, since in this case the modality switch would be expected to lead them to initially prepare for the wrong task. By comparison, on CMTS trials, a hierarchical switching mechanism that acted in this way would be expected to promote faster responding, since in this case the linkage of task and modality would lead to a correct anticipatory stance.

But why, then, should this CMTS advantage be evident to a statistically significant extent only in the shortest RSI condition? An initial consideration regarding interpretive perspective is whether it will be sufficient to consider merely the sum of the parts (*i.e.*, the length of the RSI), or whether such a dutifully parsimonious approach might fail to capture the essentials of the phenomenon; put another way, is there something special about the combination of a short CTI and a very short PCI that produced the significant nonmonotonicity. While this is properly an empirical question for future research endeavors, it does have implications for the manner in which the CMTS advantage, and more specifically the relative lack thereof in the two long RSI conditions, should be treated theoretically. In particular, if the length of the RSI is assumed to be the determining factor, then a single mechanism will be sufficient to account for the divergent results. On the other hand, if the particulars of the components of the RSI each contribute something unique to the effect, then the explanation for the relative similarity of MS and CMTS RTs in the 480/200 condition will necessarily differ from the explanation for the 27/653 condition.

At the risk of introducing unnecessary complications, the latter perspective is the one favored here, for several reasons. First, to the extent that lengthening the overall RSI leads to passive decay of activation from the preceding trial, this would be expected to have, if anything, an effect opposite the one that was observed, since on MS trials this decay would presumably reduce the magnitude of any repetition benefit that might otherwise obtain, and on CMTS trials the decay would counteract any carryover of interference from the abandoned task set. Of note, however, TS costs were not affected by the length of the PCI, contrary to what would have been expected if passive decay were impacting switch cost magnitudes. Second, the differential effectiveness of the CTI with respect to switch RTs in general, and MS as compared to TS RTs more specifically, calls into question the wisdom of treating a portion of the CTI in the 27/653 condition as if it were causatively equivalent to a portion of the PCI in the 480/200 condition. Third, in a similar vein, experiments reported later in this thesis provide examples of: a) two different RSI conditions with equivalent overall durations having different effects on CMTS relative to MS RTs (Experiment 2), as well as b) two RSI conditions with different overall durations producing statistically indistinguishable CMTS costs (Experiments 4 and 5A). Finally, the complications resulting from prospectively distinguishing between the effects of a long CTI and a long PCI are minimal, and the account derived from adopting this more detailed perspective is straightforward and demands little accommodation from the suggestions already made regarding the findings for MS and CMTS RTs.

To explain the relative absence of a significant CMTS advantage in the 480/200 condition, it is proposed that the execution of a response was followed by a transient state of heightened sensitivity to the task-switch-inducing influences of an unexpected shift in the modality of the cue. One possible reason for such an effect might be that it simply takes time after having selected and produced a response for subjects to reorient themselves to the structure of the overarching session-level task set such that they are able to treat the next trial as a discrete event in which the task and stimulus modality are free to vary independently of one another; in effect, an intentional blink. More adventurously, a critical period in post-response processing (*c.f.*, Meiran, 2000a) could potentially expose a functional vulnerability to a hierarchical switching mechanism. In either case, if this susceptibility was reduced at 480 ms relative to 27 ms post-response, as seems possible, then presenting the cue in a new modality would be expected to lead to a larger relative facilitatory effect on average in the 27/200 condition than in the 480/200 condition, as was observed.

Turning to the comparison of the 27/200 and 27/653 conditions, by the preceding comments the same reactive shift of task set would be expected in both cases when the modality of the current trial differed from the modality of the preceding trial. However, given the considerable effectiveness of the CTI in bringing about preparatory reductions in TS costs, as well as in reducing both MS and CMTS RTs, it seems reasonable to expect that any advantage the task set not carried out in trial n – 1 might have enjoyed immediately after presentation of the cue would be somewhat diminished by the time the imperative stimulus was presented in the long CTI condition. Put more simply, the suggestion is that initial shifts of task-set on trials in which the modality switched were triggered hierarchically in both the 27/200 and 27/653 conditions, but in the latter case this early bias was largely overcome by active reconfiguration based on the identity, and not merely the modality, of the cue. Evidence consistent with the notion that preparatory processing in the explicit cuing paradigm can lead to a reduction in the magnitude of a potentially hierarchically dependent underadditivity has also been presented by Kleinsorge, Heuer and Schmidtke (2002), in an experiment featuring a 1500 ms RSI with a 1200 ms CTI (see also, Hahn, Andersen & Kramer, 2003).

To this point it has been argued that the prevailing determinant of the findings regarding MS and CMTS RTs was ubiquitous, rather than nonexistent task set interference. There is, however, some reason to believe that some of the interference present on TS trials may have been bypassed by switching modalities. In particular, recall that a paradoxical asymmetry was observed in TS costs, which were larger on trials in which the more dominant Ipsilateral task was performed. There did not, however, appear to be any evidence that such an effect was present when the modality switched. This is in fact something of a challenge for the account already proposed concerning the slowing observed on MS trials and the concurrent absence of CMTS costs. Specifically, if RTs in both cases were slowed because interference related to task switching was present whenever the modality switched, then why was clear evidence of a paradoxical asymmetry not obtained for these switch types? One general point that should be made is that task set interference often leads to RT slowing without any apparent asymmetry, paradoxical or otherwise, between the tasks. Thus, the absence of an asymmetry is not diagnostic of the absence of task-set interference. That said, the nearly significant trend wherein the paradoxical asymmetry in TS costs appeared to have been reduced in the long CTI condition may offer a clue to understanding this dissociation. In particular, the marginal effectiveness of the CTI in this regard could be taken as an indication that the interference that led to the paradoxical asymmetry was largely isolated to the S-sets. For example, performance of the Contralateral task may have been accomplished in part by a biasing of early perceptual processing (or the output thereof) to deemphasize the natural tendency to orient to stimulus onset. Under these circumstances, the deleterious effects of having previously inhibited the Ipsilateral task set in favor of the

Contralateral task set would not be expected to survive a switch of stimulus modality without some attenuation. Note also the relevance of this result to the more general theoretical question of whether TSI can be overcome by advance reconfiguration (*c.f.*, Yeung & Monsell, 2003b). The current findings suggest that, at least under conditions in which TSI is realized at the level of S-sets, such preparatory reductions may indeed be possible (for a similar example see Hunt et al., 2006).

The remaining effects that were determined to be significant, most of which center on the modality in which stimuli were presented, were of less consequence for the primary research aims in Experiment 1 than the results already discussed, but are briefly summarized here for the sake of completeness. Performance tended to be better for Visual relative to Auditory trials, and this was true for repeat RTs as well as for TS costs. The relative standing of MS and TS RTs was also markedly affected by stimulus modality, with greater comparative slowing for the former evident on Auditory trials. Similarly, the dominance of the Ipsilateral task was more evident in Auditory than in Visual RR RTs, as was the effectiveness of preparation in speeding up responding on repeat trials. This collection of results seems likely to reflect at least in part the specific tasks chosen for this experiment, and the relative ease with which stimuli can be localized in the visual modality, but might also result from differences in the ease of identification of Visual as opposed to Auditory cues, or from subjects using auditory processing resources to covertly name the tasks indicated by the cues as a way of determining which task they were supposed to perform. The only other statistically significant result was the finding that the Ipsilateral advantage on RR RTs tended to be most pronounced with a long CTI. It is not clear why this would have occurred. Perhaps the carryover of inhibition that produced the paradoxical asymmetry in TS costs tended to persist across repeat trials and the CTI permitted it to be overcome on RR trials, as well as on TS trials. Another alternative might be that performance in the Contralateral task was more sensitive to phasic alertness, and therefore that the effect should be interpreted as indicating that the Contralateral disadvantage was greatest with the long CTI because attention was more likely to waiver prior to the presentation of the imperative stimulus in this condition. The notion that performance of a relatively weaker task set might suffer more from attentional suboptimality will be revisited in the discussion of Experiments 3A and 3B.

Summary

The goal of this chapter was the introduction of the CMTS paradigm, the presentation of Experiment 1, and the situation of the results and their implications within the context of previously published work. The findings reported above constitute not only a strong replication of the increasingly robust observation that TS and MS costs combine underadditively, but further indicate that a CMTS advantage can in fact be observed under certain circumstances. The findings regarding CMTS costs in Experiment 1 were argued to reflect functional overlap of processing between MS and CMTS trials, where RTs in both cases were assumed to suffer from task-related interference. The involvement of a hierarchical switching mechanism in producing the CMTS advantage, specifically, was also inferred. The primary contribution of Experiment 1 to the scientific understanding of CMTS-related phenomena centers on its systematic use of multiple RSI levels in a within-subjects design, an approach that had not been employed in previous studies in the paradigm. The findings concerning the effects of preparation stand in opposition to the claims made by Murray et al. (2009), in that preparation did not lead to a preferential reduction in CMTS relative to MS RTs, thus supporting the claim made in this manuscript that the CMTS cost differences observed between experiments in Murray et al.'s study resulted from a cue-switching confound. Experiments 2, 3a and 3b, all of which involve manipulations centered on the manner in which both task and modality are cued, are presented in the next chapter.

CHAPTER 3

The results of Experiment 1 were argued to provide evidence that both expanded upon, and in some cases contradicted previous claims regarding performance in the CMTS paradigm. These included: a) the documentation of a CMTS advantage, possibly mediated by a hierarchical switching mechanism, where other investigators had only convincingly demonstrated an underadditivity of TS and MS costs, b) the first systematic investigation of the effects of preparation in which, contrary to suggestions made by Murray et al. (2009), preparation in response to within-modality cues was not observed to preferentially benefit CMTS relative to MS RTs, and c) replication of a surprising correlational result first reported by Murray et al., albeit with supporting evidence that was argued to be more consistent with the interpretations of CMTS effects offered in this manuscript than with the interpretations of Murray et al.

Given these observations, a critical question is to what extent the results of Experiment 1 are generalizable. For example, do the divergences evident in results across experiments, including those of Hunt and Kingstone (2004), and Murray et al. (2009), concerning the extent of the CMTS underadditivity merely reflect differences in the amount of practice subjects had and/or the statistical precision achieved (greater care was taken in Experiment 1 to avoid contamination of results by practice effects early in the session than was the case in prior CMTS studies, and the larger sample size – more subjects and more trials – led to narrower confidence intervals than in Murray et al.)? Or did the chosen tasks – with Hunt and Kingstone employing numerical classification tasks, Murray et al. mixing classification and spatial judgment tasks, and Experiment 1 of this thesis utilizing two spatial tasks – exert a sizable influence on the switch cost profile? Alternatively, did the use of an explicit cuing paradigm in Experiment 1 and in Murray et al., as opposed to an alternating runs paradigm in Hunt and Kingstone, impact results, and if so does the amount of information contained in the cue (task only, modality only, task and modality, or neither) have any effect over and above that which might be due to switching, as opposed to repeating, the cue? Drawing firm conclusions with regard to any of these questions is not possible given the evidence thus far presented. The experiments described in the remainder of this thesis are intended to provide some insight into these matters. Experiments 2, 3A and 3B, detailed in

Chapter 3, deal specifically with the last set of issues centering on the use of an explicit cuing versus alternating runs paradigm, and the nature of the precues provided in each case. To facilitate comparisons with the results from Experiment 1, the tasks and imperative stimuli were carried over without modification.

Experiment 2

Although interpreted as providing evidence inconsistent with Murray et al.’s (2009) suggestion that advance reconfiguration can reduce CMTS costs so long as cues are presented in the modality of the upcoming stimulus, in so far as Experiment 1 manipulated only the preparation interval while using strictly unimodal and modality-predictive cues, the methodology did not provide for a direct test of the possibility that cuing for the modality in addition to the task, relative to when only the upcoming task is cued, would differentially impact CMTS and MS RTs. This possibility was tested in Experiment 2 by employing two Cue Types in a within subjects design. In the Unimodal condition cues were presented in the modality of the subsequent imperative stimulus, as in Experiment 1. In the Bimodal condition cues were presented in both modalities simultaneously irrespective of the modality of the imperative stimulus. Two different RSI levels, equivalent in overall duration but differing in the relative magnitudes of the PCI and CTI, were also utilized to permit evaluation of the assumption that preparation occurred and examination of the possibility that the length of the preparation interval would mediate the effects of modality-specificity in the cues. To prevent contamination of the results by a CS confound, the cuing protocol was modified from Experiment 1 so that a 2:1 cue identity-to-task mapping was employed in both the Unimodal and Bimodal conditions, and cues were sequenced so that each trial, regardless of whether the task and/or modality switched or repeated, involved a switch of cue identity from the previous trial.

The account offered by Murray et al. (2009), makes a relatively straightforward prediction regarding the differences in CMTS effects that should be observed for the two cuing protocols. Specifically, if they were correct in their interpretation of the relatively minimal CMTS costs observed in the visual modality of their own Experiment 2, wherein preparation in response to a modality-specific cue was argued to underlie the result, then we should expect to see a more pronounced CMTS advantage (or at least a reduced CMTS cost) in the Unimodal relative to the Bimodal condition, with the length of the CTI serving to amplify the effect. While the results of Experiment 1 are comparatively mute with regard to any possible difference between the Unimodal and Bimodal conditions on CMTS relative to MS RTs, as well as on trials in which the modality switches versus those in which it does not, the failure therein to obtain evidence that preparation benefited CMTS RTs to a greater extent than MS RTs suggests that a comparable pattern of relative susceptibility to advance reconfiguration should be expected in Experiment 2.

These hypotheses, of course, assume that the modifications to the cuing protocol, including but not limited to the introduction of the 2:1 cue identity-to-task mapping and the concurrent restriction that cue repetitions never occur on successive trials, will not themselves systematically alter the switch cost profile. This assumption may be difficult to justify, however (*c.f.*, Altmann, 2006). One specific factor that should be mentioned centers on alterations to the conditional switching probabilities under the modified cuing protocol of Experiment 2. In Experiment 1, as in all experiments presented in this thesis, the probabilities associated with each of the four Switch Types (RR, TS, MS and CMTS) were constrained to be nearly equivalent. Thus, the conditional probability of a switch of task given that the cue had switched was approximately equal to .67. By comparison, in Experiment 2, since every trial involved a CS, the conditional probability of a switch of task given that the cue switched was approximately equal to .5. In other words, cue switches were predictive of a switch of task in Experiment 1, but not in Experiment 2. It is in fact possible that this characteristic of Experiment 1’s design could have contributed to the CMTS advantage if it encouraged subjects to begin task set reconfiguration any time the cue switched. It therefore seems possible that the procedure adopted in Experiment 2 could reduce or reverse the numerical CMTS advantage obtained in Experiment 1.

Depending on the extent to which this occurred, it would be interesting to observe whether this might also produce a dissociation between MS and CMTS RTs with respect to preparation. Recall, for example, that the only direct evidence that MS and CMTS RTs might have been differentially influenced by advance reconfiguration in Experiment 1 came in the comparison of the 27/200 and 27/653 RSI conditions, wherein MS RTs were observed to benefit more than CMTS RTs from the longer CTI/RSI. The hypothesis proposed to explain this finding assumed that subjects may have anticipatorily shifted tasks whenever a shift in stimulus modality was detected, interfering with processing on MS trials and providing for a head start in task-set reconfiguration on CMTS trials. From this it was argued that larger preparatory reductions in MS RTs were evident because the preparation interval was sufficient to permit subjects to overcome much of this differential maladaptive bias on MS RTs. Applying this logic to the considerations above regarding the conditional probability of a TS given a CS in Experiment 2, we might anticipate that if the modified cuing protocol results in a reduction of the bias effects on MS and CMTS trials, this could open the door for preparation to preferentially benefit performance on CMTS relative to MS trials. Note that, in contrast to the hypothesis favored by Murray et al. (2009), this argument does not imply that any preparation effects on CMTS costs/advantages should differ as a function of whether cues are unimodal or bimodal.

Experiment 2 Methods

Participants

Twenty-four undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

The materials and stimuli were the same as those used in Experiment 1 with the exception of the cues, which consisted of visual and/or auditory presentations of the vowels ”A”, “E”, “I”, and “O”. The auditory presentations of the cues consisted of a female voice pronouncing the appropriate vowel sound. The sound files for each letter cue were time compressed to a duration of 200 ms and the amplitudes were normalized. Sound file recording and editing was carried out using Cool Edit 2000. For visual presentations the capital letter was displayed in white at the center of a black screen in 50-point boldface Tahoma font.

Procedure

The basic procedure of Experiment 2 was the same as Experiment 1 with the following alterations. The primary modification centered on the Cue Types that were utilized, of which there were two varieties: Unimodal and Bimodal. The Unimodal condition was effectively identical to the cuing protocol of Experiment 1 with the exception of the use of letter, instead of color/tone cues and the additional fact that immediate repetitions of the cue identity (ignoring modality) in trial $n - 1$ did not occur. Unimodal cues were presented in the same modality as the following target stimulus, and informed subjects of the task (Presence vs. Absence) they were to perform on each trial. As in Experiment 1 task sequencing was unpredictable so the cues were necessary for accurate performance. In the Bimodal condition cues were presented simultaneously in both the visual and auditory modalities. In this case the cue identities were always in agreement between the two modalities (i.e., a visual “A” was always paired with an auditory “A”), and the cues contained no information about the modality of the subsequent target stimulus, which was always either visual or auditory, but never both, throughout the experiment. As in the Unimodal condition, lag-1 cue identity repetitions did not occur.

Two RSI levels, each comprising three temporal components, were also used in the experiment, and were manipulated independently of cuing protocol. The Short-CTI condition featured a PCI of 507 ms, followed by presentation of the cue for 200 ms, and finally by a pre-target interval (PTI) of 67 ms, in which the screen was blank and the headphones were silent. This condition therefore yielded a total CTI of ~ 267 ms. The Long-CTI condition featured a PCI of 160 ms, and a PTI of 413 ms, yielding a total CTI of ~ 613 ms. Overall RSI was thus also held constant at ~ 774 ms throughout the experiment.

Subjects completed a total of 52 blocks of 66 trials each. Both RSI and Cue Type were manipulated between blocks, with RSI alternating every block, and cuing protocol alternating every two blocks. This meant that subjects encountered every possible two-trial sequence at least once every four blocks. The cuing protocol (Unimodal versus Bimodal) that subjects encountered first, as well as the RSI level with which it was initially paired, were counterbalanced across subjects, as was the assignment of cues to tasks.

Results

Analysis of the RT Switch Type Profile

Preparation of the data for analysis followed the procedure outlined for Experiment 1. The first 12 blocks for each subject were excluded from the analyses. Means for the four Switch Types corresponding to each Cue Type and RSI combination, and to each Task and Modality combination, are presented in Figures 3 and 4, respectively. The associated contrasts, as well as the tests for marginal switch costs, are presented in Table 2. To summarize, the effects of RSI on RR RTs differed as a function of the Cue Type, in that speedup associated with the Long-CTI condition was evident only with Unimodal cues. RR RTs were also faster on Visual as compared to Auditory, and Ipsilateral as compared to Contralateral trials. The magnitude of the former effect was found to be reduced in the Long-CTI condition, $F(1, 23) = 21.87$, $p < .001$. TS and MS costs were both observed, and were found to be larger on Auditory relative to Visual trials. The former were reduced in the Long-CTI condition, indicating a significant effect of preparation. The significant result for the comparison of MS and TS RTs as a function of RSI appeared to be part of a more complex pattern that was not adequately captured by the *a priori* hypothesis testing structure. A *post hoc* contrast examining the interaction between MS costs, Cue Type and RSI yielded $F_S(6, 23) = 13.88$, $p_S < .068$, reflecting the divergence between the parallelism of MS costs in the Unimodal and Bimodal conditions with a Long-CTI on the one hand, and the relative lack thereof with a Short-CTI on the other. Finally, CMTS costs were again absent, with a nonsignificant trend in the direction of a CMTS advantage, and lengthening the CTI was observed to preferentially benefit CMTS relative to MS RTs irrespective of whether the cues were Unimodal or Bimodal. 95% confidence intervals for CMTS costs as a function of RSI yielded CI = [-8, 11] ms for the Short-CTI condition, and CI = [-25, -7] ms for the Long-CTI condition.

