Computational and Empirical Studies of Task Switching

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Abstract

This thesis is concerned with the study of executive function using the task switching paradigm. It is argued that theoretical accounts of task switching fall into two categories: a) those that attribute the reaction time cost of task switching to interference caused by previous performance of a different task (‘task carryover’ accounts) and b) those that attribute the ‘switch cost’ to the duration of one or more executive control processes that are specific to task switching (‘extra process’ accounts). These accounts have different implications for the study of executive function with the task switching paradigm: according to extra process accounts, control processes involved in task switching may be investigated directly, whereas according to task carryover accounts there can only be an indirect relationship between the operation of control processes and the duration of switch costs. A parallel distributed processing (PDP) model is presented, implementing a version of the task carryover account. This model simulates reaction times when subjects switch between word reading and colour naming in response to Stroop stimuli, providing a good fit to a large body of empirical data. Further simulations with random parameter settings show that the model simulates this data as a result of its architecture and general processing principles, rather than the specific parameter settings that were chosen. Six empirical studies are then reported, investigating task switching performance when subjects simultaneously perform a working memory task. The model is used to relate patterns of switch costs in these experiments to the underlying control processes and strategies that may be employed by subjects. It is concluded that task switching may provide a suitable methodology for the study of executive function, but that the relationship between switch costs and executive function is complex and indirect.
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Chapter 1

‘Executive function’: A review of theoretical and empirical investigations

1.1 Introduction

The past few years have seen increasing interest in the ways in which cognitive processes are controlled and organised so as to achieve various goals (e.g., Baddeley, 1996, 1998; Cooper, 2002; Duncan, Johnson, Swales, & Freer, 1997; Miyake, Friedman, Emerson, Witzki, Howarter, & Wager, 2000; Monsell, 1996; Monsell & Driver, 2000; Rabbitt, 1997a; Shallice & Burgess, 1996). It is frequently remarked that the processes responsible for cognitive control (i.e. ‘control processes’) are poorly understood. Indeed, some authors have gone so far as to refer to this domain as an ‘embarrassing zone of almost total ignorance’ (Monsell, 1996, p. 93). Yet, the control of cognitive processes, while perhaps somewhat neglected in the past, has not been ignored (e.g. Atkinson & Shiffrin, 1968; James, 1890; Logan, 1979, 1980, 1985; Luria, 1966; Milner, 1963; Norman, 1981; Norman & Shallice, 1980, 1986; Posner & Snyder, 1975; Reason, 1979; Rylander, 1939; Shiffrin & Schneider, 1977; Spelke, Hirst, & Neisser, 1976). This review sets itself three objectives: a) to provide a brief summary of what has been learned from empirical studies of control processes, up to the present time, and the theoretical positions that have been put forward to account for these data; b) to discuss the reasons for the gloomy assessment by many recent authors of the current state of knowledge; and c) to discuss whether this pessimism is justified and, if so, what steps might be taken to improve this situation.

1.2 Hierarchical organisation of cognitive processes
Before discussing any empirical data, it may be valuable to examine some of the assumptions that have been made in much of this research. Foremost among these is that cognitive processes are hierarchically organised, with those relating to the processing of specific perceptual inputs and the generation of specific motor outputs at the bottom, and abstract goals and intentions at the top (see e.g. Grafman, 1995; Norman & Shallice, 1986; Tyrell, 1993; for a contrasting view see Botvinick & Plaut, submitted). An important consequence of this hierarchical view of cognitive processes (or, perhaps, the intuition that motivates it) is that higher-level processes have a greater generality than lower-level processes: higher-level processes may apply to a wide range of different situations, whereas lower-level processes are tied to more specific perceptual and motor computations. Thus, insofar as higher level control processes are separable from lower level processes, they should potentially be involved in a wider range of perceptual and motor circumstances.

This view fits with a conception of a fundamental process in cognitive control being the modulation of lower-level processes by those at a higher level. Such modulation may take place at different levels of the hierarchy: a higher-level goal to obtain a drink may activate a lower-level goal to pour a glass of water; this goal in turn may activate lower-level perceptual and motor processes involved in generating the appropriate behaviour. The ‘top-down’ chain of command in such examples has given rise to the metaphor of different levels of organisation in a large company, with lower-level processes being controlled by ‘managerial’ knowledge (Grafman 1989, 1995) or a ‘central executive’ (Baddeley & Hitch, 1974). Hence, the domain of cognitive control has often been labelled ‘executive function’. The term will be used here, interchangeably with ‘cognitive control’, or ‘control processes’, although use of the phrase ‘executive function’ does not necessarily entail the assumptions sketched above.
One consequence of the view that cognitive control entails the modulation of lower-level processes by higher-level ones is that it will not be possible to study putative higher-level control processes independently of the lower-level processes that they modulate. Thus, it will only be possible to study control processes indirectly (Burgess, 1997). This might seem to be a particularly acute problem for neuropsychological methodologies: if a patient fails a particular test, supposedly sensitive to control processes, how can one be sure that the failure results from impaired higher-level processes, rather than the lower-level processes that are, of necessity, also involved in the task? Neuropsychological patients typically do not have small, focal lesions, so several cognitive processes may potentially be affected. Yet neuropsychological work has historically been the chief source of evidence for research into control processes. The literature contains many examples of patients who, typically following lesions involving the frontal lobes, are able to perform simple tasks adequately, but have difficulty organising their behaviour in order to achieve specific goals.

The discussion of this research will be restricted, for the present, to two observations. First, such patients are often thought to suffer from an excessive behavioural rigidity: they will tend to perseverate (Sandson & Albert, 1984), producing behaviour that was previously appropriate but is not any more. Secondly, and in apparent contrast, such patients may also show excessive distractibility, resulting in some rare cases in 'utilisation behaviour' (Lhermitte, 1983; Shallice, Burgess, Schon, & Baxter, 1989), in which the patient will frequently produce behaviours appropriate for the objects that surround them (e.g. dealing from a pack of cards placed in front of them) even when this behaviour is not appropriate for the current context (a neuropsychological examination, where no reference has been made to the cards) and the patient expresses no intention to act in this way. Similar patterns of behaviour may be observed in normal subjects. Studies of action slips are of particular interest,
typically involving diary studies where normal subjects record any action that was not as planned, over a period of time (Norman, 1981; Reason, 1979, 1984). Errors resulting from excessive behavioural rigidity may occur, where a previously appropriate behaviour is repeated in inappropriate circumstances (e.g. following a familiar route despite intending to take a different one). In addition, errors resulting from excessive distractibility are frequently reported, especially 'capture errors' (Norman, 1981) where one behaves in a way that is often appropriate in a certain situation but is not presently. Such errors result from behaviour being inappropriately triggered by the environment, a well known example being James' (1890) tale of a man going to his bedroom to dress for dinner and finding himself, moments later, in bed.

1.3 Theoretical frameworks

The evidence referred to above suggests that inappropriate behaviour may be produced as a result of failures at the level of higher-level control processes, rather than any problems in the production of lower-level behavioural routines. Before discussing this evidence in detail, some of the theoretical frameworks to describe this evidence will be introduced. The most influential of these was originally presented by Norman and Shallice (1980, 1986; see also Shallice, 1982, 1988; Shallice & Burgess, 1996; Cooper & Shallice, 2000). According to this model, behaviour is controlled by a hierarchically organised set of 'schemas', each of which is associated with a level of activation. These schemas select and coordinate the elementary processes involved in simple cognitive tasks. Activation of higher-level 'source' schemas (such as the schema for driving) leads to some activation of lower-level schemas (such as those for braking, steering, indicating, changing gear and so on), so that they can be selected when the appropriate 'trigger conditions' arise. Lower-level schemas activate specialised input-output pathways: for example, a schema for reading aloud might link
together a visual word-form input system and a spoken output system. In addition, schemas may control lower-level attentional processes (e.g. those involved in orienting visual attention in space). It is assumed that schemas have mutually inhibitory connections when they require overlapping effector resources. For further details of the organisation and processing in schema networks, see Cooper and Shallice (2000).

A crucial feature of the Norman-Shallice model is that there are two distinct ways in which schemas may become selected. They can be triggered by environmental cues, following which a process called ‘contention scheduling’ selects the most active schema for the control of action. This may depend both on environmental events and the activation levels of lower-level schemas, dependent on which source schemas have been activated. The process of contention scheduling is appropriate in routine circumstances where environmental cues are sufficient to determine behavioural responses. However, Norman and Shallice (1986) discuss five circumstances in which the operation of the contention scheduling system alone is not appropriate:

1) those that involve planning / decision making
2) those that involve error correction / trouble shooting
3) situations where responses are not well-learned or contain novel sequences of actions
4) dangerous or technically difficult situations
5) situations that require the overcoming of a strong habitual response or resisting temptation

In these circumstances, reliance on learned associations between environmental cues and behavioural responses is either not possible (e.g. in cases of novelty) or liable to lead to inappropriate behaviour (e.g. in cases where a prepotent
response is not appropriate). Norman and Shallice propose that in such circumstances, the activation level of schemas may be modulated by a 'Supervisory Attentional System' (SAS), which operates in a way that is qualitatively different from the contention scheduling system. The SAS is characterised as having 'access to a representation of the environment and of the organism's intentions and cognitive capacities' (Shallice, 1988, p. 335), and is anatomically associated with the frontal lobes. Importantly, it does not directly generate behaviour itself. Rather, it modulates the contention scheduling system by activating or inhibiting pre-existing schemas. Shallice and Burgess (1996) also suggest that this system is able to control behaviour by creating temporary schemas to deal with novel situations.

An advantage of this model is that it can provide a unified account of the seemingly conflicting consequences of impaired cognitive control: excessive rigidity and, by contrast, excessive distractibility. In cases where environmental triggers lead to the selection of one schema, but where an alternative schema must now be selected, the supervisory system is required to activate the now-appropriate schema. Damage to this system will therefore make it more likely that the previously relevant schema, triggered by environmental events, will continue to be selected, leading to perseveration (i.e. excessive behavioural rigidity). In other circumstances, where the task in hand is not strongly cued by environmental events, a failure of supervisory input to the contention scheduling system may lead to the triggering of inappropriate behaviours by salient objects in the environment, leading to excessive distractibility. Thus, a failure of the supervisory system could explain both patterns of behaviour. Consistent with this view, studies of action slips in normal subjects have shown that they tend to occur when subjects rate themselves as being distracted (Reason, 1984), at which times the supervisory system may be operating less reliably.
The Norman-Shallice model has similarities with the tripartite model of working memory presented by Baddeley and Hitch (1974), consisting of a 'central executive' and a pair of 'slave' systems (the phonological loop and visuo-spatial sketchpad). Baddeley (1986) suggests that the central executive could be seen as equivalent to Norman and Shallice's (1986) supervisory system. These models draw on the earlier work of Atkinson and Shiffrin (1968), who distinguished between relatively permanent cognitive structures, such as short- and long-term memory, and control processes which harness those fixed structures in order to attain specific goals. For example, Atkinson and Shiffrin (1968) propose that when a piece of information needs to be maintained in short term memory for a limited period of time (e.g. a telephone number), the control process of rehearsal can operate on the short term store. Likewise, Atkinson and Shiffrin (1968) give the example of generating visual imagery related to the information that needs to be remembered as a control process that can operate on long term memory in order to store information more effectively.

A similar approach has been taken within cognitive psychology with the distinction between 'automatic' and 'controlled' processes (e.g. LaBerge & Samuels, 1974; Posner & Snyder, 1975; Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977). These models typically characterise controlled processes as being deliberate, slow, serial, effortful, and capacity-limited. Thus, they are consistent with dependence on a centralised control system such as the SAS. Automatic processes are fast, may take place without intention, and are not severely limited in capacity. In this way, they are compatible with the operation of a decentralised control system such as contention scheduling. Theoretical models of the transition from controlled to automatic processing have emphasised the importance of consistent mapping between stimulus and response (e.g. Schneider & Shiffrin, 1977), in common with models of executive
function (e.g. Norman & Shallice, 1986) which emphasise the role of higher-level processes in non-routine behaviour.

Other theoretical accounts have similarly stressed the importance of higher-level cognitive processes, without putting so much emphasis on their role in novel situations. For example, Meyer and Kieras (1997) have attempted to explicitly characterise the role of higher-level processes in various cognitive tasks with the 'EPIC' model. In this model, executive processes are characterised as condition-action rules in a production system architecture, which insert or delete task goals in the working memory system, and monitor and schedule task processes. The precise nature of these executive processes may be quite specific, and tailored for the requirements of an individual well-learned task. The involvement of higher-level processes in familiar situations has also been remarked upon by Grafman (1989, 1995, 2002), who emphasises the knowledge stored, at various degrees of abstraction, in 'structured event complexes'. According to this account, such knowledge is required for the appropriate control of behaviour in different types of situation (e.g. visiting a restaurant).

How might one go about evaluating these models? In the Norman-Shallice model, the contention scheduling mechanism, and the schema hierarchy, are specified in some detail. Indeed, Cooper and Shallice (2000) were able to implement this system explicitly in a computational model (based on the interactive activation framework; see McClelland & Rumelhart, 1981), and showed that it is successfully able to simulate the production of goal-directed sequential action, as well as the characteristic patterns of errors that can be made by normal subjects and by patients. However, the supervisory system is underspecified, making detailed evaluation of the Norman-Shallice model difficult. How does the supervisory system know when the contention scheduling system cannot produce appropriate behaviour by itself, and requires
additional activation or inhibition of schemas? How does it know which schema(s) to activate/inhibit? The use of mentalistic language here ('how does it know...') is revealing. At present, the supervisory system appears to have the properties of a processing homunculus. That is, the abilities ascribed to the supervisory system, such as problem solving or planning are abilities that should properly be ascribed to persons; the high-level cognitive abilities of persons that stand in need of explanation are simply ascribed to a component of the theory. Thus, the ability of the Norman-Shallice model to explain properties of the human cognitive system that are typically described at the personal level (e.g. planning) is limited by its inability to reduce these properties to a description at the subpersonal level (see Duncan, 1986 for a similar argument; for discussion of the distinction between personal and subpersonal levels, and the role of these levels in psychological explanation, see Cummins, 1983; Dennett, 1969). A similar criticism may be made of the concepts of control in other frameworks, such as Atkinson and Shiffrin's (1968) model of memory ('The model of memory is there all right... However, the control structure is completely absent and is used as a deus ex machina to concoct separate models for each task'; Newell, 1973, pp. 297-8).

Two observations might be made in defence of the Norman-Shallice framework. First, the supervisory system could stand as a placeholder, to be elucidated by further research (Baddeley, 1998), without rejecting other elements of the theory. Thus, to rule out the Norman-Shallice model on the basis of the underspecification of the supervisory system would be to risk throwing out the baby with the bathwater. Second, as Dennett (1978, p.124) points out, 'homunculi are bogeymen only if they duplicate entirely the talents they are rung in to explain.' Recent work has attempted to fractionate the supervisory system into various subsystems (e.g. Shallice, in press; Shallice & Burgess, 1996; Stuss,
Thus, the supervisory system is at least slightly more attractive as a theoretical entity than a homunculus.

1.4 Outstanding issues

Two outstanding issues are of crucial importance. First, to what extent are control processes separable from the lower-level processes that they putatively control? This question could be interpreted in several different ways. Control processes may be separable from lower-level processes anatomically, or as a matter of cognitive theory, or both, or neither. If control processes are theoretically separable from lower-level processes, this could be in the sense that control processes are implicated in some types of behaviour but not others (e.g. non-routine behaviour), or in the sense that they play a different information-processing role to lower-level processes, despite being involved in all types of behaviour. Thus, a wide range of views is possible on the question of the separability of control processes from lower-level processes. Norman and Shallice (1986) take the strong view that control processes and lower-level processes are separable both anatomically and theoretically, and that control processes are involved in some types of behaviour (i.e. non-routine behaviour) but not others (i.e. routine behaviour). Other authors (e.g. Grafman, 1989, 1995, 2002; Schwartz, Reed, Montgomery, Palmer, & Mayer, 1991) apparently accept that control processes are theoretically separable from lower-level processes, but claim that they are involved in both routine and non-routine behaviour. Others still (e.g. Allport, 1980a; Brooks 1991; Minsky, 1985) have raised the possibility that control processes and lower-level processes may not even be separable as a matter of cognitive theory: lower-level processes may be self-organising, without any requirement for higher-level control. Of course, the separation between control processes and lower-level processes may admit of various degrees, rather than being all-or-nothing. For example, most current accounts of automaticity
posit a continuum between controlled and automatic processes (e.g. Cohen, Dunbar & McClelland, 1990; MacLeod & Dunbar, 1988), rather than a strict separation between the two (e.g. Shiffrin & Schneider, 1977). A second important unresolved theoretical issue is how far it is possible (if at all) to subdivide control processes (or fractionate the supervisory system) into more elementary processes or systems.

1.5 Neuropsychological evidence

Before discussing these two questions, the following sections survey some of the tests that may be failed by patients with frontal lobe lesions, so that the types of processes involved in cognitive control may be discussed below. These tests will be organised into categories of ‘inhibition’, ‘shifting’, ‘random generation’, and ‘planning/problem solving’, on the basis of the surface demands of the tasks. However, this does not imply that the computational processes involved in the tasks necessarily cluster in this way.

1.5.1 Inhibition

Suggestive evidence for a failure of inhibition in frontal lobe patients comes from the phenomenon of utilisation behaviour, described above (Lhermitte, 1983; Shallice et al., 1989). This could be viewed as resulting from a failure to inhibit the actions associated with objects in one’s environment. Another source of evidence for a failure of inhibitory control in such patients comes from cognitive tasks which have been considered to place heavy demands on the inhibition of irrelevant information. The best known of these is the Stroop task (Stroop, 1935), where subjects must name the ink colour of a colour-word (e.g. ‘GREEN’ written in red ink); this presumably requires the suppression of the prepotent response of reading out the word (MacLeod, 1991). Perret (1974) reported that patients
with frontal lobe lesions perform the colour-naming task with excessive slowness when the word and ink colour are incongruent (see also Stuss, Floden, Alexander, Levine, & Katz, 2001).

An alternative task that has been used to study the ability of subjects to inhibit a prepotent response is the antisaccade task (Hallett, 1978; see Everling & Fischer, 1998 for a review). In this task, subjects must make an eye movement in the opposite direction to a peripheral target with a sudden onset, thereby suppressing the tendency to fixate the target. Patients with frontal lobe lesions have been reported to make abnormally high numbers of incorrect reflexive saccades (e.g. Fukushima, Fukushima, Miyasaka, & Yamashita, 1994).

1.5.2 Shifting

Probably the most well known task that has been used in studies of control processes in frontal lobe patients is the Wisconsin Card Sorting Test (WCST; Grant & Berg, 1948), where subjects sort a pack of cards according to one dimension (e.g. the number of objects on the card), until they learn that this is incorrect. They must then discover, by trial and error, the correct new sorting dimension (e.g. the colour of the objects) and sort according to this. Milner (1963) found that frontal lobe lesions were associated with poor performance on this task. Subsequent investigations, with modified card sorting tests, have suggested that this often results from perseverative errors, where patients continue to sort according to a previously correct dimension, even when they have been told that this is incorrect (Nelson, 1976). Thus, these patients appear to have a difficulty with discovering new sorting rules, or shifting away from previously correct ones.
A test involving switching between tasks in a less constrained manner is the Six Elements Test developed by Shallice and Burgess (1991; see also Burgess, Veitch, de Lacy Costello, & Shallice, 2000). Subjects were presented with three tasks, each split into two sections. They were told that it was not possible to complete all tasks in the allocated time, and that they should try to score as many points as possible by partially completing the tasks. Subjects were encouraged to switch between the tasks because more points were available in the earlier stages of each task; in addition, they were instructed that they were not allowed to carry out the first section of any task followed by the second section of the same task. Patients with lesions involving the frontal lobes carried out the individual tasks as effectively as control patients. However, they scored fewer points because they attempted fewer subtasks (i.e. they did not switch between tasks as frequently as they should have done to obtain maximum points) and more frequently broke the rules by attempting the second section of a task immediately after the first.

1.5.3 Random generation

In random generation tasks, subjects must produce a sequence of responses (e.g. numbers) which approximates randomness as closely as possible (see e.g. Baddeley, Emslie, Kolodny, & Duncan, 1998). These tasks may rely on control processes on account of the necessity of monitoring output (i.e. to check if it appears random), switching between different production strategies, and inhibiting the tendency to produce stereotyped (e.g. counting) response sequences. Spatt and Goldenberg (1993) showed that patients with frontal lobe lesions produced responses that deviated from randomness to a greater extent than control subjects.

Another task that requires subjects to generate targets out of an internally represented response set (this time words, rather than numbers), which are
unrelated to currently available stimuli, is the Hayling task (Burgess & Shallice, 1996). In part A, subjects are presented with incomplete sentences such as 'He mailed the letter without a ...' and are required to complete them with a single contextually appropriate word (i.e. 'stamp'). As in the example given above, these sentences constrain the final word to a high degree. Then, in part B, subjects perform the same task but must this time complete sentences with a word that would make no sense in the context. Subjects are penalised if the word could potentially complete the sentence, or if it is semantically related to any of the words in the sentence. Burgess and Shallice (1996) found that patients with lesions involving the frontal lobes took longer to produce their responses in part A, compared with control patients with posterior lesions, but produced equally appropriate responses. In part B, the anterior patients had a greater error score, indicating that they were less able to produce responses unrelated to the sentences with which they were presented. This task has some similarities with 'fluency' tasks, in which subjects produce as many responses of a certain sort as possible (e.g. words beginning with the letter 'F', or unnameable drawings consisting of four lines) in a given time period. Such tasks also require subjects to generate responses from an internally represented response set, and are sensitive to frontal lobe lesions (Benton, 1968; Butler, Rorsman, Hill, & Tuma, 1993; Jones-Gotman & Milner, 1977).

1.5.4 Planning / Problem Solving

Problem solving may be required in circumstances where there is no obvious means of achieving a goal state, given the current state of the environment. Frequently this involves planning, i.e. generating an internally represented sequence of actions in order to bring oneself closer to the goal state. In tasks requiring the solution of complex puzzles, patients with frontal lobe lesions have often been found to find solutions less frequently (e.g. Klosowska, 1976). For
example, in the ‘cognitive estimates’ test (Shallice & Evans, 1978), subjects are asked questions such as ‘How long is the average man’s spine?’ Since it is unlikely that subjects are able to provide learned answers to these questions, they must discover a way in which they can use other knowledge to produce a sensible estimate (e.g. through consideration of the average length of a shirt). Shallice and Evans (1978) found that patients with frontal lobe lesions produced bizarre responses (i.e. outside the range produced by control subjects) more frequently than those with posterior lesions.

A task frequently used to assess planning ability is the Tower of Hanoi (ToH; e.g. Anzai & Simon, 1979) and its variant, the Tower of London (ToL; Shallice, 1982). Subjects must move discs of different sizes (ToH) or differently coloured beads (ToL), placed on three pegs, from one position to a goal position, in accordance with certain rules (e.g., a larger disc cannot be placed on top of a smaller one in the ToH and, in the ToL, two of the three pegs are limited in the number of beads that can be placed on them). Typically, several moves are required in order to reach the goal state; one way of measuring the difficulty of each problem is by the number of moves required. Patients with frontal lobe lesions have been reported to perform poorly on both tasks (Goel & Grafman, 1995; Owen, Downes, Sahakian, Polkey, & Robbins, 1990; Shallice, 1982), especially with problems which involve moving pieces away from their goal position (Morris, Miotto, Feigenbaum, Bullock, & Polkey, 1997).

A more complex task which may involve planning/problem solving is the Multiple Errands Task (MET; Shallice & Burgess, 1993). In this task, based around a shopping centre, participants are given some money and a set of instructions. They are required to buy various items, find out certain information, and be at a certain location at a specific time. There are also a number of rules to be followed, such as ‘you must not enter a shop other than to
buy something’. Patients with frontal lobe lesions tended to break the rules, leave items unfinished, and made several departures from social convention (e.g. refusing to pay for items, or clambering up onto a display of fruit outside a grocer’s shop to peer through the shop window).

Now that the types of test typically failed by patients with frontal lobe lesions have been described, it is possible to consider the evidence for the separability of control processes from other cognitive processes, and the fractionation of executive function. Before doing so, however, it should be pointed out that although the summary above focused on studies involving patients with frontal lobe lesions, several other populations have been claimed to suffer from impaired executive function, on the basis of their performance on these and similar tests. Such populations include children (e.g. Jacques & Zelazo, 2001); the elderly (West, 1996); children with ADHD (e.g. Shallice, Marzocchi, Coser, Del Savio, Meuter, & Rumiati, 2002); autistic children (Russell, 1997); those suffering from schizophrenia (e.g. Sullivan, Shear, Zipursky, Sager, & Pfefferbaum, 1994), Parkinson’s disease (e.g. Morris, Downes, Sahakian, Evenden, Heald, & Robbins, 1988), Alzheimer’s disease (e.g. Baddeley, Baddeley, Bucks, & Wilcock, 2001), obsessive-compulsive disorder (e.g. Head, Bolton, & Hymas, 1989), depression (Elliott, 1998), and so on. In addition, it should be pointed out that not all patients with frontal lobe lesions perform poorly on these tests (e.g. Eslinger & Damasio, 1985), and neither do patients with posterior lesions necessarily perform well (e.g. Corcoran & Upton, 1993; Hermann, Wyler, and Richey, 1988). Thus, it may be more appropriate to refer to ‘dysexecutive’ patients, rather than frontal lobe patients (Baddeley & Wilson, 1988), and to ‘executive tasks’ rather than frontal lobe tasks.

1.6 Evidence from correlational studies
Some theoretical accounts have stressed the importance of a single process for adequate performance of executive tasks. These have often been neurobiological theories of prefrontal cortex function. For example, Goldman-Rakic (1996) suggests that 'dorsolateral prefrontal cortex as a whole has a generic function – "on-line" processing of information or working memory in the service of a wide range of cognitive functions' (p. 1446). According to this view, the sustained representation of information, so that it can influence and modulate various cognitive processes, is at the heart of executive function (see Miller & Cohen, 2001; O'Reilly, Braver, & Cohen, 1999 for related accounts).

A conceptually related position has been put forward by Duncan (1986, 1993, 1995; see also Duncan et al., 1997), who claims that tests of executive function are sensitive to a general process of 'goal weighting'. This 'reflects a process of forming an effective task plan by activation of multiple goals or action constraints' (Duncan et al., 1997, p.714). Duncan (1995) claims that not only is the efficiency of this general process responsible for individual differences in tests of executive function, it is also responsible for individual differences in the 'g' factor of general intelligence (Spearman, 1927). This departs from conventional wisdom that frontal lobe lesions have relatively little impact on measures of IQ (e.g. Hebb, 1945). However, Duncan, Burgess, and Emslie (1995) distinguished between tests of 'crystallised intelligence', which are knowledge based and may be sensitive to g at the time of learning rather than the time of test (Cattell, 1971), and tests of 'fluid intelligence', which may have a closer relationship with g. They showed that three patients with frontal lobe lesions showed large impairments on Cattell's Culture Fair test (Institute for Personality and Ability Testing, 1973), which is supposedly a more faithful measure of g at the time of test. However, since this finding was based on such a small sample, it requires further investigation.
One source of evidence for the hypothesis that executive function reflects a general goal weighting process comes from correlational studies in which large numbers of patients (e.g., Duncan et al., 1997) or normal subjects (e.g., Obonsawin, Crawford, Page, Chalmers, Cochrane, & Low, 2002) perform a battery of executive tasks (such as the ones described above) as well as tests thought to be sensitive to Spearman’s $g$ (e.g., Cattell’s Culture Fair, or the WAIS-R; Wechsler, 1958, 1981). Duncan et al. (1997) found that in a sample of 90 brain-injured patients, correlations between conventional executive tests (e.g., WCST, verbal fluency) were uniformly low (around .2 - .3), and no higher than correlations with other tests typically not considered to reflect executive function (e.g., recognition memory; forward digit span). In a follow up experiment, involving 24 patients with a wide range of performance on these tests, Duncan et al. (1997) administered additional executive tests (e.g., SET) as well as Cattell’s Culture Fair test, as a measure of Spearman’s $g$. Partialling out the score on this test effectively reduced the correlations between performance on the executive tasks to zero, suggesting ‘little in common between tests besides their $g$ component’ (p.734).

In a similar study involving 123 normal subjects, representative of the general population, Obonsawin et al. (2002) administered the WAIS-R as a measure of general intelligence, along with a battery of seven executive tasks: verbal fluency, WCST (Nelson, 1976), Stroop, ToL, cognitive estimates, a paced auditory serial addition task (Gronwall & Wrightson, 1974) and a fluency task in which subjects had three minutes to generate as many uses as possible for a common object (e.g., a brick, see Butler, Rorsman, Hill, & Tuma, 1993). There were significant correlations between scores on the executive tasks, and between performance on the executive tasks and WAIS-R scores ($r = .2 - .5$). However, when WAIS-R scores were partialled out, correlations between the executive tasks were reduced, and of the 13 significant correlations in the initial analysis only four remained significant. Thus, Obonsawin et al. (2002) conclude that performance
on executive tasks, to a large extent, reflects the operation of a general process measurable by standard psychometric $g$.

However, as Burgess (1997) points out, even if different elements of executive function are theoretically separable, empirical evidence for such a fractionation may be hard to obtain. It is possible, for example, that two theoretically independent processes might underlie planning: one to create the plan itself, and another to actually implement it behaviourally. But any planning task would implicate both of these, so it would be difficult to dissociate these processes empirically (but not necessarily impossible: requiring subjects to verbally report their intentions before acting, for example, might dissociate plan generation from plan implementation). Thus, an inability to discern a factor structure in correlations between performance in various executive tasks might not imply that they all reflect a single control process: the tasks might be too coarse to reveal the independent contributions of several theoretically orthogonal control processes. The relatively high correlations between executive tasks and measures of $g$ may therefore result from the latter measures providing a broad sample of elements of executive function rather than reflecting a single process.

A more basic problem for accounts of executive function in terms of a single process is that the correlations between executive tasks tend to be universally low, not universally high. There could be several reasons for this, even if such tasks place heavy demands on control processes. Since cognitive control cannot be studied independently of the cognitive processes being controlled, performance may differ between various tasks as a result of their lower-level discrepancies, even if they rely on common control processes. Thus, Shallice and Burgess (1996, p. 1408) argue that ‘for observations of low correlations across frontal tasks to be theoretically interesting the tasks must at least involve the same type of material’. An additional problem is that executive tasks tend to
have low test-retest and internal reliabilities (Rabbitt, 1997b; O’Carroll, Egan, & MacKenzie, 1994), which may account for the low correlations across tasks. A possible cause of this is that executive tests depend critically on adopting an effective strategy, which can vary from one test session to the next (Stuss, Shallice, Alexander, & Picton, 1995). These considerations suggest that, even if it is possible to fractionate control processes, it may not be easy to do so with conventional correlational methodologies. Thus, the null result reported by Duncan et al. (1997) and Obonsawin et al. (2002) - the absence of an easily discernible factor structure in correlations between various executive tasks - should be interpreted with caution. It seems that this result is far from a conclusive demonstration of the unity of control processes.

A contrasting correlational study, which did provide evidence for a fractionation of control processes, was recently reported by Miyake et al. (2000). This study, rather than investigating correlations between specific tasks, used confirmatory factor analysis to relate scores from a number of tasks with latent variables representing three putative executive functions: ‘Shifting’ (between tasks or mental sets), ‘Updating’ (of working memory representations, as well, conceivably, as concomitant monitoring processes), and ‘Inhibition’ (of prepotent responses). 137 normal subjects (all undergraduate students) performed a battery of tasks, including three corresponding to each of the postulated executive functions as well as a series of more complex tasks: WCST (Kimberg, D’Esposito, & Farah, 1997), ToH, random number generation, operation span (Turner & Engle, 1989), and dual-task performance. A statistical model assuming that the three latent variables have correlations greater than zero but less than 1.0 fitted the covariance matrix between the individual tasks significantly better than both a model assuming complete independence of the latent variables (i.e. zero correlations) and a model assuming complete interdependence of the latent variables (i.e. correlations of 1.0). Miyake et al. (2000) argue that if there is only a
single, global control process, then each of the three latent variables should have extracted variance related to this process, and it should not have significantly improved the fit of the model to allow these latent variables to have correlations less than 1.0. By contrast, if the three executive functions are entirely separable, then the fit of the model with the correlations between the latent variables fixed at zero should not have been significantly worse. Thus, they conclude that the three postulated executive functions are indeed separable, but there are also some commonalities between them.

Next, Miyake et al. (2000) used structural equation modelling to investigate the relationship between their complex executive tasks and the latent variables extracted from the other tasks. They compared the fit of models with paths from all three of the latent variables to the score on each particular test with models where there are paths from only one or two latent variables. If the fit is not significantly worse with a path from just one latent variable (‘Updating’, for example), this suggests that the complex task primarily relies on this executive function, rather than the other two. These analyses led Miyake et al. (2000) to conclude that WCST is best modelled with one path from Shifting, ToH is best modelled with one path from Inhibition, and the ‘prepotent associates’ score of RNG is best modelled with one path from Inhibition, while the ‘equality of response usage’ score of RNG is best modelled with one path from Updating. Miyake et al. (2000) argued that the best model for the Operation Span task had one path from Updating, even though this was statistically worse than the three factor model, because this did not distort the factor structure so much. Finally, Miyake et al. (2000) found that dual-task performance was fitted no worse with a model with no paths than any of the others, suggesting that this ‘may tap an executive function that is somewhat independent of the three target executive functions’ (p. 84). The authors conclude that the three target executive functions are separable, and also have separable influences on various complex executive
tasks. On the other hand, they note that there were relatively high correlations between the latent variables in the best fitting model, so there was also evidence for some commonality between the three executive functions that were investigated.

One possible objection to this study is that performance on the tasks measuring each target executive function may have clustered as a result of peripheral similarities between the tasks, rather than reliance on a common, separable control process. Miyake et al. (2000) argue that different executive tasks might correlate poorly with each other as a result of a dependence on different peripheral, lower-level processes (i.e. the problem of 'task impurity'). However, a consequence of this is that it must also be possible that certain executive tasks will correlate more highly than others as a result of a dependence on common peripheral processes. This is especially concerning since each of the tasks from which the Shifting factor was extracted had RTs as their dependent measures, whereas the tasks loading on Updating had errors as their dependent measures. Suppose that subjects tend to apply similar speed-accuracy trade-off settings across tasks. In this case, subjects with a bias towards speed might perform comparatively well in the RT tasks, but comparatively poorly in the error tasks. This might lead to a lower than 1.0 correlation between the Shifting and Updating factors, even if they reflect a common control process. Thus, the results of Miyake et al. (2000) do not conclusively show that the executive functions they investigate, rather than the peripheral demands of the various tasks, are separable.

To summarise the work described above, there does not seem to be strong empirical support either for or against a fractionation of executive function. Correlational studies arguing for a unity or a diversity of executive function suffer from a common problem: insofar as the correlations between different
executive tasks lack a factor structure, this may reflect the discrepant low-level demands of the tasks. However, insofar as a factor structure is discernible, this may likewise reflect the discrepant low-level demands of various tasks. These studies have also failed to provide strong evidence either for or against a separation between control processes and lower-level processes. The low correlations between performance in various executive tasks suggests that it may be difficult to dissociate higher- and lower-level processes, but this difficulty may be methodological rather than theoretical in nature.

1.7 The control of routine and non-routine behaviour

An alternative means of investigating the separability of control processes from lower-level processes is through consideration of the circumstances in which executive function putatively is, or is not, required. An important commonality between tests that are thought to rely heavily on executive function is that they cannot be solved by reliance on learned associations between features of the environment and behavioural routines. In other words, executive function plays a crucial role when it would be inappropriate to base behaviour on learned associations between stimuli and responses. Thus, it is frequently assumed that executive function is called into play in circumstances of novelty, or when responses are under-constrained by environmental events, as in random generation or fluency tasks. In such situations there are no learned stimulus-response associations available. In addition, executive function is frequently thought to play an important role when there are learned stimulus-response associations available, but they are currently inappropriate, as in ‘inhibition’ tasks such as the Stroop.

Despite this characterisation of executive tasks as being ones where responses are not based on learned associations with environmental events, it would seem
mistaken to characterise responses in such tasks as being based on internal representations at the expense of external stimuli. Performance in executive tasks frequently seems to rely heavily on the state of the environment. Of course, in tasks such as the Stroop, the appropriate response is determined in a very straightforward way by features of the environment. But even in more complex tasks, where behaviour may be under-constrained by the environment, environmental cueing may nevertheless play an important role. For example, in part B of the Hayling test, a common strategy is to complete each sentence by looking around the testing room naming objects that one sees (Burgess & Shallice, 1996). Thus, even where there are no task-relevant environmental triggers, control processes may allow one to respond appropriately by enabling a relatively simple stimulus-response routine: look around the room naming the objects that one sees. The control processes involved in strategy selection may be very complex, but the processes that are then called upon to achieve the goals of the task may be relatively simple.

According to this view of executive function, an important role of control process is to modulate the flow of information through relatively low level input and output modules, presumably ‘chaining together’ (Monsell, 1996) the appropriate systems, so that suitable behaviour is produced for the current task. Of course, this is the basis for the claim in the Norman-Shallice model that the role of the SAS is to modulate the contention scheduling system, rather than to generate behaviour itself. This characteristic of control processes seems to lie behind many other models of the production of task-relevant behaviour. For example, the concepts of the ‘determining tendencies’ (Ach, 1905) or, more recently, the ‘prepared reflex’ (Hommel, 2000) or ‘sculpting the response space’ (Frith, 2000) seem to follow the same principle, that once control processes have configured

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1 Presumably some monitoring process is required in addition, to ensure that the object being named is unrelated to the context of the sentence.
the appropriate mappings between input and output, intentional behaviour may proceed in a relatively ‘bottom-up’ or stimulus-driven way.

A converging view, based on somewhat different theoretical considerations, has emerged recently in the field of robotics. In the past, it was generally assumed that the best information-processing architecture for the control of autonomous mobile robots was separable into a sensing system, a planning system, and an execution system, with a feedforward flow of information between systems (e.g. Nilsson, 1980). However, due to the complexity of the computations in the planning system (often involving exhaustive search routines), such architectures led to excessively long delays between the detection of environmental events in the sensing system (e.g. obstacles) and the generation of appropriate behaviour in the execution system. More successful mobile robots were built using a ‘subsumption’ architecture (Brooks, 1986), based on decentralised control by a number of direct input-output links. Such systems did not perform well when they were scaled up in order to produce more complex behaviours. Thus, currently the dominant approach for the control of mobile robots is a ‘three-layer’ architecture (Gat, 1999), in which processing is divided into a ‘controller’, which affords stable closed-loop control for the desired behaviour; a ‘sequencer’, which selects the primitive behaviour that the controller should use at a given time; and a ‘deliberator’, which operates on a much slower time scale and can produce plans for the sequencer to execute, or respond to specific queries from the sequencer. This architecture is remarkably similar to that produced by Norman and Shallice (1986), both in terms of the role of each system and the interactions between them. The controller would correspond with low-level input-output pathways, the sequencer with the contention scheduling system and the deliberator with the supervisory system.
An interesting consequence of this analysis of the division of labour between higher-level and lower-level systems in the production of appropriate behaviour is that an act of intention to produce a certain sort of response can *precede* the stimulus that one responds to ("The intention lies in the readying of the act, not (necessarily) its release"; Monsell, 1996, p.107; cf. Ach, 1905). This dissociation between the temporal characteristics of higher-level and lower-level process is potentially of relevance to the question of their dissociability. Presumably, in order for there to be a temporal separation between the operation of a higher-level and lower-level process, the two processes must be theoretically separable (see Shallice, 1988, p. 334-335 for a similar argument).

This speculation on the temporal characteristics of higher-level and lower-level processes invites consideration of the control of sequential behaviour. If one intends to perform a routine sequence of behaviours, making a cup of tea, for example, it seems reasonable to assume that this "act of intention" precedes the first stimulus-driven action (perhaps to reach for the kettle, in order to fill it with water). Does the act of intention also terminate before the sequence of behaviours is initiated, or must it accompany the entire sequence? This question is of theoretical significance since the case for a strict separation between higher-level and lower-level processes will be strengthened if it can be shown that there is no temporal overlap between the two in the production of routine sequential behaviour. James (1890) advocated such a position, claiming that in "habitual action... the only impulse which the centres of idea or perception need send down is the initial impulse, the command to *start*" (p.116). In other words, each act in a habitual sequence automatically gives rise to the next, so the only involvement of higher-level processes is to initiate the sequence. However, this view is challenged by recent studies of action disorganisation syndrome (e.g. Schwartz et al., 1991) which suggest that routine action can become disordered after frontal lobe lesions, leading to an extreme form of the action slips made by
normals (e.g. object substitution errors, anticipations, capture errors etc.). Schwartz et al. (1991) argue that this implies the involvement of supervisory attention even in routine behaviour, throughout the entire sequence of acts (see also Cooper, 2002; Cooper & Shallice, 2000 for discussion).

Evidence from action disorganisation syndrome suggests that higher-level processes are involved throughout the production of even routine behavioural sequences. This contrasts with a view of control processes as being involved only in non-routine behaviour (e.g. Norman & Shallice, 1986). However, according to the Norman-Shallice framework, schemas are hierarchically organised so that a source schema can modulate activity in lower-level component schemas. Thus, it is possible to explain disordered routine behaviour in terms of damage to source schemas (Cooper & Shallice, 2000, refer to this as the 'intermediate domain of action') rather than a supervisory impairment. In this way, it is still possible to preserve a distinction between two qualitatively different systems for the control of lower-level processes.

1.8 Evidence from neuroimaging

D'Esposito, Detre, Alsop, Shin, Atlas, and Grossman (1995) sought to dissociate control processes from lower-level processes anatomically, by requiring subjects to perform a pair of tasks both individually and simultaneously. They reasoned that if the individual tasks were not heavily dependent on control processes, then brain regions responsible for a control process coordinating the simultaneous performance of two tasks might not be activated by either task alone but would be activated when both tasks are performed together. Using fMRI, D'Esposito et al. (1995) scanned subjects whilst performing a semantic judgement and a spatial rotation task, both individually and in conjunction. Compared with a rest baseline, the dual-task condition activated dorsolateral prefrontal cortex.
(DLPFC) bilaterally (BA 9 and 46), and anterior cingulate cortex (ACC); these regions were not activated in either of the single-task conditions. D'Esposito et al. (1995) conclude that the executive processes involved in dual-task performance are anatomically separable from those involved in performance of the individual tasks. However, the subtractive methodology employed in this study does not seem appropriate to the question of whether control processes can be separated from lower-level processes. The results showed that DLPFC activity was similar during single-task conditions and rest periods, but significantly greater during dual-task performance. In order to conclude that the dual-task condition engaged a system that was not involved in single-task performance, it is necessary to assume that control processes were not involved in the rest periods. However, since the subjects' cognitive processes were not controlled in any way during such periods, there seems little reason to make this assumption.

An alternative to the subtraction methodology for identifying brain regions involved in task performance is to use a parametric design, in which a variable of interest is manipulated at several levels and brain regions with activity correlated with the level of this factor are identified. For example, Dagher, Owen, Boecker, and Brooks (1999) carried out a PET study investigating a computerised version of the ToL task. Dagher et al. (1999) identified a number of occipitoparietal and primary motor areas that were activated by performance of the ToL task but were not affected by problem complexity (i.e. the number of moves required to complete the problem). By contrast, there were a number of other regions, virtually all in the frontal lobe, that showed differential activity depending on the complexity of the problem. A similar approach was taken by Garavan, Ross, Li, and Stein (2000), who used fMRI to scan subjects while they performed a working memory task which involved keeping running counts of the number of times two different sorts of stimuli appeared during each trial. Garavan et al. (2000) split the trials into three categories, depending on the
number of times the subjects had to switch attention from one running count to the other on that trial. Like Dagher et al. (1999), Garavan et al. (2000) identified a number of brain regions activated by task performance, but not all of them had different activity levels for different levels of the factor under investigation, in this case switching frequency. The regions that did show an effect of switching frequency were widely distributed, including large areas of activation in prefrontal, parietal, and cerebellar regions. There were also activations in occipital, temporal, and subcortical (thalamus and caudate) areas. These regions were frequently adjacent to others that were activated by the task but did not show any effect of switching frequency. Thus, these studies show that some distinction may be made between brain regions involved in control processes (especially in the frontal lobes) and those involved in lower-level processes. However, it may be difficult with such studies to distinguish between regions necessary for control processes, and others that differ in activity as a result of modulation from higher-level processes. For example, attentional modulation of low-level visual areas, even including V1 (Posner & Gilbert, 1999), does not imply a role of these areas in the control of attention. In future, the greater temporal resolution afforded by event related fMRI (Josephs & Henson, 1999) might help to separate the contributions of different brain regions to executive tasks (see e.g. Rowe, Toni, Josephs, Frackowiak, & Passingham, 2000).

Turning now to the fractionation of executive function, functional neuroimaging seems to be a promising technique, since it offers the promise of dissociating distinct processes that may be difficult to separate behaviourally. Some suggestive evidence is available, pointing towards different executive processes being carried out by distinct regions of prefrontal cortex. For example, Shallice (in press) considers a number of studies involving memory encoding and retrieval tasks (e.g. Fletcher, Shallice, & Dolan, 1998a; Fletcher, Shallice, Frith, Frackowiak, & Dolan, 1998b). Fletcher et al. (1998a) provided evidence that
activity in left DLPFC is associated with the use of a strategy in memory encoding; Fletcher et al. (1998b) investigated memory retrieval and demonstrated a double dissociation between right DLPFC, which was activated more strongly by an internally cued recall task than an externally cued paired-associate recall task, and a more ventral, posterior right hemisphere region that was activated more strongly by the externally- than the internally-cued task. On the basis of such findings, Shallice (in press) suggests that left DLPFC may be involved in top-down modulation of schemas in contention scheduling, right DLPFC may be involved in monitoring and checking of behaviour with respect to a variety of internally generated criteria, and right ventrolateral prefrontal cortex may be involved in the specification of a required memory trace during cued recall.

A contrasting view comes from Duncan and Owen (2000), who carried out a meta-analysis of functional imaging studies that have investigated several different types of manipulation common in executive tasks, viz. response conflict, task novelty, working memory (both number of elements and delay duration) and one manipulation not conventionally thought of as associated with executive function - perceptual difficulty. They found that all of these manipulations produced activation clusters in dorsolateral prefrontal cortex, ventrolateral prefrontal cortex and the anterior cingulate. However, it was not possible to identify different clusters associated with the different types of tasks. Thus, while the studies examined by Duncan and Owen (2000) consistently produced quite specific regional activations, these activations did not seem to be dependent upon the task that was performed.

Even if Duncan and Owen (2000) are correct, and it is not possible to distinguish between the frontal activations obtained in different executive tasks, it might be possible to distinguish control processes at an information-processing level that apply to a variety of executive tasks, rather than seeking to differentiate the
control processes required by these different tasks. For example, it may be possible to dissociate the 'level' of executive processes mediated by ventrolateral and dorsolateral prefrontal cortex (Owen, 2000; Petrides, 1995). According to this hypothesis, ventrolateral prefrontal cortex implements low-level executive processes, such as those required by active maintenance of information in working memory, whereas dorsolateral prefrontal cortex plays a role in higher-level processes such as the manipulation of that information. In addition, it may be possible to dissociate the implementation of attentional 'set' in dorsolateral prefrontal cortex (Banich et al., 2000; Nathaniel-James & Frith, 2002) from a role of the anterior cingulate in monitoring processes such as the detection of response conflict (Botvinick et al., 1999; Carter et al., 2000; Gehring & Fencsik, 2001; MacDonald et al., 2000), or more general autonomic processes (e.g. Critchley, Corfield, Chandler, Mathias, & Dolan, 2000). Since processes such as the implementation of attentional set can be assumed to be required by a wide variety of tasks, it is not surprising that seemingly diverse tasks activate common brain regions.

1.9 Conclusions

1.9.1 Reasons for pessimism

There seems to be near-universal agreement that a theoretically useful distinction between higher- and lower-level processes may be made. However, there is less consensus on the nature of higher-level processes. Are such processes qualitatively different from lower-level processes? Are they only involved in non-routine behaviour, or do they also play a role in routine behaviour? Can they be fractionated, and if so, into which subprocesses? What are the neural correlates of such processes? At present, these questions have not received clear answers. Thus, it does not seem a great exaggeration to refer to the information-
processing characteristics of control processes as a 'zone of almost total ignorance' (Monsell, 1996, p.93).

A serious problem is the scarcity of experimental paradigms where a clear link can be made between a subject's behaviour and the nature of the implicated control processes. In other words, there is a poor 'process-behaviour correspondence' (Burgess, 1997). As a result, much of the empirical evidence in the domain of executive function has come from the relatively crude approach of characterising the type of situation that would require the operation of putative control processes and showing that certain types of patient perform poorly in those situations (e.g. Shallice, 1982; Shallice & Evans, 1978). Of course, in the early stages of theory development this type of evidence plays an important role in supporting the theoretical framework. However, it is less informative with respect to the precise computational nature of the impaired processes in the relevant set of patients. Unfortunately, relatively little progress has been made in this more detailed phase of theory elaboration, which strives for a more precise characterisation of the entities posited by the theory.

1.9.2 Reasons for optimism

Notwithstanding the difficulties outlined above, the progress made in recent years should not be underestimated. This has come largely as a result of methodological advances, especially involving studies of normal subjects using new behavioural paradigms (e.g. Miyake et al., 2000) or neuroimaging techniques (Collette & Van der Linden, 2001), to complement earlier work which often employed neuropsychological methodologies (Shallice, 1988).
Another important advance has been the development of computational models to account for phenomena associated with control processes (e.g. Braver & Cohen, 2000; Changeux & Dehaene, 2000; Cooper & Shallice, 2000; Kimberg & Farah, 1993; Logan & Gordon, 2001; Meyer & Kieras, 1997a, 1997b; O'Reilly, Braver, & Cohen, 1999). By explicitly implementing these models, and comparing them with data from both normal subjects (e.g. Meyer & Kieras, 1997b) and patients (e.g. Cooper & Shallice, 2000), it is possible to provide a more detailed examination of the fit between theory and data. This is especially important in the domain of executive function for two reasons. First, models of executive function typically suffer from underspecification, making it difficult to derive specific, testable predictions. Computational modelling techniques require theories to be specified precisely, allowing a better assessment of the relationship between theoretical assumptions and empirical results. Second, if executive function is only indirectly related to observable behaviour, via its modulatory influence on other cognitive processes, the relationship between theoretical executive processes and behavioural output may be complex. Thus, implemented models can provide a bridge linking theoretical descriptions of high-level processes with features of observable behaviour. Such models have also been used to link the description of high-level cognitive processes to lower-level neurobiological descriptions (e.g. Changeux & Dehaene, 2000).

Detailed examination of experimental paradigms suitable for testing normal subjects, combined with explicit theorising about the control processes involved in such paradigms (e.g. through computational modelling) seems to offer a promising direction for future research into executive function. In the following chapters, this approach is taken in order to investigate the role of control processes in task switching.
A review of task switching research

2.1 Introduction

The task switching paradigm has emerged recently as a leading means of studying executive function in normal human subjects. As in standard reaction-time experiments, task switching experiments consist of a series of stimulus-response episodes ('trials'). However, the task performed by the subject may differ from trial to trial. In this manner, it seems possible to study the ways in which cognitive processes may be configured in order to perform one out of the many tasks afforded by each stimulus. At least when the same set of stimuli is used for more than one task, responses cannot be based on learned stimulus-response associations. Thus, task switching appears to depend critically on the operation of control processes, which modulate the lower-level processes responsible for the production of various possible responses. What may be learned about such processes from the task switching paradigm? In particular, how do they relate a) to the cognitive processes involved in task performance when there is no requirement to switch between tasks and b) to other control processes?

Before discussing results from the task switching paradigm, a crucial concept will be introduced: the 'task set'. The concept of 'set' has a long history in psychology, referring to a diverse range of phenomena (Gibson, 1941). In discussions of task switching, a task set typically refers to the set of cognitive operations required to effectively perform a specific task, which may be in place even before the relevant stimulus for the task arrives. For example, in a Stroop experiment one might adopt a colour-naming task set, meaning that one is ready
to name the colour of the next word with which one is presented. Thus, it is possible to adopt a task set without knowing which of the task-relevant stimuli will be presented, and hence which specific S-R mapping will be required ("To form an effective intention to perform a particular task, regardless of which of the range of task-relevant stimuli will occur, is to adopt a task-set"; Rogers & Monsell, 1995, p.208, emphasis in original). For further discussion of this concept see Monsell (1996) and Monsell, Taylor, and Murphy (2001).

2.2 Methodologies for task switching experiments

One approach to task switching experiments is simply to compare pure and mixed (or ‘alternating’) blocks, i.e., to compare blocks where the subject performs the same task on every trial with blocks in which s/he must alternate between two tasks on successive trials (e.g. Allport, Styles, & Hsieh, 1994; Jersild, 1927; Spector & Biederman, 1976). However, it is not possible unequivocally to attribute any differences between pure and mixed blocks to the effects of task switching. Since mixed blocks require subjects to maintain the rules for two tasks, whereas pure blocks require the rules for just one task to be maintained, any differences between the two types of block may arise from greater working memory demands in mixed blocks, or differences in arousal between the two types of block due to their perceived difficulty (Rogers & Monsell, 1995). There are also methodological difficulties if subjects are presented with lists of stimuli and the list-completion time is the dependent measure (e.g. Allport et al., 1994; Baddeley, Chincotta, & Adlam, 2001; Jersild, 1927). In this case, reaction times for individual stimuli are not available, and hence it is not possible to investigate differences between the two tasks in mixed blocks. Results may also be affected by preview of forthcoming stimuli (Spector & Biederman, 1976). A final problem with this list-based methodology is that it is not possible to exclude trials on which subjects make errors.
An alternative methodology that avoids many of these difficulties is the 'alternating runs' paradigm (Rogers & Monsell, 1995), where subjects are required to switch tasks predictably every \( n \)th trial, where \( n \) is at least 2. This has the advantage that 'switch' trials (where the task differs from the one performed on the previous trial) can be compared with 'nonswitch' (or 'repeat') trials within the same block. Thus, the effect of task switching is no longer confounded with differences in arousal or working memory requirements between the two types of block.

In a third methodology for task switching experiments, each trial is preceded by a cue which instructs the subject which task to perform (e.g. Meiran, 1996; Meiran, Chorev, & Sapir, 2000; Sudevan & Taylor, 1987). This makes the requirement to switch tasks unpredictable. Hence it is possible using this paradigm to unconfound the effects of temporal remoteness from the previous trial (affected by the response-cue interval, or RCI) and the effect of foreknowledge of the currently required task (affected by the cue-stimulus interval, or CSI).

### 2.3 Tasks

Typically in task switching experiments, the subject switches between a pair of choice reaction time tasks, with different S-R mappings for each. For example, Rogers and Monsell (1995) presented their subjects with stimuli such as ‘G7’ and required a classification of the letter as a consonant or vowel in one task, and the number as odd or even in the other. In this experiment the two S-R tasks were mapped onto the same responses so that a button-press with the left index finger might indicate a consonant in the first task and an odd number in the second. Three types of stimuli are possible in such experiments: a) incongruent stimuli,
which command different responses depending on which task is being performed (e.g. ‘A1’ using the above response mappings), b) congruent stimuli, which receive the same response in both tasks (e.g. ‘A2’), and c) neutral stimuli, which only afford a response in one of the tasks (e.g. ‘A#’ where the ‘#’ character is not associated with any task).

2.4 Basic findings

Studies of task switching using each of the methodologies described above have consistently found that there is a cost in reaction time, and typically also in error rate, for switch compared with nonswitch trials (or mixed compared with pure blocks). Another consistent finding is that the ‘switch cost’ is highly sensitive to stimulus congruity. A striking demonstration of this can be found in the study by Jersild (1927). In one experiment, subjects were presented with lists of two-digit numbers. In pure blocks, they either added or subtracted 3 from each number; in alternating blocks they alternated between the two tasks. Jersild (1927) found a large cost of task alternation, amounting to several hundred milliseconds per item. However, when subjects switched between subtracting 3 from two-digit numbers and naming the antonym of common adjectives there was no switch cost; in fact there was a small switch benefit. Spector and Biederman (1976) replicated this finding, although they showed that the switch benefit disappears when subjects are no longer able to preview forthcoming stimuli. Thus, large switch costs appear to be generated only when the stimuli afford both of the currently relevant tasks. In a study using the alternating runs procedure, measuring individual RTs, Rogers and Monsell (1995, Experiment 4) found that there was a small switch cost associated with switching between neutral stimuli, around 50 ms per item. However, this was much smaller than the cost, around 150 ms per item, in an otherwise identical experiment using a combination of neutral, congruent and incongruent stimuli.
2.5 Theoretical accounts

Theoretical accounts of the switch cost have fallen into two broad families. On
the one hand, Allport and colleagues (e.g., Allport et al., 1994; Allport & Wylie,
1999, 2000; Wylie & Allport, 2000) have suggested that switch costs reflect an
interference effect caused by the carryover of a previous task set into switch
trials. Such hypotheses will be referred to as 'task carryover' accounts. For
example, the version of the task carryover account put forward by Allport et al.
(1994) described this carryover as 'task set inertia' (TSI). According to this
hypothesis, a task set active on one trial will persist, involuntarily, on the next.
As well as a carryover of the previously active task set on switch trials, Allport et
al. (1994) hypothesise that inhibition of competing task sets can also persist on
switch trials, when the previously competing task set is now required. Thus,
various task sets may be primed, positively or negatively, depending on the task
performed on the previous trial. On a switch trial, the required task set may be
negatively primed, and an inappropriate task set may be positively primed. As a
result, responses may be slower and more error-prone, hence the switch cost.
According to this account, in common with other task carryover accounts, there
is no need to posit differences in the higher-level cognitive processes that take
place on switch and nonswitch trials. Rather, one or more of the processes that
take place on nonswitch trials may be prolonged by competition on switch trials.
Typically, it is assumed that the process most likely to be prolonged in this way
is response selection.

Task carryover accounts, such as the TSI hypothesis, may be contrasted with
'extra process' accounts (e.g. Monsell, Yeung, & Azuma, 2000; Rogers & Monsell,
1995; see also De Jong, 2000; Hübner, Futterer, & Steinhauser, 2001; Rubinstein et
al., 2001). According to these accounts, switch costs reflect the duration of one or
more stage-like control processes that reconfigure the cognitive system for the upcoming task. For example, in the version of the extra process account put forward by Rogers and Monsell (1995), there are two extra control processes that must take place on switch trials, but do not occur on nonswitch trials. One process cannot be completed until the arrival of the first stimulus of the new task (i.e. it is a stimulus-driven ‘exogenous control process’). An additional component of the switch cost is hypothesised by Rogers and Monsell (1995) to reflect the operation of an ‘endogenous control process’ that can be executed before the arrival of the first stimulus in the new task.