Analysis of the Error Switch Type Profile

Preparation of the data for examination of results for errors proceeded as indicated in Experiment 1, and following preparation of RT data for Experiment 2 as noted previously. Median error percentages for the interactions of Switch Type, Cue Type and RSI, and Switch Type and Trial Type, are presented in the bottom panels of Figures 3 and 4, respectively. While the numeric trend for error percentages on CMTS trials to be reduced relative to error percentages on MS trials that was present in Experiment 1 was not evident here, there was no evidence that the underadditivity of TS and MS RT costs could be attributed to a speed-accuracy tradeoff. The same is true for the effects of RSI on RT at S_{CMTS}, in so far as error CMTS costs were not observed to be numerically larger in the Long- relative to the Short-CTI condition.

RT Correlations

Data preparation for correlational analyses proceeded as before. CMTS RTs were again correlated between modalities prior to partialing, $r(22) = .9$, $p < .001$, but not once MS RTs had been taken into account, $r(20) = -.05$ $p < .83$. Within-modality correlations between CMTS and MS RTs were again extremely strong, $r(22) = .99$, $p < .001$, in each modality.

Discussion

The observation that the effects of a CMTS relative to an MS within each RSI level were parallel across Cue Type directly contradicts the claim of Murray et al. (2009) that the numerical reduction in Visual CMTS costs that they observed reflected an inherent sensitivity of CMTS costs to modality-specific preparation. At the same time, the finding that lengthening the CTI favored CMTS RTs relative MS RTs given a constant RSI constitutes an empirical divergence relative to Experiment 1 of this thesis, in which performance on MS trials appeared to benefit from advance reconfiguration at least as much as CMTS RTs, if not more so. As noted in the introduction of Experiment 2, however, the reduction in CMTS costs (or rather, the emergence of a CMTS benefit) in the Long-CTI condition is consistent with the suggestion that the modified cuing paradigm, and the consequent reduction of the conditional probability of a TS occurring given that a CS also occurred, created a situation in which subjects were less likely to engage in anticipatory reconfiguration of task set on trials in which the modality of the imperative stimulus switched.

The question therefore becomes one of whether the remaining aspects of the data – in particular the effects of Cue Type and RSI on the overall switch cost profile, which produced several unanticipated empirical complexities – present any challenges to this hypothesis. Consider first the significant $\mathbf{s}_r \otimes \mathbf{c}_{s\text{-}l}$ and $\mathbf{s}_r \otimes \mathbf{c}_{cxr}$ contrasts, which were framed above as resulting from the effectiveness of the longer CTI in reducing RR RTs in the Unimodal, but not in the Bimodal, condition. This pattern can also be interpreted as reflecting the fact that whereas a Bimodal advantage was evident when the CTI was short, $F_S(3, 23) = 18.68$, $p_S < .003$, a trend (not attaining significance by *post hoc* standards, but of a comparable magnitude) towards a Unimodal advantage was observed when the CTI was long, $F_S(3, 23) = 5.03$, $p_S < .2$. Combining these two perspectives and the outcome of the *post hoc* test of the RSI by Cue Type interaction on MS costs, the results can be characterized as indicating that the makeup of the RSI in the Long-CTI condition facilitated RR performance following Unimodal cues to an extent that was sufficient to overcome a Bimodal advantage on RR trials in the Short-CTI condition, and further that the divergent effects of the two RSI structures in the Unimodal versus Bimodal conditions were in evidence to an even greater extent on MS trials than on RR trials.

At first glance, a relatively straightforward explanation for these effects might seem to be available in the form of anticipatory task-set reconfiguration on Unimodal RR and MS trials. In particular, TS RTs are known to be both slower and more sensitive to preparation than RR RTs, and this basic description is similarly applicable to the comparison of the Unimodal (slower in the Short-CTI condition and more sensitive to preparation) and Bimodal (faster in the Short-CTI condition but less sensitive to preparation) conditions. It therefore seems reasonable to ask whether the modified cuing protocol used in Experiment 2 did not merely serve to motivate the adoption of a hierarchical switching mechanism on Unimodal trials in which the stimulus modality repeated (RR and TS trials, leading to aniticpatory task set switching on RR trials), while doing nothing to discourage the operation of a similar mechanism on trials in which the modality switched (MS and CMTS RTs, leading to anticipatory task set switching on MS trials, as was hypothesized to have occurred in Experiment 1). Critically, an answer in the affirmative to this query would directly contradict the previous assertion that the obligatory switching of cues discouraged such hierarchically mediated switching, and would consequently call into question any attempts to attribute the effects of preparation on CMTS trials in the Unimodal and Bimodal conditions to a common cause. However, this contrarian proposal leads directly to a fairly strong prediction concerning the manner in which this interaction should propagate through to TS and CMTS costs. In particular, if the shifting of cues on a trial-to-trial basis contributed to the operation of a hierarchical switching mechanism that selectively interfered with RR and MS performance in the Unimodal condition, this same mechanism would be expected to lead to the reverse effect on TS and CMTS trials, since in these cases obligatory and reactive task set reconfiguration would be adaptive. No such effect appeared to be present, however, as lengthening the CTI was observed to have similar effects on TS relative to RR trials, and CMTS relative to MS trials, irrespective of the modality-specificity of the cue. The hypothesis of anticipatory task set biasing in the Unimodal condition alone does not, therefore, appear well supported by the available data.

But then what is to be made of the complex effects of Cue Type and RSI on the switch cost profile? To begin with, there are at least four candidate hypotheses that seem capable of explaining why the Short-CTI Bimodal advantage was observed on trials in which the modality of the imperative stimulus repeated. First, it is possible that in a very general sense cue processing was more efficient when the information was presented in two modalities simultaneously, potentially reflecting either co-activation of the cue identity or the outcome of a race between the two available perceptual representations. A second possibility is that as the PCI increased, the probability that subjects would have their attention focused on the modality in which the preceding imperative stimulus had been presented tended towards chance levels, so that even on trials in which the modality repeated there would have been some portion of trials in the Unimodal condition in which attention was misdirected at the time of cue onset. This would not, of course, be expected to lead to a similar impairment on Bimodal trials since the attentional orientation in this condition could never be “wrong” with respect to the cue, at least. Third, in so far as subjects may have tended to be somewhat uncertain as to which task was required at the time of imperative stimulus onset in the Short-CTI condition on some percentage of trials, regardless of Cue Type, selecting a correct response might have required not merely the application of a cued task set to an imperative stimulus, but also the coordination of function between concurrent perceptual representations of both the imperative stimulus and the cue. The ready availability in the Bimodal condition of a representation of the cue in a perceptual modality that differed from the modality of said imperative stimulus could have facilitated response selection in such cases if a division of labor between visual and auditory processing centers was capable of effecting a reduction in the difficulty of this particular subtask. In principle, the mechanism of such a real-time adaptation could have been either active, such that subjects were motivated to discard the ipsimodal (relative to the current imperative stimulus) representation of the cue once the modality of the imperative stimulus became known, or passive, in that subjects could have simply allowed the imperative stimulus to push the ipsimodal cue representation out of the way. To the extent that TS and CMTS costs were roughly equivalent for the two Cue Types, and in so far as an active mechanism might be expected to interfere with task-set reconfiguration, the latter possibility seems somewhat more likely. The final hypothesis concerns the possibility that the speed with which cues can be identified is influenced by both low-level (*i.e.*, modality-specific) perceptual priming, as well as by amodal semantic priming. Because the same set of cue identities were employed for both Cue Types in Experiment 2, there were actually twice as many individual cues in the Unimodal condition as there were in the Bimodal condition. In so far as the letter identity of each cue was the only dimension from which the appropriate task could be determined, this should not have resulted in any general imbalance in the LTM demands (matching letter identities to tasks) between the two Cue Type conditions. Nonetheless, note that in the Bimodal condition, if the cue “A” were presented on trial n and also on trial $n - k$, ($k > 1$), cue processing on trial n would be expected to benefit from both semantic and perceptual priming stemming from trial $n - k$ regardless of whether or not the imperative stimuli were presented in the same modality on both trials. In the Unimodal condition, on the other hand, these dual sources of potential priming facilitation would only be concurrently realized under the condition that the modality of the imperative stimulus on trial *n* and trial *n* – *k* were in agreement. Of course, this possibility would only be tenable under a restricted subset of time-course functions that could potentially govern the decay of priming facilitation.

Turning to the most general aspects of the differential effectiveness of CTI length in the two Cue Types, all four of the hypotheses just outlined imply that the Bimodal advantage could be overcome by preparation. In the first case, this is because the Long-CTI condition presumably permitted subjects more than enough time to identify and begin to prepare the appropriate task set regardless of the nature of the cue. In the second case, the constant RSI meant that the PCI was short in the Long-CTI condition, so that attention would have been less likely to wander prior to cue onset. The third and fourth hypotheses predict greater speedup in the Unimodal condition for much the same reason as the first. To these we can also add the possibility that readiness to respond to a unimodal imperative stimulus can be optimized for a particular stimulus modality through adequate preparation. This need not depend on faster processing of the imperative stimulus itself, but would more likely reflect a biasing of the connections between perceptual and response-selection centers. For example, if it were known in advance that the imperative stimulus would not be auditory, it might be possible to adopt an attentional setting that discounted perceptual output that did not conform to the expected stimulus parameters, and in so doing permit a lowering of the activation threshold necessary to trigger a response without risking increases in errors. This latter possibility alone among the others discussed leads directly to the prediction that Unimodal RTs should be shorter than Bimodal RTs in the Long-CTI condition.

None of the considerations outlined above present any real challenges to the hypothesis that CMTS RTs in the Unimodal and Bimodal conditions benefited from an extended CTI not merely to a similar extent, but also for similar reasons. We may therefore expand the inquiry to evaluate the implications of subjects' performance on trials in which the modality switched. Take, for example, the greater strength of the Cue Type by RSI interaction on MS relative to RR trials. Perhaps the simplest available explanation for this effect is that whereas the presentation of a Unimodal cue on RR trials creates a situation in which the modality of the cue is also the same as the modality of the imperative stimulus to which subjects have just responded, on MS trials processing of the cue itself requires a reorientation of attention to the previously irrelevant modality, potentially entailing inefficiencies in initial processing of the cue. By comparison, no such attentional shift would be required for processing of Bimodal cues on MS (or CMTS) trials, since in these cases it would always be possible to identify the appropriate task solely on the basis of information gleaned from the sensory modality in which the immediately preceding imperative stimulus had been presented.

Critically, however, even if accurate this perspective offers no insight into what are ultimately the most challenging aspects of the results of Experiment 2, namely: a) that outside of an apparent reduction in TS costs (problematically attributable at least in part to a speed-accuracy tradeoff) there was no evidence in the RT data to suggest that performance in the Bimodal condition benefited overall from a longer preparation interval, and furthermore b) that the differential effectiveness of preparation for a task switch given a shift of imperative stimulus modality with Bimodal cues could be attributed entirely to numerically reduced MS costs in the Short- relative to the Long-CTI condition. It should be emphasized at this point that the clear and consistent absence of CMTS costs in Experiment 2 strongly implies that modality specificity in explicit cues was not a determinant of CMTS cost magnitudes in Murray et al. (2009), irrespective of whether preparation can be concluded to have selectively benefited CMTS RTs. Nonetheless, consideration of the surprising results noted above is indispensable for interpretation of the generality of the effects of lengthening the CTI. Counterarguments to the questions posed by these results *vis a vis* the theoretical claim that preparation benefited performance on CMTS relative to MS trials regardless of Cue Type include the high degree of parallelism evident in the comparison of the Long-CTI Unimodal and Bimodal conditions, the possibility that RTs in the Bimodal Long- relative to Short-CTI condition could have been affected by lapses in phasic alertness or relative underactivation in the connections between perceptual processing output and response selection input, and the observation that accuracy was slightly better on Bimodal CMTS trials in the Long- relative to the Short-CTI condition, $T = 82$, $p < .017$. These points noted, however, it must be acknowledged that the evidence supporting the efficacy of preparation on CMTS relative to MS RTs following Bimodal cues, specifically, is at this point largely circumstantial. To briefly note one alternative possibility that derives from considerations outlined previously, it could be that the relatively shorter PCI in the Long-CTI condition was in fact associated with some degree of anticipatory set switching, and that this cue-driven misdirection, rather than task set reconfiguration in the traditional sense, was responsible for the numerical CMTS relative to MS RT advantage in the Long-, but not in the Short-CTI Bimodal condition.

Ultimately, the verdict concerning the effects of preparation on CMTS costs in the absence of modality-predictive cues depends on the determination of the root cause of the relatively fast MS and CMTS RTs in the Short-CTI Bimodal condition (as compared to the Unimodal and Long-CTI Bimodal conditions). Although the effect should not be treated as more than a trend and will require replication, the following speculative hypothesis seems worthy of mention. As a starting point, assume: a) that activation tended to spread from the internal representation of the cue encountered on the immediately preceding trial to the internal representation of the complementary cue (*i.e.*, the alternate cue used for the most recently performed task set) during the extended PCI in the Short-CTI condition, and furthermore b) that this cross-cue priming was to some extent perceptually specific, and was directly determined by the modality of the immediately preceding cue so that, for example, a visual cue on trial $n - 1$ would most strongly activate its visual complement during the PCI on trial n. From this there are at least two possible mechanisms of facilitation on Bimodal Short-CTI MS RTs. First, recall from the discussion of the RSI by Cue Type interaction with respect to modality repeat trials the suggestion that having both visual and auditory representations of the cue available could have facilitated processing of the imperative stimulus by permitting a segregation of cue and imperative stimulus processing between the two modalities. Cue complement priming during the PCI would arguably minimize the effect of such a division of labor on RR trials since in this case the primed cue would have been the one that was actually presented. This correspondence would be expected to reduce the percentage of Unimodal Short-CTI trials in which the appropriate task had not been determined from the cue by the time the imperative stimulus was presented, masking the benefits of having two representations of the cue available in the Bimodal Short-CTI condition. On the other hand, when the modality of the imperative stimulus switched in the Unimodal condition, so too did the modality of the cue, and so it follows that any facilitation from activation of the complementary cue specifically in the preceding modality would no longer have the same effects, resulting in an unmasking of the partially latent dual-cue advantage in the Bimodal Short-CTI condition.

This argument is limited, however, in that it does not provide an explanation for why CMTS RTs should have been similarly affected. Extending the hypothesis to accomodate this aspect of the results, note that because of the constraint that no cue-identity repetitions occurred it was possible, in principle, for subjects to determine which task to perform by means of a simple comparison of the cue presented on trial *n*, and the complement of the cue presented on trial $n - 1$, with a match indicating that the task from trial $n - 1$ should be repeated, and a mismatch indicating that the required task had switched. For purposes of the current discussion, this strategy would: a) provide an alternative and entirely accurate means of identifying the appropriate task that would not require explicit consideration of the cue-task mappings, b) impact performance irrespective of whether the task switched or not, and c) be maximally applicable in the Bimodal Short-CTI condition, specifically. The first point is important because it is conceivable that by effectively bypassing (or preempting) the LTM retrieval components of cue processing during the CTI via spreading activation between LTM representations during the PCI, the speed with which the appropriate task could be determined could have been significantly reduced, leading to faster RTs than would otherwise be possible. The realization of this facilitation in the Bimodal Short-CTI condition follows for two reasons. First, the longer PCI in this condition would presumably increase the likelihood that subjects would have had sufficient time to activate the perceptual representation of the complement of the cue from trial *n* – 1 in advance of the presentation of the cue on trial *n*. Second, it is only in the Bimodal condition that the activated perceptual representation of the complement would have been assured of providing a viable (*i.e.*, modality-appropriate) referent to which the cue on trial *n* could be matched. Thus, by proposing that subjects may have processed cues in the Bimodal Short-CTI condition in a fundamentally different and more efficient way, the cue-matching hypothesis, although clearly in need of direct testing, can be seen to provide a basis for the argument that the comparatively fast RTs in the Bimodal Short-CTI condition, particularly on MS and CMTS trials, do not necessarily preclude the possibility that preparation in response to Bimodal cues was effective in overcoming task set interference on CMTS trials.

The remaining significant results – faster Visual than Auditory, and Ipsilateral than Contralateral RR RTs, larger TS and MS costs for Auditory as compared to Visual trials, and a larger effect of CTI length on RR RTs in the Auditory as compared to the Visual condition – all replicated results from Experiment 1, and can be interpreted in line with the considerations laid out in the discussion thereof. The correlational analyses were also generally consistent between the two experiments, as an approximately nil between-modality correlation between CMTS RTs, partialed with respect to MS RTs, was observed, but once again under conditions in which the strengths of the linear associations between MS and CMTS RTs within either modality were extremely high.

In contrast to Experiment 1, a statistically significant paradoxical asymmetry of TS costs was not observed. However, to the extent that TS costs differed numerically between the two tasks the effect was in the predicted direction. Interestingly, this trend was paralleled in the comparison of CMTS “costs” between the Ipsilateral and Contralteral task, although again the magnitude of the asymmetry was not statistically significant. Another noteworthy difference between Experiments 1 and 2 concerned the overall steepness of the Switch Type profile, which appeared considerably flatter in Experiment 2 relative to Experiment 1. A reduced magnitude of switch costs relative to the RR baseline is not, in and of itself, surprising given the fact that RR trials involved switches of cue identity in Experiment 2. It is somewhat puzzling, however, that faster switch RTs rather than slower RR RTs in Experiment 2, particularly in the Auditory modality, appeared to account for the bulk of the difference between experiments. This would seem to suggest either the involvement of strategic factors in Experiment 1 that exacerbated existing differences in generalized task difficulty between the two modalities, or perhaps an adaptation to the cuing protocol in Experiment 2 that sped up processing of the cues and consequently identification of the appropriate task. The hypothesis suggested above regarding the possible use of cue matching could in fact predict such an effect if the spreading of activation to cue complements affected both perceptual and conceptual representations thereof. Alternatively, given that the primary cross-experiment gains were observed for Auditory trials, it could be that identification of letter cues was somewhat easier than distinguishing between high- and low-pitch tones. A final possibility that could also be relevant to the parallel trend towards a paradoxical asymmetry of TS and CMTS costs concerns the use of overlapping cue-identity sets for the two tasks in Experiment 2 but not in Experiment 1. Specifically, this factor could have encouraged subjects to represent the tasks more centrally in the cognitive architecture, bringing TSI in amodal processing centers into play, and at the same time reducing conceptual complexity in the representation of S-R rules to reduce switch trial RTs.

Experiments 3A and 3B

Hunt and Kingstone (2004) previously observed an underadditivity of TS and MS costs using parity and magnitude tasks in an alternating runs approach, but CMTS costs did not appear to be eliminated, much less inverted in their findings, in contrast to the results of Experiments 1 and 2. To evaluate the extent to which Hunt and Kingstone's use of predictable task sequencing in the absence of explicit task cues was responsible for this discrepancy, Experiments 3A and 3B employed an alternating-runs paradigm in which the task switched predictably every two trials while the modality of the imperative stimulus continued to vary unpredictably from trial to trial. There are at least two reasons why the paradigmatic variants in question rather than, or perhaps in addition to, other experimental factors might play a critical role in determining whether switching tasks cross-modally leads to RT impairments relative to cross-modal task repetitions. First, depending on the degree to which external support for keeping track of the task sequence is or is not made available to subjects in an alternating-runs paradigm, it is possible that the additional working memory load imposed by having to maintain one's bearings in the absence of task cues could exacerbate the effects of differential levels of task set interference between MS and CMTS trials. While this is unlikely to have played a prominent role in Hunt and Kingstone's findings, since in their experiment the spatial position of imperative stimuli unambiguously indicated the appropriate task, Experiments 3A and 3B offered no such guidance to subjects. Second, the reader may recall that evidence from studies of TS using only visual stimuli has indicated that while asymptotic repeat trial performance in experiments that use random task sequencing may be somewhat elusive, requiring multiple consecutive repeat trials to achieve, reconfiguration may be relatively complete after only a single trial when task sequencing is predictable (Rogers & Monsell, 1995). To the extent that MS trials benefit from the effects of task repetition, they may likewise benefit selectively (relative to CMTS trials) from predictable task sequencing, leading to the unmasking of CMTS costs even in the absence of any paradigmatically derived slowing on CMTS RTs themselves.

Briefly outlining several further relevant design features of Experiments 3A and 3B, the Ipsilateral and Contralateral response tasks from Experiments 1 and 2 were carried over with no changes to the imperative stimuli to facilitate comparisons with the experiments detailed thus far. As modality was not constrained to vary predictably, four different task-nonspecific cuing protocols were used between the two experiments to permit evaluation of the extent to which unpredictability of stimulus modality relative to task set, and the presence and modality-specificity of warning signals might influence results. Finally, the preparation interval was held constant using only a single short RSI, primarily because of practical limitations on the number of conditions that could be effectively tested in a single experimental session. Because of the use of a short RSI, and on the basis of the results of Experiments 1 and 2 – wherein conditions that were argued to have suffered from task set interference or uncertainty to a relatively greater extent evinced greater preparatory facilitation than conditions that were less profoundly handicapped, either for intrinsic reasons or because of maladaptive anticipatory reconfiguration – the reader may note that Experiments 3A and 3B were comparatively optimized for detecting the presence of differential amounts of task set interference across the various Switch Types, in particular in the MS and CMTS conditions. To simplify and streamline interpretation, discussion of the findings from Experiments 3A and 3B will be postponed until the methods and principle results of each have been presented.

Experiment 3A Methods

Participants

Twenty-four undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

The materials and stimuli used were the same as in Experiment 1, except that only the red screen and high pitch tone were employed as cues.

Procedure

The procedure was similar to that of previous experiments with the following exceptions. First, an *AABB* alternating runs design was used for task sequencing. Tasks were not explicitly cued, and subjects were required to keep track of the task sequence on their own. Stimulus modality varied unpredictably from trial to trial. Second, only a single RSI level, featuring a 27 ms PCI and a 240 ms CTI was used, yielding a total RSI of 267 ms. Three different cuing protocols (Cue Type) were employed. In the first of these, the Ipsimodal condition, the cue was presented in the same modality as the subsequent imperative stimlus. In the second, the Contramodal condition, the modality of cues and targets always differed (a visual cue would precede an auditory target, and vice versa). In the final condition, No-Cue, only imperative stimuli were presented with no intervening cues. The overall RSI duration of 267 ms was preserved in this condition as in the other two. Cue Type was constant within blocks, and changed between each block, with the order counterbalanced across subjects. The experiment consisted of a total of 48 blocks of 67 trials each, and blocks were structured so that subjects encountered each possible two-trial sequence at least once every three blocks.

The experimental trials commenced once subjects had received instructions on the proper performance of the tasks and the nature of the task and modality sequencing. A screen at the beginning of each block informed subjects of the task they were to perform for the first two trials of each block, after which the task would alternate every two trials as noted above. The appropriate starting task alternated from one block to the next so that half of the blocks commenced with a pair of Presence Trials, and half with a pair of Absence trials. Incorrect responses triggered an error feedback screen informing subjects that they had responded incorrectly, and specifying the appropriate task on the next two or three trials; enough to allow them to determine where they were in the sequence. This screen remained visible until the subject pressed a button to resume the experiment.

Experiment 3A Results

Analysis of the RT Switch Type Profile

Preparation of the data for analysis proceeded as in previous experiments, with data from the first 12 blocks excluded. Means for the four Switch Types corresponding to each Cue Type, and to each Task and Modality combination, are presented in Figures 5 and 6, respectively. The associated contrasts, as well as the tests for marginal switch costs, are presented in Table 3. The results can be summarized as follows. RR RTs were statistically significantly faster in the Ipsimodal condition than in the Contramodal conditon, while the trend for faster responding on RR trials in the No-Cue relative to the Ipsimodal condition did not reach significance. TS and MS costs were both present, and a *post hoc* contrast comparing MS costs in the No-Cue condition to the average of the Ipsimodal and Contramodal conditions yielded $F_S(4, 23) = 16.02$, $p_S < .014$, indicating larger costs in the former relative to the latter. CMTS costs were also evident, but were reduced in the No-Cue condition relative to the Ipsimodal condition. 95% Confidence intervals around CMTS costs for each of the three Cue Types yielded CI = [21, 95] ms for the Ipsimodal condition, CI = [34, 120] ms for the Contramodal condition, and CI = [-15, 57] ms for the No-Cue condition. With regard to Trial Type, RR RTs were faster on Visual relative to Auditory trials, and for the Ipsilateral relative to the Contralateral task. Both TS costs and MS costs were statistically significantly larger in the Auditory modality, and MS costs were also considerably reduced for the Ipsilateral task. The trend towards a paradoxical asymmetry of TS costs as a function of task approached but did not reach significance. A paradoxical asymmetry of CMTS costs was clearly evident, however, and was more pronounced in the Auditory relative to the Visual modality.

Analysis of the Error Switch Type Profile

Preparation of data for evaluation of patterns in the error data proceeded as before. Median error percentages are presented in the lower panels of Figures 5 and 6. As can be seen, the error data corresponds reasonably well to the findings from the RT data, and no clear indications of a speed-accuracy tradeoff are evident.

RT Correlations

Correlations were calculated on RT data prepared in the same manner as was described for analysis of the Switch Type profile. CMTS RTs were correlated across modalities at $r(22) = .92$, $p < .001$, and MS and CMTS RTs were correlated within modality at $r(22) = .91$, $p < .001$ for the Visual modality, and $r(22) = .93$, $p < .001$ for the Auditory modality. The partial correlation between Visual and Auditory CMTS RTs eliminating Visual and Auditory MS RTs yielded $r(20) = .82$, $p < .001$.

Experiment 3B Methods

Participants

Twenty-two undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

The materials and stimuli used were the same as in Experiment 3A, with the exception that a blue screen was used in place of the red-screen for cues.

Procedure

The procedure was identical to Experiment 3A, with the exception of the cuing protocols, of which there were two varieties in the current experiment. The first, No-Cue, was the same as was used in Experiment 3A. In the second, Both-Mod, cues were presented in the visual (blue screen) and auditory (high-pitch tone) modalities simultaneously. Cuing protocol was held constant within blocks and alternated between blocks with the order counterbalanced across subjects so that subjects encountered each possible two-trial sequence at least once every two blocks.

Experiment 3B Results

Analysis of the RT Switch Type Profile

Preparation of the data for analyses followed the procedure outlined for previous experiments. Means for the interactions between Switch Type and Cue Type, and Switch Type and Trial Type, are presented in the top panels of Figures 7 and 8, respectively. Outcomes of the preplanned contrasts are presented in Table 4. Results indicated RR RTs were faster when the imperative stimulus modality was Visual, and when the task was Ipsilateral. The statistically significant marginal advantage of the No-Cue relative to the Both-Mod condition on RR RTs appeared to be due primarily to the Auditory modality, $F(1, 21) = 12.02$, $p < .003$. TS and MS costs were both present, and the reversal of monotonic ordering of TS and MS RTs in the two Cue Types also produced a statistically significant interaction contrast. As this effect appeared to extend to RR and CMTS RTs as well, a *post hoc* contrast was carried out to compare trials on which the imperative stimulus modality repeated (RR and TS) to those on which the imperative stimulus modality switched (MS and CMTS) as a function of Cue Type, and yielded $F_S(7, 21) = 24.22$, $p_S < .013$. TS and MS costs were both larger on Auditory than on Visual trials, and the latter, but not the former, were likewise of a greater magnitude on Contralateral trials. Marginal CMTS costs, 95% CI = [-1, 59] ms, did not reach statistical significance, although trends favoring the existence of CMTS costs that were larger with Auditory stimuli, and for the Ipsilateral task, were evident.

Analysis of the Error Switch Type Profile

Error data were prepared as in previous experiments, and median error percentages are presented in the lower panels of Figures 7 and 8. While some unusual patterns are visible, particularly for the Task/Modality combinations in Figure 8, in general the directions of effect are consistent with expectations (*e.g.*, the trend towards larger error TS costs for the Ipsilateral task, $T = 65.5$, $p < .031$, and the marginal accuracy advantage for the Ipsilateral relative to the Contralateral task in the Auditory modality, $T = 87.5$, $p < .003$).

RT Correlations

Correlational analyses were performed on data prepared as previously indicated. CMTS RTs were correlated between modalities at $r(20) = .9$, $p < .001$, and were correlated with MS RTs within modalities at $r(20) = .72$, $p < .001$, for Visual trials, and $r(20) = .85$, $p < .001$, for Auditory trials. The intermodality correlation between CMTS RTs partialed with respect to MS RTs yielded $r(18) = .85$, $p < .001$.

Discussion of Experiments 3A and 3B

Experiments 3A and 3B produced several results that clearly diverged from the findings of Experiments 1 and 2. With regard to the Switch Type profile, the most notable among these was the presence of CMTS costs, which reached statistical significance in Experiment 3A. Even in the No-Cue (in both experiments) and Both-Mod (in Experiment 3B) conditions – which were united in failing to provide any information about the modality of the upcoming imperative stimulus – where the evidence in favor of CMTS costs was weaker, the results appeared highly consistent across the two studies. Further novelties were evident in the interactions between Switch Type and Task, wherein MS costs were found to be reduced for the Ipsilateral task in both experiments, while CMTS costs were concurrently increased (albeit not quite significantly in Experiment 3B). The sharpest distinction between the previously discussed experiments and the current ones, however, came from the correlational analyses. Although no significant relationships of the kind had been observed in Experiments 1 or 2, or by Murray et al. (2009), CMTS RTs were found to be strongly correlated across modalities in Experiments 3A and 3B even after MS RTs had been taken into account. In so far as the results for the four Cue Type conditions in Experiments 3A and

3B appeared to share more in common with one another than any of them did with Experiments 1 and 2, and since the same tasks and imperative stimuli were used across all four experiments, these basic patterns can be attributed with high confidence to the change over from the task-cuing paradigm to the alternating-runs paradigm.

In the introduction to Experiments 3A and 3B it was suggested that CMTS costs might be found if the predictable task sequencing permitted more complete reconfiguration of task set after a single trial provided that reconfiguration facilitated responding to stimuli according to the rules of the most recently adopted set in either modality, or if the presumably greater working memory load incurred by having to keep track of the appropriate task without the aid of explicit cues interfered with task set reconfiguration on CMTS trials. There is some reason to think, however, that the divergent results of Experiments 3A and 3B on the one hand, and Experiments 1 and 2 on the other, reflect a more fundamental set of differences between the cognitive processes that supported task performance in the alternating-runs and task-cuing methodologies, respectively. The reductions in MS costs as a function of task, and the strong between modality partial correlations for CMTS RTs in Experiments 3A and 3B were not simply quantitatively amplified replications of previous results, but were in fact qualitatively novel. It may therefore be worthwhile in considering the current results to step back from the interpretive context established for the task-cuing paradigm and ask whether a more direct and parsimonious account can be constructed. In this respect, note that the focus to this point in quantifying the CMTS underadditivity has been on tracking the cost of switching tasks cross-modally rather than the cost of switching modalities as a function of whether or not the task switches. This approach owes to several factors, among them Murray et al.’s (2009) emphasis on, and claims regarding CMTS costs as calculated relative to the MS baseline, as well as the fact that these investigations were initially undertaken to expand upon current knowledge of task switching phenomena. Retrospectively, this strategy can be further justified by pointing to its utility in guiding interpretation of Experiments 1 and 2, in particular the documentation of and theoretical position taken regarding the CMTS advantage.

That said, there are at least two principle arguments in favor of a shift to an analytic perspective that assigns higher priority to the repetition versus shifting of task set in organizing thinking about cognitive functionality in the alternating-runs CMTS paradigm. The first is largely evidentiary, and concerns four aspects of the results from Experiments 3A and 3B: a) the observation that MS costs tended to be larger on Contralateral trials represents a nonparadoxical asymmetry that is at odds with what the typical result for TS costs using tasks of this nature, b) CMTS costs tended to vary inversely with MS costs, both as a function of Cue Type, and as a function of Task, c) CMTS costs were strongly correlated across modality even after taking performance on MS trials into account; both this observation and the preceding one suggest a decoupling of performance on trials in which the modality switched as a function of whether or not the task also switched, in contrast to the extremely strong relationship between MS and CMTS RTs that was noted in Experiments 1 and 2, and finally, d) in comparison to the divergent behavior of CMTS and MS RTs in Experiments 3A and 3B, CMTS and TS RTs tended to correspond in magnitude relatively consistently across Cue Types and Task/Modality combinations. Two possible exceptions to this last point, neither significant by *post hoc* testing standards, were apparent. In the Auditory/Ipsilateral condition of Experiment 3A CMTS RTs appeared slower on average than TS RTs. Even without adjustment for *post hoc* testing, however, the difference did not reach significance, $F(1, 23) = 2.67$, $p < .117$. The numerical reduction in CMTS relative to TS RTs in the Both-Mod condition of Experiment 3B was smaller but appeared somewhat more reliable, yielding an unadjusted $F(1, 21) = 11.57$, $p < .003$ (adjusted $p_S < .176$). The reader may note that this latter trend was situated within the broader context of the statistically significant interaction between the two Cue Types as a function of whether the imperative stimulus modality switched, which itself clearly indicates that modality-switching did in some cases have effects that were evident regardless of whether the task switched. This observation aside, however, the overall picture painted by the results outlined above is one in which TS, MS and CMTS RTs can be most efficiently distinguished and grouped according to whether or not a shift of task set was required.

A second argument, which can be developed on *a priori* grounds and is consequently somewhat more compelling, holds that in contrast to the explicit-cuing paradigm, wherein the control settings on each trial are likely to be relatively invariant regardless of whether the task repeats or not – perceive a cue, determine which task to perform, perceive an imperative stimulus, perform the task – in the alternating-runs paradigm with no external support for the task sequence the control settings could differ markedly as a function of whether the task repeats or not. For example, with respect to repeat trials, to the extent that carryover of set from the preceding trial occurs, and to the extent that subjects are capable of taking advantage of the predictability of the task repeat, some degree of relatively effortless perseveration of the previously relevant S-R mappings is to be expected, and consequently a subject's primary concern outside of simply repeating an already strongly instantiated task set may be to avoid losing track of the sequence for the sake of preserving the capacity to produce a correct response on the subsequent switch trial. By comparison, responding correctly on trials in which the task switches would depend on the internal recognition that a change in S-R mappings was required, upon the actual updating of those S-R mappings, and also possibly upon the adoption of a control setting that would be more or less optimal with regard to the subsequent repeat trial, and in line with the conjectures outlined just previously. An additional factor that is to some extent implied by the above is that an external processing focus may be possible on repeat trials, but relatively untenable on trials in which the task switches, both because of the reconfiguration requirements in the latter case, and because of the danger of allowing the output from perceptual processing of the imperative stimulus to impinge upon response selection circuitry before the task set has been updated.

Proceeding, the results of Experiment 3A and 3B can be accounted for in the following way. To begin with, the underadditivity of TS and MS costs on CMTS trials does not, in the present case, appear to be attributable to the elicitation of task set reconfiguration, either anticipatory or otherwise, on MS trials, at least not to the same extent as in Experiments 1 and 2. Instead, it is proposed that the bulk of the switch cost interaction reflects three basic effects: a) a reduced susceptibility to external sources of distraction on TS and CMTS versus RR and MS trials, owing to a necessarily heightened intentional grasp of the current position in the task sequence in the former relative to the latter, b) a tendency for switches of stimulus modality to disrupt the maintenance of intentional synchrony with the cyclical task sequence under conditions in which conscious monitoring of the current position in the sequence is relaxed or deemphasized (*i.e.*, on MS trials), and c) the slowing of response selection on MS trials, in proportion to task difficulty, as a function of interference generated by concurrent attempts at sequence reacquisition following the disruption of sequence tracking engendered by a shift in stimulus modality.

This hypothesis can be concretized through consideration of its implications for each of the individual Switch Types. The arguments above imply that performance on RR trials benefited from carryover of the task set relevant on the previous trial, as well as a general strategic approach wherein subtasks that differentially taxed working memory resources (*e.g.*, active sequence updating), were delegated to trials on which a task switch would be required. By comparison, the shifting of responsibility for the most daunting portions of the working memory load to TS trials, as well as the absence of task set carryover, both would have led to slower TS RTs. On MS trials, although task set carryover would have been present to some extent, the switch of stimulus modality is assumed to have created an external processing environment that destabilized sequence tracking during the lapse of intentional focus on predictable task repeats, with the practical result that the effort required to review the event files in working memory and reestablish a firm cognitive grasp on one's current location in the sequence led to a disruption of task set, the severity of which varied inversely with the dominance of the current task. Finally, on CMTS trials, the hypothesis holds that heightened intentionality corresponding to the increased working memory demands associated with switching tasks in response to an internally maintained task sequence served to insulate higher-level cognitive processing from the perceptual interference that plagued MS RTs, putting CMTS and TS trials on a similar footing.

While the suggestions above are sufficient to accommodate the presence of TS, MS, and CMTS costs relative to the RR baseline, the similarity of TS and CMTS RTs, variations in MS costs across tasks, and the decoupling of MS and CMTS RTs in the current experiments, and also the fact that the Switch Type profile reported by Hunt & Kingstone (2004) was characterized by MS and CMTS RTs that both appeared inflated relative to TS RTs (since there was external support for the task sequence in their design), further elaboration is necessary in order to explain the interactions that were found between Switch Type and Cue Type, which included larger MS costs in the No-Cue condition of Experiment 3A, and the finding that switches of stimulus modality impacted performance in the Both-Mod and No-Cue conditions of Experiment 3B differently, with no evidence that the effect interacted with task switching/repetition. In so far as both of these results involve differences between trials that do and do not involve switches of modality, it will be worthwhile to consider whether modality shifts are likely to have had any effects on performance that go beyond mere distraction. One possibility that was discussed in introducing the CMTS paradigm, but has played little role in interpretation to this point, is that switching modalities slowed stimulus identification or otherwise interfered with early perceptual processing. Combining this idea with the considerations already outlined, and recalling Oriet & Jolicoeur's (2003) finding that stimulus contrast effects and TS costs combined additively, the Cue Type by Switch Type interactions observed in Experiments 3A and 3B can be plausibly, if not authoritatively explained by attributing: a) the reduced MS costs in the two cued conditions of Experiment 3A to a tendency for single-modality cues to mediate against the distracting effects of switches of imperative stimulus modality, and b) the interaction documented in Experiment 3B to reduced interference in early perceptual processing of cross-modal stimuli in the Both-Mod condition. The divergent arguments are to some extent necessitated by the fact that in the former case the effect was restricted to MS costs, specifically, whereas in the latter the effect did not appear to depend on whether the task switched or not. These suggestions are highly data-driven, however, and should be regarded with some skepticism.

Finally, it should be noted that there is at least one alternative possibility that is relevant to the divergent MS cost magnitudes for the two tasks. If the Ipsilateral task were implemented in such a way that processing in both modalities was biased in favor of making a same-side response, whereas the Contralateral task were established in a way that predominantly impacted processing only in the perceptual modality of the current imperative stimulus, then it would be expected that selecting a same-side response to an imperative stimulus on MS trials would proceed with relative efficiency because of task set carryover, whereas no such benefit would be present when an opposite-side response was called for. One cognitive strategy that might support such a dissociation would be the adoption of an ipsilateral orienting protocol as the default attentional setting irrespective of modality, in concert with a more stable biasing of response selection circuitry to favor the propagation of activation stemming from low level orienting responses in an ipsilateral fashion (*i.e*, leftward orienting begets a left-hand response), so that performance of the Contralateral task involved updating of orienting protocols at a level where clear distinctions were still present between the internal representations of visual and auditory stimuli. This hypothesis encounters considerable difficulty in accommodating other findings, however, in particular the fact that the effect it seeks to explain was not observed on CMTS trials. The considerations just outlined would seem to imply that, for example, Ipsilateral CMTS RTs should have been subject to reduced task-set interference since on the preceding trial the Contralateral task would have been implemented in the now-irrelevant modality.

Chapter 3 Summary

Experiments 2, 3A and 3B explored several issues related to the cuing and sequencing of both the upcoming task and imperative stimulus modality. The results of Experiment 2 replicated a highly statistically reliable absence of CMTS costs first documented in Experiment 1, and also directly challenged theoretical arguments made by Murray et al. (2009) concerning the role that modality specificity in implicit cues plays in the determination of CMTS cost magnitudes in the explicit cuing paradigm. While some evidence was found that was consistent with the possibility that preparation effects on CMTS relative to MS RTs were similar in the Unimodal and Bimodal cuing conditions, other aspects of the results were sufficiently divergent with respect to the expected overall pattern to discourage concluding as much with any confidence.