It is important to have a clear definition of exactly what separates task carryover and extra process accounts. It is clearly not an adequate characterisation of task carryover accounts to say merely that the switch cost arises from the effects of the task performed on the previous trial. Since the only difference between switch and nonswitch trials is the task performed on the previous trial, the distinction between the two types of account would collapse under this characterisation. The definition of extra process accounts is fairly straightforward: they claim that the switch cost measures the duration of one or more cognitive processes that take place on switch trials but do not take place on nonswitch trials. Task carryover accounts will be defined here as the negation of this. They claim that there is no need to posit additional cognitive processes on switch trials in order to explain the switch cost, either because no such additional cognitive processes exist, or because the duration of such processes is not typically what is being measured by the switch cost. According to such accounts, the switch cost reflects an interference effect (i.e. a form of priming), rather than measuring the duration of any control process.

Although task carryover accounts have been characterised as the negation of extra process accounts, the two types of account need not be mutually
incompatible. It is of course possible that switch trials are slowed both by one or more additional processes (in line with extra process accounts) and a priming effect, leading to slowed execution of the processes that normally take place on nonswitch trials (in line with task carryover accounts). One such hybrid account has been presented by Meiran (2000a, 2000b). Evidence for extra process or task carryover accounts does not, therefore, logically exclude the other type of account. However, if it could be shown that task carryover or extra process accounts are able to provide an adequate explanation of task switching data on their own, this would make the other sort of account unnecessary. The assumption of extra processes on switch trials, for example, would not serve any explanatory role if it could be shown that a task carryover effect was able to explain all of the data relevant to the debate between the two sorts of account. In this limited sense, then, the two types of account can be seen as competing. The various types of account are illustrated in Figure 2.1, adapted from Monsell et al. (2000, Figure 2).

These accounts differ in their implications for the relationship between task switching performance and executive function. According to extra process accounts there is a relatively straightforward relationship between control processes and the switch cost. Since switch trials are extended by the duration of one or more stage-like control processes, the RT increase on switch trials can be assumed to reflect the duration of these processes, independent of the lower-level processes involved in task performance. Some authors have claimed that if the switch cost simply reflects a priming effect, as proposed by task carryover accounts, it would therefore be irrelevant to the study of control processes. For example, Rubinstein et al. (2001) describe Allport et al.’s (1994) TSI hypothesis as

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7 In order to prefer a task carryover over an extra process account in this circumstance, it would also be necessary to show that there is some data that can be more adequately explained by the task carryover account. Otherwise, the two types of account would be equally adequate.
Theoretical accounts of the RT switch cost. 'Basic S-R processes' refer to the processes that take place on nonswitch trials. According to extra process accounts, one or more extra processes slow reaction time on switch trials, while the duration of the basic S-R processes is unchanged. Task carryover accounts attribute the switch cost to extended basic S-R processes on switch trials.
attributing switch costs to ‘conflicts at a basic level of task processing without any assumption of control being exerted from a higher supervisory level’ (p. 790). However, task carryover accounts need not rule out the involvement of control processes in task switching (‘Undoubtedly, additional control processes are brought to bear on the pre-existing task-set, on a “switch” trial. If not, the previous task would continue to be executed’; Allport & Wylie, 1999, p. 280). Such accounts merely deny that these control processes are measured in any direct way by the switch cost. Information about control processes might nonetheless be inferred from task switching data. Thus, even according to task carryover accounts, the task switching paradigm may be of relevance to the study of executive function. However, according to such accounts the relationship between control processes and switch costs is indirect.

2.6 Evidence for task carryover accounts

Evidence for task carryover accounts has come from findings indicating an effect of performance of an earlier task on subsequent task switching performance (e.g. Allport et al., 1994; Allport & Wylie, 1999, 2000; Mayr & Keele, 2000; Waszak, Hommel, & Allport, in press; Wylie & Allport, 2000).

2.6.1 Asymmetric switch costs

Allport et al. (1994, Experiment 5) carried out an experiment where subjects switched between word reading and colour naming in response to incongruent Stroop stimuli. As expected, word reading yielded faster reaction times than colour naming (see MacLeod, 1991). However, this experiment also produced a most unexpected result. Reaction times for word-reading trials were slower when colour naming had been performed on the previous trial (i.e., there was a switch cost), but Allport et al. failed to detect a switch cost for colour-naming
trials that followed performance of the word-reading task. In other words, the switch cost appeared to be confined to the switch from the nondominant into the dominant (i.e., better learned, easier) task.

Subsequent experiments replicated this asymmetry in switch costs (Allport & Wylie, 2000; Wylie & Allport, 2000). Allport and Wylie found that there is a cost for a switch into the colour-naming task, but it is smaller than the cost of a switch into the word-reading task. Meuter and Allport (1999) reported an analogous finding when subjects switched between digit naming in their dominant and nondominant languages. This paradoxical finding – larger reaction time costs for a switch into a better learned, more dominant task – is difficult to explain if switch costs reflect the duration of a process that reconfigures the cognitive system for the upcoming task. Why should a switch into an easier, better learned task take longer to complete than a switch into a less familiar task? Allport et al. argue that the result can be explained if the primary determinant of the switch cost is the nature of the previous task. When subjects name the colour of an incongruent Stroop stimulus, Allport et al. hypothesise that inhibition of the word-reading task may be required. According to the TSI account, this inhibition will persist on a switch trial where word-reading is now appropriate, leading to a large switch cost. But in the absence of any requirement to suppress colour naming in order to perform the word-reading task, there will be no carryover of inhibition into colour naming switch trials, hence the small or absent switch costs.

2.6.2 Reverse Stroop interference

Allport et al. (1994, Experiment 5) also included a condition involving sequences of neutral stimuli (colour words shown in black for word reading, and "xxxxx" shown in one of the possible colours for colour naming). By comparing reaction
times to incongruent and neutral stimuli, Allport et al. were able to assess the interference from the other task, both for switch and repeat trials. They found that, on nonswitch trials, there was a large interference effect in the colour-naming task (i.e. a large RT difference between incongruent and neutral trials), and little interference in the word-reading task. This is the usual pattern of results in studies of interference effects in Stroop tasks (MacLeod, 1991). However, on switch trials, interference from colour naming was very much greater in the word-reading task, i.e. a “reverse Stroop effect” was now detectable. This provides evidence for an intrusion of the previous task (colour naming) into word-reading switch trials, supporting task carryover accounts of switch costs.

2.6.3 Effect of prior stimulus congruity

If switch costs reflect a carryover of task set from the previous task, it should be possible to modulate their size simply by changing the nature of the task from which the subject is switching. An experiment by Allport and Wylie (2000, Experiment 1; also reported in Wylie & Allport, 2000, Experiment 1) illustrates the successful modulation of switch costs in this way. In this experiment, subjects again switched between word-reading and colour-naming tasks, and the main focus was the cost of a switch into the word-reading task. The experiment was carried out in three phases. First of all (phase one), subjects switched between word reading and colour naming, using neutral stimuli for both tasks. This gave rise to small, symmetrical switch costs in both tasks. Next (phase two), incongruent Stroop stimuli, instead of neutral stimuli, were used in the word-reading task. However, stimuli in the colour-naming task were kept identical (i.e., neutral). Despite the change from neutral to incongruent stimuli for word reading, switch costs were practically identical in these two phases. However, when subjects were also presented with incongruent stimuli for colour naming,
in phase three, the word-reading switch costs were increased by a factor of almost three. This was despite no change in the word-reading task itself. Thus, Allport and Wylie conclude that it is the task being switched from, and not the task being switched into, which is the primary determinant of the size of switch costs.

2.6.4 Longer lasting carryover effects

The evidence presented so far, supporting the TSI account of switch costs, could be explained by a short-term carryover effect, operating between immediately adjacent trials only. However, other studies have provided evidence for longer lasting carryover effects. For example, in an experiment by Mayr and Keele (2000; see also Mayr, 2002), there were three possible tasks. This permitted a comparison between two types of switch trial: a) a switch into a task different from the one performed on either of the two previous two trials (i.e. an ‘ABC’ sequence) and b) a switch into the same task that was performed two trials previously (i.e. ‘CBC’). In both cases, the final trial involves performance of task ‘C’ preceded by task ‘B’. However, Mayr and Keele (2000) found that RT is slower on the final trial of a ‘CBC’ sequence than the final trial of an ‘ABC’ sequence. They claim that when subjects switch from one task to another, they inhibit the no-longer required task set. Thus, when they switch back into this task (i.e. in a ‘CBC’ sequence) they are slower to respond since the ‘C’ task set is still inhibited. Evidently, this phenomenon (referred to by Mayr and Keele, 2000 as ‘backward inhibition’) can survive at least one intervening trial.

An investigation into the longevity of carryover effects was carried out by Allport and Wylie (2000, Experiment 2; also reported in Wylie & Allport, 2000, Experiment 2). This experiment was based on the earlier finding, described above, that word-reading switch costs are larger when subjects switch from
colour naming with incongruent rather than neutral stimuli (Allport & Wylie, 2000, Experiment 1). Using the alternating runs paradigm, subjects switched between word-reading and colour-naming tasks. Each block consisted of a repeating sequence of 24 neutral trials, followed by 24 incongruent trials, followed by a further 24 neutral trials and so on. Thus, Allport and Wylie were able to examine to what extent the enhanced word-reading switch cost remained, or was dissipated, over the course of 24 trials with neutral stimuli for both tasks. The results indicated that the word-reading switch cost did indeed become smaller over the course of each 24-trial mini-block of neutral trials, but nevertheless it was still enhanced at the end of this mini-block, in comparison with a similar experiment (Allport & Wylie, 2000, Experiment 1) where subjects were presented with neutral trials before being exposed to the incongruent condition. Thus, the carryover effect seems to be relatively long lasting (see Allport et al., 1994, Experiment 4 for further evidence of relatively long lasting carryover effects).

2.6.5 Task set inertia versus task set retrieval

In the TSI version of the task carryover account (Allport et al., 1994), the carryover effect was seen as a persisting activation or inhibition at the level of the task set. As such, it should not be affected by the identity of the stimuli presented in each task, since task sets, by definition, apply to all task-relevant stimuli. However, Allport and Wylie (2000, Experiment 5) provided evidence for a strongly item-specific task carryover effect. Subjects alternated between short runs of colour naming and word reading, but only a subset of the stimuli that appeared in the word-reading task were also presented for colour naming. Thus, it was possible to compare two types of stimuli in the word-reading task: 'primed' stimuli, which had also appeared recently in the colour-naming task, and 'unprimed' stimuli, which were only ever seen in the word-reading task.
Allport and Wylie found that the reaction time to primed stimuli on switch trials was slower than the reaction time to unprimed stimuli, but there was no reliable difference in reaction time between primed and unprimed stimuli on nonswitch trials. Thus, there was a greater switch cost when the stimulus on the switch trial was primed. This finding suggests that stimuli might themselves evoke task sets with which they were recently associated, even when this task set is not appropriate. As a result, Allport and Wylie (2000) have updated the TSI theory of Allport et al. (1994). They propose an alternative task carryover account, according to which stimuli are able to evoke recently associated task sets from memory (this will be referred to as the ‘associative-TST’ theory). When a stimulus appears on one trial, associated with task A, and then reappears on a subsequent switch trial, requiring performance of task B, Allport and Wylie hypothesise that task set A may nevertheless be evoked by the presentation of the stimulus (cf. perceptual ‘trigger conditions’ in Norman & Shallice, 1986). The resulting competition between task sets A and B may lead to an extended response selection process, explaining the enhanced switch cost obtained for word reading when the stimulus was recently presented for colour naming.

2.7 Evidence for extra process accounts

One empirical challenge for task carryover accounts has resulted from the finding that switch costs between tasks of different levels of difficulty are not necessarily ‘paradoxical’ (i.e. larger in the easier task). Monsell et al. (2000) consider a number of cases (involving tasks with different degrees of stimulus-response compatibility, for example) where the switch cost was larger for a switch into the more difficult task. Similarly, Rubinstein et al. (2001, Experiments 3-4) have provided evidence that a switch from classifying stimuli on a more

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3 The idea that stimuli might directly trigger higher-level representations, independently of one's present intentions or even in conflict with them, has also been put forward in the social psychology literature. For examples see Bargh and Gollwitzer (1994) and Shah and Kruglanski (2002).
familiar dimension to a less familiar dimension can sometimes incur a greater cost than a switch in the opposite direction. Thus, it does not seem to be a universal rule, as predicted by Allport et al. (1994), that a switch from a more familiar to a less familiar task yields a smaller switch cost than a switch in the reverse direction. However, this does not strongly challenge task carryover accounts since there is no reason for such accounts to be incompatible with larger switch costs in less familiar tasks.

A more problematic finding for task carryover accounts of switch costs has come from studies using the alternating runs paradigm with more than two trials before each switch. Experiments with run lengths of four (Rogers & Monsell, 1995, Experiment 6) and eight (Monsell, Azuma, Eimer, Le Pelley, & Strafford, ...
1998) trials before each switch have found that the cost of task switching is confined to the first trial of a run (see Figure 2.2). In other words, there seems to be no further improvement in RT after the first nonswitch trial. Rogers and Monsell (1995) argue that if switch costs result from a carryover effect from the previous task, they should dissipate gradually over a series of trials, rather than being eliminated after the very first switch trial. This pattern of results seems more easily explained if switch costs reflect a one-off stage-like process of task set reconfiguration at the beginning of switch trials.

Other studies, in contrast to Rogers and Monsell (1995), have found that RT does speed up over successive nonswitch trials (e.g. Meiran et al., 2000; Salthouse, Fristoe, McGurthy, & Hambrick, 1998). Thus, it is not a universal rule that switch costs are confined to the first trial in a run. Nevertheless, it still appears to be a challenge to task carryover accounts that switch costs can be confined to the first trial in a run, at least sometimes. Similarly, the finding of larger costs of a switch into a dominant task (e.g. Allport et al., 1994), even if it is not universal, appears to challenge extra process accounts.

2.7.1 Multiple 'extra processes'

Most versions of the extra process account have posited more than one extra stage of processing on switch trials. These multiple stage-like control processes are typically assumed to have different characteristics and to be affected by different factors. Thus, support for such models has come from evidence suggesting that certain factors selectively affect specific stages involved in task switching, independently of any effect on the lower-level processes required to perform each task. For example, Rogers and Monsell (1995) posit two independent control processes required to switch tasks: a) an endogenous control process, and b) an exogenous control process.
Rogers and Monsell (1995, Experiment 3) supported this distinction by investigating the effects of manipulating the inter-trial interval in the alternating runs paradigm, and hence affecting the amount of time subjects had to prepare for the forthcoming task. They showed that switch costs were reduced over the first 600 ms or so of this preparation interval (from 207 to 130 ms). However, after a further 600 ms of preparation, a 'residual switch cost' of 115 ms remained. Rogers and Monsell (1995) argue that the reduction in switch costs over the first 600 ms or so of the preparation interval reflects the operation of the endogenous control process. The remaining switch cost at the longest preparation interval is hypothesised to correspond with the time taken by the execution of the exogenous control process, which must await stimulus presentation and is therefore insensitive to the preparation interval.

A similar account has been put forward by Rubinstein et al. (2001), who attribute switch costs to the duration of a 'goal shifting' stage, which can be executed before the arrival of a stimulus, and a 'rule activation' stage, which must await stimulus presentation. In order to support their account, Rubinstein et al. (2001) employ the additive factors logic (Sternberg, 1969, 1998), which seeks to identify separable processing stages by investigating the effects of more than one factor on performance. If the effect of two factors on reaction time is additive, this is taken as evidence that the two factors affect discrete processing stages. If the factors interact, this is taken as evidence that there is at least one processing stage that is affected by both factors. First, Rubinstein et al. (2001, Experiment 1) seek to establish that control processes involved in task switching are independent of the processes involved in basic task performance. They show that stimulus discriminability affects reaction time equally in alternating-task and pure blocks. In contrast, the complexity of the classification rules (unidimensional or bidimensional) in the basic tasks had a greater effect on the completion time of
alternating-task blocks than repetitive-task blocks. Thus, some factors increase RT independently of the cost of task switching, whereas others boost this cost. Rubinstein et al. (2001) conclude from this that there are control processes involved in task switching which are separable from those involved in performance of the component tasks, since basic task performance and the cost of task switching seem to be affected by different factors (but see section 2.10 below).

Next, Rubinstein et al. (2001) attempt to fractionate the switch cost into components corresponding to goal shifting, which is assumed to be affected by the presence or absence of task cueing, and rule activation, which is assumed to be affected by the complexity of the tasks between which the subject must switch. Rubinstein et al. (2001, Experiment 2) show that task cueing and rule complexity have additive effects on switch costs. Thus, they argue that task cueing and rule complexity affect independent stages of task switching. In a related account, Mayr and Kliegl (2000) have proposed that one important process in task switching is the retrieval of the currently appropriate task from long term memory. This plays a similar role to the goal shifting stage in Rubinstein et al.’s (2001) account.

2.8 Hybrid accounts

Monsell et al. (2000, p. 254) have suggested an intimate link between the extra process(es) they claim are required on switch trials and task carryover effects: ‘it is perfectly possible that an extra “control” process is required precisely in order to overcome ... interference, once it arises as the result of the stimulus retrieving a recently activated task-set or the inhibition associated with a recently suppressed task-set.’ According to this suggestion, the switch cost would still measure the duration of an extra process, but this process would be triggered by
a task carryover effect. An alternative possibility is that the switch cost measures a combination of one or more extra control processes and an extended response selection process, caused by interference resulting from a task carryover effect. This possibility has been taken seriously by Meiran (2000a, 2000b; see also Meiran et al., 2000), who has presented a hybrid model involving elements of both task carryover and extra process accounts.

Meiran (2000a, 2000b) proposes that task sets are multifaceted. When both stimuli and responses are bivalent (e.g. in the standard colour-word Stroop task), a stimulus task set (S-Set) is required as well as a number of response task sets (R-Sets), one for each possible response. The S-Set generates a more abstract representation of the stimulus that is biased towards the currently relevant dimension. The R-Sets associate each response with the relevant stimulus feature for the current task. Meiran claims that S-Sets and R-Sets must be updated separately. The S-Set can be updated prior to task performance in a stage-like control process. The operation of this process during the preparation interval gives rise to the reduction in switch costs that is typically observed. The R-Sets, on the other hand, are adjusted by an incremental learning process that takes place after responding, even if they are set incorrectly for the upcoming trial. Since R-Sets will be set incorrectly on a switch trial (if they were used most recently for the other task), this leads to an extended response selection process on switch trials, hence the residual switch cost. Thus, the model attributes the switch cost to both the duration of an extra control process (to update the S-Set) and an extended response selection process resulting from a carryover of R-Set settings.
In order to support this model, Meiran (2000a) first shows that a computational implementation is able to capture a number of empirical phenomena, including the reduction of switch costs by the preparation interval (caused by updating of the S-Set), the existence of residual switch costs (caused by inappropriately set R-Sets), and the increased switch cost with incongruent stimuli (since incongruent trials are more dependent on correct setting of the S-Set than neutral or congruent trials). The model also simulates a finding reported by Rogers and Monsell (1995): repetition of the same response as the previous trial, compared with the production of a different response, leads to faster RTs on nonswitch trials, but slower RTs on switch trials. Meiran (2000a, 2000b) is able to explain this since the R-Set for the repeated response will favour the previous task, whereas the R-Sets for alternative responses will be less biased, or unbiased between the two tasks. Thus, repeated responses will favour the correct task on nonswitch trials, but the incorrect task on switch trials, in comparison with other responses (see Kleinsorge & Heuer, 1999 for an alternative account of this finding). Meiran (2000b) also reports evidence consistent with two predictions of the model: a) that when stimuli are bivalent and responses univalent (i.e. each response associated with only one task), preparation should reduce the switch cost to zero, and b) that when stimuli are univalent and responses are bivalent, there should be a significant switch cost which is relatively impenetrable by preparation. These predictions were made because, in case (a), the switch cost should be caused by an inappropriate S-Set, which may be overcome by the preparation interval, whereas in case (b) the switch cost results from inappropriately set R-Sets, which are relatively difficult to alter during the preparation interval.

2.9 Effects of preparation on task switching
To what extent does foreknowledge of the next task allow subjects to avoid the cost of task switching? This simple question has provoked much controversy. As discussed above, Rogers and Monsell (1995, Experiment 3) obtained a reduction in switch costs over the first 600 ms or so of the preparation interval, although a significant residual cost remained. However, in a similar experiment where the preparation interval varied randomly from trial to trial, rather than being held constant across each block, there was no effect of the preparation interval on switch costs (Rogers & Monsell, 1995, Experiment 2). Using word-reading and colour-naming Stroop tasks, Allport et al. (1994, Experiment 5) obtained only a small, nonsignificant reduction in switch costs over a preparation interval of 1100 ms.

Thus, it is not always possible to obtain any reduction of switch costs with the preparation interval. In those cases where the switch cost has been reduced by a preparation interval, it is not always clear what processes have led to this reduction. As Meiran (1996) points out, in experiments where the task sequence is predictable, increasing the inter-trial interval could reduce switch costs for two reasons: a) passive decay of the previous task set from one trial to the next and b) active preparation for the forthcoming task. Meiran (1996) presented subjects with a random task sequence, where each trial was preceded by a cue indicating the correct task. Since task switching is unpredictable in this paradigm, it is possible to unconfound the length of the preparation interval from the remoteness from the previous trial by inserting a variable delay between the previous trial and the cue for the next trial. Thus, two switch trials can be compared, where the distance from the previous trial is identical, but the amount of time the subject has had to prepare for the required task is not. Under such conditions, Meiran obtained a reduction in switch costs with the longer preparation interval, indicating that advance reconfiguration of task set is
possible, at least to some degree. To complement this finding, in a more recent experiment Meiran et al. (2000, Experiment 1) held the cue-stimulus interval (CSI) constant and manipulated the response-cue interval (RCI). In this case, subjects have an equivalent amount of time for advance preparation on each trial. Nevertheless, extending the RCI led to a small but reliable drop in switch costs, indicating that passive dissipation of the previous task set may also play a role in preparation-related decreases in switch costs.

2.9.1 Residual switch cost

A residual switch is typically obtained, even after preparation intervals as long as 8 seconds (Kimberg, Aguirre, & D'Esposito, 2000), or after subjects have delayed stimulus onset until they feel fully prepared (Meiran, Hommel, Bibi, & Lev, 2002). One explanation of this phenomenon has been that there is a component process in task switching which cannot take place until the first stimulus for the new task has been presented (Rogers & Monsell, 1995; Rubinstein et al., 2001), or after the subject has responded to the first stimulus (Meiran, 2000a, 2000b). Although such explanations are able to explain the data, they are somewhat unprincipled, since they do not explain why it is that the process in question cannot take place before stimulus presentation.

An alternative account of the residual switch cost (the ‘failure to engage’ or FTE hypothesis) has been proposed by De Jong (2000; see also Nieuwenhuis & Monsell, 2002). According to this hypothesis, subjects are either fully prepared for the new task at the beginning of switch trials or they are not at all prepared. Thus, mean reaction times on switch trials mix together two sorts of trial: those
where the subject was fully prepared for the task, the mean reaction time of which is the same as for nonswitch trials, and those where the subject had failed to prepare for the task at all, with a larger mean reaction time incurred by the operation of a control process at the moment of stimulus presentation. As the preparation interval increases, the proportion of switch trials for which the subject is fully prepared increases. This explains the drop in switch costs with the preparation interval. However, the FTE hypothesis proposes that subjects are still not prepared for a large proportion of switch trials, even after long preparation intervals, explaining the residual switch cost.

Evidence for this theory has come from analyses of RT distributions in task switching paradigms. De Jong (2000) shows that it is possible to provide a good fit to the RT distributions on switch trials with long preparation intervals by combining two other distributions, corresponding to a completely unprepared state (based on switch trials with a short preparation interval) and a completely prepared state (based on nonswitch trials after a long preparation interval). De Jong (2000) also demonstrates that when subjects are more strongly motivated to prepare for switch trials (e.g. in shorter blocks of trials), the RT distributions for switch trials with long preparation intervals are best modelled with a higher proportion of fully prepared trials. This hypothesis is attractive since it is able to explain the data with only one postulated control process on switch trials. However, one difficulty is that the proportion of fully prepared switch trials is typically rather low, even after long preparation intervals (e.g. Nieuwenhuis & Monsell, 2002). It seems difficult to provide a convincing explanation for the hypothesised reluctance of subjects to prepare for task switches. Nevertheless, in at least some cases, it does seem possible for the preparation interval to eliminate the switch cost entirely. One example is reported by Tornay and Milán (2001), who employed the same tasks as Rogers and Monsell (1995), odd-even digit
classification and vowel-consonant letter classification. When the sequence of
tasks was predictable (as in Rogers & Monsell’s study), a sizeable switch cost
remained after a preparation interval of 1200 ms. But when the task sequence
was random (as in Meiran’s 1996 study) the switch cost was reduced to zero by a
1200 ms preparation interval. Tornay and Milán (2001) claim that subjects
prepare for a task switch more effectively when the ordering of the tasks is
random. However, other studies involving random switching have found
residual switch costs at long preparation intervals (e.g. Meiran, 1996; Meiran et
al., 2000).

Of course, a version of De Jong’s hypothesis is also compatible with task
carryover accounts: it is possible that subjects are able to engage in an active
process which minimises the carryover effect from the previous trial, but they
may not do so effectively on every trial. According to this version of the FTE
hypothesis it is not necessary to assume that subjects are either fully prepared or
not at all prepared; it is possible that the residual switch cost results from
subjects typically only achieving a partially prepared state on switch trials with
long preparation intervals.

2.9.2 An alternative view of preparation

Each of the studies discussed above has assumed that any reduction of the
switch cost by the preparation interval results from processes which reduce the
disadvantage of switch trials, compared with nonswitch trials. This might result

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4 The carryover effect may be minimised by activating the task set for the forthcoming task and/or
inhibiting the task set for the previous task.
either from active engagement of the forthcoming task during the preparation interval (e.g. Meiran, 1996) or passive disengagement of the previous task (e.g. Meiran et al., 2000). However, a very different view has recently been put forward by Dreisbach, Haider, and Kluwe (2002; see Sohn & Carlson, 2000 for a similar account). According to Dreisbach et al. (2002), preparation leads to an 'expectation advantage' for the forthcoming task, which is potentially of equal benefit for switch and nonswitch trials. However, these authors claim that subjects in task switching experiments implicitly expect task repetition trials. Thus, the manipulation of the preparation interval may have less effect on nonswitch trials, which were (implicitly) expected anyway, compared with switch trials, which are unexpected. In other words, whatever it is that makes switch trials less efficient than nonswitch trials is not affected by the preparation interval. Rather, the preparation interval is able to lead to a general improvement in performance, which is less evident on nonswitch trials since subjects prepare for them by default.

To support this hypothesis, Dreisbach et al. (2002) report a series of experiments where each trial was preceded by a cue which announced the forthcoming task probabilistically. There were four tasks, each associated with a different colour. The task cue consisted of four coloured squares. If all four squares were blue, for example, this predicted the 'blue' task with 100% probability. If three squares were blue and one square was red, this predicted the blue task with 75% probability and the red task with 25% probability. By cueing the task in this manner, Dreisbach et al. (2002) were able to compare the performance of each task when it was predicted with 25%, 50%, 75% or 100% probability. They found that performance improved with increasing probability (i.e. there was an 'expectation advantage'), but this improvement was equal for switch and nonswitch trials. Thus, task preparation speeded responses without influencing
the switch cost. Dreisbach et al. (2002) therefore conclude that switch costs arise from an automatic carryover effect from the previous task that cannot be overcome by advance reconfiguration.

The results from these experiments certainly seem to indicate that there is at least one factor, related to task expectancy, which improves performance without affecting the cost of task switching. This is consistent with the hypothesis put forward by Dreisbach et al. (2002), but it does not establish that the switch cost is always impenetrable by preparation. This strong conclusion rests on the assumption that the probabilistic cueing method employed by Dreisbach et al. (2002) is an unbiased methodology for assessing the effect of the preparation interval on switch and nonswitch trials, whereas other task switching methodologies are biased by subjects' implicit expectation of nonswitch trials. However, in the absence of direct evidence for this assumption, it seems premature to conclude that preparation can never reduce switch costs by leading to an advance reconfiguration of task set.

One relevant finding for the evaluation of Dreisbach et al.'s (2002) hypothesis is that preparation, while reducing switch costs, typically does not seem to reduce the amount of Stroop-like interference, or 'cross-talk', between tasks (e.g. Allport et al., 1994; De Jong et al., submitted). Dreisbach et al. (2002) would be able to explain this finding if they assume that task expectancy, as well as having equal effects on switch and nonswitch trials, also has equal effects on incongruent and

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neutral trials. For alternative accounts which are able to explain this finding, see Rubinstein et al. (2001) and Meiran (2000a). However, a reduction in Stroop-like interference with preparation has been found by other researchers (Goschke, 2000; Sudevan & Taylor, 1987), which is problematic for theories that rule out any effect of preparation on the amount of cross-talk between tasks.

In summary, many unresolved questions remain concerning the effects of preparation on task switching. In particular, it is still not clear whether there is some inexorable limit to the ability to prepare for a switch, or whether it is possible in some circumstances to eliminate the switch cost by preparation. If it is possible to eliminate the switch cost, it is not clear which circumstances promote this possibility. Another outstanding question concerns what subjects actually achieve through preparation (e.g. full reconfiguration of task set, 'expectancy' for the forthcoming task, 'goal shifting', etc.). It does not seem possible at present to arbitrate between task carryover and extra process accounts of switch costs on the basis of the evidence derived from studies of preparation.