Experiments 3A and 3B transitioned from the use of a task-cuing paradigm to an alternating-runs approach with no external support for the task sequence. This changeover led to several clear differences in the results that were argued to reflect interacting effects of working memory, executive control, and perceptual distraction. At the most basic level, the results of these experiments, in comparison with the results of Experiments 1 and 2, indicate that not only CMTS cost/advantage magnitudes, but also the sources of variance in cognitive processing that go into determining those magnitudes (as measured by the correlational analyses), can vary markedly across experimental conditions even when the task and stimulus sets that are employed are invariant.

CHAPTER 4

Experiments 2, and 3A and 3B in the preceding chapter explored several alternative task sequencing and cuing paradigms. Although the manipulations were successful in producing changes in the RT profile for the four Switch Types, they failed to produce results that mimicked those reported by Murray et al. (2009) and Hunt & Kingstone (2004), respectively, implying that there are other important determinants of CMTS cost magnitudes beyond those that have thus far been systematically investigated. While the experiments presented in Chapters 2 and 3 can be distinguished from Hunt & Kingstone's and Murray et al.'s studies on a range of variables, the most obvious choice for extending the investigation at this point is to focus on the tasks that subjects were asked to perform. In addition to being a highly salient feature of the methodology, there is considerable evidence from the broader TS literature that the tasks subjects perform can impact switch cost magnitudes, and the results of Experiments 3A and 3B provided a clear indication that CMTS cost magnitudes can differ as a function of the task subjects perform as well. There is also some reason to think that the specific tasks utilized in Experiments 1 though 3B might be particularly well suited to demonstrations of nonadditivity, at least in the task-cuing paradigm. To the extent that the requisite functional distinctions between same-side and opposite-side spatial judgments can be implemented in modality-specific processing centers, it is conceivable that switching between, as opposed to repeating spatial judgments across modalities could be inherently poorly suited to the observation of differential amounts of cross-modal task set interference on CMTS relative to MS trials, assuming that the modality-specificity in the implementation of task sets contributes to the bypassing of a portion of the task set interference on CMTS trials, and also to the bypassing of a portion of the task repetition benefit on MS trials. Considered relative to a hypothetical pair of tasks that are only conceptually, rather than perceptually defined, this logic dictates that CMTS costs should have been reduced using the spatial tasks relative to what would be expected if task sets were implemented more centrally, since in this latter case we would predict that carryover of activation in amodal task processing centers would result in increased repetition benefits on MS trials, as well as increased task set interference on CMTS trials.

Evaluation of the possibility that CMTS costs are sensitive to the tasks that subjects perform served as the starting point for the experiments detailed in this chapter. The issue was taken up explicitly in Experiment 4, the results of which served as the motivation behind Experiments 5A, 5B, which themselves made use of the tasks from Experiment 4 to address the possibility that factors related to the way in which stimulus sets were defined and differentiated for the two modalities contributed to the results thereof. A general discussion of the research presented in this thesis follows consideration of the results of Experiments 5A and 5B. The aim of this section will be to review the primary findings, their place in the existing literature, and their implications for the theoretical understanding of CMTS performance, as well as to briefly outline several ideas relating to the continued evolution of the CMTS paradigm going forward.

Experiment 4

As indicated above, a potentially important characteristic of the preceding experiments was the use of spatial tasks that could have in principle been responded to through modality-specific orienting responses. This possibility was tested in Experiment 4 by having subjects switch between Parity (odd/even) and Magnitude (greater-than/less-than) judgments in response to numeric stimuli in a design that was otherwise structured to mimic that of Experiment 1 as closely as possible. Methodological factors of note include the use of the same explicit cues and response options, and, most importantly, the restriction of the size of the stimulus set. Two factors were critical in this respect. First, only incongruent numbers (with incongruence determined by comparison of the judgment-to-response mappings defined for each task) were presented, so that switching tasks always entailed a strict reversal of S-R mappings. Although somewhat artificial in the present tasks, this was necessary to approximate the inherent incongruence between ipsilateral and contralateral spatial judgments. Second, the imperative stimulus set was comprised of only two numbers, which were equivalent for the two modalities. This was done to replicate the restricted number of basic stimulus types utilized in the spatial tasks, and can be seen to provide an appropriate match conditional on the assumption that the cognitive representation of left-versus right-sidedness of imperative stimuli in Experiments 1 through 3B was equivalent for the two modalities, irrespective of the obvious perceptual differences, in much the same way that would be expected for two-ness or nine-ness on cognitive dimensions related to numerical identity. The core prediction was that if the spatial tasks utilized in previous experiments were amenable to cognitive strategies that could minimize differential cross-modal task set interference on CMTS relative to MS trials, and if such an effect contributed to the results of those experiments, then the inherently amodal processing requirements of the numerical tasks in the present design should contribute to the observation of CMTS costs.

Experiment 4 Methods

Participants

Thirty-two undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

The materials and cue stimuli were the same as those used in Experiment 1. Imperative stimuli were changed from the white circle and white noise stimuli of previous studies to number stimuli presented either visually or auditory. The stimulus set for the current study was restricted to the numbers “2” and “9”. On visual trials a single number was displayed in white at the center of a black screen in 50-point boldface Tahoma font for 307 ms before the screen went blank. On auditory trials subjects heard a female voice pronounce the numbers. Sound files were edited as in Experiment 2, with the duration set to 307 ms.

Procedure

The basic procedure was the same as Experiment 1 with the exception of the tasks that subjects were required to perform, and some alterations in timing variables. The two tasks in the current study involved determining on the one hand whether a presented number was odd or even (Parity task), and on the other hand whether it was greater than or less than a given comparison number (Magnitude task). The tasks were introduced to subjects during the instruction phase as “Even/Odd” and “Greater/Lesser”, respectively. For half the subjects the comparison number for the Magnitude task was given as “5”, and for the other half the comparison number was “6”. Both tasks shared a common response set, again the “S” and “K” keys on the keyboard, and the mappings from tasks onto responses were counterbalanced across subjects. These mappings were also chosen so that all trials were incongruent. That is, if the appropriate response for a “2” on the Parity task was a left-hand key press, the appropriate response for a “2” on the Magnitude task would be a right-hand key press, whereas for a “9” the correct responses would be reversed.

Subjects completed 40 blocks of 66 trials each. Two RSI levels were used. Both featured a 200 ms PCI. Cue presentation in the Short-RSI condition lasted 200 ms, whereas in the Long-RSI condition cue presentation lasted 653 ms, thus the total duration of the former was 400 ms, and the total duration of the latter was 853 ms. RSI was held constant within blocks, and alternated between each block, with the ordering of the RSI conditions counterbalanced across subjects. Task sequence within blocks was unpredictable, and blocks were structured so that subjects encountered each possible two-trial sequence at least once every two blocks. The duration of the imperative stimulus on each trial was 307 ms, after which time the monitor displayed a blank screen and the headphones were silent. This continued until the subject responded, at which point the PCI for the next trial would begin.

Results

Analysis of the RT Switch Type Profile

Preparation of the data for analysis followed the procedure outlined for previous experiments. The first 14 blocks for each subject were excluded from the analyses. Means for the four Switch Types corresponding to each RSI level, and to each Task and Modality combination, are presented in Figures 9 and 10, respectively. The associated contrasts, as well as the tests for marginal switch costs, are presented in Table 5. The results can be summarized as follows. RR RTs were reduced in the Long-RSI condition, and also for the Visual modality, with more pronounced benefits from the Long-RSI evident on Auditory RR trials, $F(1, 31) = 55.51$, $p < .001$. No consistent statistically significant effects of Task were observed, although the monotonic ordering of RR RTs in the two tasks was opposite between the two modalities. TS, MS and CMTS costs were all observed. A 95% confidence interval on the latter value spanned CI = [42, 104] ms. The length of the RSI was not found to be effective in reducing TS or CMTS costs, although MS costs did benefit from the longer preparation interval relative to TS costs. Finally both TS costs and MS costs were larger in the Auditory modality.

Analysis of the Error Switch Type Profile

Preparation of the data for examination of errors proceeded as indicated in previous experiments. Median error percentages for the interactions of Switch Type and RSI, and Switch Type and Trial Type, are presented in the bottom panels of Figures 9 and 10, respectively. Of primary importance is the fact that error percentages on CMTS trials were consistently numerically larger than error percentages on MS trials, indicating that the presence of CMTS costs could not be attributed to a speed-accuracy tradeoff.

RT Correlations

Data preparation for correlational analyses proceeded as before. CMTS RTs were positively associated both prior to, $r(30) = .55$, $p < .002$, and after partialing $r(28) = .64$, $p < .001$. The within modality correlations between MS and CMTS RTs were $r(30) = .87$, $p < .001$, and $r(30) = .93$, $p < .001$, for Visual and Auditory trials, respectively.

Discussion

The results of Experiment 4 were generally consistent with the hypothesis that the amodal nature of the parity and magnitude tasks in Experiment 2 led to increased cross-modal task set interference on CMTS relative to MS trials. Two pieces of evidence stand out in this regard. First and foremost was the observation of statistically significant CMTS costs in excess of 70 ms, which had been either absent or reversed in Experiments 1 and 2. At the same time, and again in stark contrast to the results of earlier explicit cuing studies that used spatial tasks, CMTS RTs were found to be positively correlated across modalities even after their shared variance with MS RTs had been taken into account. Together these findings point to the conclusion that subjects suffered from interference on CMTS trials that was to some extent distinct from the source of the slowing documented for MS trials, and was also to some extent consistent across perceptual modalities, as was predicted from the amodal and conceptual nature of the tasks subjects performed.

Another finding that differed from those produced by Experiments 1 and 2 that was not expected was the failure of the longer preparation interval to lead to a reduction in either TS or CMTS costs. It has been previously noted that preparation effects are not always observed in task switching research (*c.f.*, Altmann, 2004); still, the absence of a switch-specific effect in the current data was surprising given that the cues that were used had produced effects characteristic of advance reconfiguration in an earlier experiment, and that multiple RSIs were used and were manipulated between blocks. As will be seen in Experiments 5A and 5B, this result was ubiquitous with the current set of tasks, and could reflect any of a number of factors, including but not necessarily limited to the abstract nature of the tasks, the abstract relationship between the cues and the tasks, the arbitrary mappings of judgments to responses, or the fact that the tasks sets were not distinguishable on the basis of perceptual factors (Meiran, 2000a). Considerable speedup in baseline RR RTs was apparent, likely reflecting the combined benefits of a longer ITI and the extra time available to identify the cues and the tasks they indicated. Further RSI-related speedup was apparent when the modality of the imperative stimulus switched, with no evidence that the effect differed as a function of whether a switch of task was also required, perhaps indicating that the time course of cue identification was somewhat slowed on cross-modal trials. The reversal of monotonic ordering between the two tasks in RR RTs did not replicate in Experiments 5A and 5B, and is therefore considered suspect.

The remaining significant results generally seemed to indicate that performing the tasks, as well as switching between them and performing them in a new modality, were all more difficult when the current trial was Auditory. The fact that the effect has been most clearly evident in the present results and in Experiment 1 lends credence to the suggestion that the high/low pitch distinction required for identification of the appropriate task in the Auditory modality in these studies was at the root of this effect.

Experiments 5A and 5B

The results of Experiment 4 appeared to provide clear and relatively straightforward support for the hypothesis that the spatial tasks utilized in earlier experiments did permit some degree of bypassing of carryover of task set activation on trials in which the modality of the imperative stimulus switched, and that this bypassing contributed to the absence of CMTS costs in Experiments 1 and 2. Attribution of these effects to the tasks, themselves, however, is contingent upon the validity of the assumption that the restrictions on the stimulus set used in Experiment 4 provided for a suitable approximation of the essential characteristics of the stimulus set used in Experiments 1 through 3B. There are several reasons to question this assumption. For one thing, Murray et al. (2009), did observe CMTS costs of a magnitude comparable to those observed in Experiment 4 under conditions in which one of the two tasks they had subjects perform was spatial in nature; although on this point it should be noted that it is far from a given that the inclusion of one spatial task among non-spatial tasks should necessarily lead to the same results as the restriction of the task space in its entirety to the spatial domain. More compelling evidence comes from recent research that suggests that cross-modal linkages may be a prominent feature of the deployment of attention to spatial locations (Driver & Spence, 2004), challenging the notion that spatial tasks should be intrinsically unique with respect to the extent of modality-specificity that they permit in task set implementation.

This latter point, in fact, implicates another characteristic of the design of the earlier experiments that may have been critical in enabling the observation of a CMTS advantage in Experiment 1. Although judgments of stimulus location for the Ipsilateral and Contralateral tasks expressly required only a dichotomous partitioning of space along a single dimension, stimuli nonetheless originated from specific points in three-dimensional space. Furthermore, because Visual stimuli were presented on a computer screen positioned in front of subjects and Auditory stimuli were presented through headphones, stimuli in each modality could be distinguished from stimuli in the other modality according to their relative depth in space. If subjects were sensitive to this characteristic of the design (*c.f.*, Spence, MacDonald, & Driver, 2004), it seems possible that this discontinuity between the spatial positioning of visual and auditory stimuli could have enabled a parallel dissociation of the task space, effectively bypassing some portion of the task-set interference that would have otherwise been present on CMTS trials, and also some of the facilitation from carryover that would have sped responding on MS trials under other circumstances.

These considerations are critical for the interpretation of the CMTS costs in Experiment 4 as reflecting the difference between spatial and non-spatial tasks, per se, because the restriction of the stimulus set to the numbers “2” and “9” in Experiment 4 was predicated on the assumption that left-side and right-side stimuli in Experiments 1 through 3B were, with regard to those aspects of processing that contribute to and determine switch cost magnitudes, cognitively differentiated according to the same criteria regardless of whether they were visual or auditory. Violation of this assumption would mean that segregation of stimulus sets, rather than task sets, between the two modalities could have been at the heart of the divergent results. Consequently, a representative translation of the essential characteristics of Experiments 1 through 3B would seem to require specification of two disjoint stimulus sets, one for each modality, with each set in some sense parallel to the other with respect to the judgments they afford in the parity and magnitude tasks. This manipulation was affected in Experiments 5A and 5B by doubling the number of numerical identities that subjects were presented with, and assigning two numbers to each modality, with the constraint that the stimulus set in each modality afforded both responses for both tasks, and that the stimuli within each set were equidistant from the average magnitude comparison reference number. Experiment 5A was otherwise identical to Experiment 4, whereas in Experiment 5B the two RSI durations were equalized by increasing the duration of the PCI in the Short-CTI condition, and the tasks were introduced to subjects as “Parity” and “Magnitude” tasks, respectively. If the hypothesis that spatially distinct stimulus sets, as opposed to spatial tasks, led to the absence of CMTS costs in earlier explicit cuing studies is correct, then CMTS costs should be absent in Experiments 5A and 5B. Methods and results sections for each experiment are presented first, and an integrated discussion follows.

Experiment 5A Methods

Participants

Thirty-two undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

Materials and stimuli were the same as those used in Experiment 4, with the exception that the size of the imperative stimulus set was expanded and differentiated between the two modalities. Specifically, imperative stimuli on Visual trials consisted of the numbers “4” and “7”, and on Auditory trials of the numbers “2” and “9”. Formatting and duration of the stimulus presentations was identical to Experiment 4.

Procedure

Experiment 5A was procedurally identical to Experiment 4.

Experiment 5A Results

Analysis of the RT Switch Type Profile

Preparation of the data for analysis followed the procedure outlined for Experiment 4. Means for the four Switch Types corresponding to each RSI level, and to each Task and Modality combination, are presented in Figures 11 and 12, respectively. The associated contrasts, as well as the tests for marginal switch costs, are presented in Table 6. Results indicated that RR RTs were faster in the Long-RSI condition, as well as on Visual trials, with the length of the preparation interval primarily affecting RR performance in the Auditory condition, $F(1, 31) = 58.9$, $p < .001$. TS and MS costs were evident, with the latter more pronounced on Auditory than on Visual trials. CMTS RTs were not observed to be statistically significantly different than MS RTs, and preparation in the Long-RSI condition was not observed to alter switch costs. A 95% confidence interval around the difference between CMTS and MS RTs yielded CI = [-7, 45] ms.

Analysis of the Error Switch Type Profile

Preparation of the data for examination of errors proceeded as indicated in previous experiments. Median error percentages for the interactions of Switch Type and RSI, and Switch Type and Trial Type, are presented in the bottom panels of Figures 11 and 12, respectively. Two observations are worth noting. First, there was some evidence to suggest that preparation may have resulted in a reduction in error TS costs, $T = 119$, $p < .025$. Second, relatively strong support for the presence of CMTS error costs in the Visual modality was obtained, $T = 216$, $p < .001$.

RT Correlations

Data preparation for correlational analyses proceeded as before. CMTS RTs were positively associated prior to, $r(30) = .82$, $p < .001$, as well as after partialing with respect to MS RTs, $r(28) = .45$, $p < .013$. The within modality correlations between MS and CMTS RTs were $r(30) = .87$, $p < .001$, and $r(30) = .98$, $p < .001$, for Visual and Auditory trials, respectively.

Experiment 5B Methods

Participants

Thirty-two undergraduate students at the University of California, San Diego participated for experimental credit.

Materials and Stimuli

The materials and stimuli in Experiment 5B were identical to those used in Experiment 5A.

Procedure

Experiment 5B was procedurally identical to Experiment 5A with three exceptions. First, the tasks were presented to the subjects under the headings “Parity” and “Magnitude”, instead of “Even/Odd” and “Greater/Lesser”. Second, the total durations of the two RSI conditions (referenced as Short- and Long-CTI conditions) were equalized by lengthening the PCI of the (formerly) Short-RSI condition from 200 ms to 653 ms, while retaining the 200 ms CTI. Finally, to accommodate the somewhat longer average trial length the number of blocks subjects were required to complete was decreased from 40 to 36.

Experiment 5B Results

Analysis of the RT Switch Type Profile

Preparation of the data for analysis followed the procedure outlined for Experiment 5A. Means for the four Switch Types corresponding to each RSI level, and to each Task and Modality combination, are presented in Figures 13 and 14, respectively. The associated contrasts, as well as the tests for marginal switch costs, are presented in Table 7. The pattern of statistically significant results was identical to that of Experiment 5A, with the lone exception that CMTS costs were observed in Experiment 5B. A 95% confidence interval for CMTS costs yielded CI = [13, 62] ms.

Analysis of the Error Switch Type Profile

Preparation of the data for examination of errors proceeded as indicated in previous experiments. Median error percentages for the interactions of Switch Type and RSI, and Switch Type and Trial Type, are presented in the bottom panels of Figures 13 and 14, respectively. There was again some indication that error TS costs were reduced by preparation, $T = 141$, $p < .007$. Although some variability was evident, marginal CMTS error costs were found $T = 165$, $p < .001$.

RT Correlations

Data preparation for correlational analyses proceeded as before. CMTS RTs were positively associated, $r(30) = .64$, $p < .001$, although only a trend towards a significant relationship was found after partialing out MS RTs, $r(28) = .31$, $p < .093$. The within modality correlations between MS and CMTS RTs were each $r(30) = .93$, $p < .001$.

Discussion of Experiments 5A and 5B

Whereas CMTS costs in Experiment 4 averaged 73 ms, in Experiments 5A and 5B the average differences between CMTS and MS RTs were reduced to 19 and 38 ms, respectively, providing some indication that the segregation of the stimulus sets between the Visual and Auditory modalities in the latter two cases did lead to a reduction in CMTS costs, and further implying that the spatial dissociation of auditory and visual stimulus location may have contributed to the absences and reversals of CMTS costs when subjects switched between Ipsilateral and Contralateral judgments in the explicit cuing paradigms of Experiments 1 and 2. At the same time, the results of Experiments 5A and 5B did not strictly conform to the precedents set by Experiments 1 or 2, either. The significant CMTS advantage in the 27/200 condition of Experiment 1 arguably provides for a loaded comparison because of the longer PCI used in the current designs; however, the presence of statistically significant CMTS costs in Experiment 5B, and the less pronounced trend in Experiment 5A, are generally at odds with, for example, the lack thereof in Experiment 2 and also in the 480/200 condition of Experiment 1. The same is true to a limited extent for the results of the error analyses in comparison to Experiment 1, as well as for the correlational analyses, which indicated that CMTS RTs in the Visual and Auditory modalities had shared sources of variance that could not be accounted for by performance on MS trials in Experiment 5A, with the same trend evident to a lesser and nonsignificant extent in Experiment 5B. Thus, although the results do provide support for the hypothesis that disjoint sets of bivalent stimuli between the two modalities can contribute to reductions in CMTS costs, they also seem to suggest that other variables are likely to have contributed to the divergent results across studies.

One possibility, of course, is that the critical variables were intrinsic to response selection in the spatial domain, specifically. However, there are at least two other factors that cannot be ruled out. First, incompatibility of S-R mappings between Ipsilateral and Contralateral judgments is an unavoidable characteristic of those tasks, whereas for numerical judgments the ubiquitous incongruence of imperative stimuli was achieved artificially (clearly, for example, there exist numbers that are both even and greater than 5 even though none were included in the designs of Experiments 5A and 5B). Note, however, that it is not entirely clear that coordinating performance between task sets that are exact inverses of one another should be expected to lead to isolated increases in MS RTs or decreases in CMTS RTs, as would be required by this suggestion; as an illustrative counterexample, it seems logical that an innate incompatibility might in fact serve to insulate MS RTs against interference from task set uncertainty. On the other hand, it could also be the case that the greater complexity of the full span of potential S-R mappings when switching between tasks that are not definitionally incongruent contributes to inefficiencies when configuring a new task set, consistent with the observation of statistically significant CMTS costs in Experiment 5B.

The second hypothesis concerns the possibility that it was not merely the stimulus sets that were disjoint between the two modalities in Experiments 1 through 3B. In particular, the fact that Visual and Auditory stimuli could be localized to different depths in three-dimensional space could have contributed to a cognitive reliance on two disjoint, albeit parallel decision spaces as a function of the modality in which the stimulus was presented in the spatial tasks. By comparison, no such clear prepotent distinction was available in distinguishing 4s and 7s from 2s and 9s in Experiments 5A and 5B, and although the differences in depth between visual and auditory stimuli were present in Experiments 4 through 5B, this information was entirely unrelated to the judgments subjects were making, and may have therefore been discounted (*c.f.*, Mondor and Amirault, 1998). From these considerations we would expect that a relatively unified decision space would have been invoked to guide responding in the Parity and Magnitude tasks, leading to the prediction that carryover of activation should have played a larger role in determining RTs to judgments of numerical identity on cross modal trials than was the case for judgments of spatial position.

With regard to the apparently larger CMTS costs in Experiment 4 as compared to Experiments 5A and, to a lesser extent 5B, the most obvious question to be asked is whether the effect was due primarily to changes in the magnitude of MS costs across experiments, or whether segregation of stimulus sets between the two modalities acted primarily to reduce task set interference specific to CMTS trials. In fact, the only clear and directionally consistent difference to emerge involved MS costs specifically on trials in which the response repeated. Restricting consideration to Long-CTI trials (since the length of the PCI in the Short-CTI condition differed across Experiments 4, 5A and 5B), these reached 274 ms, with a 95% CI = [220, 329] in Experiment 4, whereas in Experiments 5A and 5B, MS costs on response repeat trials were 438 ms, CI = [350, 525], and 339 ms, CI = [272, 405], respectively. Although in need of further direct study, this observation is noteworthy for two principle reasons. First, it indicates that bypassing of repetition benefits, as opposed to task set interference, in Experiments 5A and 5B, was likely at the root of the divergent CMTS cost magnitudes, and more generally reinforces the importance of taking account of changes in baseline performance before interpreting CMTS costs as evidence of bypassed interference. Second, in so far as response repetitions on MS trials in Experiment 4, but not in Experiments 5A or 5B, involved stimulus repetitions, the apparent specificity of the MS cost inflation in the latter two experiments indicates that the primary effect of using separate stimulus sets for the two modalities with the current set of tasks may not have been a restructuring of task sets around the modality-specific stimulus sets, but rather the simple elimination of stimulus repetition benefits on a subset of MS trials (*c.f.*, Hubner et al., 2004).

The remaining patterns evident in the data – the predominant ineffectiveness of preparation, as well as slower responding, larger costs associated with switching modalities, and larger effects of preparation on baseline performance for Auditory trials – all replicated earlier findings and do not appear to require further qualification beyond comments made in the discussions of other experiments.

General Discussion

Primary Empirical Findings

The principle aim of this thesis has been the exploration of the cognitive processes and behavioral phenomena associated with switching tasks cross-modally. Two published studies have provided evidence that was interpreted by their authors as indicating that task shifting under cross-modal stimulus conditions might be subject to reduced task set interference relative to when stimuli were presented in the same modality on both the previous and current trial. Two pieces of evidence served as the basis for these claims: a) a robust and consistent underadditivity of TS and MS costs across multiple sequencing and cuing paradigms, including an absence of statistically significant CMTS costs in one case, and b) a failure to detect any linear association between CMTS costs in the visual and auditory modalities. The results of the experiments presented in this thesis in some cases replicated, and in other cases either extended or directly challenged these findings. To review, let us first consider the following five basic methodological dimensions that were investigated and their effects: a) active preparation during a prolonged CTI, b) modality-specificity in cues, c) span and cross-modal overlap in stimulus sets, d) task set-related factors, and e) task sequencing/external sequence support.

Preparation during the CTI

The duration of the preparation interval was manipulated in five of the seven experiments, variously independently of, or in tandem with, the length of the overall RSI. Whether or not preparation impacted the costs of switching tasks depended on the tasks that subjects were performing, with no significant effects on RTs evident when subjects transitioned between parity and magnitude tasks. Of greater concern were the effects of CTI length on CMTS relative to MS RTs. Virtually every conceivable pattern was observed: MS relative to CMTS reductions in the comparison of the 27/200 and 27/653 conditions of Experiment 1, CMTS relative to MS reductions and apparent MS relative to CMTS increases in the Unimodal and Bimodal conditions of Experiment 2, respectively, and approximately parallel MS and CMTS reductions in Experiments 4, 5A and 5B, as well as in the 27/653 relative to the 480/200 condition of Experiment 1. The absences of differential preparatory effects on RTs in Experiments 4 through 5B were unexpected, but in so far as they occurred alongside similar failures of preparation with respect to RT TS costs they are of limited theoretical value relative to the primary concerns of this thesis. The inconsistent effects of preparation in Experiments 1 and 2 were generally accounted for by inferring the operation of a hierarchical switching mechanism that promoted, to varying degrees, lapses in set and/or anticipatory set switching on trials in which the modality of the imperative stimulus switched.

Modality-specificity in cues

Whether cues unambiguously and/or directly indicated the modality of the upcoming imperative stimulus was manipulated in Experiments 2 and 3A. Murray et al. (2009) have speculated that the use of modality-predictive cues may have led to a reduction of CMTS costs to statistically undetectable levels. This suggestion received no support in the present results. Experiment 2 documented parallel profiles for MS and CMTS RTs irrespective of whether cues were Unimodal or Bimodal, and in Experiment 3A CMTS costs were larger when task-nonspecific and modality-predictive cues were presented than when no cues were presented. Another important aspect of the results regarding modality-predictive cuing was the fact that the use of such cues did not universally benefit performance. On the contrary, RR RTs were nonsignificantly faster in the No-cue condition than in the Ipsimodal condition of Experiment 3A, and performance was significantly slower in the Unimodal relative to the Bimodal condition of Experiment 2 when the CTI was short. The only case in which a clear benefit of Unimodal cuing was observed was for the comparison of the effectiveness of preparation by Cue Type in Experiment 2, wherein a longer preparation interval conferred no overall RT advantage in the Bimodal condition, but in the Unimodal condition served to overcome the Bimodal Short-CTI advantage noted above, with the differential sensitivity to PCI/CTI length showing up most clearly on trials in which the modality of the imperative stimulus switched, albeit for unexpected reasons. One noteworthy implication of these results is that low-level perceptual orienting costs (expecting Visual and getting Auditory) play at most a small role in determining MS and CMTS costs, since we would otherwise expect to see stronger evidence that modality-specific cues facilitated performance when the CTI was short. This observation highlights the need to provide an alternative account for the pronounced slowing typically observed on MS trials, and calls into question the wisdom of the theoretical perspective introduced by Hunt and Kingstone (2004), wherein it was suggested that the CMTS underadditivity should be interpreted as reflecting separate bottlenecks for task and modality shifting. Somewhat more generally, these findings also paint a picture that is fundamentally at odds with predominantly reductionistic accounts of TS phenomena; *e.g.*, with De Jong's (2000) suggestion that preparatory effects and residual switch costs can be accounted for in their entirety as reflecting probabilistic successes and failures with respect to the unidimensional and dichotomous axis of intention activation.

Span and cross-modal overlap in stimulus sets

A distinctive characteristic of the experiments presented in this thesis was the use of small imperative stimulus sets. Although not originally conceived of as a dimension of primary interest, Experiments 4, 5A and 5B provided some opportunity to evaluate the possibility that the size and modality-independence of the stimulus sets would impact performance. CMTS costs were largest when the stimulus set was, arguably, smallest (Experiment 4), a result that appeared to depend to a considerable extent on MS RT facilitation from cross-modal stimulus identity repetitions. A similar effect has been reported in studies of task switching in response to purely visual stimuli (Hubner et al., 2004), pointing to the conclusion that the previously undocumented absences and reversals of CMTS costs in Experiments 1 and 2 cannot be explained as a byproduct of the restricted size of the stimulus sets used in the present collection of studies. On the other hand, the differences that were apparent between the results of Experiment 4 on the one hand, and Experiments 5A and 5B on the other hand, do favor the conclusion that estimates of CMTS costs, and perhaps more importantly the underadditivity of TS and MS costs, may be sensitive to the extent to which stimulus sets for the two modalities overlap unless within-modality and between-modality transitions are equated with respect to stimulus repetitions. To relate these considerations to the previously published studies of CMTS, Hunt & Kingstone (2004) utilized a larger stimulus set (eight numbers) and did not control for stimulus repetitions in design or analysis. It therefore seems likely that the relatively small percentage of expected cross-modal stimulus repetitions (probability = .125), may have contributed both to the observation of CMTS costs in their design, as well as to the somewhat reduced magnitude of those costs relative to what was found in Experiment 4, in which the cross-modal stimulus repetitions occurred with probability = .5. Stimulus presentation in Murray et al. (2009) was similar to stimulus presentation in the experiments detailed in this thesis (monitor for visual stimuli, headphones for auditory stimuli), and on the basis of this commonality one might have expected similar results in their spatial task as were obtained for the Ipsilateral task in Experiments 1 and 2. They also employed a fairly large stimulus set for their classification task (30 sounds and 30 line drawings), indicating that stimulus repetitions should have been rare, and were arguably impossible on MS and CMTS trials owing to the distinctiveness of the visual and auditory stimulus sets. Given that both of these observations would tend to favor the prediction that CMTS costs should have been absent or minimal, it is interesting that significant CMTS costs were the norm in their experiments.

Task set-related factors

One implication of the considerations just outlined regarding Murray et al.’s (2009) findings is that the tasks that subjects perform, and perhaps the relationships between those tasks, may play a critical role in determining CMTS costs. Unfortunately, Murray et al. collapsed across task in analyzing and reporting their results, so it is unclear whether their results differed as a function of which task subjects were performing. CMTS performance in the current set of experiments was evaluated using two different pairs of tasks, the first purely spatial, and the second requiring judgments of numerical identity. Although switch costs were numerically underadditive in either case, some differences were apparent even after cross-modal stimulus repetitions were controlled for by segregating the stimulus sets for the two modalities: significant CMTS costs in Experiment 5B and a significant partial correlation between Visual and Auditory CMTS RTs in Experiment 5A, as opposed to statistically reliable absences or reversals of CMTS costs, and no evidence that CMTS RTs were correlated across modalities after partialing with respect to MS RTs, in Experiments 1 and 2. It is therefore possible that the presence of CMTS costs in the experiments reported by Hunt & Kingstone (2004) and Murray et al. (2009) reflect to some extent the nature of the tasks used by those authors. At the same time, as noted in the discussion of Experiments 5A and 5B, it is not possible to rule out the influence of other factors such as definitional task set incongruence or the adoption of modality-specific decision spaces in producing CMTS cost reversals exceeding a mere underadditivity.

Task-sequencing/external sequence support

Finally, two basic methodological approaches have dominated research on TS costs: the task-cuing paradigm, and the alternating-runs paradigm. Although some theoretical proposals have been predicated on characteristics that are unique to one or the other design type (*e.g.*, Logan & Bundesen, 2003), it is not uncommon for the distinction to be largely glossed over in literature surveys. The results of Experiments 3A and 3B, as compared to the results of Experiments 1 and 2, provide a clear demonstration that CMTS performance may differ markedly in the two paradigms, at least under certain conditions. Whereas CMTS costs were absent or reversed, and cross-modal partial correlations between CMTS RTs were not statistically detected in Experiments 1 and 2, significant CMTS costs and strong cross-modal partial correlations between CMTS RTs were present in Experiments 3A and 3B, implicating the use of an alternating-runs methodology as another possible contributor to the CMTS costs evident in Hunt and Kingstone (2004). That said, the overall Switch Type profile in Experiments 3A and 3B did not exactly replicate the monotonic pattern evident in the results of Hunt and Kingstone, as MS and CMTS RTs were not observed to be slowed significantly relative to TS RTs. This fact, among others, figured in the hypothesis put forward to account for performance in Experiments 3A and 3B, wherein it was suggested that the executive control and working memory load entailed by requiring subjects to keep track of which task to perform without any external support may have served to insulate cognitive function on CMTS trials from the distracting effects of stimulus modality shifts.

Diagnosing Interference in the CMTS Paradigm

A central premise of the theoretical interpretations of CMTS performance offered by Hunt & Kingstone (2004) and Murray et al. (2009) is that switching tasks across sensory modalities led to switch cost reductions relative to within-modality task switches because the former case was characterized by separable bottlenecks for task and modality shifting, and/or a bypassing of task-set interference. As has been previously noted, however, the presence of sizable MS costs in their results, as well as the fact that the apparently different CMTS cost magnitudes across experiments in Murray et al. could be accounted for by increased MS costs, which themselves may have resulted from an overlooked CS confound, cast doubt on their interpretations.

At issue in resolving the questions raised about Hunt and Kingstone's (2004) and Murray et al.'s (2009) results is the determination of what kind of information, exactly, can be gleaned from the analysis of CMTS cost magnitudes. Inferences about the cognitive underpinnings of task-switching abilities in general that are based on the assumption that TS and MS cost underadditivities reflect bypassed interference are likely to differ profoundly from inferences derived from an alternative set of assumptions. In the broader task-switching literature, philosophically related questions of whether within-modality TS costs reflect carryover of activation or executive control processes, and whether estimates thereof might not also be dangerously sensitive to factors that influence baseline repeat-trial performance (*e.g.*, CS confounds and the effects of previous exposure to experimental stimuli), have played a prominent role in advancing understanding of the core phenomena. The consideration of hypotheses that explain CMTS phenomena without relying so heavily on assumptions of bypassed interference are therefore worthy of consideration on grounds of empirical and theoretical precedent alike.

The account favored in this thesis, which is informed both by consideration of the results presented in the two published studies of CMTS performance, as well as by the results of the experiments detailed herein, holds that the degree of overlap in cognitive functionality between MS and CMTS trials, and the transparency with which this overlap manifests in the relative magnitudes of MS and CMTS RTs, can vary markedly as a function of: a) the characteristics of the tasks subjects perform, with tasks that require amodal judgments tending to produce larger CMTS costs, which in this case can be interpreted as at least a relative index cross-modal task set interference, b) the stimulus sets they are presented with, such that cross-modal stimulus repetitions will tend to promote the observation of CMTS costs, whereas dissociations between stimulus sets for the two modalities – particularly when defined by prepotent dimensional criteria such as spatial depth – will tend to have the opposite effect; in these cases variations in CMTS costs can be a misleading index of cross-modal task set interference, and finally c) the nature of the control requirements entailed by the sequencing and cuing paradigms employed, wherein use of the task-cuing paradigm can promote extensive overlap in cognitive function between MS and CMTS trials that masks CMTS costs without enabling a corresponding reduction in cross-modal task set configuration requirements, and use of the alternating-runs paradigm without external sequence support can unmask CMTS costs by permitting greater task-repetition facilitation on MS trials, even as strategic factors may bring about a greater sensitivity to perceptually driven interference on trials in which the tasks repeats relative to those in which the task shifts.

To justify these claims, let us first distinguish those experiments that have produced evidence of pronounced functional similarities on MS and CMTS trials, from those that have produced evidence of relatively greater dissimilarity. The clearest examples of the former are Experiments 1 and 2. In particular, not only were CMTS costs reliably absent or reversed in these two studies, but correlations between MS and CMTS RTs within each modality were exceedingly strong, and between-modality partial correlations of CMTS RTs eliminating MS RTs were entirely absent. Experiments 4, 5A and 5B stood apart from other task-cuing CMTS studies now on record in documenting positive between-modality partial CMTS correlations that either trended towards or reached statistical significance. Furthermore, despite attempts to methodologically equate these experiments with the design of Experiments 1 and 2, the same was true of CMTS costs. The occurrence of cross-modal stimulus identity repetitions did appear to play a role in the magnitude of these differences, however, a possibility that was most clearly indicated by the observation of inflated MS cost magnitudes. This fact is noteworthy because it indicates that CMTS cost estimates, like TS cost estimates, may differ between experimental contexts (4, 5A and 5B) that are otherwise closely matched with respect to variables that arguably exert their influence at the level of the task set. Variations in partial correlation magnitudes also hint at the possibility that these analyses may be similarly sensitive to stimulus repetitions. The clearest evidence of a dissociation between processing on MS and CMTS trials was found in Experiments 3A and 3B. CMTS costs in these experiments either trended towards or reached significance, and tended to vary inversely with MS cost magnitudes, which themselves differed nonparadoxically as a function of the tasks subjects performed. Furthermore, strong between-modality partial CMTS correlations were observed, in contrast with the fact that such associations were entirely absent in Experiments 1 and 2 using the exact same task and imperative stimulus sets.

Returning to the question of whether and to what extent results such as those that figured prominently in the hypotheses favored by Hunt and Kingstone (2004) and Murray et al. (2009) may be taken as evidence of bypassed interference, let us begin by considering three findings or trends in the present experiments that were consistent with this possibility. First, the fact that CMTS costs were reliably absent, and that between modality partial correlations for CMTS RTs were not observed in Experiments 1 and 2, whereas both effects trended towards or reached significance in Experiments 5A and 5B, suggests that the tasks and stimuli used in the former case may have been amenable to some degree of modality-specific task set implementation, and consequently may have enabled a bypassing of some portion of the task set interference that was present on TS trials. As noted in the discussion of Experiments 5A and 5B, while this may have been a consequence of the differences between the tasks themselves, a partitioning of the decision space around the different spatial depths of stimulus presentation on Visual and Auditory trials in Experiments 1 and 2 may have also played a role. The second piece of evidence concerns the failure to observe a paradoxical asymmetry in MS or CMTS costs in Experiment 1, even though such an effect was present in TS costs, which was also interpreted as pointing to a modality-specific implementation of the task sets. Finally, although not significant by *post hoc* testing criteria, there was some indication that CMTS RTs might have been faster than TS RTs in the Both-Mod condition of Experiment 3B. A similar monotonic ordering of TS and CMTS RTs was also evident in Murray et al.'s data, although the authors did not comment on the trend's significance.

In all of these cases, however, it should be noted that a cognitive architecture that bypasses one source of interference might only be capable of doing so by bringing other forms of interference into play. The robust observations of MS costs that in many, but not all cases (*e.g.*, Experiment 2), greatly exceeded any expectations that might reasonably be derived from empirical precedent in studies of the MSE suggests that this was in fact likely to have been the case. This was especially true for Experiment 1, which produced evidence that was argued, *vis a vis* the mechanism of hierarchical switching, to be consistent with the presence of qualitatively and quantitatively redundant task set interference and/or uncertainty on MS and CMTS trials on the basis of within- and between-modality correlations, and interactions between MS and CMTS costs on one hand, and PCI and CTI duration on the other. Even in Experiments 4, 5A and 5B, wherein task sets were almost assuredly implemented to a considerable extent in amodal processing centers and correlational results did not lend themselves to the hypothesis of a near complete overlap between the sources of variance responsible for slowing in MS and CMTS RTs, costs associated with switching stimulus modality approached or exceeded 300 ms. Finally, the clearest evidence that MS RTs might have been subject to less interference than TS RTs came in the experiments that made use of the alternating-runs paradigm. While this should have maximized, in relative terms at least, the chances of detecting a hypothetical absence of task set interference on CMTS trials, Experiments 3A and 3B in fact ended up yielding results that indicated a clear dissociation between MS and CMTS performance, consistent with preserved task set-related interference in the later. The results of these experiments were better accommodated by the hypothesis that switching tasks predictably and without external support served to insulate performance against interference resulting from a shift of imperative stimulus modality than vice versa.