It is perhaps not surprising that the nature of preparation for task switching is ill understood, when one considers the number of factors which have been implicated in this process. The role of preparation appears to be affected by the use of random versus regular task sequences (Tornay & Milán, 2001), whether the preparation interval is random or blocked (Rogers & Monsell, 1995), the nature of task cueing (e.g. verbal task cues versus spatial cues, cf. Arbuthnott & Woodward, 2002), whether task cues are probabilistic (Dreisbach et al., 2002), and whether the stimuli and responses are univalent or bivalent (Meiran, 2000b). Few studies have independently manipulated cue-target and response-cue intervals, in order to separate the effects of decay of the previous task set versus
active preparation for the forthcoming task (cf. Meiran et al., 2000). In addition, the number of tasks between which the subject must switch has differed between studies. Whereas most studies have employed two tasks (e.g. Rogers & Monsell, 1995), others have used three (e.g. Arbuthnott & Woodward, 2002) or four (Dreisbach et al., 2002). Finally, there have been differences between studies in the nature of the tasks that subjects perform. Some studies have used simple spatial S-R tasks (e.g. Meiran, 1996), whereas others have used more complex classification tasks, involving simple perceptual classifications (e.g. Rubinstein et al., 2001), simple semantic classifications of single characters (e.g. odd vs. even, or vowel vs. consonant; Rogers & Monsell, 1995; Tornay & Milán, 2001) or more complex semantic classifications of words (e.g. animacy; Mayr & Kliegl, 2000). Systematic investigation of each of these factors, and their interrelations, may result in a better understanding of the complex processes involved in preparation.

### 2.10 Relationship between task switching and executive function research

What has been learned so far about executive function from studies of task switching? Two crucial issues in the study of executive function were raised in Chapter 1. First, to what extent can executive processes be dissociated from the lower-level processes they are assumed to influence? Second, can executive function be fractionated into more elementary cognitive control processes, and if so, what is the nature of those elementary processes? The contribution of task switching research to these two questions will be considered in turn.

Several authors have argued that results from task switching suggest a dissociation between control processes involved in switching and the cognitive
processes involved in performance of the elementary tasks. For example, Rubinstein et al. (2001, Experiment 1) argue that stimulus discriminability affects a stage that takes place on both switch and nonswitch trials, hence the equal effects of this factor on alternating- and repetitive-task blocks. However, since the effects of rule-complexity were larger in alternating-task than repetitive-task blocks, Rubinstein et al. (2001) argue that rule-complexity affects a process that is specific to task switching. The authors conclude that it is possible to distinguish between cognitive processes involved in elementary task performance, which are affected by factors such as stimulus discriminability, and cognitive processes involved in task switching, which are affected by factors such as rule-complexity. However, it is possible to resist this conclusion on the basis that rule complexity, while interacting with the cost of task switching, nevertheless also affected repetitive-task blocks, albeit to a lesser degree. Thus, at least some of the cognitive processes affected by rule complexity evidently play a role in both alternating- and repetitive-task blocks, rather than being specific to task switching.

Other studies have sought to provide evidence for the separability of cognitive processes in task switching and those involved in elementary task performance by investigating task switching performance in populations who have been hypothesised in the past to suffer from a deficit in executive function. Enhanced switch costs have been reported in patients with left-hemisphere frontal lobe damage (Rogers, Sahakian, Hodges, Polkey, Kennard, & Robbins, 1998), patients with Parkinson’s disease (Hayes, Davidson, Keele, & Rafal, 1998), patients with schizophrenia (Meiran, Levine, Meiran, & Henik, 2000), children (Cepeda, Kramer, & Gonzalez de Sather, 2001), the elderly (Kramer, Hahn, & Gopher, 1999), and children with ADHD (Cepeda, Cepeda, & Kramer, 2000). Each of these populations has also been reported to perform poorly on other measures of
executive function. However, the interpretation of these studies suffers from the same problem that was mentioned in relation with the Rubinstein et al. (2001) study: in each case, although the subjects hypothesised to suffer from diminished executive function had larger switch costs, they also performed more poorly on nonswitch trials, or repetitive-task blocks. Thus, insofar as executive mechanisms are implicated in task switching, these studies also suggest a role of the same mechanisms in the performance of the elementary tasks. Consequently, such studies offer little support for the existence of cognitive processes that take place on switch but not nonswitch trials. They are equally compatible with the presence of similar higher-level processes on both switch and nonswitch trials. Switch trials may be more sensitive to the efficiency of these processes than nonswitch trials, as a result of carryover effects, hence the enhanced switch costs in dysexecutive populations.

There is also some positive evidence for interdependence between higher-level and lower-level processes involved in task switching, at least in certain paradigms. Allport and Hsieh (2001; see also Allport et al., 1994) investigated a modified task switching paradigm, where subjects had to monitor a rapid serial visual presentation (RSVP) stream for targets. On some trials, a cue was presented during the RSVP stream, instructing subjects to switch target criterion. Accuracy dropped dramatically immediately after the target-switch cue, and gradually recovered over the course of the next five or so stimuli (in contrast with standard task switching paradigms, where the RT switch cost is generally confined to the first trial in a run). Allport and Hsieh (2001) showed, by varying the rate of the RSVP stream, that the recovery in performance following the target-switch cue was determined by the number of stimuli that had been presented, rather than the amount of time that had elapsed since the cue. Thus, they conclude that, at least in this paradigm, the ‘shift of set appears to be
directly “stimulus-driven” (p. 54), in contrast with the view that control processes are independent of processing in lower-level systems.

How do the control processes involved in task switching relate to other control processes? One approach to this question has been to require subjects to switch between a pair of tasks at the same time as performing an additional concurrent task in order to interfere with specific cognitive processes involved in task switching (this methodology, and its relationship with task switching will be discussed in greater detail in Chapter 5). Results from these dual-task studies have been mixed. Some experiments have found no interaction between task switching and the performance of a concurrent working memory task (Allport & Styles, 1990; Morcom, 2000, Experiment 2) or even reduced switch costs in dual-task conditions (Morcom, 2000, Experiment 3). However, two studies that have investigated the effects of concurrent articulation during task switching (i.e. 'articulatory suppression', see e.g. Baddeley, Eldridge, & Lewis, 1981) have reported enhanced switch costs (Baddeley, Chincotta, & Adlam, 2001; Emerson & Miyake, in press; see also Goschke, 2000, and Hsieh, 2002, for similar findings). Consistent with these findings, a study of patients with frontal lobe lesions (Mecklinger, von Cramon, Springer, & Matthes von Cramon, 1999) found that impaired task switching performance was essentially limited to those with language or speech disorders. Thus, there is some evidence that inner-speech plays a role in the control of task switching performance.

A correlational methodology has also been used for investigations of the relationship between control processes involved in task switching and those involved in other tasks, as discussed in Chapter 1. Ward et al. (2001) found relatively strong correlations between switch costs when subjects alternated
between different pairs of tasks, but little or no correlation between switch costs and the amount of interference in Stroop-like tasks, concluding that the control mechanisms involved in task switching are separable from those involved in the regulation of Stroop interference. Miyake et al. (2000) found that performance on various task switching tasks correlated significantly, and that performance on these tasks correlated moderately with performance on other tasks involving 'Updating' and 'Inhibition'. They also investigated five additional executive tasks: Wisconsin Card Sorting Test (WCST), Tower of Hanoi, random number generation, operation span, and dual tasking. Of these, only the WCST was most strongly associated with task switching performance, rather than Updating or Inhibition. Thus, there is evidence that while the control processes involved in task switching do not overlap entirely with those involved in other executive tasks, nor is there a total separation between the executive processes required in these various tasks.

Similar conclusions might be drawn from functional neuroimaging studies of task switching. Several authors have reported frontal lobe activations in fMRI studies comparing task switches with task repetitions (e.g. MacDonald, Cohen, Stenger, & Carter, 2000; Nagahama, Okada, Katsumi, Hayashi, Yamauchi, Oyanagi, Konishi, Fukuyama, & Shibasaki, 2001; Sohn, Ursu, Anderson, Stenger, & Carter, 2000), suggesting a possible neuroanatomical dissociation between processes involved in task switching and those involved in performing the elementary tasks. Strong evidence for this comes from a combined fMRI and TMS study by Rushworth, Hadland, Paus, and Sipila (2002). In the first phase of the experiment, fMRI was used to localise a region of dorsomedial prefrontal cortex (corresponding to the presupplementary motor area, or pre-SMA) that was activated more strongly on switch than nonswitch trials. Next, the same subjects performed the same tasks with or without TMS applied to this region.
TMS led to slowed reaction times on switch trials, but had no effect on repeat trials. However, other imaging studies (e.g. Dove, Pollmann, Schubert, Wiggins, & von Cramon, 2000) have found that areas activated by a task repetition are activated more strongly on switch trials, without any additional areas being recruited by task switching. A similar result was reported by Kimberg, Aguirre, and D'Esposito (2000): regions activated by task repetitions were activated more strongly on switch trials; in addition, a region in the left superior parietal lobule showed significant switch-related activity.

There has been little anatomical consistency in the results from imaging studies investigating frontal lobe activations in task switching. For example, the results of Rushworth et al. (2002) implicated a region of medial frontal cortex (corresponding to the pre-SMA) in task switching. In contrast MacDonald et al. (2000) identified a more lateral region (BA 9). The loci of frontal activations in task switching experiments may depend on the tasks used. Nagahama et al. (2001) suggested that a region in the inferior frontal sulcus (close to the region identified by MacDonald et al., 2000) is involved in reversing stimulus-response associations, whereas switching attention between stimulus dimensions led to an additional activation in a more anterior and dorsal region corresponding with BA 46. Rushworth et al. (2002) found that TMS over the pre-SMA increased switch costs when subjects had to reverse S-R mappings, but not when they had to switch attention between different stimulus dimensions. Thus, different prefrontal regions have been implicated in different sorts of task switching paradigms. In addition to different regions being activated by different types of task, some authors have claimed that various prefrontal regions play different roles within the same task. For example, in an event-related fMRI study by Sohn et al. (2000), greater activity in BA 46/45 during the preparation period before a switch was associated with faster reaction times, whereas higher activation in
superior prefrontal cortex (BA 8) during the switch period was associated with slower responses. In the study by MacDonald et al. (2000), where subjects switched between Stroop tasks, left dorsolateral prefrontal cortex (BA 9) was more strongly activated during the preparation interval before a colour-naming trial than a word-reading trial. MacDonald et al. (2000) also reported a greater anterior cingulate activation for incongruent than neutral trials; however, there was no difference in anterior cingulate activity during the preparation interval before colour-naming compared with word-reading trials. They concluded that dorsolateral prefrontal cortex is involved in the implementation of control, whereas the anterior cingulate responds to response conflict.

In summary, the neuroimaging data have provided some evidence for separable executive processes involved in task switching (e.g. Rushworth et al., 2002), as well as evidence for common executive processes in switch and nonswitch trials (e.g. Dove et al., 2000). In addition, there is some evidence for a distinction between the executive roles played by various frontal regions (e.g. Sohn et al., 2000). However, it is difficult to draw strong conclusions from these studies since the results have been somewhat inconsistent. This is not altogether surprising, since these studies have used different primary tasks, different task-switching paradigms (e.g. random versus predictable switching) and different data analysis techniques.

2.11 Conclusions

At present, task switching research has suggested many promising avenues for investigations of executive function, but few conclusions have been established. It seems that the control processes involved in task switching are partially, but
not fully, separable both from the lower-level processes involved in task performance and from the control processes involved in other executive tasks. However, these control processes are still ill-understood, making it difficult to study their relationship with executive function more generally. They may include such processes as cueing (including internally-generated linguistic cueing, e.g. Baddeley et al., 2001), holding two or more tasks in working memory, retrieving the now-appropriate task from memory (Mayr & Kliegl, 2000), and ‘reconfiguring’ S-R mappings from moment to moment, which might subdivide into processes of activating the forthcoming task set (Meiran, 1996) and inhibiting the task set that is no longer required (Mayr & Keele, 2000). An additional process of overcoming inhibition associated with a previously abandoned task set may be required (Monsell et al., 2000). And each of these processes may subdivide into ‘endogenous’ and ‘exogenous’ components (Rogers & Monsell, 1995). Even if the switch cost reflects a task carryover effect, top-down biasing of stimulus-response pathways, in accordance with the currently relevant task, would still be an important process (Miller & Cohen, 2001).

If task switching is to be used to further our knowledge of executive function, a more precise understanding will be required of the role (if any) played by each of these cognitive processes in task switching paradigms. A basic problem is that the debate between task carryover and extra process accounts is still unresolved. According to task carryover accounts, the operation of control processes involved in task switching will be revealed indirectly, if at all. In order to make inferences about such processes, therefore, a detailed understanding of the hypothetical task carryover process and its consequences will be required. Alternatively, if a version of the extra process account is correct, the control processes involved in task switching may be investigated more directly, but their
precise cognitive roles will need to be established. According to hybrid accounts, both of these steps will be necessary.

There seems to be little disagreement that task carryover effects have some effect on performance in task switching paradigms. For example, Monsell et al. (2000, p. 254) 'certainly accept that there are relatively long-term carry-over effects of the kind that Allport and colleagues have demonstrated'. However, they are inclined to doubt whether the dramatic improvement in RT from switch trials to immediately successive nonswitch trials can be explained 'even in part' by a carryover effect. Thus, the crucial issue appears to be whether it is necessary to posit one or more extra processes on switch trials in addition to a task carryover effect. A fruitful means of investigating this issue would be to examine the performance of an explicit computational implementation of a version of the task carryover account. If it is able to capture the relevant set of task switching phenomena, it would seem that the extra process(es) invoked by alternative accounts may be unnecessary to explain the cost of task switching.

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6 This would still be compatible with extra process accounts (rather than hybrid accounts) if the carryover effect had a negligible impact on the size of switch costs (i.e. had similar effects on switch and nonswitch trials).
Chapter 3

A PDP model of task switching: Simulations of basic phenomena

3.1 Introduction

Extra process accounts of switch costs, discussed in Chapter 2, claim that a carryover effect is insufficient to explain the cost of task switching. In order to test this claim, this chapter describes an implementation of a version of the task carryover account in a computational model. An existing computational model of task performance in pure-block conditions will be extended in accordance with this account of switch costs. Its performance in mixed-blocks will then be tested against the empirical data. The logic of this investigation is as follows. If an existing computational model of pure-block performance, augmented with the mechanisms required by a task carryover account of task switching, can produce similar behaviour to subjects in comparable experimental conditions, then this task carryover account would be strengthened. Furthermore, if the model is successful, this would indicate that, contrary to the arguments of Rogers and Monsell (1995), Monsell et al. (2000), and Rubinstein et al. (2001), a task carryover account can offer a sufficient explanation of the relevant set of task switching phenomena. Thus, the extra process(es) invoked by alternative accounts may be unnecessary to explain the cost of task switching.

The experimental tasks that will be simulated are word-reading and colour-naming in response to Stroop stimuli. This domain was chosen for three reasons. First, a relatively large corpus of data has been accumulated concerning the effects of switching between dominant and non-dominant tasks on reaction
times, in domains such as Stroop word-reading / colour-naming (Allport et al., 1994; Allport & Wylie, 1999; 2000; Monsell et al., 2000), bilingual language switching (Meuter & Allport, 1999) and pairs of tasks with different S-R compatibility (Monsell et al., 2000). Thus, there is enough data to allow the evaluation of a computational model against the performance of human subjects.

A second reason for this choice is the theoretical weight attached to the finding of 'paradoxical' asymmetric switch costs (Allport et al., 1994; Monsell et al., 2000), which requires a pair of tasks of different 'strengths'. The final reason is that an influential model of the Stroop effect has been developed by Cohen and colleagues (Cohen, Braver, & O'Reilly, 1996; Cohen, Dunbar, & McClelland, 1990; Cohen & Huston, 1994; Cohen & Servan-Schreiber, 1992; see also Phaf, Van der Heijder, & Hudson, 1990; Zhang, Zhang, & Kornblum, 1999). Thus, an existing computational model of pure-block performance is already available.

This strategy is similar to the approach taken by Logan & Gordon (2001), who added control processes to an existing model of visual attention (Bundesen, 1990) in order to simulate situations that require switching between tasks. However, the type of model employed by Logan & Gordon (2001), and the phenomena simulated are rather different from those tackled here, making direct comparison between the two models difficult.

3.2 The model

Although the present model is based on the earlier models of the Stroop task by Cohen, Dunbar, & McClelland (1990) and Cohen & Huston (1994), it also has many modifications. A full description of the present model will be provided below, followed by a brief comparison with the earlier models on which it is based.
The model is based on the interactive activation framework of McClelland and Rumelhart (1981). It is composed of two separate pathways, for word-reading and colour-naming (see Figure 3.1). In each pathway, there are three input units (representing, in the word pathway, the words ‘red’, ‘green’ and ‘blue’ and, in the colour pathway, the colours red, green and blue). In addition, each pathway has three output units, representing the responses ‘red’, ‘green’ and ‘blue’. In other words, each possible response is represented twice, once in the word-reading pathway and once in the colour-naming pathway. Thus, the model has a total of six input units and six output units. Each input unit has a positive connection with its corresponding output unit. For example, in order to simulate a stimulus of the word ‘red’ written in green ink colour, the ‘red’ word input unit and the ‘green’ colour input unit would both be activated. This would send activation to the ‘red’ output unit in the word-reading pathway and the ‘green’ output unit in the colour-naming pathway.

Processing in the model, i.e. the passing of activation between units along their connections, is iterated for a number of cycles. This allows the simulation of reaction time: on each cycle, ‘evidence’ is collected from the activation values of the six output units, two of which represent each possible response, ‘red’, ‘green’ or ‘blue’. When the evidence for one of these three responses passes a fixed threshold, the trial is terminated. In this way, it is possible to compare the number of cycles required for the model to reach its response threshold with the mean reaction time of human subjects.

The connection strengths from the input to the output units are stronger in the word-reading pathway than in the colour-naming pathway. This simulates the greater experience that people have of naming written words than colours. As a result, the word-reading output units become more strongly active than the colour-naming output units when the model is presented with a Stroop stimulus.
Figure 3.1 Architecture of the present model
The evidence for the response represented in the word-reading pathway is therefore greater than the evidence for the response represented in the colour-naming pathway and as a result the model will tend to respond by 'reading out' the word that it is presented with. However, since people are able to name the ink colour of a colour-word, even when the colour and word are incongruent, some mechanism is required to prevent the model from always executing the word-reading task. This is provided by the colour-naming and word-reading 'task demand' units, which send activation to their corresponding pathways. For example, when the colour-naming task demand unit is activated, it sends activation to the output units in the colour-naming pathway, allowing them to win competition with the output units in the word-reading pathway. As well as sending a positive input to the output units of their corresponding pathway, the task demand units also send a negative (i.e. inhibitory) input to the output units of the other pathway.

The word-reading and colour-naming output units also send activation back to the task demand units. This introduces feedback, as well as feedforward connectivity into the model, allowing activity in the colour and word pathways to modulate activity in the task demand units. Similar connections were introduced into the model of Cohen & Huston (1994) in order to simulate phenomena such as attentional capture, where stimuli are able to 'draw attention' to themselves (e.g. Posner, 1980). The task demand units receive an additional 'top-down control input', which specifies which task the model should perform. The word and colour output units are interconnected, so that congruent word and colour response units (e.g. the two 'red' units) have reciprocal positive connections and incongruent pairs of units (e.g. word 'red',

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1 In the present model, the task demand units do not modulate activity in the word or colour input units. However, such perceptual selectivity may play a role in task switching, and could be incorporated into a more complex model.
colour green) have reciprocal negative connections. Finally, there are lateral inhibitory connections between all units within the word output ‘module’ (i.e. set of word output units), colour output module and task demand module. This encourages the network to settle into stable states with no more than one unit active in each module.

### 3.2.1 Comparison with earlier models

Although the model is based on the earlier models of Cohen, Dunbar & McClelland (1990) and Cohen & Huston (1994), its architecture differs from them in two main respects. First, there are three possible words and colours (red, green and blue) in the present model, as opposed to just two in the earlier models. This was chosen because the Cohen et al. (1990) model has been criticised for failing to capture differences between word-reading and colour-naming when the set size is increased beyond two (Kanne, Balota, Spieler & Faust, 1998; but see Cohen, Usher & McClelland, 1998 for a reply).

A second difference is that, unlike the present model, the earlier models included a ‘winner-take-all’ response layer. In the Cohen et al. (1990) model, the units corresponding to the word and colour output units in the present model sent inputs into a pair of response units. Thus the word-reading ‘red’ output unit and the colour-naming ‘red’ output unit sent activation to a single ‘red’ response unit. Evidence was collected from these response units in order to determine when each trial should end. These additional units are unnecessary in the present model: since the word and colour output units are interconnected, there is a beneficial effect of activating congruent output units and a detrimental effect of activating incongruent output units. This plays a role similar to the convergent
inputs into the response units of the Cohen et al. (1990) and Cohen & Huston (1994) models.

3.2.2 Implementation of the task carryover account

The model was extended in two ways in order to implement a task carryover account of task switch costs. First, rather than reinitializing the network at the beginning of every trial, the model was modified so that the state of the task demand units can persist into successive trials. As a result, the most recently implemented task set remains active at the beginning of the next trial and the most recently inhibited task set remains inhibited. This can be seen as an implementation of the hypothesis put forward by Allport et al. (1994) that control states persist, involuntarily, from one trial to the next. The most simple way to implement this would be to start each trial with the task demand units in the state they were in at the end of the previous trial. However, this could well lead to 'perseverative' behaviour, with the model unable to switch from one task into the other. Thus, a 'squashing' parameter was introduced so that the activation levels of the task demand units following each response are squashed, i.e. set to some proportion of their activation levels at the end of the previous trial. Such reductions in activation levels between trials are common in models of sequential processes (e.g. Burgess, 1995; Dayan, 1998; O'Reilly & Farah, 1999), and seem to be biologically plausible. The task demand units are thought to reflect activity in prefrontal cortex (see Miller & Cohen, 2001) and single-cell recording studies of monkeys performing cognitive tasks have demonstrated a sharp reduction in the firing rate of cells in prefrontal cortex following the production of a response (see Fuster, 1997, pp. 121-134; for an example in some ways akin to the task switching paradigm see Asaad, Rainer, & Miller, 2000).
An additional requirement for an implementation of the associative-TSI account, as opposed to the earlier theory of Allport et al. (1994), is some mechanism for individual stimuli to evoke task sets with which they have recently been associated. In order to achieve this, a connection was added from each of the stimulus input units to both task demand units. Thus, stimuli are able to evoke task sets by providing an extra input into the task demand units. The weights of these connections between stimulus input and task demand units are determined by Hebbian learning at the end of each trial, so that the weights between co-active units are adjusted in proportion to the product of their activation values.

One potential danger with this learning algorithm is that the weights between a pair of repeatedly co-active units will grow without bound, which may lead to such a strong input into the task demand units that the model is unable to switch task. In order to avoid this problem, the weights between stimulus input and task demand units are reset to zero at the end of each trial, before the new weights are calculated, so that the effects of learning on trial N persist only for trial N+1. This is a simplifying assumption rather than a theoretical position adopted here. There is strong evidence for such item-specific priming effects lasting for longer than one trial (e.g. Allport & Wylie, 2000, Experiment 5; Waszak, Hommel, & Allport, in press). However, long-term priming effects, though clearly of great interest, are not addressed in this work.

3.2.3 Multiple inputs to task demand units

As well as the control input that indicates which task is to be performed, each task demand unit also receives an input from the stimulus input units, from the colour and word output units and from the other task demand unit. Discussion
of the model’s performance will refer especially to two of these inputs: the control input which indicates which task is currently appropriate (referred to as the ‘top-down input’) and the input that the task demand units receive from the stimulus input units (referred to as the ‘bottom-up input’). One crucial feature of the top-down input is that it is not equal for the two task demand units. It is assumed that the control mechanism provided by the task demand units is required more for the colour than the word task, since the colour naming pathway is weaker (see e.g. LaBerge & Samuels, 1974; Posner & Snyder, 1975 for similar ideas). Thus, the top-down input received by the colour task demand unit, when colour-naming is the required task, is greater than the top-down input received by the word task demand unit on word-reading trials. It will become clear below that this difference in top-down input plays an important role in the model’s behaviour.

3.2.4 Operation of the model

The steps taken to simulate a trial are as follows:

- For trials other than the first, the task demand units are set to a proportion of their activation values at the end of the previous trial. This proportion is set by the squashing parameter discussed above. The activations of the stimulus input and output units are set to zero.

- The appropriate top-down input is added to the net input of the colour or the word task demand unit, depending on which task is required. This input is added to the task demand unit’s net input on every cycle.

- The preparation interval begins. With all of the stimulus input units set to zero, the top-down input is applied to the task demand units for the number
of cycles set as the preparation interval. The activation levels of the output units are not updated during this period.

- After the end of the preparation interval, the appropriate task demand unit is activated as before and either one or two of the stimulus input units have their activation values set to the maximum value, until the end of the trial. In order to simulate neutral trials (affording a response in only one of the tasks, e.g. ‘xxxxx’ written in blue ink, or ‘green’ written black ink), just one of the stimulus input units is activated in this way. Congruent (e.g. ‘red’ in red ink) and incongruent (e.g. ‘red’ in blue ink) trials are simulated by activating two stimulus input units, one in the word-reading pathway and one in the colour-naming pathway.

- Activation is allowed to propagate until a response threshold is reached (see below). The number of cycles since stimulus presentation is recorded as the ‘reaction time’.

3.2.5 Activation level and weight update

Activation levels are determined by the standard interactive activation equations (McClelland & Rumelhart, 1981). On every cycle, the net input for each unit is calculated by summing the activation values of every unit from which it receives a connection, multiplied in each case by the appropriate connection weight. In addition, units in the task demand, word and colour modules each have a bias, a constant which is added to their net inputs on every cycle as well as the inputs received from other units. Each unit’s activation value is then updated according to the following equations:
If the net input is positive:  \( A_{\text{act}} = \text{step} \times \text{net} \times (\text{max} - \text{act}) \)  
(3.1)

If the net input is negative:  \( A_{\text{act}} = \text{step} \times \text{net} \times (\text{act} - \text{min}) \)  
(3.2)

Where \( \text{act} \) = current activation, \( \text{step} \) = step size, \( \text{net} \) = net input, \( \text{max} \) = maximum activation value, and \( \text{min} \) = minimum activation value. The step size parameter determines the magnitude of the change in activation on each cycle, setting the speed of processing. When the activation values have been updated for each unit, the net inputs are calculated again and a new cycle begins. On each cycle, a random noise term is also added to the activation values of each unit. This term is drawn from a normal distribution, with a mean of zero; the standard deviation of this distribution determines how much disruption is caused by noise on each cycle. After noise has been added, the activation levels of any units outside the maximum and minimum values are reset to the relevant extreme.

At the end of each trial, in order to implement the Hebbian learning discussed above, the weights between the stimulus input and task demand units are set according to the following equation:

\[
W_{ij} = \text{lr} \times a_j \times a_i
\]  
(3.3)

In this equation, \( W_{ij} \) is the weight of the connection from unit \( j \) to unit \( i \), \( \text{lr} \) sets the learning rate (i.e. the magnitude of the change in weights for each trial) and \( a_j \) and \( a_i \) are the activation levels of units \( j \) and \( i \) respectively. Note that this equation does not take into account the previous weight of the connection between the two relevant units. Thus, these weights are calculated anew at the
end of each trial, i.e. the weights derived from the activation levels of the units at the end of trial N only affect the model’s behaviour for trial N+1.

3.2.6 Response threshold

The purpose of the response threshold is to provide a way to determine the moment when enough evidence has been accumulated from the word and colour output units for a response to be emitted. This is implemented as follows. At the end of each cycle, the word or colour output unit with the greatest level of activation is compared with each other word and colour output unit, except for the unit which commands the same response in the other stimulus dimension (e.g. the red colour-naming output unit if the red word-reading output unit has the highest activation level). The difference in activation between the most active unit and the next most active unit is calculated. When this difference passes a fixed threshold the trial is terminated. Thus, a response is simulated as occurring when the amount of evidence for that response exceeds the evidence for any other response by a fixed amount. A similar response threshold, based on the amount of evidence for the different responses, was used by Cohen et al. (1990).

3.3 Performance of the model: Simulation of basic phenomena

The parameters, including the connection weights, were initially set by hand and remain fixed for the first set of simulations described below, except where stated (see Appendix A for parameter values). It will be shown later that the model’s behaviour generally remains similar when the parameters are set to random values within a certain range. Thus, the properties of the model result from its
general processing characteristics rather than the specific parameter values that were chosen.

Whenever incongruent stimuli are used in the simulations reported below, the same stimulus is presented on every trial, whichever the task (except where stated). In most experimental studies of task switching, a small number of possible stimuli are used, which seems likely to lead to stimulus-driven priming effects when the stimulus presented on one trial was associated with a different task on an earlier trial. Since the effects of learning only last for one trial in the model, it was necessary in the basic simulations to use the same stimulus on every trial in order to simulate this item-specific effect. This is of course a major simplification, which will be addressed further in a later section.

In each of the following simulations, mean reaction times are based on 50,000 simulated reaction times. For every simulation below where the performance of the model is being compared with a specific set of empirical data, the model's simulations are reported in milliseconds, based on a linear regression of the simulation data (in cycles) on the empirical data (in milliseconds). Where the model is not being compared with any specific data set, simulated reaction times are based on the regression equation derived from the model's simulation of the Stroop interference and facilitation data (see below). Trials where the model produced an incorrect response, or where reaction time was greater than 400 cycles (corresponding to around 2500 msec according to the linear regressions of simulated empirical data) were counted as errors and excluded from the reaction time analyses.
3.3.1 **Stroop interference and facilitation**

The first aspect of the model's performance to be investigated is whether it can accurately predict the reaction times of subjects in pure blocks of word-reading and colour-naming with neutral, incongruent and congruent Stroop stimuli (see MacLeod, 1991 for a review). Cohen et al. (1990) demonstrated that their model was able to simulate three important findings in pure blocks of the Stroop tasks: word-reading reaction times are faster than colour-naming reaction times; the word-reading task interferes more with the colour-naming task than vice versa (i.e. there is a greater difference between neutral, incongruent and congruent trials in the colour-naming task); and the cost of incongruent trials, in comparison with neutral trials, is greater than the benefit of congruent trials.

![Graph showing empirical and simulation data for Stroop interference and facilitation](image)

**Figure 3.2** *Performance of the model in pure blocks of the standard Stroop tasks, with equivalent empirical data, based on Dunbar and MacLeod (1984, Experiment 1B). Simulated RTs = 5.8 * cycles + 318 ms*
Pure blocks of word-reading and colour-naming were simulated, with no preparation interval. As shown in Figure 3.2, the model accurately reproduces all three aspects of the data which were discussed in relation to the Cohen et al. model. Errors were infrequent (< 0.5% for every trial type).

Each of these phenomena can be readily explained in terms of the architecture and processing of the model. Word-reading responses are faster than colour-naming responses because of the stronger connection strengths in the word pathway, which lead to stronger activation of the output units and therefore a faster rate of evidence accumulation. The colour-naming task suffers much greater interference from the word-reading task than vice versa because the connections in the word pathway are sufficiently strong to cause strong activation in the output units, even when the task demand units favour the colour-naming task. The greater cost of incongruent trials, in comparison with the benefit of congruent trials, can be explained in terms of the activation function used (equations 3.1 and 3.2). The effect of a positive net input into a unit is reduced, the higher the activation value of that unit (the same is true of the somewhat different activation function used by Cohen et al., 1990 and Cohen & Huston, 1994). Thus, the greater the positive input to a unit, the less will be the contribution of additional positive inputs. Consequently, adding an extra positive input to an active output unit from a congruent stimulus dimension will have less effect than adding a positive input to a less active (incorrect) output unit from an incongruent stimulus.

The simulation of the Stroop interference and facilitation data is not a demanding test of the present model which, despite several differences, is based on an existent model of the Stroop task (Cohen et al., 1990; Cohen & Huston, 1994).
more interesting question concerns the ability of the model to generalize beyond the mean reaction times found in pure blocks and predict reaction times when subjects switch between the two tasks within the same block.

3.3.2 Task switching in mixed blocks

To test the ability of the model to predict reaction times in mixed blocks of word-reading and colour-naming, the alternating runs paradigm (Rogers & Monsell, 1995) was simulated with a run length of four before each switch of task. If switch costs are found, this would then allow one to investigate whether they are confined to the first trial of a run, as the empirical data would suggest (Rogers & Monsell, 1995, Experiment 6). Again, there was no preparation interval in this simulation.

The mean reaction times of the model when it performs four trials of word-reading, followed by four trials of colour-naming, and then a further four trials of word-reading are illustrated in Figures 3.3a (with item-repetition allowed between the two tasks) and 3.3b (without item repetition). The model was reset after each 12-trial run, so that the mean reaction time on the first word-reading trial was not affected by a previous colour-naming trial. Four features of the model’s performance are noteworthy. First, the model does produce switch costs, that is, switch trials have longer reaction times than nonswitch trials. Second, this switch cost appears to be confined to the first trial, as found by Rogers & Monsell (1995). Inspection of Figure 3.3a shows that there is a very small increase in RT on the first repeat trial compared with the second: 19.7 msec for the word task and 6.3 msec for the colour task. However, this effect is negligible in comparison
Figure 3.3 Performance of the model in a block of 12 trials (a) using the same stimulus throughout and (b) without item repetition. Simulated RTs were calculated with the regression equation used in Figure 3.2.
with the RT drop from the first to the second trial: 273 msec for the word task and 123 msec for the colour task.

The third noteworthy feature of Figure 3.3, especially Figure 3.3a, is that switch costs are markedly asymmetrical: much larger for the word than the colour task. This is the pattern reported by Allport et al. (1994) and Allport and Wylie (2000). The switch cost asymmetry is less extreme in Figure 3.3b, in line with some recent empirical evidence (Waszak et al., in press); this will be discussed below. Finally, the model produces a slightly increased RT on the first trial of the run, even though this does not follow the performance of any other tasks. Such 'restart costs' have been observed by Allport and Wylie (2000) and Gopher, Armony, and Greenshpan (2000).

Errors were relatively infrequent (< 5% in all conditions), but were more common in the colour task than in the word task, and on switch in comparison with nonswitch trials. However, since reaction times have received greater attention than errors in studies of task switching, error rates will not be discussed in the following simulations.

3.4 Explanations of the observed phenomena

3.4.1 Switch / restart costs
In order to understand why the model produces switch costs, one must look at
the role of the task demand units in its behaviour. The task demand units bias
processing in the word and colour pathways so that the intended task is
facilitated (i.e. receives positive connections) and the unintended task is inhibited
(i.e. receives negative connections). Thus, the task demand units help to resolve
competition between the two tasks, leading to one of the output units in the
correct response dimension (colour or word) becoming most active. The model
was run without noise in order to produce Figure 3.4, which illustrates the
activation levels of the two task demand units over the course of a switch and a
nonswitch trial, both for the colour and the word task. The vertical lines indicate
the moment of response execution, i.e. the moment when the difference between
the activation levels of the most active and the second most active output unit
passed the threshold. Although the activation levels after the end of the trial are
shown in Figure 3.4, for illustrative purposes, the next trial immediately follows
response execution when the model is actually run.

On switch trials, the task demand units take longer to reach the activation levels
required to facilitate the intended task and inhibit the unintended one. Thus,
competition between the responses is extended, in comparison with nonswitch
trials, because the task demand units are less effective at biasing the network
towards the correct task. This is for two reasons. First, the activation values of the
task demand units have the wrong sign at the beginning of the trial and so take
longer to reach activation levels which would facilitate the intended task and
inhibit the unintended one. This is caused by the carryover of the task demand
units’ activation values from the previous trial. A second reason for the relative
ineffectiveness of the task demand units on switch trials results from the
network’s learning mechanism and the repetition of the same item for both tasks.
This item-specific effect will be investigated in more detail below.
Figure 3.4 Activation of the task demand units during the course of switch and nonswitch trials, for both the word and the colour tasks. Vertical lines indicate the moment of response execution.
Restart costs arise because, on the first trial of a run, the network lacks the facilitation caused by a carryover of task set from switch to subsequent nonswitch trials. Since they only reflect the absence of a facilitatory effect, rather than being caused by an inhibitory effect combined with the carryover of a competing task set, restart costs are more modest than switch costs.

3.4.2 Switch costs confined to the first trial in a run

A determinant of the size of switch costs in the model is the state of the task demand units at the end of the trial preceding the switch. This determines the magnitude of the carryover of task set: if the task demand units are very active at the end of one trial, this will cause a large carryover into the subsequent trial. As Figure 3.4 shows, although the task demand units have inappropriate activation levels at the start of switch trials, their activation levels become appropriate for the required task by the end of such trials. The stimulus input units become associated with these new values by Hebbian learning. Consequently, the appropriate task demand activations are carried over into the immediately successive nonswitch trial. In other words, the original cause of the switch cost (incorrect task demand unit activations) is dissipated by the end of the switch trial. It therefore does not contribute to successive nonswitch trials in the run. (At the end of a switch trial, the task demand units generally have a weaker activation than they do at the end of a nonswitch trial, since they started with inappropriate activations at the beginning of the trial. As a result, the first nonswitch trial after a switch may receive a weaker carryover of the appropriate
task than subsequent nonswitch trials. This accounts for the very slight improvement which is observed between the first and second repeat trial.)

One might ask whether this account would be tenable in a model where there was long-term learning between the stimulus input and task demand units (to account for long-term item-specific priming effects). Long-term connections between stimulus input and task demand units may contribute to the switch cost, and these connections will not have dissipated by the end of the switch trial. However, it is assumed that the long-term connection weights will have a slow enough decay that the contribution of these connections will not be noticeably different from trial to trial. Thus, the effect of long-term connection weights between stimulus input and task demand units will effectively be constant over successive trials in a run and will not cause a gradual reduction in RT from trial to trial.

Note that the model’s simulation of switch costs confined to the first trial in a run is not dependent on the presence of a learning rule, which lasts for one trial. The same pattern of data is seen in Figure 3.3b, when the model is run with no item repetition (and hence there are no effects of the learning rule). Rogers and Monsell (1995) are therefore incorrect to argue that task carryover accounts of switch costs are incompatible with such costs being limited to the first trial of a run. As Figure 3.3 shows, a model in which the only source of switch costs is a carryover of task set is capable of producing this pattern of data.

3.4.3 Asymmetric switch costs
Examination of Figure 3.4 also reveals a reason for asymmetric switch costs. Comparison of the word and colour switch trials shows that, on the colour switch trial, the task demand units quickly become strongly activated in the appropriate manner for the colour-naming task but on the word switch trial the task demand units continue to favour the colour task until just before the moment of response execution. This is due to the difference in the strength of top-down inputs for the two task demand units: a larger value is added to the net input of the colour task demand unit on colour trials than is added to the net input of the word task demand unit on word trials. Thus, on switch trials, reconfiguration of the task demand units for the intended task is slower for the word-reading task than the colour-naming task, hence the paradoxical asymmetry in switch costs.

It would be problematic if the model rigidly predicted asymmetric switch costs, with a larger cost of switches into the dominant task. As discussed above, a number of experiments have been carried out which have yielded an asymmetry of switch costs in the reverse direction (i.e. greater switch costs for switches into the weaker task; see Monsell et al., 2000). However, it will be shown later that the asymmetry in switch costs observed in Figures 3.3a and (to a lesser degree) 3.3b is sensitive to certain parameter manipulations. Thus, it is possible for the model to produce asymmetric switch costs with a larger cost for switches into the weaker task. The circumstances that give rise to this will be discussed in the following chapters.

3.5 Simulation of additional phenomena
3.5.1 Reverse Stroop interference

One of the pieces of evidence for the task carryover account of switch costs is the 'reverse Stroop' interference seen on word switch trials (e.g. Allport et al., 1994; Allport & Wylie, 2000). On switch trials, word-reading is substantially slower for incongruent than neutral stimuli but the difference is generally negligible on nonswitch trials, suggesting a carryover of the colour task set into word switch trials. This contrasts with colour-naming, which shows a large difference between incongruent and neutral stimuli for both switch and nonswitch trials. As can be seen from Figure 3.5, the model also shows this effect. On word nonswitch trials, the colour pathway is too weak to interfere noticeably with activity in the word pathway. As a result there is little difference in reaction time to neutral and incongruent stimuli on word nonswitch trials. But on word switch trials, the carryover of task set facilitates the colour pathway and inhibits the word pathway. This causes the colour pathway to be strong enough, and the word pathway to be weak enough, for interference to occur, hence the difference in reaction time between incongruent and neutral stimuli on switch trials. When the task is colour-naming, the word pathway is sufficiently strong to cause interference even on nonswitch trials when the colour and word are incongruent. Thus, there is a large difference between reaction times to incongruent and neutral stimuli both on switch and nonswitch trials.

3.5.2 Item specific switch costs

In an experiment which was discussed in the introduction, Allport and Wylie (2000, Experiment 5) investigated the extent to which switch costs might be
Figure 3.5  Simulated and experimental effects of incongruent versus neutral stimuli on switch and nonswitch trials. Empirical data for word reading is based on Allport and Wylie (2000, Fig. 5). Data for colour naming is based on Allport and Wylie (2000, Fig. 1). Simulated RTs = 4.9 * cycles + 288 ms.
contingent on repetition of the same stimulus items between the two tasks. They found that the cost of a switch into the word task was substantially higher if the stimulus on the switch trial had earlier appeared in a colour-naming trial. However, there was no significant difference in reaction time between these 'primed' and 'unprimed' stimuli on nonswitch trials. Thus, there appears to be a strongly item-specific component of the switch cost. It is clear that priming from the colour task cannot affect subsequent word nonswitch reaction times in the model, for the simple reason that learning between the stimulus input and task demand units only lasts for one trial. As a result, even if there is a greater reaction time on word switch trials for primed stimuli (i.e. stimuli just seen in the colour-naming task), this priming will have decayed before the following word nonswitch trial.

This is not a satisfactory account of the interaction between stimulus repetition and switch / nonswitch trials because there is abundant evidence for priming effects that last for more than one trial (Allport & Wylie, 2000, Experiment 5; Waszak et al., in press). The simplifying assumption that learning effects last for only one trial does not allow a test of the model's simulation of the interaction between item repetition and switch / nonswitch trials. Thus, to provide a stronger and more principled test of the model, learning was allowed to last beyond a single trial for this simulation only.

Equation 3.3 was modified so that the weight at the end of the last trial was added to the new weight, i.e. there was no decay in the connection strengths between the stimulus input and task demand units. First, the model was presented with a sequence of four non-repeating incongruent stimuli (referred to
as ‘ABCD’), the first two for colour naming, the last two for word reading, in order to obtain baseline reaction times for unprimed stimuli. Then, starting afresh, an ‘ABBC’ sequence was presented, again with colour-naming for the first two trials and word-reading for the last two. Finally, again starting afresh, an ‘ABCB’ sequence was presented. (As before, each of these simulations was run 50,000 times). By comparing the mean reaction time to the second B stimulus in the ABBC sequence with the C stimulus in the ABCD sequence, it is possible to assess the effect of item repetition on word switch trials. Likewise, comparing the mean reaction time to the second B stimulus in the ABCB sequence with the D stimulus in the ABCD sequence should reveal the effect of item repetition on word nonswitch trials. Since there was no decay in the connection strengths between the stimulus input and task demand units, the distance between prime

\[ \text{Simulated RTs} = 5.6 \times \text{cycles} + 221 \text{ ms.} \]

* In order to obtain these unprimed baseline RTs, Hebbian weights were only allowed to persist for one trial (as in the earlier simulations), and different stimuli were presented on consecutive trials. This was necessary because there are only three possible incongruent stimuli with non-overlapping stimulus representations.
and probe trials had no effect on the model’s behaviour. Figure 3.6 shows the results of these simulations.

The model does an excellent job of simulating the interaction between item repetition and trial type (switch / nonswitch) for word-reading reaction times. Not only does item repetition between the colour and word tasks lead to slower response times in the word-reading task, this effect is essentially confined to switch trials. The explanation is as follows. When a stimulus is presented for colour-naming, the Hebbian learning at the end of the trial leads to a positive connection weights from the stimulus input units to the colour task demand unit, and negative connection weights from the stimulus input units to the word task demand unit. If the same stimulus is presented on a subsequent word-reading trial, the word and colour input units send some activation to the colour task demand unit and inhibition to the word task demand unit. This means that the task demand units take longer to reach appropriate activation values for the word task, since the top-down activation they receive is countered, to some extent, by the inappropriate bottom-up (i.e. stimulus-driven) activation. On nonswitch trials, the task demand units start off favouring the word task and inhibiting the colour task. Since the word pathway is much stronger than the colour pathway, the precise values of the task demand units have very little effect on word-reading trials when they already favour the word task. Thus, an incorrect bottom-up activation of the task demand units on a word nonswitch trial has little effect on the model’s behaviour.

Similar simulations were run to assess the effects of priming from word-reading to colour-naming trials; these revealed two differences between the two tasks. First, although the mean reaction time was slower for primed than unprimed
colour-naming switch trials, this effect was smaller than in the word-reading task. This is because the colour-naming top-down input is stronger, allowing it to oppose the inappropriate bottom-up activation more effectively. Second, unlike the word-reading task, where the effect of item-repetition is very small on nonswitch in comparison with switch trials, the relative size of the effect on colour-naming nonswitch trials, in comparison with switch trials, was greater than half. This is because the colour-naming pathway, unlike the word-reading pathway, is heavily dependent on the task demand units even on nonswitch trials. Support for these two predictions was recently obtained, using a picture-word Stroop task, by Waszak et al. (in press), who found that the effect of item repetition on reaction times in the less dominant picture-naming task was smaller than the effect on word-reading switch trials, and it was just as large on switch and nonswitch trials. Note that the model’s simulation of item-repetition increases the switch cost substantially in the word-reading task, but very little in the colour-naming task where switch and nonswitch trials are affected similarly. This accounts for the greater asymmetry in switch costs between the two tasks when item-repetition is allowed (compare Figures 3.3a and 3.3b), as found by Waszak et al. (in press).

3.5.3 Effects of neutral trials

Many experiments (e.g. Allport & Wylie, 2000, Experiment 4; Rogers & Monsell, 1995, Experiment 1) have found that switch costs are reduced, but not eliminated, when neutral stimuli are used. Inspection of Figure 3.5 shows that the model also produces reduced switch costs when the stimulus is neutral, both in the word and colour tasks. This reduction in switch costs with neutral stimuli can be explained in terms of the earlier explanations of reverse Stroop
interference. Incongruent switch trials are slower than neutral switch trials, but there is a smaller difference in reaction time between incongruent and neutral nonswitch trials. Thus, the difference in reaction time between switch and nonswitch trials (i.e. the switch cost) is larger for incongruent than neutral stimuli. This is because response selection in incongruent trials is particularly slow following a switch of task.

A second example of the effect of neutral trials on switch costs is reported by Allport and Wylie (2000, Experiment 1; also reported in Wylie & Allport, 2000, Experiment 1). In this experiment, the focus was the effect of neutral trials in the previous task on subsequent switch costs, rather than the effect of neutral stimuli on switch trials. Allport and Wylie found that the cost of a switch into the word-reading task was greatly increased when the stimuli in the previous colour-naming task were changed from neutral to incongruent, even though the word-reading stimuli were always incongruent. Similarly, the model’s mean word-reading switch cost increases from approximately 151 to 273 msec when the stimulus for the previous colour-naming trial is changed from neutral to incongruent, using incongruent stimuli for word-reading in both cases.

The main cause of this effect is the absence of item repetition between the two tasks when a neutral stimulus is used. Another contributing factor to the reduced switch cost following the appearance of neutral stimuli in previous trials is that, on neutral trials, a response can be selected before the task demand units have become strongly active. This means that the task demand units have smaller activation values at the end of neutral trials than at the end of incongruent trials. As a result, there is a greater carryover of task set into trials following an incongruent stimulus in the other task than into those following a neutral
stimulus. Thus, even when items are not repeated between the two tasks, the model produces a larger switch cost following incongruent stimuli.

3.5.4 Preparation

Preparation is simulated by activating the task demand units in advance of the stimulus input units, without activating the output units. As Figure 3.7 shows, the preparation interval has a large facilitatory effect on the reaction times for switch trials, but little effect on nonswitch RTs: the preparation interval is therefore able to reduce switch costs, as found by Rogers and Monsell (1995, Experiment 3) and Meiran (1996). In this respect, the model's simulation of the empirical data is successful. However, one aspect of the model's behaviour does not fit the experimental evidence. In the model, the switch cost is entirely eliminated at the longest preparation interval of 150 cycles; this corresponds to an interval of approximately 1200 msec according to the regression equation.
derived from the model's simulation of the Stroop interference and facilitation data. In contrast, some experimental investigations into task switching have found residual switch costs even at preparation intervals as long as eight seconds (Kimberg, Aguirre, & D'Esposito, 2000). This discrepancy between the model's behaviour and the empirical data will be discussed in Chapter 4.

Another potential shortcoming of the model's simulation of preparation is that some authors have reported that preparation, whilst reducing switch costs, does not seem to reduce the amount of Stroop-like interference between tasks (e.g. Allport et al., 1994; De Jong et al., submitted). The model would predict some reduction in such interference with preparation. However, this reduction in Stroop-like interference with preparation has been found by other researchers (Goschke, 2000; Sudevan & Taylor, 1987), so it is not clear whether this should be considered a problem for the model. In fact, the model makes a specific prediction here. As Figure 3.8 shows, the difference in reaction time between incongruent and neutral word-reading switch trials is reduced considerably by the preparation interval. However, there is much less reduction of the difference between incongruent and neutral colour-naming switch trials. The explanation for this is similar to the explanation for the presence of Stroop interference in the colour-naming task even on nonswitch trials. The word-reading pathway is sufficiently strong to cause interference on colour-naming trials, even when there is a long preparation interval. Since previous studies investigating the effects of the preparation interval on congruency effects (e.g. De Jong et al., submitted, Goschke, 2000; Meiran, 1996; Sudevan & Taylor, 1987) have used pairs of tasks of equal strength, rather than dominant and non-dominant Stroop-like tasks, there does not seem to be any empirical data, at present, against which to test this prediction.
3.6 Discussion

The model provides a good fit to many of the phenomena discussed in Chapter 2. It predicts the facilitation and interference effects found in Stroop colour-naming and word-reading, with asymmetric interference between the two tasks and greater interference than facilitation, the existence of switch costs, the asymmetry of those switch costs between dominant and non-dominant tasks, the confinement of those switch costs to the first trial in a run, reverse Stroop interference and its confinement to switch trials, the effects of neutral stimuli on switch costs when they appear in the task being switched from and when they appear in the task being switched into, item-specific priming effects, and the interaction of item repetition with switch versus non-switch trials in the word-reading task. The model also simulates the reduction of switch costs by the preparation interval, although it does not produce residual switch costs at long
preparation intervals. Thus, the simulations reported in this chapter show that the model is able to capture a large body of empirical evidence.

However, since there were many parameters, all of which were set by hand so that the model's performance would approximate the empirical data, it is not clear whether this demonstration necessarily strengthens the theoretical account that the model implements. Although the parameter settings were fixed for each of the simulations above (except where stated), it is possible the model has sufficient flexibility that it could simulate any set of data with the appropriate parameter settings. In this case, the model could not be said to support the version of the task carryover account it implements, since its ability to simulate the empirical data would be based on its specific parameter settings, rather than the general principles that govern its operation. Thus, a full discussion of the model's performance, and the theoretical issues that it raises, will follow an investigation of the model's sensitivity to parameter manipulations in the next chapter.
Chapter 4

Further investigations of the model and implications for the study of task switching and executive function

4.1 Introduction

The model presented in Chapter 3 was able to simulate a large body of empirical data. Of course, this is valuable in itself. It provides a quantitative demonstration that a version of the task carryover account is able to capture the data that the account, expressed verbally (e.g. Allport & Wylie, 2000), was claimed to explain. However, this does not necessarily lend it strong support. In order for a model to provide strong support for a theoretical position, it should be shown not only that the model is able to simulate the relevant set of empirical data, but also that the model is sufficiently inflexible that it rules out patterns of data that are not obtained empirically. As Roberts and Pashler (2000) point out, a 'prediction is a statement of what a theory does and does not allow. When a theory has adjustable parameters, a particular fit is only one example of what it allows.' Thus, 'if a theory does not constrain possible outcomes, the fit is meaningless' (p. 359; see also Cooper & Shallice, 1997). This chapter seeks to show that the model presented in Chapter 3 does indeed 'constrain possible outcomes', before providing a full evaluation of its performance, and considering its implications for theories of task switching and executive function.

4.2 Effects of parameter settings

In order to make sure that the model has the properties that it does as a result of its architecture and general processing characteristics, rather than the specific
parameter settings that were chosen, it is necessary to investigate the effect of those parameter settings on its behaviour. If the model produces a wide range of results with different parameter settings, this would suggest that the successful simulations in Chapter 3 result from the parameter settings that were chosen, rather than the theoretically important aspects of the model such as the way in which the carryover of task set is implemented.

The model was run 1000 times with a random value selected for each weight and each other free parameter on every run, with the exception of minimum and maximum activation values and step size (see Appendix A for a list of parameters and their possible values). Each run consisted of 500 simulated reaction times in each condition, to provide mean reaction times. In keeping with a central assumption of the model, the connections from stimulus input to output units had a greater strength in the word pathway than in the colour pathway, to reflect the greater strength of the word task (Cohen et al., 1990). On each run, two random values were chosen for the connection strengths from the stimulus input units to the word and colour output units. If the value for the colour units was greater than the value for the word units, another pair of values was chosen until the word output units received a stronger connection than the colour output units. Similarly, the level of top-down control input for the colour task demand unit was greater than the level of top-down control input for the word task demand unit.

Before these simulations could be run, it was necessary to establish limits for the random values chosen for each parameter. This was done by taking the model with the standard parameter settings and adjusting each parameter in turn, to establish its maximum and minimum values, outside of which the model
produced unacceptably high error rates. 'Unacceptably high' was defined as an error rate of 20% or more for one or more trial types in a repeating run of word-reading and colour-naming trials using the same incongruent stimulus on every trial.

The level of the top-down control input to the colour task demand unit had no upper limit above which the model's performance deteriorated. It was therefore given a maximum value which made the standard parameter setting exactly midway between the maximum and the minimum values. The noise parameter was allowed to vary between zero and its standard setting. It was assumed that, since the maximum and minimum parameter values were chosen with the noise parameter at its standard setting, making this the maximum noise setting should ensure that the majority of simulations do not produce an excessive number of errors.

4.2.1 Results

Models that produced an error rate of greater than 10% on any trial type were excluded from the following analyses⁹ (as before, reaction times of greater than 400 cycles were counted as errors). This led to 366 simulations out of 1000 being excluded.

Thirteen empirical phenomena simulated in Chapter 3 were categorised into those that relate to performance in pure blocks of the Stroop tasks (A1-A3) and

⁹ When thresholds of 15% or 20% were investigated, the results were virtually identical to those described below.
Table 4.1 13 properties of the model and the percentage of models showing those properties when their parameters are set to random values

<table>
<thead>
<tr>
<th>Property</th>
<th>Percentage of models</th>
</tr>
</thead>
<tbody>
<tr>
<td>A1) Word-reading is faster than colour-naming in pure blocks</td>
<td>88.5</td>
</tr>
<tr>
<td>A2) In pure blocks there is a greater effect of incongruent and</td>
<td>76.3</td>
</tr>
<tr>
<td>congruent stimuli in the colour task than in the word task</td>
<td></td>
</tr>
<tr>
<td>A3) In pure blocks, interference from incongruent stimuli is</td>
<td>66.9</td>
</tr>
<tr>
<td>greater than facilitation from congruent stimuli</td>
<td></td>
</tr>
<tr>
<td>B1) Switch trials are slower than repeat trials</td>
<td>98.9</td>
</tr>
<tr>
<td>B2) Switch costs are confined to the first trial in a run</td>
<td>72.7</td>
</tr>
<tr>
<td>B3) There is a greater cost of switches into the word task than</td>
<td>77.1</td>
</tr>
<tr>
<td>switches into the colour task</td>
<td></td>
</tr>
<tr>
<td>B4) Reverse Stroop interference occurs on word switch trials</td>
<td>100.0</td>
</tr>
<tr>
<td>B5) Less reverse Stroop interference occurs on word nonswitch trials</td>
<td>99.5</td>
</tr>
<tr>
<td>B6) Reaction time is slower on word switch trials when the stimulus</td>
<td>98.3</td>
</tr>
<tr>
<td>was seen on the previous colour-naming trial</td>
<td></td>
</tr>
<tr>
<td>B7) There is a smaller effect of item repetition between the two tasks,</td>
<td>93.9</td>
</tr>
<tr>
<td>or no effect, on word nonswitch trials (for those models with property</td>
<td></td>
</tr>
<tr>
<td>B6).</td>
<td></td>
</tr>
<tr>
<td>B8) Smaller switch costs occur when the subject is presented</td>
<td>99.2</td>
</tr>
<tr>
<td>with neutral stimuli than when incongruent stimuli are used</td>
<td></td>
</tr>
<tr>
<td>B9) Switch costs in the word task, using incongruent stimuli, are</td>
<td>99.8</td>
</tr>
<tr>
<td>greater following colour-naming with incongruent stimuli than</td>
<td></td>
</tr>
<tr>
<td>following colour-naming with neutral stimuli</td>
<td></td>
</tr>
<tr>
<td>B10) A reduction in switch costs occurs with the preparation interval</td>
<td>99.7</td>
</tr>
</tbody>
</table>
those that relate to task-switching in mixed blocks (B1-B10). Table 4.1 shows the percentage of simulations with each of these properties. Operationally, property B2 (switch costs confined to the first trial in a run) was defined as a reduction in RT between the first and the second nonswitch trial that amounted to less than 6.5% of the reduction in RT between the switch trial and the second nonswitch trial, averaged over both tasks (this is approximately the reduction shown by the model with its standard parameter settings). Property B10 was satisfied if switch costs were reduced by a 30 cycle preparation interval.

Binomial tests showed that the models with random parameter settings were more likely to have each of these properties than not (p < .0001 for every phenomenon). Thus, these phenomena are simulated by the model because of its architecture and general processing principles, rather than as a result of any 'curve-fitting' manipulation of the parameter settings\textsuperscript{10}.

Only two of the task-switching phenomena are shown by less than 90% of the models – switch costs confined to the first trial of a run (property B2, shown by 73% of the models) and paradoxical asymmetric switch costs (property B3, shown by 77% of the models). Interestingly, both of these phenomena have not

\textsuperscript{10} Only a subset of the parameter space was tested, in accordance with the assumption that the bottom-up connections should be stronger in the word than the colour pathway and the top-down input stronger for the colour than the word task. It is also important to show that the model fails when these constraints are not met. This can be seen when one considers that the only difference between the word and colour pathways (i.e. the reason for one being called ‘word’ and not the other) is the strength of the bottom-up and top-down connections. Thus, if the constraints concerning the strength of bottom-up and top-down connections were reversed, this would be equivalent to renaming the word task ‘colour’ and the colour task ‘word’ in the present simulations. In this case, the three phenomena that reflect a direct comparison between the two tasks (A1, A2, and B3) would not be shown by the majority of simulations currently showing them.
always been reported in the empirical literature (see Meiran et al., 2000 and Salthouse et al., 1998 for evidence that switch costs need not be confined to the first trial of a run, and Monsell et al., 2000 for evidence that switch costs need not be 'paradoxically' asymmetrical). Thus, the phenomena that the models with random parameter settings are least likely to produce are also those which have not always been observed in the empirical data. In order to investigate further the model's simulation of switch costs being confined to the first trial in a run, a frequency histogram was generated of the drop in reaction time from the first to the second nonswitch trial, as a percentage of the drop in reaction time from the switch trial to the second nonswitch trial (Figure 4.1). As the figure shows, the

![Property B2](image)

**Figure 4.1** Frequency histogram of the reduction in reaction time from the first to the second nonswitch trial, as a percentage of the reduction from the switch trial to the second nonswitch trial.
The majority of simulations were in the bars representing 0-3% and 3-6%, indicating that there was generally little or no further drop in reaction time from the first to the second nonswitch trial (as found by Rogers & Monsell, 1995). However, there were some simulations that showed a larger drop in reaction time between nonswitch trials, and a few that showed an increase in reaction time from the first to the second nonswitch trial.