In summary, the results presented in this thesis seem to favor the conclusion that while switching modalities may under certain circumstances permit subjects to bypass some aspects of the interference that would otherwise be expected from within-modality task switches, in general the proportion of interference that is bypassed is likely to be negligible in many cases, or, if of a less trivial magnitude, may ultimately be replaced by alternative sources of interference stemming from the task-switching requirements of the experimental context. While arguably a less desirable conclusion than the one favored by Hunt and Kingstone (2004) and Murray et al. (2009), studies of the nature of such interference substitution patterns may prove to be of considerable scientific and practical import in their own right. For example, in complex sequences of more than two tasks performed in response to stimuli occurring in different modalities, overall time to completion of the sequence as well as the frequency of errors incurred in performance of the sequence could prove to vary markedly as a function of the order of tasks and modalities in the sequence.

Future Directions in the CMTS Paradigm

While this thesis has explored a range of issues that had been previously overlooked, or only ineffectively addressed in previous CMTS studies, the range of conceivably worthwhile manipulations has by no means been exhausted. In concluding, it will be worthwhile to consider some potentially fruitful avenues for further scientific exploration in the CMTS paradigm, starting with five pending theoretical questions from the current set of experiments. Beginning with Experiment 1, a central feature of the interpretation of the CMTS advantage and RSI effects in those data was the suggestion that a hierarchical switching mechanism may have acted to promote anticipatory task shifting on trials in which the modality of the imperative stimulus switched. This account makes at least two fairly straightforward predictions. First, Kleinsorge, Heuer and Schmidtke (2004) demonstrated that the nonmonotonicity of switch costs predicted by their hierarchical model was only observed when all switch types were possible within a single block of trials, and that combined switches were slower than unidimensional switches when only a single type of switch (judgment only, dimension only, judgment and dimension) occurred in each block. From this, it is a simple matter to predict that the CMTS advantage observed in Experiment 1 should only be replicable under conditions in which RR, TS, MS and CMTS trials are intermixed. Second, the argument that it was the short PCI in particular that was responsible for the effect being most pronounced in the 27/200 condition was predicated on the hypothesis that the effects of anticipatory switching were overcome during the CTI in the 27/653 condition. Under the assumption that anticipatory switching and preparatory reconfiguration are not inextricably intentionally linked, it should be possible to unmask clearer evidence of hierarchical switching in a short-PCI/long-CTI condition if preparation does not occur. Several attempts at doing so by varying RSI randomly within blocks (*c.f.*, Rogers and Monsell, 1995), were unsuccessful in discouraging preparation. An alternative approach that is worth considering would be to hold PCI/CTI constant across the experimental session (*c.f.*, Altmann, 2004).

The primary challenge in interpretation of the results of Experiment 2 stemmed from the failure to observe consistent CTI-related speedup in the Bimodal condition, even as the Switch Type profile in the Long-CTI Bimodal condition appeared to parallel the Switch Type profile in the Long-CTI Unimodal condition. A speculative hypothesis put forward to explain this phenomenon postulated that the combination of the Bimodal cues and long PCI could have jointly permitted the adoption of a cue-matching strategy for identifying the appropriate task in the Short-CTI Bimodal condition. An obvious prediction of this account is that if PCI were equated between the Short- and Long-CTI conditions this strategy should be unavailable to subjects and a more typical effect of preparation in the Bimodal condition should obtain if the effects of preparation on the processes that lead to switch costs are, in fact, equivalent in response to Unimodal and Bimodal cues.

The third and fourth issues both concern the results of Experiments 3A and 3B, in particular the role of the heightened executive control and working memory requirements associated with tracking the task sequence, a feature that was not shared by the experiment reported by Hunt and Kingstone (2004). To the extent that this was not the only methodological difference between the two studies, however, it remains possible that the distinctive results of Experiments 3A and 3B could be accounted for: a) by the predictable sequencing inherent to the alternative-runs sequence relative to the task-cuing methodology in Experiments 1 and 2, or b) by the tasks and stimuli used in Experiments 3A and 3B relative to those that were used by Hunt and Kingstone. Determination of which variables were critical should be possible by replicating Hunt and Kingstone's design without providing subjects any information about the appropriate task on each trial, or by replicating Experiments 3A and 3B while providing subjects with information that could be used to determine the appropriate task on each trial. Systematic manipulation of RSI length in an alternating-runs variant of the CMTS paradigm has also not yet been explored. In light of the extent to which the results of Experiments 3A and 3B deviated from, for example, results for the 27/200 condition of Experiment 1, it is possible that preparation might likewise differentially affect MS and CMTS trials in an (unsupported) alternating-runs design to an extent not seen in task-cuing designs.

The fifth issue concerns the characteristics of the stimulus sets subjects are presented with and the extent to which they overlap between the two modalities. The results of Experiments 4, 5A and 5B were interpreted as indicating that cross-modal stimulus repetitions could inflate CMTS cost estimates by selectively facilitating performance on stimulus-repeat MS trials. While direct application of this conclusion to the results of Experiments 1 and 2 would seem to indicate that left-side visual stimuli and left-side auditory stimuli were not perceived or processed as being any more equivalent than a visual "2" and an auditory "4", such a conclusion is still based on a conjecture that may or may not be valid. Achieving a more complete understanding of the effects of cross-modal stimulus overlap in spatial judgment paradigms will require refinement of the stimulus presentation protocols used in the service of such studies. One strategy that can be recommended would be to place participants in soundproofed, deadened rooms with speakers and LEDs placed in overlapping locations. As an example, positioning four speakers and four LEDs so that they comprised a 2 x 2 grid and having subjects switch between judgments of laterality and height would achieve at least three things: a) optimal cross-modal stimulus overlap in the spatial paradigm, b) inclusion of congruent as well as incongruent stimuli, and c) facilitation of direct comparisons to previous work by Meiran and colleagues (*e.g*., Meiran, 2000a) using analogous tasks under unimodal stimulus conditions. Further elaboration of the numerosity and positioning of stimulus-delivery devices could also permit more formal study of the perceptual/cognitive partitioning and implementation of spatial decision spaces around stimulus depth in a three dimensional environment, and the effects of such partitioning on task shifting performance.

A final commentary derives not from the results of the experiments presented in this thesis, but rather from a consideration of the basic theoretical question of whether and to what extent task set interference can survive switches of stimulus modality, and the somewhat more practical consideration of what other methodological means might be available for resolving the matter more conclusively. One approach that would seem to have great promise in this regard would be to adapt the paradigm used to study lag-2 repetition effects, from which numerous researchers have inferred the existence of BI (*e.g.*, Mayr and Keele, 2000). Considering the simplest case, if switching tasks cross-modally suffers from task-set interference of a sort that is similar to what is generally thought to be responsible for slowing on TS relative to RR trials, then switching from an Auditory Task *C*, to a Visual Task *B*, to an Auditory Task *A* (*AC* – *VB* – *AA*) should lead to faster RTs on the third trial (*AA*) than if the first of the three trials had also been an *AA*. Furthermore, if the switching of modalities makes no difference to the amount of task set interference then the same should be true for a comparison of two otherwise equivalent sequences that begin with either a Visual Task *C* (*VC*) or a Visual Task *A* (*VA*), respectively, and also for a comparison of two sequences that begin *AC* – *VB* and *AA* – *VB*, but end with *VA* (*i.e.*, BI from an earlier CMTS should also be evident on TS trials). Alternatively, if switching modalities bypasses interference then on the basis of results reported by Ruthruff et al. (2001), the opposite result might be expected, as recent performance of the current task set would be expected to facilitate performance on a third trial *AA* given that trial *n* – 2 had also featured *AA*. More adventurously, if BI operated in a modality-specific fashion, it might be possible to observe opposite effects of lag-2 task repetitions as a function of the modality sequence. For example, if BI from trial *n* – 1 only affected the task set on trial *n* – 2 in the modality from trial *n* – 2, then performing that same task on trial *n* in response to a stimulus in the opposite modality might facilitate performance, even as performing that same task in response to a stimulus in the same modality was made more difficult. More explicitly, we could expect an *AC* – *VB* – *AA* relative to *AA* – *VB* – *AA* advantage because of BI in the latter case, but a *VC* – *VB* – *AA* relative to *VA* – *VB* – *AA* disadvantage because of repetition facilitation in the latter case. On the other hand, if BI from trial *n* – 1 only affected the task set from trial *n* – 2 in the modality of trial *n* – 1, then we would only expect BI to be evident on TS trials and not on CMTS trials. Such evidence would not only be valuable for its relevance to the understanding of carryover of activation in CMTS performance, but would also contribute more generally to elucidation of the distinctions between BI and other task switching phenomena.

FIGURES AND TABLES

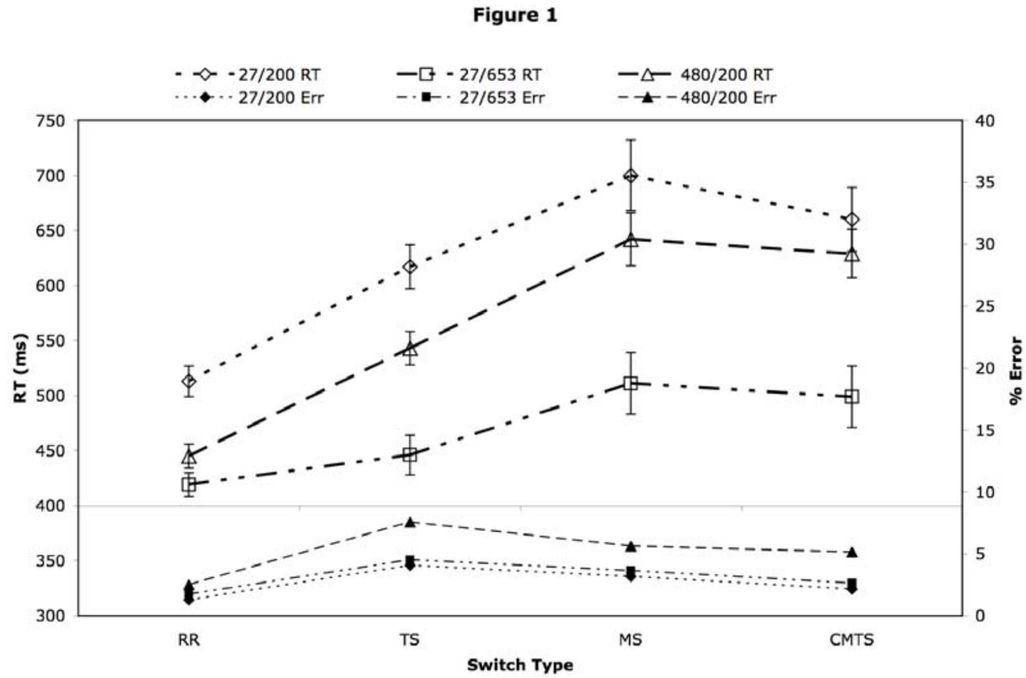

Figure 1. Experiment 1. Upper panel: mean RT as a function of Switch Type and RSI. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and RSI.

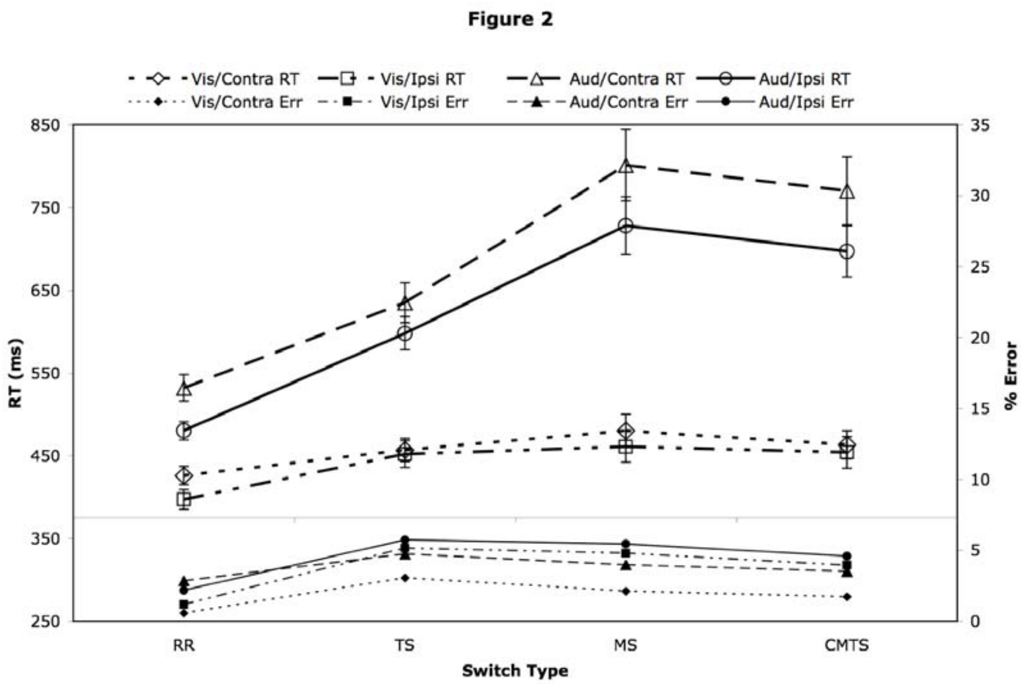

Figure 2. Experiment 1. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

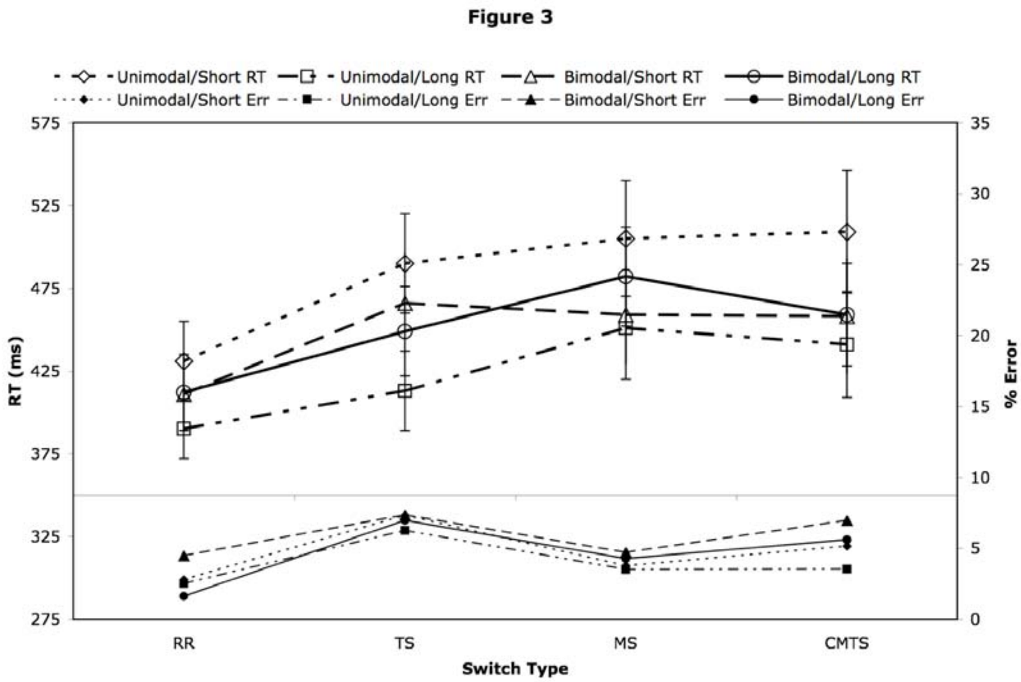

Figure 3. Experiment 2. Upper panel: mean RT as a function of Switch Type, Cue Type and RSI. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type, Cue Type and RSI.

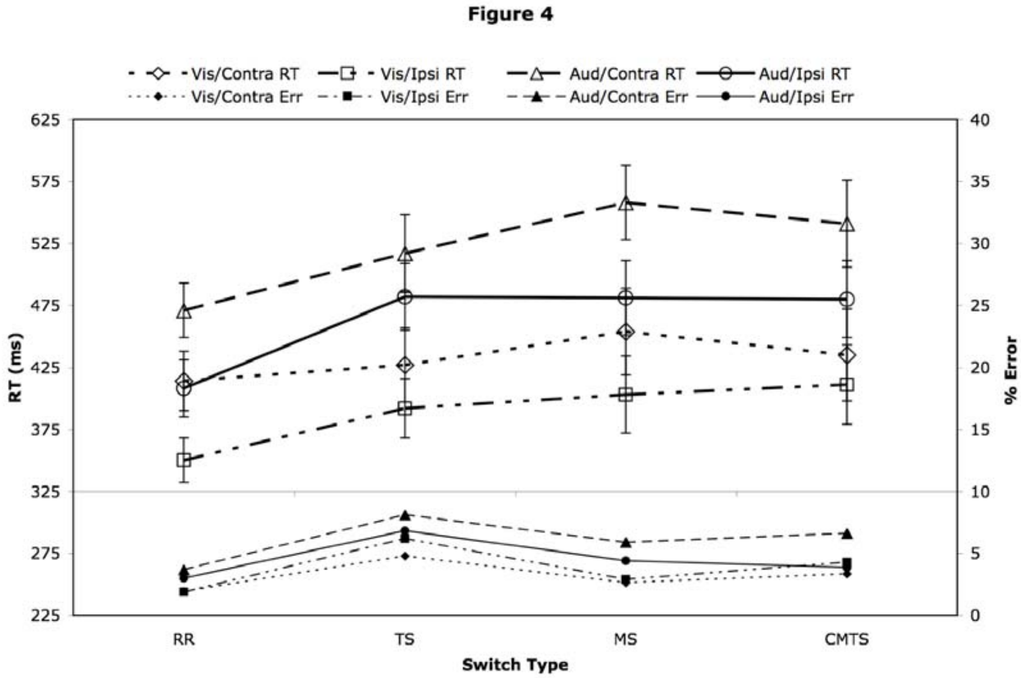

Figure 4. Experiment 2. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

Figure 5

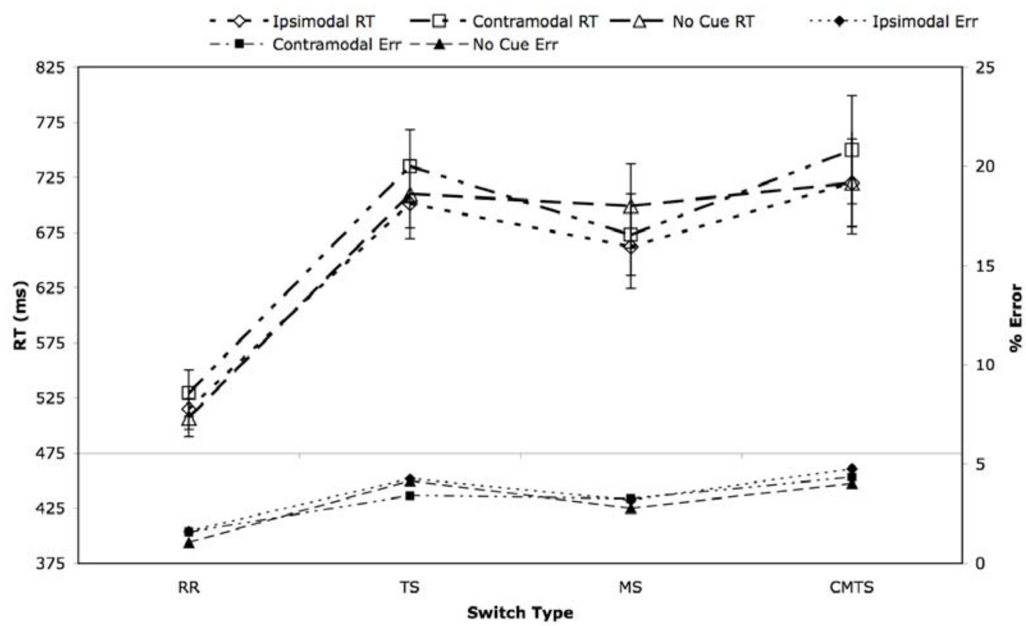

Figure 5. Experiment 3A. Upper panel: mean RT as a function of Switch Type and Cue Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Cue Type.