4.2.1 Parameter settings and asymmetric switch costs

Although it has been shown that the model does not rigidly predict a paradoxical asymmetry of switch costs when the parameters are set to random values, the specific parameter manipulations responsible for this variability in behaviour have not yet been investigated. Only four of the model's parameters have effects which are specific to one of the two tasks: colour and word pathway connection strengths and colour and word level of top-down control input. Thus, any differences between the model's performance in the two tasks must result from differences between the settings of these parameters for the colour and word tasks. As an important assumption of the model, the word pathway has greater connection strengths than the colour pathway and the colour top-down control input is greater than the word top-down control input. In order to investigate the effects of these differences on asymmetric switch costs, Figure 4.2 shows the mean difference between the pathway strengths (favouring the word task) and between the top-down control inputs (favouring the colour task) separately for

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11 There could be additional effects of other parameters on asymmetric switch costs, as long as there is already a difference between the colour and word pathway strengths and/or top-down inputs. However, since the other parameters in the model should apply to a wide variety of tasks, these parameters seem unlikely to be responsible for the finding that some pairs of tasks, but not others, yield a paradoxical asymmetry of switch costs.
Switch Cost Asymmetry

Figure 4.2 Mean difference in the strength of top-down inputs for the two tasks (favouring the colour task) and mean difference between the bottom-up pathway strengths (favouring the word task), plotted separately for models producing paradoxical and non-paradoxical asymmetric switch costs. Error bars represent standard error of the mean.

models which produce paradoxical and non-paradoxical asymmetric switch costs.

The 'paradoxical models' show a greater difference in top-down input than the 'non-paradoxical models', but the non-paradoxical models show a greater difference in pathway strength than the paradoxical models. Thus, non-paradoxical asymmetric switch costs seem to be produced by the combination of a relatively small difference in top-down control input for the two tasks and a relatively large difference in pathway strength.
The effect of top-down control input on asymmetric switch costs makes sense in terms of the earlier explanation of asymmetric switch costs. Since switch trials are slowed by the need to reconfigure the state of the task demand units, a smaller top-down control input will lead to slower reconfiguration of the task demand units and therefore larger switch costs. Thus, a large difference in the level of top-down control input for the two tasks, favouring the colour task, will lead to larger switch costs in the word than the colour task. But this is clearly not the full explanation of asymmetric switch costs. If the only reason for asymmetric switch costs were the difference in top-down control inputs for the two tasks, a paradoxical asymmetry would be expected every time, since the top-down control input is always larger for the colour task.

When the top-down control inputs for the two tasks are equal, but the pathway strengths are different, the model still produces asymmetric switch costs. This time, however, the switch cost is larger for switches into the non-dominant task. Thus, two opposing effects seem to be occurring. When the level of top-down control input for two tasks is different, all else being equal, larger switch costs are produced in the dominant task (i.e. the task with the smaller top-down control input). But different pathway strengths (all else being equal) will produce larger switch costs in the non-dominant task (i.e. the task with the weaker pathway strength). This is because the non-dominant task is more dependent on the task demand units than the dominant task. Thus, the effect of an inappropriate state of activation in the task demand units (the cause of switch costs in the model) is greater for the non-dominant than the dominant task. It appears that the balance between these two forms of asymmetry in task strength is largely responsible for the nature of the asymmetric switch costs. Further simulations and experimental investigations in the following chapters will return to this issue.
In a recent study, Ward et al. (2001) investigated correlations between Stroop interference costs and task switching costs. Large numbers of subjects were presented with lists of colour-word Stroop stimuli and digit-related Stroop stimuli (Morton, 1969). They performed the two tasks afforded by each type of list in either pure or alternating conditions. In addition, subjects performed pure blocks of the harder task in each pair (e.g. colour-naming with the colour-word Stroop stimuli) in both incongruent and neutral conditions. Thus, for each subject, Ward et al. (2001) were able to derive two measures of Stroop-like interference (by comparing incongruent with neutral pure blocks with each type of stimulus) and two measures of task-switching costs (based on the comparison between pure and alternating blocks with each type of stimulus). They reasoned that, insofar as 'Stroop-costs' and 'switch-costs' rely on common executive mechanisms for the suppression of unwanted task-sets, these costs should correlate with each other. Alternatively, if separate executive mechanisms are responsible for the performance of Stroop-like tasks and for switching between such tasks, little or no correlation should be expected between Stroop- and switch-costs. Ward et al. (2001, Experiment 1) found that the switch-costs in the two pairs of Stroop tasks correlated relatively highly with each other (r = .58), yet there was only a weak (but significant) correlation between the two Stroop-costs (r = .12). There was a small (significant) correlation between switch- and Stroop-costs in the colour-word tasks (r = .16), and no significant correlation between switch- and Stroop- costs in the digit tasks (r = -.004). Ward et al. (2001, p.499) conclude on the basis of this that ‘executive control mechanisms responsible for “switching” appear to be distinct from the separate mechanisms underpinning the two different Stroop-costs’. 
This conclusion appears to challenge the model's simulation of colour-word Stroop interference and task-switching, since in both cases the state of the task demand units plays a crucial role. If the task demand units are not strongly activated, there is greater competition between the word and colour pathways, leading to greater Stroop interference. The inappropriate state of the task demand units at the beginning of switch trials is also responsible for the model's simulation of switch costs. Thus, it might seem that the model should produce a strong correlation between Stroop interference and switch costs. In order to investigate this, Stroop- and switch-cost values for each model with random parameter settings were derived in a similar manner to Ward et al. (2001). The Stroop-cost value was calculated by subtracting the mean colour-naming reaction time in pure blocks of neutral trials from that in pure blocks of incongruent trials. The switch-cost value was based on the difference between switch and nonswitch reaction times, averaged across both tasks. The correlation between these two measures in the models with random parameter settings was weak and only marginally significant ($r = .07, p = .079$), in line with the findings of Ward et al. (2001).

To further investigate the relationship between Stroop-costs and switch-costs in the model, the two costs were calculated separately for the word-reading and colour-naming tasks. Switch-costs in the two tasks correlated relatively highly ($r = .41$), but the correlations between the Stroop-costs were modest ($r = .18$). The difference between these correlations was highly significant ($z = 4.32, p < .00001$). The correlations between the Stroop-costs and switch-costs were also modest in each task (word-reading: $r = .08$; colour-naming: $r = .14$). Thus, the model replicates the finding of Ward et al. (2001) that switch-costs correlate relatively
highly across tasks, but Stroop-costs do not, and nor do switch-costs correlate highly with Stroop-costs. However, Ward et al. (2001) studied Stroop-costs and switch-costs in different pairs of Stroop-like tasks, whereas the above analysis compared Stroop-costs and switch-costs for the complementary tasks with colour-word stimuli. Thus, the data from the model are not based on a simulation of precisely the same circumstances that were tested empirically by Ward et al. (2001).

The explanation for this discrepancy between Stroop-costs and switch-costs in the model, despite their common dependence on the state of the task demand units, can be seen by investigating the effects of parameter settings on each cost. Table 4.2 illustrates the correlations between each of the parameters and the Stroop- and switch-costs in the word-reading and colour-naming tasks. For each cost, only the five parameters with the highest correlations are shown (p < .005 for all correlations). The strength of the connections from task demand units to output units has a relatively strong effect on Stroop-costs in the word-reading task, and both switch- and Stroop- costs in the colour-naming task. This seems to explain the small correlations between the Stroop-costs and between the switch- and Stroop-costs in the colour-naming task (r = .18 and .14 respectively). Other than this, the costs share few correlations with each other, or correlate with parameters in opposite directions, except in the case of the two switch-costs, which share three correlations out of five in the same direction. Thus, it is not surprising that the correlation between the switch-costs is relatively high, whereas the other-costs only correlate modestly with each other. The factors that influence the switch-costs (e.g. squashing of task demand units, learning rate between stimulus and task demand units) would seem to affect many different tasks, whereas those that influence the Stroop-costs may be more specific to the individual tasks (e.g. bottom-up colour-naming pathway strength).
Table 4.2 Correlations between the model’s parameters and the size of Stroop- and switch-costs in the two tasks

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Word switch</th>
<th>Colour switch</th>
<th>Word Stroop</th>
<th>Colour Stroop</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Weights:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stimulus input units to word output units</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stimulus input units to colour output units</td>
<td></td>
<td></td>
<td>.220</td>
<td>-.122</td>
</tr>
<tr>
<td>Task demand units to word / colour output units</td>
<td>-.372</td>
<td>-.332</td>
<td>-.302</td>
<td></td>
</tr>
<tr>
<td>Word / colour output units to task demand units</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Connections between colour and word output units</td>
<td>.359</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Within module inhibitory connections</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Other parameters:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bias for word / colour output units</td>
<td>-.138</td>
<td>-.214</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bias for task demand units</td>
<td></td>
<td></td>
<td>-.200</td>
<td></td>
</tr>
<tr>
<td>Top-down control input into colour task demand unit</td>
<td>-.266</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Top-down control input into word task demand unit</td>
<td>-.380</td>
<td>-.398</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Learning rate between stimulus input and task demand units</td>
<td>.253</td>
<td>.189</td>
<td>-.172</td>
<td></td>
</tr>
<tr>
<td>Response threshold</td>
<td>.262</td>
<td></td>
<td>.137</td>
<td></td>
</tr>
<tr>
<td>‘Squashing’ of task demand unit activations</td>
<td>-.472</td>
<td>-.405</td>
<td></td>
<td></td>
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<td>Noise (i.e. standard deviation of the distribution used to provide noise terms added to activation values)</td>
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Ward et al. (2001, p.496) comment that their conclusion is based upon the assumption that 'the shared [executive] processes were the critical factor in determining individual differences in switch- and Stroop-tasks'. However, the analysis above suggests that this assumption may not be valid. Similar executive processes might be involved in both switch- and Stroop-tasks without there being high correlations between the switch- and Stroop-costs.

A similar correlational approach was taken by Kramer, Hahn, and Gopher (1999) in order to provide evidence for the separability of task switching processes from processes involved in the component tasks. Kramer et al. (1999) showed that residual variances in task switching performance, unaccounted for by nonswitch performance, were significantly correlated across experimental sessions and tasks. In other words, the size of the switch cost in one task, or in one experimental session, tended to correlate with the size of switch costs in the other task, or other experimental sessions, even when individual differences in nonswitch performance were controlled for. Kramer et al. (1999) conclude that 'the processes that underlie task switching performance ... are distinct from the processes which underlie the performance of the individual tasks' (p. 357). However, it is possible that, as in the model, the same processes might be involved in both switch and nonswitch performance, with various factors having somewhat different influences on different types of trial.

4.3 Discussion
It has now been demonstrated that not only is the model able to reproduce a large number of empirical phenomena that have been reported in the literature, it is able to do so with a wide variety of parameter settings. Thus, the general principles governing the model’s performance, rather than the specific parameter settings that were chosen for the simulations in Chapter 3, seem responsible for the qualitative aspects of the model’s performance.

4.3.1 Preparation

One shortcoming of the model is its simulation of preparation. While many empirical studies have demonstrated a reduction in switch costs with longer preparation intervals, this reduction is typically imperfect: a significant residual switch cost usually remains (e.g. Allport et al., 1994; Rogers & Monsell, 1995), even after preparation intervals as long as 8 seconds (Kimberg et al., 2000), or after subjects have delayed stimulus onset until they feel fully prepared (Meiran et al., 2002). By contrast, a relatively short preparation interval in the model (corresponding to approximately 1200ms, according to regressions of simulated on empirical data) is able to reduce the switch cost to zero. However, this may reflect a problem with the way that preparation is simulated in the model, rather than a more fundamental weakness. The cognitive processes that take place during a long preparation interval in task switching experiments are likely to be complex, possibly involving recall of the correct task to be performed next (see Mayr & Kliegl, 2000) and perceptual adjustments in preparation for the new task, as well as advance reconfiguration of task set. In addition, Meiran et al. (2000) claim that performance is affected by a number of independent processes such as phasic arousal and prediction of stimulus onset. They conclude that ‘referring to preparation as a unitary process makes little sense’ (p.251). Thus, it is clear that
simply activating the task demand units in advance of stimulus presentation is a very crude simulation of the processes that may be taking place in the preparation interval. If one assumes that the task demand units do not necessarily become fully activated during task preparation in the same way as they do during task performance, then there need be no discrepancy between the model's performance and the existence of the residual switch cost.

Why might it not be possible to activate task demand units during a preparation interval as effectively as during task performance? Three potential answers will be considered below. First, it seems plausible that motor pathways are inhibited during the preparation interval before a response is made (Brunia, 1993). This might also involve the inhibition of abstract motor representations, such as the task demand units in the model. Second, as discussed in Chapter 2, De Jong (2000) may well be correct to claim that subjects sometimes fail to make use of the opportunity to prepare for a new task before the next stimulus arrives. Further evidence for this claim comes from a recent study by Tornay and Milán (2001), which found that when the sequence of tasks was unpredictable, a situation which the authors claim encourages advance task preparation, the switch cost was reduced to zero.

A third, more speculative reason for believing that it may be difficult to update the state of the task demand units during a preparation interval, follows a suggestion made by Cohen et al. (1996) (see also Braver, Barch, & Cohen, 1999; Braver & Cohen, 2000; Cohen, Braver, & Brown, 2002; O'Reilly, Noelle, Braver, & Cohen, 2002; for a somewhat different approach, see Frank, Loughry, & O'Reilly, 2001). These authors discuss a number of models which are computationally similar to the present one, since they are also based on earlier work by Cohen
and colleagues (e.g. Cohen et al., 1990; Cohen & Servan-Schreiber, 1992). What these models have in common is that direct routes from stimulus to response are modulated by activity in ‘context’ units (akin to task demand units), according to the demands of the task. These context units are thought to reflect activity in the prefrontal cortex (PFC; see Miller & Cohen, 2001). Cohen et al. (1996) suggest that the dopamine (DA) neurotransmitter system may play a role in regulating the access of information to PFC (i.e. context units). Specifically, Cohen et al. hypothesised that DA may play a role in a ‘gating’ mechanism so that transient changes in DA activity may modulate the sensitivity of PFC to its inputs, allowing the PFC to maintain its current state of activity without interference in some conditions, and to rapidly update and modify that information under different conditions. Converging evidence that is compatible with this suggestion comes from neurobiological studies of the effect of DA on PFC activity (e.g. Goldman-Rakic, 1992), computational analyses (Hochreiter & Schmidhuber, 1997) and a successful computational implementation of the theory by Braver et al. (1999) which captured some aspects of schizophrenic performance when noise was added to the simulated DA system.

If one assumes that the transient changes in DA activity which are responsible for this gating function are stimulus locked (see e.g. Schultz, 1992), this has the consequence that it will be more difficult to update the information stored in context units (or task demand units) until the DA activity which occurs at the moment of stimulus presentation ‘opens’ the gate. In other words, it will be more difficult to reconfigure the state of the task demand units during the preparation interval, because the gate is still closed, but the DA activity at the beginning of the switch trial will open the gate. Thus, on switch trials, the task demand units can only begin to be fully reconfigured at the moment of stimulus onset, hence the residual switch cost. This explanation of residual switch costs receives
support from two experiments in which switch costs were reduced to zero over
the preparation interval (De Jong et al., submitted; Meiran, 1996, Experiment 5).
In both cases, a cue which was similar in appearance to the forthcoming stimulus
instructed the subject which task to perform. According to the explanation of
residual switch costs sketched above, the cue may have been sufficient for DA
activity to allow new information to be gated into the task demand units, thereby
reducing the switch cost to zero. (Note that a similar explanation is available
under the account put forward by Rogers and Monsell, 1995: the cue may have
been sufficient to trigger the exogenous control process).

4.4 Comparison with other models

4.4.1 Task carryover accounts

The model implements a version of the task carryover account of task switching,
the main principle being that the cost of task switching arises from interference
cau...
input and task demand units allows stimuli to re-evoke task sets, as required by the ‘associative-TSI’ theory of Allport and Wylie (2000).

Both of these mechanisms are necessary for the model to simulate the task switching data. If the model did not incorporate persisting task demand activation from one trial to the next, it would be unable to simulate switch costs when different stimuli are presented in the two tasks, despite the empirical data that switch costs are present even when neutral stimuli are presented (e.g. Rogers & Monsell, 1995, Experiment 4). Even if the stimulus-driven retrieval of task set was modified so that stimuli could evoke task sets associated with similar stimuli (e.g. those presented on the same computer monitor, or in a similar font etc.), this would have very little effect on word reading reaction times. In order for bottom-up activation of the task demand units to have a large effect on word reading reaction times, the task demand units must be in an inappropriate state at the beginning of the trial, as evidenced by the effect of item repetition on word reading switch, but not nonswitch, trials. Thus, the model would not be able to simulate the task switching data if there was no persisting activation in the task demand units from trial to trial. In addition, of course, the Hebbian learning mechanism is required in order to simulate the item specific effects reported by Allport and Wylie (2000, Experiment 5) and Waszak et al. (in press). Thus, both the TSI mechanism suggested by Allport et al. (1994) and the associative-TSI mechanism suggested by Allport and Wylie (2000) are required in order for the model to simulate the task switching data.

One discrepancy between the model and the theoretical position of Allport et al. (1994; see also Allport & Wylie, 1999, 2000; Meuter & Allport, 1999) concerns the explanation of paradoxical asymmetric switch costs. Allport and colleagues
propose the following explanation. The colour task requires strong suppression of word reading, but not vice versa. As a result, the word task is strongly suppressed following a switch from colour naming, generating a large switch cost. However, since the colour-naming task is not strongly suppressed during the performance of word reading, it is possible to switch from word reading to colour naming with little cost.

If, as this explanation claims, word switch trials are slowed by a time consuming process of releasing the word task set from inhibition, why are colour switch trials not slowed equally by a time consuming process of inhibiting the word task set in the first place? It seems that the explanation of asymmetric switch costs put forward by Allport et al. (1994) is tenable only if one assumes that there is some difference between inhibiting a representation and reactivating it, such that the latter is more time consuming than the former (a similar argument is made by Yeung, 1999). In the absence of any independent evidence for this, the model's explanation of asymmetric switch costs, in terms of differences in the top-down inputs for the two tasks, seems preferable. This difference in top-down inputs is independently motivated by the assumption that the colour-naming task requires stronger activation of the task demand units than the word-reading task in order to avoid errors. It also plays a role in another aspect of the model's performance: the larger cost of item-repetition between tasks on word-reading than colour-naming reaction times. The assumption of differential top-down inputs for the two tasks is also supported by recent neuroimaging data. In an fMRI experiment by MacDonald et al. (2000), subjects switched between Stroop word-reading and colour-naming tasks. Greater activation of dorsolateral prefrontal cortex, associated with top-down control (Miller & Cohen, 2001), was found on colour-naming trials, in comparison with word-reading trials. Nevertheless, Allport et al. (1994) could be right that differences in 'competitor-
suppression' exist between the two tasks. Equal excitatory and inhibitory weights from the colour and word task demand units were only implemented as a simplifying assumption.

Yeung (1999) has also put forward a task carryover model of switch costs, implemented as a set of mathematical equations relating reaction time to theoretical variables such as 'task readiness'. Although very different in implementation, this model is similar to the present one at a deeper theoretical level. As in the current model, Yeung's model simulates switch costs as an extended response selection process resulting from a carryover of task set, although Yeung's model incorporates separate response activation and response resolution stages rather than the single response selection stage that takes place in the present model. Yeung's model, like the present one, explains paradoxical asymmetric switch costs in terms of a greater top-down control input for the nondominant task. Unlike the present model, however, the carryover of task set is identical every time the subject switches task (it is a constant added to the 'readiness' of the most recently performed task). One further difference is that there is no mechanism for stimulus-driven retrieval of task set in this model, although it could presumably be accommodated by a small extension of the model. Yeung points out that his model is intended to be 'descriptive rather than explanatory with respect to the nature of the response selection process.' By implementing the present model in a PDP network, it is hoped that the computational principles that may underlie the behavioural effects of task switching have been made more explicit.

4.4.2 Extra process accounts
As discussed in Chapter 2, some theories have explained switch costs in terms of
the duration of one or more stage-like executive control processes that operate
only on switch trials (e.g. De Jong, 2000; Hübner, Futterer, & Steinhauser, 2001;
Kleinsorge & Heuer, 1999; Monsell et al., 1998, 2000; Rogers & Monsell, 1995;
Rubinstein et al., 2001). Hybrid accounts (e.g. Meiran, 2000a, 2000b) explain the
switch cost in terms of both a stage-like control process and a carryover effect,
leading to an extended response selection process on switch trials. However, the
present model indicates that the assumption of an extra process only present on
switch trials is not necessary to account for the findings reported so far. The
crucial difference between the present model and the extra process models
described in Chapter 2 is that, in the present model, precisely the same
computational processes occur on switch and nonswitch trials. The only
difference between the two types of trial is in the initial state of the network.
Thus, the switch cost does not measure the duration of any distinct process.

There have been some attempts to implement extra process accounts (e.g. Kieras
Meyer, Ballas, & Lauber, 2000) and hybrid accounts (Meiran, 2000a) in explicit
computational models. However, these models have so far been used to simulate
rather small sets of empirical data, in the case of Kieras et al. (2000) from only a
single unpublished study. In addition, the sensitivity of these models to
parameter settings has not been investigated, so their robustness has not been
established. Thus, it is difficult, at present, to compare the present model with
implementations of alternative theoretical accounts of task switching.

Some of the proposals discussed above, concerning the failure of the model to
simulate the residual switch cost, appear similar to extra process accounts. In
particular, the proposal of a DA gating system that requires stimulus
presentation for an updating of the task demand units has similarities with the exogenous control process envisaged by Rogers and Monsell (1995). Both accounts entail that a state of unpreparedness for a task switch may persist until stimulus presentation. However, according to the 'gating hypothesis', even if the task demand units cannot be effectively updated until the stimulus has been presented, they are still subject to the same influence from the top-down inputs on switch and nonswitch trials. Thus, it does not seem appropriate to classify this hypothesis as an extra process account.

Of course, it is quite possible that, at least on some occasions, additional processes may take place on switch trials. For example, a time-consuming process of retrieving the now-appropriate task from memory may occur more often on switch trials, in some circumstances (Mayr & Kliegl, 2000). Indeed, one could examine the behaviour of the model when such additional assumptions are made. However, it is clear that the large body of task switching data simulated by the model does not require the assumption of an extra control process on switch trials.

To what extent, if at all, does the evidence from the present model rule out extra process accounts of the switch cost? It was argued in Chapter 2 that the two types of account do not logically exclude one another, but evidence from one type of theory may threaten to make the other redundant. Thus, if it can be shown that evidence taken as supporting extra process accounts can also be explained by task carryover accounts, this would count as strong evidence for the latter type of theory. This is especially so since it seems implausible that extra process accounts would be able to explain all of the data (e.g. paradoxically asymmetrical switch costs) on their own.
The present model simulates at least two findings that have been taken to challenge task carryover accounts: switch costs confined to the first trial in a run (Rogers & Monsell, 1995) and non-paradoxically asymmetrical switch costs (Monsell et al., 2000). It was argued above that the existence of residual switch costs need not challenge the model, if certain additional assumptions are made. Other findings are difficult to simulate with the model in its present form. For example, the interaction between task switching and response repetition (e.g. Rogers & Monsell, 1995) was not investigated above, since in the standard version of the model there was no persisting activation of the word and colour output units, and hence no way to simulate response repetition. In preliminary investigations with such persisting activation allowed, the model produced faster reaction times if the response was repeated, on both nonswitch and switch trials. However, this effect was much larger on nonswitch trials. Thus, the interaction between task switching and response repetition was in the same direction as the empirical data, but the effect of response repetition on switch trials was to speed responses rather than to slow them.

Of more relevance to the debate between extra process and task carryover accounts is the finding from Rubinstein et al. (2001) that stimulus discriminability does not affect the size of switch costs and that ‘rule complexity’ and task cueing have additive effects on switch costs. The factors used by Rubinstein et al. (2001) to affect ‘stimulus discriminability’ and ‘rule complexity’ cannot be simulated with the present model\textsuperscript{12}, so it is difficult to investigate these findings from the theoretical perspective adopted here. However, initial testing

\textsuperscript{12} Stimulus discriminability was manipulated by requiring subjects to categorise stimuli along different dimensions, which were more easily or less easily discriminated. Rule complexity referred to the use of unidimensional or bidimensional classification rules.
with the model showed that reducing the activation level of the stimulus input units had approximately equal effects on nonswitch and switch trials, in line with Rubinstein et al.'s (2001) first finding. In addition it should be pointed out that additive factor effects have in the past been explained with models that do not contain separate processing stages (e.g. McClelland, 1979), so the discovery of such effects does not necessarily imply the existence of separate, discreet stages. In summary, although the model does not simulate all of the data that has been put forward in support of extra process accounts, it is not clear that such data necessarily challenges the model. Thus, the present model offers fairly robust support to task carryover accounts of task switching. It remains to be seen whether an explicitly implemented version of the extra process account can do a better job of simulating the relevant set of data.

4.5 Implications for the study of executive function

One aspect of the simulations reported above that is of direct relevance to the study of executive function is the finding of low correlations between switch costs and Stroop costs in the models with random parameter settings. Empirical studies have also generally reported low correlations between performance in various tests of executive function, both in normal subjects (e.g. Chan, 2001) and patients (e.g. Duncan et al., 1997). Various explanations have been put forward for this, including low test-retest and/or internal reliability of the individual tests (e.g. Miyake et al., 2000; Rabbitt, 1997b), which may be caused by strategic differences between different testing sessions (Stuss et al., 1995). An alternative explanation is that various executive tests may correlate poorly on account of their different non-executive loadings (e.g. Burgess, 1997). In the present simulations, different measures of executive function (i.e. switch- and Stroop-costs in the two tasks) correlated relatively poorly since variability in
performance was caused mainly by parameters relating to lower-level factors such as the bottom-up pathway strengths of the two tasks. Thus, the simulations suggest that the discrepant lower-level demands of various executive tasks may offer a sufficient explanation of their low correlations. However, this does not rule out an additional contribution of other factors, such as low reliabilities of individual tests.

If the cost of task switching results from an interference effect caused by previous performance of a different task, rather than the duration of an executive task reconfiguration process, it may seem that task switching is of little relevance to the study of executive function. However, this is not necessarily the case. For example, it may be possible to attribute new findings in more complex situations to the duration of executive processes that operate in addition to the effects simulated by the model. In addition, even though the model does not simulate the switch cost by equating it with the duration of some control process, control processes (i.e. those processes relating to the task demand units and their top-down control inputs) nevertheless play a crucial role in the model’s behaviour. In fact, the elements of the model can be mapped quite easily onto the framework for understanding executive function that was put forward originally by Norman and Shallice (1986; see Chapter 1 for a full description of the theory). Schemas, in the Norman-Shallice framework, would correspond with the task demand units in the model. Environmental triggering of schemas corresponds with the connections that form between the stimulus input units and the task demand units, and the supervisory attentional system (SAS) corresponds with the top-down control input into the task demand units.
However, one apparent discrepancy is that, whereas in the standard version of the theory the SAS is only operative when nonroutine behaviour is required, the top-down control input into the task demand units is always present. Thus, the present model does not entail a strict division between routine behaviour mediated by contention-scheduling and nonroutine behaviour which also requires the SAS. The model is equally consistent with other models where there is a single action-controlling hierarchy (e.g. Schwartz, Reed, Montgomery, Palmer, & Mayer, 1991). The top-down control input into the task demand units may reflect an input from higher level to lower level schemata.

If the framework of the model is appropriate for understanding the cost of task switching, how might task switching be used to investigate executive function? Of course, the crucial area of ignorance is the nature of the processes represented by the 'top-down control input' in the model (or the SAS in the Norman-Shallice model). How are these control inputs generated? What factors influence their strength? To what extent are the top-down inputs for a variety of tasks generated by the same mechanism, and how does this mechanism relate to other aspects of cognitive control? The model is able to provide an explicit link between these top-down inputs and the expected pattern of switch costs observed in behavioural experiments. Thus, it should be possible to investigate the relationship proposed in the model between the control processes that regulate task switching performance (i.e. the top-down inputs and task demand units) and the patterns of switch costs that they (indirectly) give rise to (see Yeung, 1999 for a similar approach). The following chapters attempt such an investigation.
Chapter 5

The effects of 'divided attention' on task switching

5.1 Introduction

This chapter moves away from the modelling work described in earlier chapters and reports an empirical investigation. Specifically, it will consider the effects of a dual-task paradigm on task switching. If subjects switch between tasks A and B, as in standard task switching studies, but also perform task C concurrently, how is performance on tasks A and B affected? Do switch and repeat trials differ in their sensitivity to concurrent task performance? If so, what conclusions may be derived from this concerning the control of cognitive processes in task switching?

This choice of a dual-task investigation is based on two considerations. First, dual-task studies have been used in the past as a means of investigating executive function in normal subjects. Thus, if task switching is sensitive to the operation of control processes then combining this paradigm with a dual-task paradigm may offer a means of investigating the relationship between various sorts of control process. Second, it is possible to derive a specific prediction concerning the effects of dual-task performance on task switching from the model presented in Chapters 3 and 4. The dual-task paradigm can therefore offer an empirical test of the model.

The plan for the chapter will be as follows. A review of past investigations into dual-task performance will attempt to place the current investigation in context. This will be followed by simulations to illustrate the expected effect of dual-task
performance on task switching, according to the model. An experiment will then be reported to test this prediction.

5.2 Dual task performance: An overview

Early experiments involving dichotic listening (e.g. Cherry, 1953) pointed to a catastrophic drop in performance when subjects shadow one stream of text, presented auditorily to one ear, whilst monitoring a stream of text presented to the other ear for semantic information. Such evidence led to the development of single-channel theories of attention (e.g. Broadbent, 1958), according to which human information processing is severely limited in capacity, as a result of one or more crucial processing stages being limited in the amount of information that can be dealt with. Single-channel theories explain impaired dual-task performance in terms of competition between various tasks for access to a single system that must be shared between all tasks. The specific tasks that the subject is performing will therefore only be relevant to the extent of the dual-task decrement insofar as they make greater or lesser demands on this system. While Broadbent (1958) identified the limited capacity stage as a 'perceptual system', other versions of the single-channel theory claim that a dual-task performance decrement will be detectable when the tasks make greater or lesser demands on a different system or processing stage, such as response selection (e.g. Pashler, 1990; Welford, 1952) or the actual initiation or execution of responses (Keele, 1972).

Alternative theories have explained dual-task interference in terms of competition between various tasks for some resource or capacity that applies to the information processing system as a whole, rather than any particular system or stage in processing (e.g. Kahneman, 1973; Moray, 1967). Kahneman (1973) compares this resource to a power supply that must be shared between several
appliances (hence the use of the term ‘divided attention’ to refer to dual-task studies). If the requirements of two tasks for this general resource are high, performance will deteriorate. Such ‘resource’ theories are similar to single-channel theories in that they assume that various combinations of tasks will be difficult to perform together primarily as a result of common demands on a single resource, which must be shared between tasks of all types.

5.2. General versus specific processing conflicts

The idea that dual-task interference results from competition for a single, general purpose resource was supported by empirical studies showing considerable interference between apparently dissimilar tasks (e.g. Posner & Boies, 1971). However, subsequent experiments demonstrated that dual-task interference can be strongly dependent on the nature of the tasks performed (e.g. McLeod, 1978) and that certain pairings of individually demanding tasks can be combined with little or no drop in performance (e.g. Allport, Antonis, & Reynolds, 1972). A debate has therefore arisen between those that see performance limitations in dual-task paradigms as resulting primarily from competition for a single shared resource or processing stage (e.g. Broadbent, 1958, 1982; Moray, 1967; Norman & Bobrow, 1975; Kahneman, 1973; Pashler, 1984, 1990; Posner & Boies, 1971) and those that see performance limitations as arising primarily out of conflicts resulting from specific pairings of tasks (e.g. Allport, 1980a; McLeod, 1977, 1978; Navon & Gopher, 1979; Navon, 1984). Supporters of ‘multiprocessor’ theories have emphasised the task-dependency of dual task interference (e.g. Allport et al., 1972; McLeod, 1978). However, supporters of ‘general resource’ or single-channel theories have pointed to the almost universal finding of impaired performance on pairs of tasks with no obvious processing requirements in common, and suggested strategic factors which may allow certain pairs of tasks...
to be combined successfully, even if they both rely on a single system (e.g. Broadbent, 1982; Pashler, 1993).

Despite this theoretical controversy, the empirical evidence is relatively clear. There is strong evidence that dual-task interference is much greater when there is an obvious processing conflict between the two tasks. For example, shared input modalities (e.g. Allport et al., 1972) or shared output modalities (McLeod, 1977, 1978; Pashler, 1990) for the two tasks can greatly increase interference. Even when the two tasks have different input and output modalities, dual-task performance can be strongly dependent on the input-output mappings employed. For example, Shaffer (1975) showed that skilled typists were able to simultaneously perform auditory shadowing and copy-typing tasks, but were not able effectively to combine audio-typing with reading aloud. Of course, resource or bottleneck theories of attention might explain such cases in terms of the general difficulty of various input-output mappings. However, Allport (1980b) argues that in cases such as this, the stimuli for one of the tasks may serve as 'calling-patterns' for responses in the wrong output modality. Since auditory input is presumably a strong cue for vocal output, it may be difficult to perform a task with auditory input and manual output at the same time as a different task requiring vocal output. A contrasting view comes from McLeod and Posner (1984) who claim that certain input-output mappings (or perhaps just the single mapping of auditory input to speech output) act as 'privileged loops' which may operate independently of other mappings, with little or no dual-task interference.

Alongside these various demonstrations of the task-dependency of dual-task interference, a number of studies have also documented interference between tasks with no obvious processing requirements in common, though such
interference is typically much smaller than that found when there is an obvious processing conflict. Bourke, Duncan and Nimmo-Smith (1996) investigated the performance of several seemingly unrelated tasks and showed that the size of the dual-task interference effects could be well accounted for by a model that assumed that each task placed a specific demand on a general processing system, no matter which task it was paired with. They argued for a hybrid model, where tasks may compete for general resources when there is no specific processing conflict (giving rise to a small impairment in performance), but where stronger dual-task interference effects may result from combinations of tasks requiring some specific processing resource. Thus, there seems to be a spectrum between the view that all dual-task interference results from competition for a single resource or processing stage, and the view that all dual-task interference results from a conflict in one or more of the processing requirements specific for each task. The much greater dual-task interference effects where there is an obvious processing conflict between the tasks suggests a position towards the latter end of the spectrum.

Shallice, McLeod & Lewis (1985) were able to side-step the issue of general versus specific processing conflicts in dual-task performance, and capitalised on the large difference in performance decrements depending on whether there is a direct processing conflict between the tasks. They were interested in the question of whether there are separate systems for verbal auditory input and phonological output. When a pair of tasks requiring these systems were combined, there was a small but significant single- to dual-task decrement in accuracy (around 10%). However, when pairs of tasks were combined that would be expected to lead to 'structural' interference (i.e. between specific processing systems), there was a much larger single- to dual-task decrement (30-40%). Thus, Shallice et al. (1985) claim that the small dual-task decrement in their first experiment does not result from structural interference, and that auditory input and phonological output
systems are indeed separable. More generally, Shallice et al. (1985) argue that such a methodology offers a means of distinguishing between general dual-task inference effects (i.e. where there are no obvious processing conflicts between the tasks) and specific effects (where the tasks may rely on the same processing subsystem).

5.2.2 Dual-task performance and executive function

In recent years, the literature on the general interference effect in dual-task performance (i.e. where there are no obvious processing conflicts between the tasks) has moved closer to the literature on executive function. Many authors have argued (or assumed) that the reason for interference between pairs of tasks with ostensibly little in common is the common reliance of various tasks on executive control processes. There are at least four arguments that motivate this claim. First, there is a surface similarity between concepts in the executive function literature such as the supervisory attentional system (Norman & Shallice, 1986) or the central executive (Baddeley & Hitch, 1974) and the concept in the divided attention literature of a general pool of resources which contributes to a wide range of tasks (e.g. Kahneman, 1973; Posner & Boies, 1971). In both cases, the postulated system is somewhat underspecified except insofar as it is involved in a very wide range of tasks. Moreover, the task of coordinating dual-task performance is the type of process that a central executive might well be required for (e.g. Baddeley, 1996).

Second, there has been a convergence of interest on the effects of practice in the domains of divided attention and executive function. It has long been recognised that an important determinant of dual-task interference is the amount of practice that the subject has had at the combination of the two tasks. For example, Spelke, Hirst, and Neisser (1976) studied the ability of two subjects to read short stories
while copying a series of unrelated words that were dictated to them. At the beginning of the experiment, their reading speed and comprehension suffered seriously when they copied words at the same time, but after six weeks of practice (a total of about 30 one-hour sessions) they were able to combine the tasks with no detectable drop in reading performance. On the basis of such evidence, Hirst, Spelke, Reaves, Caharack and Neisser (1980) conclude provisionally that 'performance is constrained only by obvious peripheral factors ... and by an individual's willingness to practice' (p.114). The role of practice has also been emphasised in accounts of executive function. For example, Norman and Shallice (1986) argue that their supervisory system is especially called in to play in novel (i.e. unpractised) situations, where there are no strong links between perceptual events and action schemata. Practice also plays a crucial role in the distinction drawn by Shiffrin and Schneider (1977) between controlled and automatic processes: it is assumed that a controlled process can become automatic with practice, if the mapping from input to output is consistent (see also Logan, 1979).

A third reason to link dual-task performance with the study of executive function is that the single- to dual-task performance decrement has been reported to be greater in populations hypothesised to suffer from a deficit in executive function. This excessive dual-task impairment has been reported in patients with Alzheimer's disease (Baddeley, Baddeley, Bucks, & Wilcock, 2001), schizophrenia (Granholm, Asarnow, & Marder, 1996), frontal lobe lesions (Baddeley, Della-Salla, Papagno, & Spinnler, 1997), as well as elderly subjects (e.g. McDowd & Craik, 1988) and children (Gutentag, 1989).

A fourth line of evidence to link dual-task performance with executive function comes from neuroimaging studies that have identified frontal lobe regions associated with dual-task performance (e.g. D'Esposito et al., 1995; Shallice et al.,
1994). However, several recent imaging studies, using a variety of experimental
designs, have failed to activate frontal regions in dual-task conditions that were
not activated during the performance of the component tasks singly (Adcock,
Constable, Gore & Goldman-Rakic, 2000; Bunge, Klingberg, Jacobsen, & Gabrieli,
Thus, the evidence from neuroimaging studies implicating executive function in
dual-task performance is currently weak.

In summary, a variety of arguments, both theoretical and empirical, have
suggested that a reason for a single- to dual-task performance decrement when
the tasks are apparently unrelated is a reliance of both tasks on common
executive resources. This possibility has been recognised even by supporters of
the multiprocessor approach to dual-task performance. For example, Allport
(1980b) suggests that 'aspects of the problem-solving involved in recovery from
error in otherwise very different tasks may well share the same, specialized
processing resources, even though error-free performance of the same tasks
entails no structural competition for specific resources' (p.138).

As a result of this convergence between the divided attention and executive
function literature, several studies have attempted to probe executive control
processes in normal subjects using a dual-task paradigm. One domain in which
dual-task methodologies have been used to investigate executive function is in
the performance of the antisaccade task (Hallett, 1978; see Everling & Fischer,
1998 for a review). This task is suitable for such studies for a number of reasons.
First, since it is a task requiring subjects to behave in a way that conflicts with a
strongly prepotent response (to look toward a peripheral target stimulus with an
abrupt onset), it appears to implicate executive function as defined in most
theories (e.g. Norman & Shallice, 1986). This is strengthened by a number of
studies demonstrating impaired antisaccade performance in dysexecutive
populations such as patients with lesions to dorsolateral prefrontal cortex (Fukushima, Fukushima, Miyasaka, & Yamashita, 1994; Pierrot-Deseilligny, Rivaud, Gaymard, & Agid, 1991) and schizophrenics (Clementz, McDowell, & Zisook, 1994). The antisaccade task is also appropriate for such studies because an elegant control condition is available: prosaccade performance, where the subject simply makes a saccade to the target stimulus. Any experimental manipulation that affects prosaccades and antisaccades equally seems unlikely to be of direct relevance to the study of executive function.

In three studies, antisaccade performance was more severely impaired than prosaccade performance when paired with an "executive" secondary task. Roberts, Hager & Heron (1994) found that antisaccade performance, in terms of both latency and accuracy, decreased when subjects carried out a concurrent mental arithmetic task. There was no effect on prosaccade performance. When subjects performed a shadowing secondary task, rather than the mental arithmetic task, there was only a small reduction in antisaccade accuracy (significantly smaller than the effect of the arithmetic task) and no effect on antisaccade latencies. Thus, it was only the combination of the more demanding secondary task (mental arithmetic) with the antisaccade task that led to a large performance decrement. In a similar study, Stuyven, Van der Goten, Vandierenconck, Claesys, & Crevits (2000) paired an antisaccade task with a random time interval generation (RIG) task (Vandierendonck, De Vooght, & Van der Goten, 1998). They found that antisaccade latencies were slowed to a greater extent than prosaccade latencies. Finally, a study by Mitchell, Macrae, & Gilchrist (2002) found that the accuracy difference between prosaccades and antisaccades correlated with memory load in a $n$-back task, where subjects are presented with a series of letters and have to judge for each letter whether it matches the item that preceded it by $n$ places in the sequence.
Another domain where dual-task methodologies have been used to investigate control processes in normal subjects is episodic memory (e.g. Baddeley, Lewis, Eldridge, & Thompson, 1984; Fernandes & Moscovitch, 2000; Moscovitch, 1994; Shallice, Fletcher, Frith, Grasby, Frackowiak, & Dolan, 1994). Several studies have demonstrated that concurrent task performance has a greater disruptive effect on encoding than retrieval processes (e.g. Baddeley et al., 1984). Moscovitch and Umilta (1991) claim that this is consistent with a model where strategic processes, which are mediated by prefrontal cortex and more important for episodic encoding than most retrieval tasks, are disrupted by dual-task performance.

The control of sequential action has also been investigated with a dual-task paradigm. Humphreys, Forde, & Francis (2000) asked normal subjects to perform everyday tasks. Simultaneously they either performed a simple verbal rehearsal task (repeating the word "the" aloud as quickly as possible) or a ‘verbal trails’ task, where the experimenter named an arbitrary letter and number pair (e.g. “D7”) and asked subjects to continuously shift both the letter and the number in sequence (“E8,” “F9,” etc.). They found that subjects rarely made errors whilst carrying out the rehearsal task, but made significantly more action errors (especially step omissions, but also perseverations and step additions) whilst performing the verbal trails task. These errors resembled those made by patients with action disorganisation syndrome (ADS; e.g. Schwartz et al., 1991).

One final example of the use of a dual-task paradigm to probe executive function in normal subjects comes from a study by Baddeley, Emslie, Kolodny, & Duncan (1998). Subjects generated sequences of random keypresses at the same time as performing concurrent tasks. Concurrent performance of a verbal trails task led to much greater deviations from randomness (i.e. stereotyped sequences) than other tasks (e.g. digit span). Baddeley et al. (1998) argue that the necessity of
switching between incrementing letters and numbers in the verbal trails task interferes with the necessity of switching between various strategies to produce random keypress responses.

In evaluating these studies that seek to investigate executive function with a dual-task paradigm, the problem that Shallice et al. (1985) were able to avoid reasserts itself. These studies seek to attribute dual-task performance decrements to the common reliance of various tasks on general mechanisms of executive control (i.e. mechanisms that can apply to a variety of tasks). However, it is difficult to rule out the possibility that these various combinations of tasks interfere for reasons which in each case apply only to the specific combination of tasks (Allport, 1980a; Navon, 1984). Shallice et al. (1985) were able to avoid this problem by comparing various pairs of tasks: two pairs where structural interference could be assumed in the processing mechanisms of interest, leading to a large dual-task decrement, and one pair where the theoretical issue was whether structural interference would occur. Since the dual-task decrement was considerably smaller in the latter case, they were able to argue that the tasks did not cause structural interference and therefore relied on separate mechanisms. However, it is not possible to find one task that can be assumed to cause structural interference at the level of executive function, and compare it with a task that does not cause such interference: it does not seem possible to assume that any task is completely free of any reliance on executive function.

An alternative approach is to compare the dual-task interference caused by two tasks that differ in the extent to which they can be assumed to rely on executive function. Unfortunately, the reliance on executive function in these studies tends to be confounded with absolute task difficulty. The tasks used to place a demand on executive function tend to be more difficult than the control tasks, as evidenced by their poorer performance even when performed singly. Thus, the
results of these studies could just as well be summarised as follows: when two difficult tasks are performed concurrently, there is a greater performance decrement than when one of the tasks is performed with an easier task, or when two easier tasks are performed together. There is no direct evidence to link this finding with the demands each task is assumed to place on the subjects' control processes.

As well as theoretical difficulties in the link between dual-task studies and executive function, there are also empirical problems. For example, Hegarty, Miyake & Shah (2000) compared the performance of three tasks, paired with a variety of secondary tasks. The three primary tasks were the 'paper folding test', where participants were required to mentally fold a piece of paper, imagine a hole punched through the folded paper, and then judge what the paper would look like when unfolded; the 'card rotations test', where participants viewed a two-dimensional target figure and judged which of the test figures were planar rotations of the target figure (as opposed to its mirror image); and the 'identical pictures test', where participants viewed a target figure and judged which one of five alternative test figures was identical to the target figure.

Hegarty et al. (2000) argued that executive processes are most strongly implicated in the paper folding test and least strongly implicated in the identical pictures test, on the basis of theoretical arguments and correlations with other measures of executive function. However, when each of the three primary tasks described above were combined with a variety of secondary tasks, they found that the dual-task decrement was greatest in the identical pictures test and least in the paper folding test, with the card rotations test showing an intermediate decrement. They interpreted these results in terms of two factors which may influence dual-task interference in addition to the executive requirements of each task: response selection requirements and the possibility of a strategic trade-off.
between primary and secondary tasks. Since subjects make more responses in the identical pictures test than the paper folding test, Hegarty et al. claim that it suffers a greater dual-task decrement as a result of a response selection bottleneck. Additionally, Hegarty et al. argue that subjects are less willing to allocate resources to a secondary task when the primary task is more difficult (e.g. the paper folding test). Thus, according to the authors, easier tasks may suffer greater dual-task decrements because subjects are more willing to allocate resources away from them.

5.2.3 Dual-task performance and task switching

The rationale for a combination of a dual-task paradigm and task switching may now be made clearer. Consider the ways in which it might be possible to simulate dual-task interference in the model, supposing that the secondary task does not share inputs or outputs with the Stroop word-reading and colour-naming tasks, and does not involve linguistic materials. In this case, there seems to be no reason to assume that the secondary task will cause interference in the bottom-up word-reading and colour-naming pathways. Thus, it seems reasonable to assume that any interference caused by performance of a secondary task will mostly affect the task demand units and their top-down inputs, i.e. there may be interference at an executive level.

A crucial assumption of the model was that the top-down input is greater for the colour-naming than the word-reading task. It is therefore plausible that if the task demand units are affected by a secondary task, the strong input into the task demand units required for colour-naming will be more disrupted than the weak input required for word-reading. Thus, the most plausible way to simulate dual-task interference in the model, assuming that the secondary task involves different input and output modalities and does not involve linguistic materials,
is to reduce the top-down input for the colour-naming task. It is assumed that the
top-down input for the word-reading task is sufficiently weak that it will not be
affected by interference from the secondary task.

It is now possible to produce a testable prediction from the model, in conjunction
with the assumptions outlined above, concerning the effects of secondary task
performance on switching between Stroop word-reading and colour-naming
tasks. The simulations in Chapter 4 demonstrated that the switch cost asymmetry
is sensitive to the difference in top-down input between the word-reading and
colour-naming task. If secondary-task performance selectively interferes with the
colour-naming top-down input, this should therefore reveal itself by a
modulation of the switch cost asymmetry. Further simulations below will
illustrate the expected effect. Thus, if secondary task performance affects the
switch cost asymmetry between word-reading and colour-naming tasks, this
would provide strong evidence that the secondary task is causing interference
with the control processes that support performance of the Stroop tasks by
leading to activation of task-controlling representations (represented by task
demand units in the model).

Such a demonstration would represent a methodological advance over previous
investigations of executive function using dual-task paradigms, since a specific
pattern of data is now associated with interference at an executive level. In
previous studies, hypothesised interference between control processes involved
in two simultaneously performed tasks was measured simply by a large dual-
task performance decrement. However, as argued above, such a performance
decrement could arise from factors other than interference between control
processes involved in the two tasks. A demonstration that dual-task performance
can alter the switch cost asymmetry between word-reading and colour-naming
tasks would therefore offer more convincing evidence that the secondary task
was interfering specifically with control processes. A modulated asymmetry of switch costs in dual-task conditions could be considered to be a ‘signature’ pattern of data, indicating that interference is occurring between higher-level processes involved in task switching and in the secondary task.

5.3 Simulations

In order to establish the consequences of reducing the colour-naming top-down input in the model, it was run three times with the strength of the colour-naming top-down input at various levels (15, 11.5, and 8). The word-reading top-down input was held constant at 6 for each of these simulations. In order to increase the

| Colour-naming top-down input | 15 | 11.5 | 8 |

![Graph showing the simulated effects of reduced top-down inputs in the colour-naming task on reaction times. Note: NS = nonswitch trials; Sw = switch trials.](image)

**Figure 5.1** Simulated effects of reduced top-down inputs in the colour-naming task on reaction times. Note: NS = nonswitch trials; Sw = switch trials.
range of top-down inputs that could be investigated without the model producing an excessive number of errors, the noise parameter was reduced in these simulations from 0.006 to 0.005. Each simulation consisted of 50,000 repetitions of a four-trial sequence: two colour-naming trials followed by two word-reading trials, using the same stimulus throughout. The results of these simulations are illustrated in Figure 5.1.

As Figure 5.1 shows, decreasing the colour-naming top-down input increases the switch cost in the colour-naming task, as one would expect given the role that the top-down input plays in reconfiguring the task demand units on switch trials. In addition, there is a slight reduction in the word-reading switch cost when the colour-naming top-down input is reduced. This presumably results from a reduction in priming from the colour-naming to the word-reading task when the colour-naming top-down input is reduced. With a smaller top-down input, the colour-naming task set is established less fully (i.e. the colour-naming task demand unit becomes less strongly activated), making it easier to switch back into the word reading task. Decreasing the colour-naming top-down input also led to an increased error rate in the colour-naming task, especially on switch trials.

5.4 Previous dual-task investigations of task switching

Several recent studies have investigated the effects of concurrently performing a secondary task whilst switching between a pair of primary tasks (Baddeley et al., 2001; Emerson & Miyake, in press; Goschke, 2000; Hsieh, 2002; Morcom, 2000). Baddeley et al. (2001) and Emerson and Miyake (in press) report studies in which subjects worked through lists of digits, performing addition or subtraction tasks in pure or alternating conditions. In both studies, it was found that the difference in mean completion time between pure and alternating blocks was enhanced
when subjects simultaneously performed an articulatory suppression task, in comparison with single-task conditions (i.e. when there was no secondary task). A similar result was reported by Hsieh (2002); the difference between the mean completion time of pure and alternating blocks of a shape-matching task (where the shapes could be matched on various dimensions) was increased when subjects concurrently performed a verbal serial subtraction task. Thus, these authors conclude that performance of a secondary task may interfere with executive processes involved in task switching\textsuperscript{13}.

These studies are difficult to interpret, since pure and alternating blocks differ in several ways beyond the presence or absence of the requirement to switch tasks (Rogers & Monsell, 1995). Thus, it is not clear in these studies that the secondary tasks interfered with task switching, rather than some other process that differed between the pure and alternating blocks (e.g. maintenance of the instructions for more than one task), which may potentially have equal effects on switch and nonswitch trials. The results from studies that have investigated the effects of dual-task performance on switch and nonswitch trials within the same block have been less clear. One finding that is consistent with the results described above was reported by Goschke (2000), using primary tasks requiring the classification of the identity or colour of a letter. Switch costs were increased when subjects articulated irrelevant words during the preparation intervals (e.g. ‘Monday’), in comparison with conditions where they said a word naming the task to be performed (‘colour’ or ‘shape’), or said nothing at all. But in a study by Morcom (2000, Experiment 2), where the primary tasks involved classifying different dimensions of visual stimuli, there was no effect of an externally-paced serial addition task on switch costs, since performance on switch and nonswitch

\textsuperscript{13} Emerson and Miyake (in press) also investigated a foot tapping task, and Baddeley et al. (2001) investigated a verbal trails task. The foot tapping task had no effect on the difference between pure and alternating blocks; the verbal trails task had an even larger effect than the articulatory suppression task.
trials was slowed equally. In a further experiment (Morcom, 2000, Experiment 3), where the task was cued less explicitly (with 'switch' or 'stay' cues, rather than cues indicating the correct task), performance of the secondary task led to a reduction in switch costs, since nonswitch trials were slowed to a greater extent than switch trials.

In summary, there is only weak evidence that performance of a secondary task can increase switch costs when switch and nonswitch trials are compared within the same block. The studies described above are not suitable for investigating the effect of secondary task performance on the switch cost asymmetry, since tasks of similar strengths were used. Stroop-like tasks were used in a study by Allport and Styles (1990). Subjects performed these tasks in pure or alternating blocks, with or without a verbal working memory load. Allport and Styles (1990) found no effect of the memory load on the alternation cost. However, this study measured the completion times of these blocks, rather than individual reaction times. Thus, it is not possible to determine whether there was any effect of the memory load on the switch cost asymmetry, since results were averaged over the tasks.

5.5 Experiment 1

The purpose of this experiment was to test the prediction, based on the simulations reported above, that performing a secondary task at the same time as switching between Stroop word-reading and colour-naming tasks should affect the asymmetry in switch costs between the two tasks, so long as there are no obvious processing conflicts between the Stroop tasks and the secondary task. Specifically, it was predicted any influence of secondary task performance on task switching should reveal itself as an increase in the colour-naming switch cost, relative to the word-reading switch cost.
The secondary task chosen was a ‘1-back’ memory task, with auditory input and motor output. Subjects were presented with a sequence of auditory tones of various pitches and made a response if the stimulus matched the previous one. This task was suitable for three reasons. First, since this task did not involve any linguistic materials and had different input and output modalities to the Stroop tasks, there seemed to be no obvious processing conflicts between the low-level features of each task. Second, since the 1-back task is paced by external stimuli, it was possible to arrange the timing of these so that subjects did not make manual responses at the same time as they made vocal responses to the Stroop tasks. Thus, it was possible to ensure that the effect of the secondary task was not caused by a response-selection bottleneck. Finally, since n-back tasks have been linked to executive function both behaviourally (Mitchell et al., 2002) and neuroanatomically (Braver, Cohen, Nystrom, Jonides, Smith, & Noll, 1997), it seemed plausible that the 1-back task would cause interference with the executive demands of switching between the Stroop tasks.

This experiment also investigated item-specific priming effects (e.g. Allport & Wylie, 2000, Experiment 5; Waszak et al., in press). There was not complete overlap between the stimulus sets for the two tasks, so that a comparison could be made between responses to stimuli that were associated with just one of the tasks and those that were associated with both tasks. It was predicted that if the dual-task condition increases the switch cost in the colour-naming task, it should also increase the difference in reaction time between ‘competitor primed’ stimuli (i.e. those that could appear in both tasks) and ‘unprimed’ stimuli (i.e. those that appeared just in the colour-naming task).

5.5.1 Method
Subjects  There were 22 paid subjects, 15 female, aged 18-28 (mean: 20.9).

Apparatus  Stimuli were presented on a 15” VGA monitor, controlled by an IBM-compatible PC. Responses were collected by means of a voicekey and a response-box, both of which were connected to the serial port of the PC.

Tasks  Subjects sat approximately 60cm from the monitor, with the voicekey and response box immediately in front of them. The monitor displayed a light grey screen, bisected horizontally by a thick black line. For half of the subjects, the word ‘word’ was presented at the top of the screen, double-spaced horizontally, in 30 point bold black Ariel type, and a similarly sized colour-spectrum was displayed at the bottom of the screen. The other subjects were shown the word at the bottom of the screen and the colour patch at the top. These stimuli remained on screen from the start to the end of every block of trials.

Single-Task Condition  The beginning of each trial was marked by a horizontally centred dark grey rectangle that appeared just above or just below the horizontal line (see Figure 5.2). After 500 ms, the stimulus was presented at the centre of the grey rectangle. The stimulus always consisted of one of the words ‘red’, ‘green’, ‘blue’, ‘yellow’, ‘orange’, ‘pink’, ‘purple’ or ‘brown’ and was printed in one of those colours in 46 point bold Ariel type. Stimuli were always incongruent (i.e. the word ‘red’, for example, could not be printed in red). Subjects were instructed to read out the word if the stimulus appeared above the horizontal line and name the colour of the word if it appeared below the horizontal line (or vice versa for those subjects who were presented with the colour patch stimulus at the top of the screen and the ‘word’ stimulus at the bottom of the screen). As soon as the voice-key detected the subject’s response, or after 3000 ms if no response was detected, the grey rectangle and Stroop stimulus were removed from the screen. There was then a 1250 ms delay before the grey rectangle cueing
Figure 5.2 Sequence of events in one trial of the word-reading task in Experiment 1. If the task was colour naming, the grey rectangle would have appeared below the horizontal line.

The next trial appeared. The position of the rectangle (and hence the required task) was completely predictable. It appeared twice above the line, then twice below the line, then twice above the line and so on.

Dual-Task Condition The dual-task condition only differed from the single-task condition in the following way. 450 ms after each response was made by the subject, a tone was played through a pair of headphones. The tone had one of three pitches: low (80 Hz), medium (320 Hz), or high (1280 Hz) and a duration of 200 ms. The pitch of the tone was selected pseudo-randomly, with the constraint
that the same pitch as the previous trial was selected with a probability of 10%.
After the offset of the tone there was a delay of 600 ms until the cue for the next
trial appeared, so that the response-cue interval was the same in the single- and
dual-task conditions (1250 ms). Subjects were instructed to press a button on the
response box whenever the tone they heard was the same pitch as the tone
played on the previous trial.