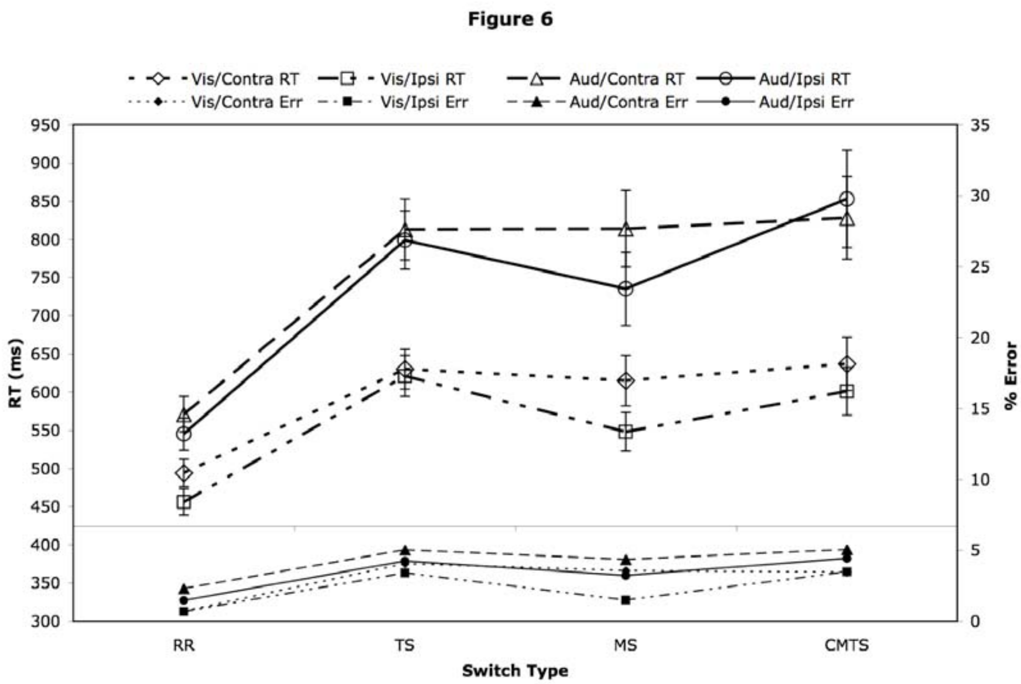

Figure 6 Experiment 3A. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

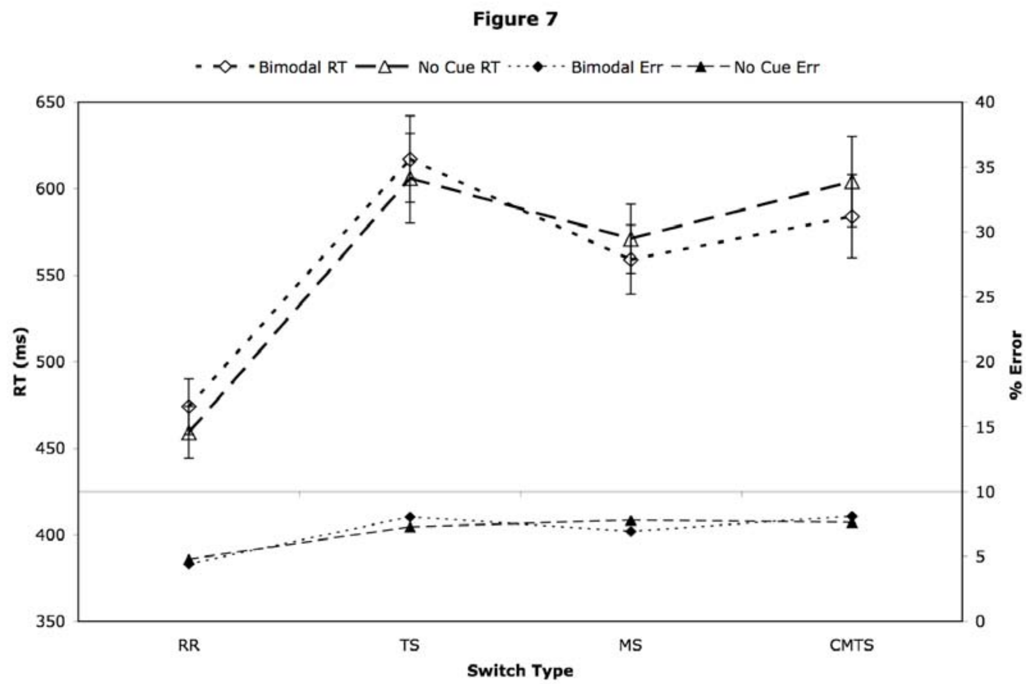

Figure 7. Experiment 3B. Upper panel: mean RT as a function of Switch Type and Cue Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Cue Type.

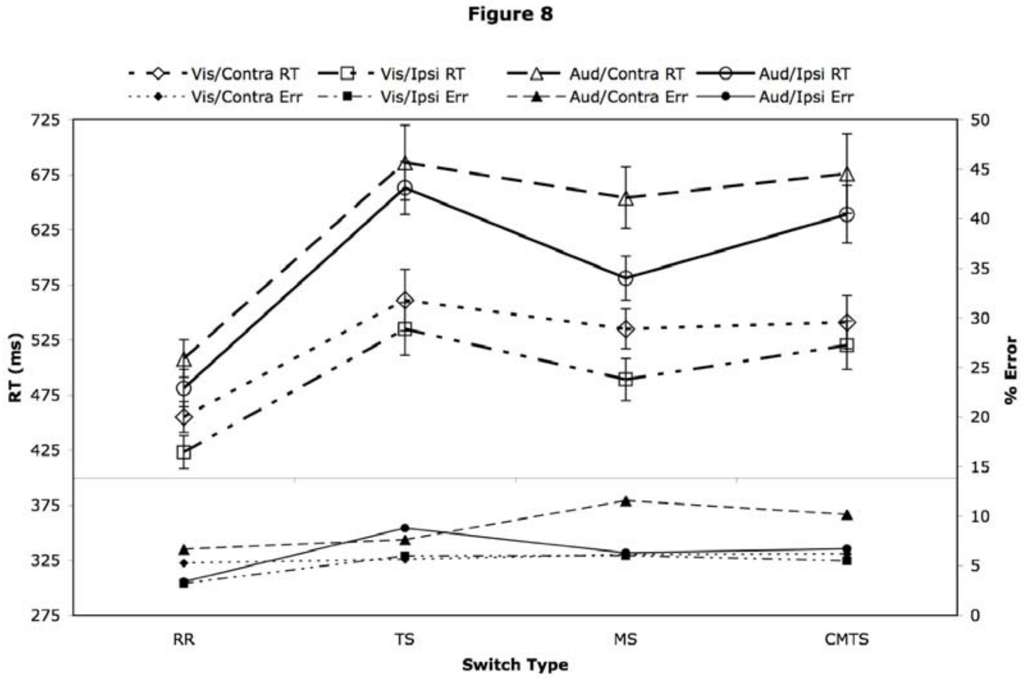

Figure 8. Experiment 3B. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

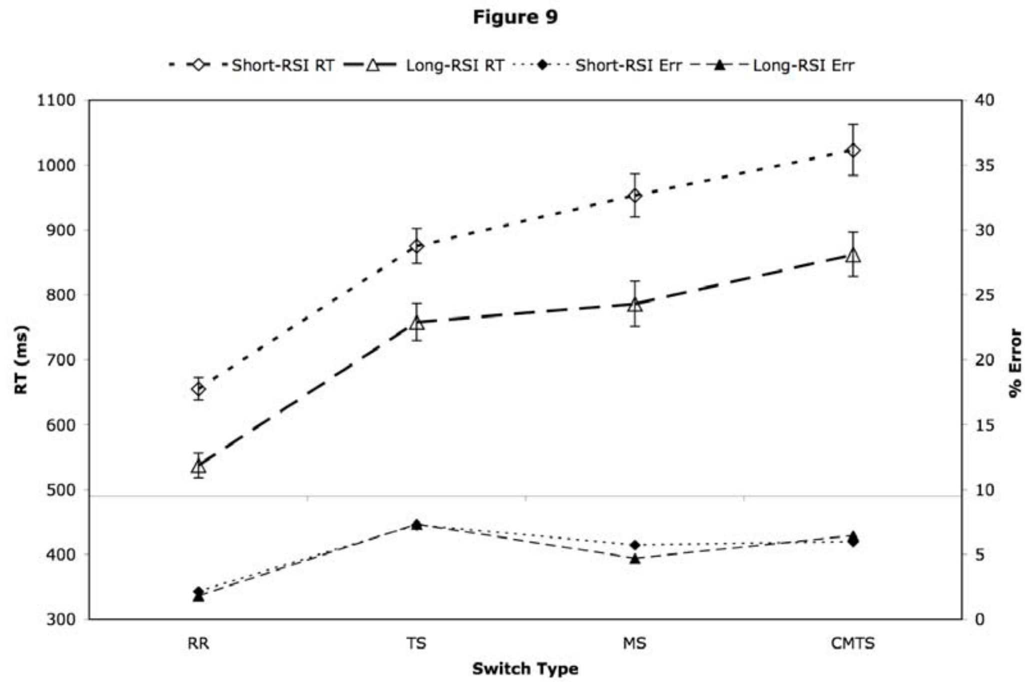

Figure 9. Experiment 4. Upper panel: mean RT as a function of Switch Type and RSI. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and RSI.

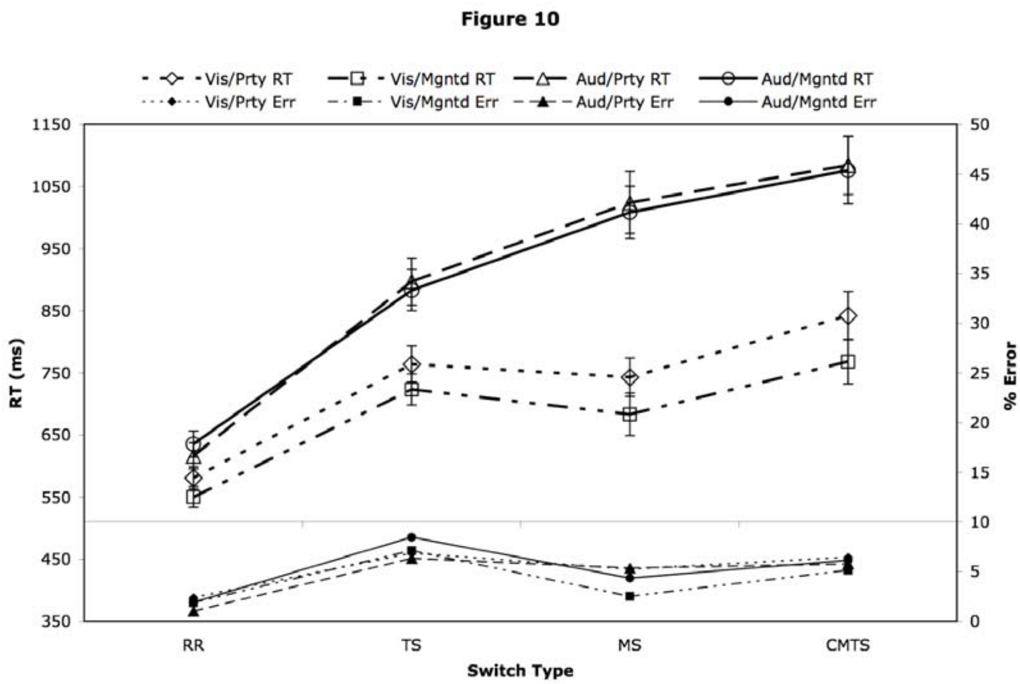

Figure 10. Experiment 4. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

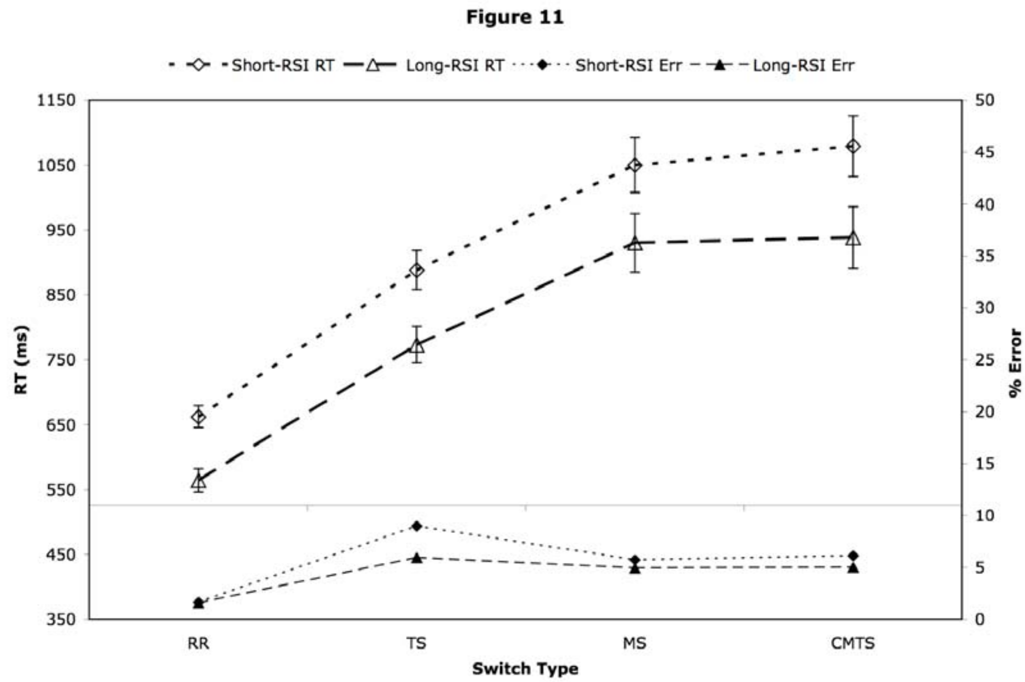

Figure 11. Experiment 5A. Upper panel: mean RT as a function of Switch Type and RSI. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and RSI.

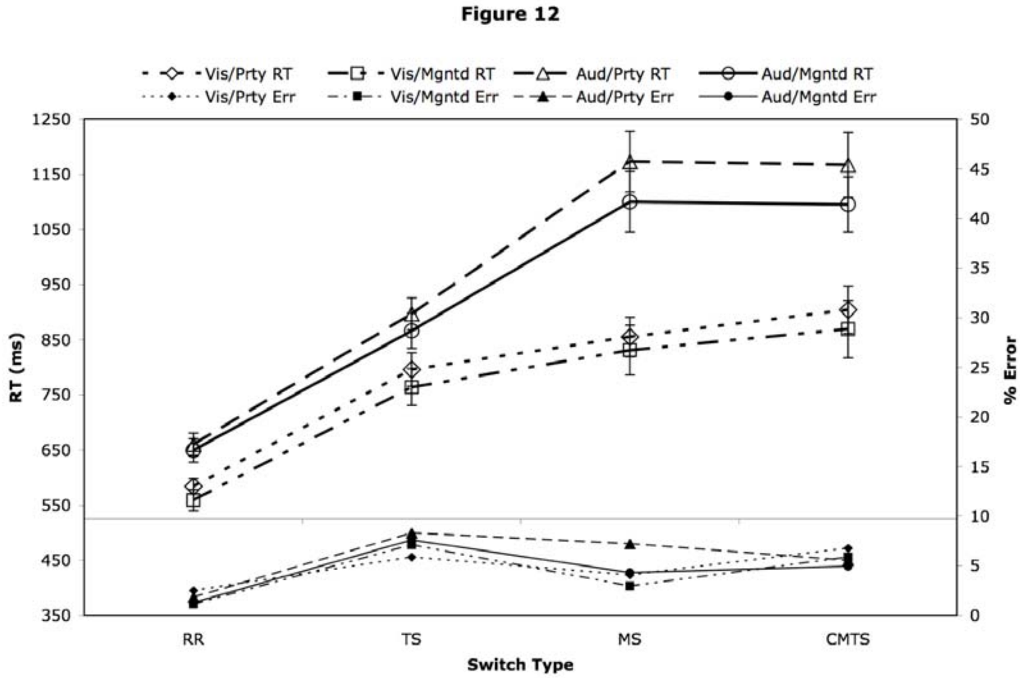

Figure 12. Experiment 5A. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

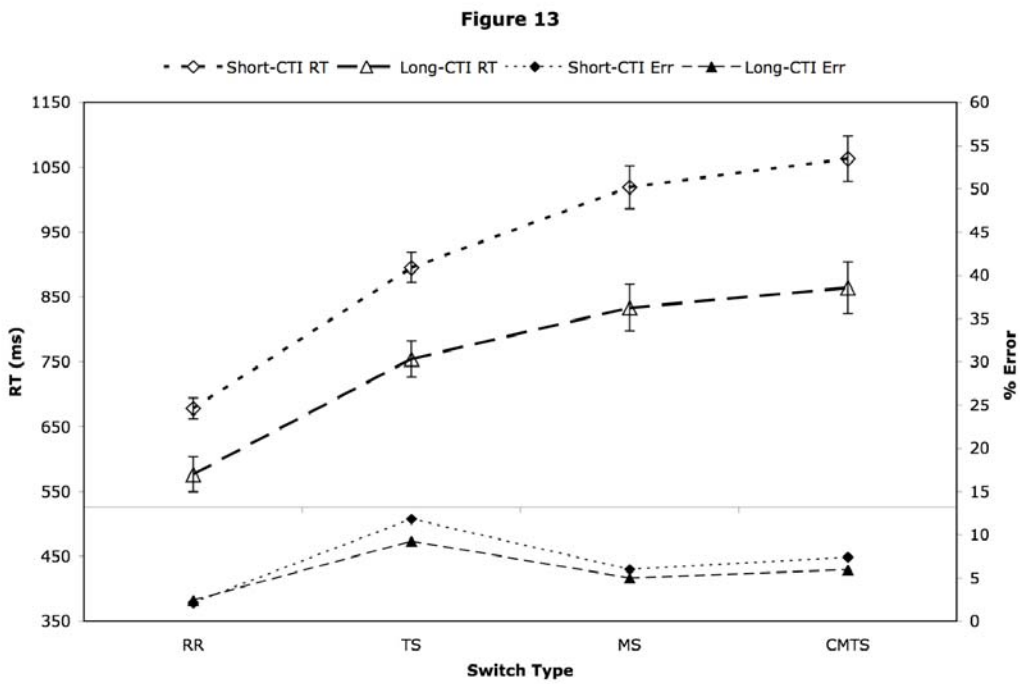

Figure 13. Experiment 5B. Upper panel: mean RT as a function of Switch Type and RSI. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and RSI.

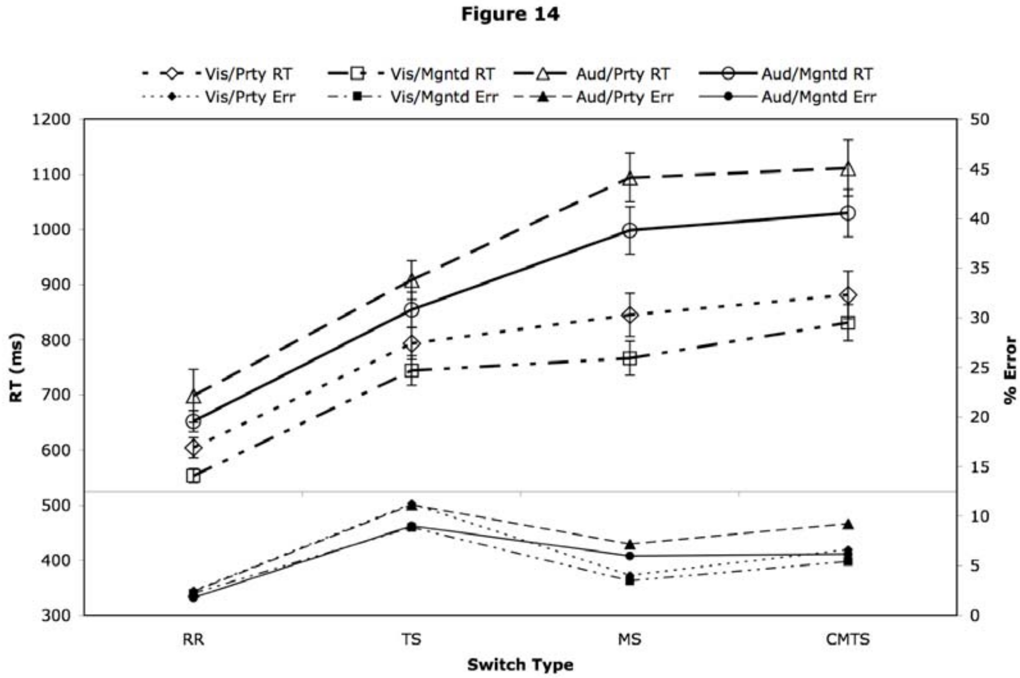

Figure 14. Experiment 5B. Upper panel: mean RT as a function of Switch Type and Trial Type. Error bars represent standard errors. Lower panel: median error % as a function of Switch Type and Trial Type.

Table 1

F Ratios and p-values for Preplanned Contrasts in Experiment 1

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
		$F(1, 23) =$			
Marginal	—	101.28*	72.41*	—	22.30*
RSI					
$\otimes\ \mathbf{c}_{cti}$	111.18*	92.12*	—	9.01*	3.97
$\otimes\ \mathbf{c}_{pci}$	83.97*	0.45	—	1.43	6.00*
Trial Type					
$\otimes\ \mathbf{t}_{mod}$	114.20*	52.00*	68.39*	—	4.85
$\otimes\ \mathbf{t}_{task}$	65.21*	8.37*	0.26	—	0.48
$\otimes\ \mathbf{t}_{mxt}$	7.00*	0.45	3.36	—	1.10
		Unadjusted $p <$			
Marginal	—	.001*	.001*	—	.001*
RSI					
$\otimes\ \mathbf{c}_{cti}$	.001*	.001*	—	.007*	.059
$\otimes\ \mathbf{c}_{pci}$	.001*	.509	—	.245	.023*
Trial Type					
$\otimes\ \mathbf{t}_{mod}$	.001*	.001*	.001*	—	.039
$\otimes\ \mathbf{t}_{task}$	.001*	.009*	.616	—	.494
$\otimes\ \mathbf{t}_{mxt}$	.015*	.509	.080	—	.306

*adjusted $p < .05$

Table 2

F Ratios and p-values for Preplanned Contrasts in Experiment 2

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
			$F(1, 23) =$		
Marginal	—	35.09*	38.45*	—	4.14
Cue & RSI					
$\otimes\ \mathbf{c}_{b\text{-}u}$	0.04	0.61	—	2.80	1.41
$\otimes\ \mathbf{c}_{s\text{-}l}$	11.88*	13.87*	—	21.83*	11.88*
$\otimes\ \mathbf{c}_{cxr}$	22.66*	3.14	—	1.45	0.84
Trial Type					
$\otimes\ \mathbf{t}_{mod}$	30.69*	29.17*	20.32*	—	0.20
$\otimes\ \mathbf{t}_{task}$	31.73*	4.15	0.00	—	4.82
$\otimes\ \mathbf{t}_{mxt}$	0.09	0.00	4.84	—	1.05
			Unadjusted $p <$		
Marginal	—	.001*	.001*	—	.054
Cue & RSI					
$\otimes\ \mathbf{c}_{b\text{-}u}$	.844	.442	—	.109	.247
$\otimes\ \mathbf{c}_{s\text{-}l}$	.003*	.002*	—	.001*	.003*
$\otimes\ \mathbf{c}_{cxr}$	.001*	.090	—	.241	.369
Trial Type					
$\otimes\ \mathbf{t}_{mod}$	.001*	.001*	.001*	—	.658
$\otimes\ \mathbf{t}_{task}$	.001*	.054	.997	—	.039
$\otimes\ \mathbf{t}_{mxt}$	.772	.971	.039	—	.318

**adjusted p* < .05

Table 3

F Ratios and p-values for Preplanned Contrasts in Experiment 3A

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
			$F(1, 23)$ =		
Marginal	—	136.25*	52.88*	—	9.03*
Cue					
$\otimes$ $\mathbf{c}_{c\text{-}i}$	10.99*	3.35	—	3.00	2.89
$\otimes$ $\mathbf{c}_{n\text{-}i}$	1.96	3.42	—	3.62	10.85*
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	101.86*	69.46*	34.38*	—	4.26
$\otimes$ $\mathbf{t}_{task}$	16.57*	5.49	20.58*	—	15.10*
$\otimes$ $\mathbf{t}_{mxt}$	1.49	0.59	0.71	—	14.46*
			Unadjusted p <		
Marginal	—	.001*	.001*	—	.007*
Cue					
$\otimes$ $\mathbf{c}_{c\text{-}i}$	.004*	.081	—	.097	.103
$\otimes$ $\mathbf{c}_{n\text{-}i}$	.175	.078	—	.070	.004*
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	.001*	.001*	.001*	—	.051
$\otimes$ $\mathbf{t}_{task}$	.001*	.029	.001*	—	.001*
$\otimes$ $\mathbf{t}_{mxt}$	.235	.451	.408	—	.001*

**adjusted* $p < .05$

Table 4

F Ratios and p-values for Preplanned Contrasts in Experiment 3B

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
			$F(1, 21) =$		
Marginal	—	82.17*	107.27*	—	4.14
Cue					
$\otimes$ $\mathbf{c}_{n\text{-}b}$	8.88*	0.28	—	8.40*	0.77
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	74.83*	33.14*	21.98*	—	4.73
$\otimes$ $\mathbf{t}_{task}$	29.34*	0.12	15.20*	—	5.25
$\otimes$ $\mathbf{t}_{mxt}$	0.40	0.00	5.34	—	0.47
			Unadjusted $p <$		
Marginal	—	.001*	.001*	—	.055
Cue					
$\otimes$ $\mathbf{c}_{n\text{-}b}$	.008*	.604	—	.009*	.391
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	.001*	.001*	.001*	—	.042
$\otimes$ $\mathbf{t}_{task}$	.001*	.736	.001*	—	.033
$\otimes$ $\mathbf{t}_{mxt}$	.533	.991	.032	—	.500

*adjusted $p < .05$

Table 5

F Ratios and p-values for Preplanned Contrasts in Experiment 4

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
			$F(1, 31) =$		
Marginal	—	193.72*	185.63*	—	23.43*
Cue					
$\otimes\ \mathbf{c}_{s\text{-}l}$	87.34*	0.00	—	8.64*	0.11
Trial Type					
$\otimes\ \mathbf{t}_{mod}$	22.54*	12.91*	55.67*	—	6.02
$\otimes\ \mathbf{t}_{task}$	0.38	2.18	4.95	—	0.04
$\otimes\ \mathbf{t}_{mxt}$	13.42*	1.12	0.57	—	1.13
			Unadjusted $p <$		
Marginal	—	.001*	.001*	—	.001*
Cue					
$\otimes\ \mathbf{c}_{s\text{-}l}$	.001*	.957	—	.007*	.743
Trial Type					
$\otimes\ \mathbf{t}_{mod}$	.001*	.002*	.001*	—	.021
$\otimes\ \mathbf{t}_{task}$	.543	.150	.034*	—	.835
$\otimes\ \mathbf{t}_{mxt}$	.001*	.299	.456	—	.297

*adjusted $p < .05$

Table 6

F Ratios and p-values for Preplanned Contrasts in Experiment 5A

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
			$F(1, 31) =$		
Marginal	—	124.00*	129.91*	—	2.16
Cue					
$\otimes$ $\mathbf{c}_{s\text{-}l}$	52.15*	1.27	—	0.07	1.05
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	47.12*	0.78	33.09*	—	5.24
$\otimes$ $\mathbf{t}_{task}$	3.91	0.95	2.54	—	0.03
$\otimes$ $\mathbf{t}_{mxt}$	0.70	0.22	3.58	—	0.13
			Unadjusted $p <$		
Marginal	—	.001*	.001*	—	.152
Cue					
$\otimes$ $\mathbf{c}_{s\text{-}l}$	.001*	.269	—	.794	.314
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	.001*	.385	.001*	—	.030
$\otimes$ $\mathbf{t}_{task}$	.058	.339	.122	—	.866
$\otimes$ $\mathbf{t}_{mxt}$	.411	.642	.069	—	.720

*adjusted $p < .05$

Table 7

F Ratios and p-values for Preplanned Contrasts in Experiment 5B

Domain	S_R	S_{RTM}			S_{CMTS}
	$\mathbf{s}_r$	$\mathbf{s}_{ts}$	$\mathbf{s}_{ms}$	$\mathbf{s}_{mxt}$	$\mathbf{s}_{cmts}$
			$F(1, 31) =$		
Marginal	—	164.11*	176.47*	—	10.15*
Cue					
$\otimes$ $\mathbf{c}_{s\text{-}l}$	26.06*	2.24	—	4.59	0.50
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	11.04*	0.64	27.54*	—	2.37
$\otimes$ $\mathbf{t}_{task}$	4.85	0.01	3.31	—	0.89
$\otimes$ $\mathbf{t}_{mxt}$	0.03	0.05	0.31	—	0.13
			Unadjusted $p <$		
Marginal	—	.001*	.001*	—	.004*
Cue					
$\otimes$ $\mathbf{c}_{s\text{-}l}$	.001*	.146	—	.041	.486
Trial Type					
$\otimes$ $\mathbf{t}_{mod}$	.003*	.430	.001*	—	.135
$\otimes$ $\mathbf{t}_{task}$	.036	.909	.079	—	.354
$\otimes$ $\mathbf{t}_{mxt}$	.876	.834	.584	—	.723

*adjusted $p < .05$

APPENDIX

Rather than analyzing results using multifactor repeated measures ANOVAs, as is typical in the literature, statistical testing was carried out primarily through the specification of sets of contrasts, with variance estimates for each contrast calculated independently. There were two primary reasons this strategy was chosen: (a) violations of the sphericity assumption were common, and in some cases severe, and (b) the standard conventions by which factors and interactions would have been specified in an ANOVA provided in some cases a relatively poor match to the goals of the analyses. The first priority in formalizing the approach centered on the manner in which the switch cost profile would be evaluated. To this end, three nonorthogonal hypothesis subspaces were defined for the four levels of the Switch Type factor (RR, TS, MS and CMTS), and a minimally sufficient set of linear weighting vectors was selected for each subspace to directly address the empirical questions of primary interest, subject to constraint that the chosen vectors comprised a basis for the subspace of interest. Ordering the Switch Types as above, the vector of weights for the first subspace, S_{RR}, was $\mathbf{s}_r = (1, 0, 0, 0)'$. This was defined merely to permit evaluation of the simple (rather than marginal) effects of secondary factors (*e.g.*, RSI, Trial Type) in the designs on non-switch, ostensibly baseline RTs. The second subspace, S_{RTM}, was defined to permit evaluation of the slowing that occurred on TS and MS trials relative to RR trials, and the extent to which the former two were differentially impacted by preparatory factors. Three sets of weights were defined for this purpose: $\mathbf{s}_{ts} = (-1, 1, 0, 0)'$, $\mathbf{s}_{ms} = (-1, 0, 1, 0)'$, and $\mathbf{s}_{m\text{-}t} = (0, -1, 1, 0)'$. As these three do not constitute a linearly independent set, only two ($\mathbf{s}_{ts}$ in addition to either $\mathbf{s}_{ms}$ or $\mathbf{s}_{m\text{-}t}$) were selected for any given application. The third subspace, S_{CMTS}, was spanned by a single vector, $\mathbf{s}_{c\text{-}m} = (0, 0, -1, 1)'$, and was focused specifically on comparisons between CMTS and MS RTs. Marginal effects (averaged across RSI, Task, Modality, etc.), in addition to interactions, were evaluated for both S_{RTM} and S_{CMTS}. The reader may note that this protocol is suboptimal for addressing the underadditivity of switch costs, per se. This was deemed acceptable given that underadditivity has now been statistically demonstrated in several published experiments; an absence of underadditivity would be the more surprising result, and maximally precise estimation of the effects of the various experimental manipulations on CMTS costs, combined with a clear summary of the inferential context, was considered to be of greater importance.

In order to evaluate the manner in which the switch cost profile was affected by other independent variables, minimally sufficient sets of contrast vectors spanning the hypothesis spaces associated with these other factors were likewise specified, and appropriate interaction contrasts were constructed. The procedure is illustrated for the case of the variables Task (taking the tasks used in Experiment 1) and Modality, which were treated in each experiment as comprising a four-level factor, Trial Type. Given the ordering Visual/Ipsilateral, Visual/Contralateral, Auditory/Ipsilateral, Auditory/Contralateral, the three Trial Type contrast vectors are given by: $\mathbf{t}_{mod}$ = (-1, -1, 1, 1)', $\mathbf{t}_{task}$ = (-1, 1, -1, 1)', and $\mathbf{t}_{mxt}$ = (1, -1, -1, 1)', and evaluate the difference between Visual and Auditory RTs, the difference between RTs for the Ipsilateral and Contralateral tasks, and the RT interaction between Modality and Task, respectively. The contrast vectors used to test the interaction between Trial Type and one of the hypothesis subspaces defined for the switch cost profile are then obtained in a straightforward fashion. For example, given an appropriate ordering and multiplicity of the factor levels (*e.g.*, RR/Visual/Ipsilateral, RR/Visual/Contralateral, …, TS/Visual/Ipsilateral, …, CMTS/Auditory/Contralateral), the contrasts evaluating the interaction of Trial Type and S_{RTM} could be obtained as $\mathbf{s}_{ts} \otimes \mathbf{t}_{mod}$, $\mathbf{s}_{ts} \otimes \mathbf{t}_{task}$, $\mathbf{s}_{ts} \otimes \mathbf{t}_{mxt}$, $\mathbf{s}_{ms} \otimes \mathbf{t}_{mod}$, $\mathbf{s}_{ms} \otimes \mathbf{t}_{task}$, and $\mathbf{s}_{ms} \otimes \mathbf{t}_{mxt}$, where $\otimes$ indicates the Kronecker product. This set of six contrasts would then be classified as a domain, the significance of which for Type I error control is discussed next. In general, $\mathbf{s}_{ts}$ and $\mathbf{s}_{ms}$ were selected to represent S_{RTM} in all preplanned contrasts, with the exception of interactions involving the duration and partitioning of the preparation interval and the types of cues presented. In these cases $\mathbf{s}_{ms}$ was replaced by $\mathbf{s}_{m\text{-}t}$ to facilitate detection of any differential sensitivity to preparatory processing on MS relative to TS trials.

Type I error probabilities for *a priori* contrasts, such as those defined by the $\mathbf{s}_i$ and $\mathbf{t}_i$ vectors, were controlled at the domain level. This was accomplished by means of the Holm stepwise testing procedure, which was implemented as follows. Given a set of *m* contrast vectors

$\mathbf{v}_i$, comprising a domain, a vector of parameter estimates **b** such that $\mathbf{v}_i'\mathbf{b}$ is an estimate of $\mathbf{v}_i'\boldsymbol{\beta}$, and a chosen level of significance α, begin by testing the intersection null hypothesis H_0: $\mathbf{v}_1'\boldsymbol{\beta} = 0 \cap \mathbf{v}_2'\boldsymbol{\beta} = 0 \cap \ldots \cap \mathbf{v}_m'\boldsymbol{\beta} = 0$, where H_0 is rejected if the smallest obtained p-value from the tests on the $\mathbf{v}_i'\mathbf{b}$ is less than α/m. If this criterion is not met, H_0 is retained and testing is stopped. If H_0 is rejected, the contrast that generated the smallest p-value is declared statistically significant and a second intersection null hypothesis is formed from the remaining $m - 1$ contrasts. This second null hypothesis is then rejected if the smallest p-value among these $m - 1$ contrasts is less than $\alpha/(m - 1)$. The process continues until all contrasts have been declared significant, or until insufficient evidence is obtained to reject one of the intersection nulls, whichever comes first. As an example, for the interaction of Trial Type and S_{RTM}, the initial intersection null would be formulated as H_0: $(\mathbf{s}_{ts} \otimes \mathbf{t}_{mod})'\boldsymbol{\beta} = 0 \cap (\mathbf{s}_{ts} \otimes \mathbf{t}_{task})'\boldsymbol{\beta} = 0 \cap \ldots \cap (\mathbf{s}_{ms} \otimes \mathbf{t}_{mxt})'\boldsymbol{\beta} = 0$, and the lowest obtained p-value would be compared to $\alpha/6$.

For all experiments, the level of significance for a given domain was set to $\alpha = .05$, and reported p-values for preplanned contrasts are unadjusted. A variant of the Scheffe procedure was employed to maintain α control in *post hoc* testing. Because pooled error terms were not used, critical values for *post hoc* contrasts were determined as $F_{crit} = (df_{effect})F[1 - \alpha; df_{effect}, n - 1]$, where df_{effect} was determined according to the smallest domain for which the particular contrast being considered was defined, and n refers to the number of subjects in the experiment. The notation adopted to indicate the use of the Scheffe procedure was, $F_S(df_{effect}, n - 1) = F_{observed}$, $p_S < p_{adjusted}$, where $F_{observed}$ refers to the value of the calculated F ratio, and $p_{adjusted}$ refers to the p-value obtained by comparing $F_{observed} / df_{effect}$ to an F distribution with numerator degrees of freedom df_{effect}, and denominator degrees of freedom $n - 1$. Because of the conservative nature of the Scheffe procedure the conventional significance level $\alpha = .05$ was not strictly adhered to. Confidence intervals for contrasts are also occasionally reported, and were in all cases calculated using an unadjusted $\alpha = .05$.

The structure and number of levels for the factors employed in this thesis, excluding Trial Type for which only the identities of the tasks changed, varied across experiments. In the remainder of this appendix additional details concerning the manner in which domains and *a priori* contrast vectors were defined are given for each experiment.

Experiment 1

Three RSI levels were utilized in Experiment 1. Ordering these levels as 27/200, 27/653, 480/200, and defining a single hypothesis subspace, the contrast vectors utilized for domain-level testing were $\mathbf{c}_{cti}$ = (1, -2, 1)' and $\mathbf{c}_{pci}$ = (1, 0, -1)', evaluating the effects of lengthening the CTI irrespective of the length of the PCI, and of lengthening the PCI given that the CTI was short, respectively.

Experiment 2

Two different partitions of the RSI were utilized in Experiment 2, Short-CTI and Long-CTI. These were factorially crossed with two levels of Cue Type, Unimodal and Bimodal. A single hypothesis subspace was defined from these four conditions for domain-level testing. Given the ordering: Unimodal/Short-CTI, Unimodal/Long-CTI, Bimodal/Short-CTI, Bimodal/Long-CTI, the contrast vectors representing the subspace were $\mathbf{c}_{b\text{-}u}$ = (-1, -1, 1, 1)', $\mathbf{c}_{s\text{-}l}$ = (1, -1, 1, -1)', and $\mathbf{c}_{cxr}$ = (1, -1, -1, 1)', representing the effects of Cue Type averaged across RSI, RSI averaged across Cue Type, and the Cue Type by RSI interaction, respectively.

Experiment 3A

Three different Cue Type conditions were utilized in Experiment 3A: Ipsimodal, Contramodal, and No-Cue. A single hypothesis subspace was defined for hypothesis testing among these three conditions. Given the ordering above, the contrast vectors chosen to span this subspace were $\mathbf{c}_{c\text{-}i}$ = (-1, 1, 0)', and $\mathbf{c}_{n\text{-}i}$ = (-1, 0, 1)', evaluating the extent to which the Contramodal and No-Cue conditions, respectively, differed from the Ipsimodal condition.

Experiment 3B

Two different Cue Type conditions were utilized in Experiment 3B: Bimodal and No-Cue. Taking that same ordering, the hypothesis subspace for comparisons between these two conditions was spanned by the contrast vector $\mathbf{c}_{n\text{-}b}$ = (-1, 1)'.

Experiment 4 and 5A

The same two RSI conditions were employed in Experiment 4 and 5A, Short-RSI and Long-RSI. Given that same ordering, the hypothesis subspace for comparisons between these two conditions was defined by the contrast vector $\mathbf{c}_{s\text{-}l}$ = (1, -1)'.

Experiment 5B

Two RSI conditions were employed in Experiment 4, Short-CTI and Long-CTI. Given that same ordering, the hypothesis subspace for comparisons between these two conditions was defined by the contrast vector $\mathbf{c}_{s\text{-}l}$ = (1, -1)'.

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