Stimuli The 56 possible stimuli were split into three groups, randomly for each
subject. 14 stimuli were assigned to the ‘word’ group, 14 to the ‘colour’ group
and the remaining 28 to the ‘both tasks’ group. On 50% of trials, one of the
stimuli from the ‘word’ or ‘colour’ groups was chosen randomly, depending on
the current task. These stimuli will be referred to as ‘unprimed’, since they can
only appear in one of the two tasks. Of course, these stimuli were positively
primed in the sense that they appeared repeatedly throughout the experiment,
but their appearance was limited to just one of the two tasks. On the other trials,
one of the stimuli from the ‘both tasks’ group was chosen randomly. These
stimuli will be referred to as ‘competitor primed’, since they could appear in
either task and may therefore carry priming from the competing Stroop task.
Since the number of stimuli in the ‘both tasks’ group was twice the number in
each of the ‘word’ and ‘colour’ groups, the probability of any individual ‘both
tasks’ stimulus being picked on a word-reading trial was half the probability of
any individual stimulus in the ‘word’ group being picked. However, since the
probability of one of the stimuli in the ‘word’ group being picked was zero on a
colour-naming trial, each of the 56 stimuli were presented, on average, an equal
number of times in the whole experiment.

Procedure Once the instructions for the single-task condition had been explained
to each subject, they were shown examples of each of the eight colours. They
then performed 20 practice trials in the single-task condition. The instructions for
the dual-task condition were then explained and the three tones were played for the purpose of familiarisation. 20 practice trials of the dual-task condition were then performed. Subjects then alternated between blocks of the single-task and dual-task condition. Each block consisted of 48 trials and there were 10 blocks in total. Every block started with a stimulus above the horizontal line. Half the subjects performed the dual-task condition in the odd-numbered blocks (i.e. starting with the first block) and the rest of the subjects performed the dual-task condition in the even-numbered blocks (i.e. starting with the second block). The accuracy of each response was coded by the experimenter.

5.5.2 Results

Two subjects were excluded, one because of an exceptionally high error rate (mean: 14.2%) and one because discussion at the end of the experiment with the experimenter indicated that he had misunderstood the instructions for the dual-task condition.

Trials where the subject failed to respond within 3000 ms were scored as errors. For RT analyses, error trials were excluded, as were trials immediately following an error, trials immediately following a button-press in the dual-task condition, trials where the voice-key triggered inappropriately and the first trial of each block. 18% of trials were excluded for one or more of these reasons. All group analyses are based on mean RTs calculated in each design cell for each subject.

Reaction times were analysed in a repeated measures ANOVA with factors of ‘Dualtask’ (whether the block was in the single- or dual-task condition), ‘Task’ (word or colour), ‘Switch’ (switch or repeat), and ‘Primed’ (competitor primed or unprimed). There were significant main effects of Dualtask \((F = 34.99; \text{df} = 1, 19; p < .001)\), Task \((F = 134.52, p < .001)\) and Switch \((F = 29.86, \text{df} = 1, 19; p < .001)\). In
addition, the Task x Switch interaction was significant (F = 22.17, df = 1, 19; p < .001). None of the other interactions reached significance, and nor did the main effect of Primed. Importantly, there was no Dualtask x Task x Switch interaction (F < 1). Thus, there was no evidence that the dual-task condition modulated the switch cost asymmetry (i.e. Task x Switch interaction). Mean reaction times and error rates for the crossing of Dualtask, Task and Switch are presented in Table 5.1.

As expected, RTs were slower on colour than word trials, and slower on switch trials then non-switch trials; in addition, the switch cost was greater in the word task than in the colour task. Reaction times were slower in the dual-task than in the single-task condition. However, contrary to the predictions derived from the model, the effect of the dual-task condition was approximately constant across different types of trial and there were no significant interactions involving the Dualtask factor (see Figures 5.3 and 5.4).

Errors The first trial of each block was excluded from the following analyses. Mean error rates in each condition were calculated for each subject and entered
Figure 5.3 Effect of Dualtask condition (Single or Dual) on RTs in the word and colour task, on switch and non-switch trials. Error bars show standard error of the mean. Note NS = nonswitch; Sw = switch

Figure 5.4 Effect of Dualtask condition on switch costs in the word and colour tasks

into a repeated measures ANOVA with factors Dualtask, Task, Switch and Primed. More errors were made in the dual-task than in the single-task condition (5.7% versus 4.4%; F = 11.77; df = 1,19; p < .01) and on colour trials compared with word trials (6.6% versus 3.5%; F = 7.12; df = 1, 19; p < .02). There was also a
greater error rate on competitor primed versus unprimed trials (5.6% versus 4.5%; \( F = 7.39; df = 1, 19; p < .02 \)). There was no significant effect of the Switch factor on error rates (\( F < 1 \)) but there was a significant Task x Switch interaction (\( F = 13.93; df = 1, 19; p < .01 \)). This reflects the greater error rate on switch trials in the word task and the reduced error rate on switch trials in the colour task (see Table 5.1). Thus, as in the RT data, there is a greater cost of switching in the word than in the colour task.

**Secondary Task Performance** Overall accuracy in the 1-back task was high. The mean hit rate was 82% and the false alarm rate was 2%. Hit and false alarm rates were converted into \( d' \) values, yielding a mean \( d' \) across subjects of 3.25.

**Dual-Task Order Effects** There were two groups of subjects, one of which performed the dual-task condition in the odd numbered blocks of the experiment (starting with block 1), the other group performing the dual-task condition in even numbered blocks (starting with block 2). In order to investigate the possible effects of this difference, a mixed-model ANOVA was conducted with factors Dualtask, Task, Switch, Primed and Group (odd or even), with repeated measures on the first four factors. There was a significant Dualtask x Group interaction (\( F = 14.6; df = 1, 18; p < .002 \)) which was qualified by a marginally significant Dualtask x Task x Switch x Group interaction (\( F = 3.48; df = 1, 18; p = .079 \)). These interactions are graphed in Figures 5.5 and 5.6.

Subjects that performed the dual-task condition in odd numbered blocks showed a larger slowing of reaction time in dual-task compared with single task blocks than subjects performing the dual-task condition in even numbered blocks. In addition, there was a suggestion that the position of the dual-task blocks (odd or even block numbers) modulated the effect of the dual-task condition on the switch cost asymmetry (i.e. the four-way interaction of Dualtask x Task x Switch
Figure 5.5 Effects of dual-task ordering (odd or even blocks) on RTs in single-task and dual-task blocks

Figure 5.6 Switch costs in the single-task and dual-task conditions for the word and colour tasks. The left-hand graph shows data from just those subjects that performed the dual-task condition in the odd-numbered blocks, while the right-hand panel shows the data from subjects that performed the dual-task condition in even-numbered blocks.

x Group). As Figure 5.6 shows, there was a tendency for subjects in the 'Even' group to show a modulation of switch costs in the dual-task condition consistent with the interaction predicted by the model; by contrast, the 'Odd' group showed a small effect in the opposite direction. To further investigate this order
effect, the mean reaction times of the ‘Odd’ and ‘Even’ groups in each block of the experiment are presented in Figure 5.7. The RTs of the ‘Odd’ group are elevated in the dual-task blocks throughout the experiment. By contrast, the ‘Even’ group only shows a strong effect of the dual-task condition on RTs at the beginning of the experiment. In the second half of the experiment, the dual-task condition appears to have very little effect on RTs in the ‘Even’ group.

The analysis of the effects of Group also revealed a significant Dualtask x Switch x Primed x Group interaction (F = 10.6; df = 1, 18; p < .005). In order to interpret this interaction, the size of the priming effects (i.e. the RT difference between competitor primed and unprimed trials) were calculated for switch and non-switch trials in the two groups of subjects. These effects are graphed in Figure 5.8. It appears that the four-way interaction is mainly caused by the different behaviour of switch trials in the two groups. In the odd group, the priming effect on switch trials is reduced in the dual-task condition, in comparison with the single-task condition, but in the even group it is increased in the dual-task condition. It is not clear what the cause of this interaction is. However, since it...
was unexpected and involves effects that are relatively small in magnitude, and due to the likelihood of Type 1 errors in an ANOVA with so many factors (Hurlburt & Spiegel, 1976), it seems premature to draw any theoretical significance from this interaction at present. There were no other significant interactions involving the Group factor, and nor was the main effect significant. A similar analysis of the error data revealed neither a significant main effect nor any significant interactions involving the Group factor.

5.5.3 Discussion

This experiment replicates a number of findings reported in previous investigations of task switching. There was a clear cost of task switching, even though the task sequence was entirely predictable and the response-stimulus interval was as long as 1750 ms. As reported previously by Allport et al. (1994)
and Allport and Wylie (2000), the switch cost was greater for a switch into the word task than for a switch into the colour task.

It was surprising that there was no clear effect of stimulus priming (i.e. whether the stimulus could appear in both or only one of the tasks) on RT (though there was a complex interaction involving the Group factor, and a main effect on error rates). A previous experiment by Allport and Wylie (2000, Experiment 5) demonstrated an effect of item-specific priming on RT switch costs with Stroop stimuli. However, immediate repetition of word or colour features across trials was not allowed in Allport and Wylie’s study (i.e. there could be no immediate repetition of words or colours, and nor could the word on one trial match the colour on the previous trial or vice versa). In the present study, immediate repetition of stimulus features was possible, and these immediate priming effects (i.e. effects of repetition of stimulus features from trial N to trial N+1) may have masked more subtle longer-term item-specific effects of the sort described by Allport and Wylie.

The predicted modulation of the switch cost asymmetry by dual-task performance was not obtained. Four alternative possible conclusions might be drawn from this. First, of course, the model described in Chapters 3 and 4 may not provide a satisfactory account of the cognitive processes involved in task switching. In this case there would be no reason to expect that its predictions would be supported empirically. Second, it is possible that the model does provide a satisfactory account of task switching performance, but the assumptions employed to derive predictions about dual-task performance were unsound. This might include the assumption that a reason for dual-task performance decrements between apparently unrelated tasks is their common reliance on shared control processes, as well as the assumption that interference between control processes is most plausibly simulated by a reduction in the
colour-naming top-down input. A third possibility is that, although the model and the assumptions about dual-task performance were sound, the performance of the secondary task did not lead to any interference with the processes represented by the top-down control input or the task demand units in the model. The control processes required to organise and co-ordinate secondary task performance, as well as those that were employed by the 1-back task itself, may be entirely separable from the control processes involved in task switching. In this case, dual-task performance would not be expected to interact with task-switching performance (though it would still be necessary to explain the main effect of dual-task performance in this experiment). The fourth possible conclusion that might be drawn from this experiment is that, although the control processes involved in dual-task performance may sometimes overlap and interfere with those involved in task switching, subjects in this experiment were able to perform the tasks in such a way as to avoid this interference.

Several pieces of evidence are consistent with the fourth possibility. First, there was the marginally significant Dualtask x Task x Switch x Group interaction, suggesting that subjects who had received more practice with the Stroop tasks before performing the dual-task condition (i.e. the ‘Even’ group) were more likely to produce a Dualtask x Task x Switch interaction in the direction predicted by the model. Thus, it is possible that there were strategic differences between the groups so that only the Even group showed the interaction predicted by the model (see Chapter 6 for further discussion). The idea of strategic differences between the groups is supported by the highly reliable Group x Dualtask interaction, suggesting differences between the ways in which the two groups of subjects performed the dual-task blocks of the experiment.

Further evidence that interference between control processes involved in dual-task performance and in task-switching is possible but not mandatory comes
from previous studies of task-switching with concurrent secondary-task performance. The results from such studies have been mixed. Some experiments have found no interaction between task-switching and the performance of a secondary task (Morcom, 2000, Experiment 2) whereas other have suggested a greater cost of task switching in dual-task conditions (Baddeley, Chincotta, & Adlam, 2001; Emerson & Miyake, in press; Hsieh, 2002) or even a reduced cost of task switching in dual-task conditions (Morcom, 2000, Experiment 3). Thus, it appears that the factors governing interactions between task-switching and dual-task performance may be complex. Methodological differences between various studies may lead to very different interactions between dual-task performance and task-switching. In light of the marginal Dualtask x Task x Switch x Group interaction in the present study, it seems premature to rule out any effect of dual-task performance on the switch cost asymmetry in Stroop tasks. However, the available evidence is currently not strong enough to distinguish between the four possible conclusions described above.
6.1 Introduction

Experiment 1 investigated the effects of concurrent performance of a secondary task while switching between Stroop word-reading and colour-naming tasks. It was predicted that this might affect the switch cost asymmetry, leading to a greater switch cost in the colour-naming task and possibly a reduced switch cost in the word-reading task. However, no such effect was obtained. Although performance of the secondary task increased mean RTs, it did not reliably modulate the switch cost asymmetry. Nevertheless, there were differences between subjects performing the dual-task condition in the first block of the experiment, and subsequently in the other odd-numbered blocks, and those performing the dual-task condition in the second block of the experiment, and subsequently in even-numbered blocks. Thus, the time at which the secondary task was introduced seemed to affect the way in which subjects combined the primary and secondary tasks. The ‘odd’ subjects, who performed the dual-task condition in the first block of the experiment, showed a greater difference in RT between single-task and dual-task blocks than the ‘even’ subjects. In addition, the groups differed in the effects of the dual-task condition on the switch cost asymmetry, with the ‘even’ group more likely to show an interaction in the direction predicted by the model. However, this unexpected difference between the groups was only marginally significant and so requires further investigation.

6.2 Practice effects in dual-task performance
As discussed in Chapter 5, a number of studies have shown dramatic improvements in dual-task performance with practice (e.g. Spelke et al., 1976; Hirst et al., 1980). One interpretation of these studies would be that better-practised tasks are more automatic (Shiffrin & Schneider, 1977), as a result of which they can be combined with a smaller single- to dual-task decrement. However, the factors underlying dual-task interference are likely to be more complex than this simple model would suggest. Several studies have illustrated dissociations between the performance of individual tasks and performance when those tasks are combined. For example, Schneider and Fisk (1982) found that dual-task performance continued to improve long after the performance of the constituent single tasks had stabilised. In many cases, the tasks performed in dual-task studies are individually well practised (e.g. reading, sight-reading, writing to dictation, copy-typing, audio-typing and so on). Nevertheless, it seems important to practice the two tasks together in order to optimise dual-task performance. Thus, the improvement in dual-task performance does not arise entirely out of the way that the component tasks, performed individually, improve with practice. In addition, practice seems to improve the way in which the two tasks are co-ordinated, independent of improvements that might be detected when the tasks are performed individually. In referring to these effects, the term 'task coordination strategy' will be used below to describe any bias or approach which primarily influences the way in which multiple tasks are performed, rather than influencing those tasks when performed individually. Specific examples of task coordination strategies will be discussed later in the chapter.

Further evidence that dual-task training can influence subjects’ dual-task coordination strategies, in addition to any influence on the way in which the tasks are performed individually, has come from studies investigating the transfer of dual-task coordination skills from one pair of tasks to another. In a
study by Damos and Wickens (1980), one group of subjects received training on a
digit classification and a short term memory task, performed together, while
another group practised them individually. In a subsequent session, both groups
of subjects performed a pair of visuomotor tracking tasks, with a joystick in each
hand. Damos and Wickens (1980) found that the subjects that had received dual-
task training performed better on the tracking tasks than the subjects that had
received single-task training, even though the tasks on which the subjects were
tested were completely unrelated to those on which they were trained. Thus,
Damos and Wickens (1980) conclude that dual-task training led to an
improvement in subjects’ general abilities to coordinate the performance of
concurrent tasks. However, since they did not test subjects’ performance on the
tracking tasks when performed individually, it is difficult to argue that the cause
of the improved tracking performance in the group that received dual-task
training was an improved dual-task coordination strategy, rather than some
other cause which may have affected the way that the individual tasks were
performed.

In a related study, Kramer, Larish, Weber, & Bardell (1999) trained subjects to
perform a tracking and a choice RT task simultaneously. Some subjects were
instructed to split their processing resources between the two tasks in variable
proportions (e.g. 80% task A, 20% task B), with the actual proportions changing
continually throughout the training phase. Other subjects were instructed to
allocate equal attention to the two tasks throughout training. The subjects
receiving ‘variable-priority’ (VP) training outperformed those receiving ‘fixed-
priority’ (FP) training. When the stimuli were unpredictable, these groups of
subjects differed on dual-task performance, but showed no differences in
performance when the tasks were performed alone. Further evidence that VP
and FP subjects differed in their dual-task coordination strategies came from
their subsequent performance on novel tasks (a visual monitoring and an
alphabet arithmetic task). Again, the groups did not differ in their performance of the individual tasks when performed alone, but the VP group performed better than the FP group when the tasks were combined, even though they were unlike the tasks on which the subjects were trained.

In light of the evidence described above, it seems that dual-task practice can influence performance both through effects on the performance of individual tasks, and also through the development of task coordination strategies. Given that the subjects in Experiment 1 performing the dual-task condition in odd- and even-numbered blocks differed in the amount of dual-task interference they suffered, it seems plausible that these groups may have differed in their task coordination strategies. The strategy adopted by the 'odd' group may have caused little interference between control processes involved in dual-task performance and in task-switching, resulting in little effect of the dual-task condition on the switch cost asymmetry. The strategy adopted by the 'even' group may have led to such interference, and consequently an effect of the dual-task condition in the direction predicted by the model.

However, there is a difference between the dual-task practice effects described above and the nature of the practice effect in Experiment 1. Whereas the studies described above investigated the effects of dual-task training on subjects' task coordination strategies, any differences between the task coordination strategies of the two groups of subjects in Experiment 1 would have arisen from the amount of single-task training before the introduction of the secondary task. The effects of single-task training on subsequent dual-task coordination strategies do not seem to have been investigated in the past so often as the effects of dual-task training. The results from Experiment 1 suggest that single-task practice leads to a smaller dual-task interference effect, since the dual-task RT increase was greater in the 'odd' subjects than the 'even' subjects. However, evidence from the
Switch cost asymmetry suggested that the dual-task condition caused interference between control processes in the 'even' group, but not in the 'odd' group. This suggests an interesting dissociation between the absolute cost of dual-task performance and the ability of the secondary task to interfere with control processes required for the primary task(s). It is possible that the 'odd' group adopted a strategy that prevented interference between control processes (as evidenced by the switch cost asymmetry data) but led to an overall greater cost of dual-task performance. The strategy of the 'even' group may have been more efficient, in terms of the absolute RT cost of dual-task performance, but this may have resulted in greater competition between the control processes involved in the Stroop tasks and the 1-back task (as well as those involved in the coordination of dual task performance). Since these speculations are only supported by a marginally significant group difference in the effects of the dual-task condition on the switch cost asymmetry, Experiment 2 seeks to provide stronger evidence by maximising group differences in single-task practice before the introduction of the 1-back task.

6.3 Experiment 2

In this experiment, rather than alternating between single-task and dual-task conditions as in Experiment 1, subjects performed one of these conditions for the first half of the experiment, and the other condition for the second half. Thus, any effects of the amount of single-task practice before the introduction of the 1-back task should be maximised in this design. On the basis of Experiment 1, it was predicted that the group performing the dual-task condition in the second half of the experiment (i.e. having practised switching between the two Stroop tasks for the first half of the experiment) should show the effect predicted by the model, viz. an attenuated or reversed switch cost asymmetry in the dual-task condition.
Subjects performing the dual-task condition for the first half of the experiment were not expected to show this effect.

6.3.1 Method

Subjects There were 37 paid subjects, 22 female, aged 18-36 (mean: 23.8).

Procedure This experiment differed from Experiment 1 in two ways. First, all possible stimuli were presented in both word-reading and colour-naming tasks in this experiment (i.e. all stimuli were ‘competitor primed’). This was because there was no reliable effect of priming on RTs in the first experiment, and item-specific effects are not the main focus of these studies. The second difference between this experiment and the previous one was the order in which the single-task and dual-task conditions were performed. There were three groups. The ‘single-task’ group performed the single-task condition throughout the experiment, and were never exposed to the dual-task condition (i.e. there was no dual-task practice block at the beginning of the experiment). The ‘dual-first’ group performed the dual-task condition for the first half of the experiment (i.e. the first five blocks) and the single-task condition for the remainder. The ‘dual-second’ group performed the single-task condition for the first half of the experiment and, like the single-task group was not given the dual-task practice block at the beginning of the experiment. The dual-task instructions were then explained after the fifth block and there was a practice block of 20 trials before the final five blocks of the experiment were performed.

6.3.2 Results

One subject was excluded due to an extremely high error rate (15%). The remaining 36 subjects were divided equally between the single-task, dual-first,
and dual-second groups. RTs were analysed as in Experiment 1, and the same
criteria were used to exclude trials (leading to 11.6% of trials being excluded for
RT analyses).

<table>
<thead>
<tr>
<th></th>
<th>Single-task</th>
<th>Dual-first</th>
<th>Dual-second</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>RT (ms)</td>
<td>Errors (%)</td>
<td>RT (ms)</td>
</tr>
<tr>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Word</td>
<td>NS 606</td>
<td>1.8</td>
<td>674</td>
</tr>
<tr>
<td>Sw</td>
<td>708</td>
<td>4.6</td>
<td>753</td>
</tr>
<tr>
<td>Half</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Colour</td>
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<td>5.1</td>
<td>871</td>
</tr>
<tr>
<td>Sw</td>
<td>867</td>
<td>5.2</td>
<td>897</td>
</tr>
<tr>
<td>Second</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
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<td>NS 579</td>
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<tr>
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<tr>
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<tr>
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<td>758</td>
</tr>
<tr>
<td>Sw</td>
<td>826</td>
<td>3.3</td>
<td>774</td>
</tr>
</tbody>
</table>

**Table 6.1** *Mean RTs and error rates in each condition for the three groups of subjects.*

*Note NS = non-switch; Sw = switch*

Full results are presented in Table 6.1. For the initial analyses, the three groups of
subjects were analysed separately in repeated measures ANOVAs, with factors
of 'Half', 'Task', and 'Switch'. The Half factor divides the experiment into two
halves, corresponding to the single- and dual-task conditions for the dual-first
and dual-second groups. All three groups showed significant main effects of
Task, colour-naming trials being slower than word-reading trials (F > 39; df = 1, 11; p < .001) and Switch, switch trials being slower than non-switch trials (F > 23; df = 1, 11; p < .001). The main effect of Half was significant for the dual-first
group, and marginally significant for the single-task group, both groups showing
a reduction in RT for the second half of the experiment (F = 47.8; df = 1, 11; p <
Figure 6.1. RT and error switch costs for the three groups of subjects, in the two halves of the experiment, and simulated RTs and error rates.
.001; and \( F = 4.1; \) df = 1, 11; \( p = .068 \) respectively). In contrast, the dual-second group was slightly slower in the second half of the experiment, but this difference was not significant (\( F = 2.3; \) df = 1, 11; \( p = .16 \)). The dual-first and single-task groups both showed significant Task x Switch interactions, reflecting a larger switch cost in the word-reading task (\( F > 7.5; \) df = 1, 11; \( p < .02 \)). The Task x Switch interaction was not significant in the dual-second group (\( F < 1 \)).

The crucial interaction of Half x Task x Switch was tested next. In the two dual-task groups this interaction reflects the effect of performing the secondary task on the switch cost asymmetry (i.e. Task x Switch interaction). It was argued in Chapter 5 that the most plausible way of simulating the effects of dual-task performance with the model described in Chapter 3 would be as a less paradoxical switch cost asymmetry (i.e. a smaller difference between the word-reading and colour-naming switch costs, or a larger switch cost in the colour-naming task than the word-reading task). Experiment 1 suggested that this effect might only be obtained in subjects that had first practised the Stroop tasks alone (i.e., in this experiment, the dual-second group). In accordance with these predictions, the Half x Task x Switch interaction was highly significant in the dual-second group (\( F = 20.1; \) df = 1, 11; \( p < .001 \)). The switch cost asymmetry in this group was completely reversed between halves: in the first half the switch cost was larger in the word-reading task, but in the second half of the experiment (when these subjects performed the dual-task condition) the switch cost became larger in the colour-naming task. The Half x Task x Switch interaction was in this direction for all 12 subjects.

In the other two groups, the Half x Task x Switch interaction was far from significant (\( F < 1 \) for both groups). Thus, there were no reliable differences in the switch cost asymmetry between the two halves in these groups. In other words, the dual-task condition failed to modulate the switch cost asymmetry in the dual-
first group, and there was no tendency for the switch cost asymmetry to differ between the two halves of the experiment when there was no dual-task condition. However, the single-task group did show a significant reduction in switch costs from the first to the second half of the experiment (Half x Switch interaction: F = 5.9; df = 1, 11; p < .05); this interaction was not significant in either of the dual-task groups (F < 1.2; df = 1, 11; p > .3).

Switch costs in the first and second half of the experiment are illustrated in Figure 6.1, which graphs the data from the three groups of subjects, as well as the performance of the model when the colour-naming top-down input is reduced from 15 to 8 (see Figure 5.1). The data from the dual-second group are graphed in full in Figure 6.2

In order to further investigate the effect of the dual-task condition on the switch cost asymmetry, pairwise comparisons between the groups were conducted. First, the dual-second group was compared with the single-task group in a Half x Task x Switch x Group ANOVA. There was a significant Half x Task x Switch x Group interaction, reflecting a modulation of the switch cost asymmetry according to Half in the dual-second group, but not in the single-task group (F = 4.3; df = 1, 22; p < .05). The Half x Group interaction was also significant (F = 5.7; df = 1, 22; p < .05); this was because the second half was slower than the first half in the dual-second group, but faster in the single-task group. Next, the single-task and dual-first groups were compared in a similar analysis. The four-way interaction was not significant (F < 1), because the switch cost asymmetry was stable across both halves of the experiment in both of these groups. However, the Half x Group interaction was significant (F = 9.8; df = 1, 22; p < .005). Although the second half was faster than the first half for both groups, this effect was larger in the dual-first group.
The dual-first and dual-second groups were also compared directly. In order to do this, the 'Half' factor was replaced by a 'Dualtask' factor, so that the two groups' single-task and dual-task conditions were compared with each other. The Dualtask x Task x Switch x Group interaction was highly significant, showing that the modulation of the switch cost asymmetry by the dual-task condition differed between the two groups (F = 14.6; df = 1, 22; p < .001). In this analysis, the Dualtask x Group interaction was also significant (F = 5.5; df = 1, 22; p < .05). Both groups were slower in the dual-task then the single-task condition, but this effect was larger in the dual-first group. However, this interaction should not necessarily be considered evidence that the RT cost of dual-task performance is larger in the dual-first than the dual-second group, since the groups performed the dual-task condition in different halves of the experiment. Thus, if there is any difference between the two halves of the experiment, independent of dual-task performance, this will give rise to a Dualtask x Group interaction. When the dual-first and dual-second groups were compared in an ANOVA with factors Half, Task, Switch and Group, there was a significant Half x Group interaction (F = 24.0; df = 1, 22; p < .0001), since the dual-first group speeds up in the second half whereas the dual-second group slows down. There was also a significant Half x Task x Switch x Group interaction (F = 11.6; df = 1, 22; p < .005) because there was a Half x Task x Switch interaction in the dual-second but not dual-first group. The main effect of Group was not significant in any of the analyses, and nor were any of the interactions involving the Group factor other than those reported above.
One puzzling feature of the data shown in Figure 6.2, from the dual-second group, is that the dual-task condition led to increased RTs on word-reading non-switch trials but not on colour-naming non-switch trials. This contrasts with the model’s simulation of dual-task performance, where the only effect on non-switch trials is a small slowing in the colour-naming task. When the non-switch trials were analysed alone in an ANOVA with factors Half and Task, this gave rise to a significant Half x Task interaction ($F = 7.9; df = 1, 11; p < .02$). Neither of the other groups showed this interaction ($F < 1$).

**Time course of dual-task effects** The time-course of the effects of the dual-task condition in the dual-task groups was investigated by discarding data from the first and last block and breaking down the data into four two-block segments, two from the first half of the experiment and two from the second half (Figure 6.3). It can be seen that the dual-first group shows a steep fall in RT when dual-task performance ceases and single-task performance begins. However, there is no modulation of the effect of task switching. The dual-second group shows an
increase in RT at the point where the dual-task condition is introduced, and also a very large increase in the colour-naming switch cost. This falls slightly between blocks 6/7 and 8/9, but it is still elevated compared to its initial level. The data from error rates seems to be broadly similar to the RT data, although it is more variable, and there is no absolute difference between word-reading and colour-naming error rates.

**Errors** The first trial of each block was excluded from the following analyses. Mean error rates in each condition were calculated for each subject and entered into repeated measures ANOVAs with factors Half, Task and Switch. All three

![Graph showing RT and error rates for dual-first and dual-second groups](image)

**Figure 6.3** Mean RTs and error rates for the dual-first and dual-second groups during the course of the experiment. In the 2nd/3rd and 4th/5th blocks, the dual-first group is in the dual-task condition and the dual-second group is in the single-task condition. The conditions are reversed in the 6th/7th and 8th/9th blocks. Note NS = nonswitch; Sw = switch.
groups showed a significant main effect of Switch, the error rate being greater on switch than non-switch trials ($F > 5.3; df = 1, 11; p < .05$). The main effect of Half was significant in the single-task and dual-first groups ($F > 20.3; df = 1, 11; p < .001$), where there were more errors in the first half of the experiment. The main effect of Task was significant in the single-task group ($F = 5.2; df = 1, 11; p < .05$), where there were more errors on colour-naming than word-reading trials. None of the other main effects or interactions were significant.

The three groups were then compared directly. The dual-first group was compared with the single-task group in an ANOVA with factors Half x Task x Switch x Group. This revealed a marginally significant Half x Group interaction ($F = 4.2; df = 1, 22; p = .053$). Both groups showed a reduction in error rate in the second half of the experiment, but this reduction was greater in the dual-first group. In contrast, the error rate in the dual-second group increased for the second half of the experiment. When the dual-second and single-task groups were compared in a similar analysis this yielded a significant Half x Group interaction ($F = 14.4; df = 1, 22; p < .001$). Finally, the dual-first and dual-second groups were compared in a Dualtask x Task x Switch x Group ANOVA. There was a significant Dualtask x Group interaction ($F = 9.4; df = 1, 22; p < .01$). Although the error rate was higher in the dual-task condition for both groups, this effect was larger in the dual-first group. However, this may simply reflect a difference between the two halves of the experiment, as in the RT data. The main effect of Group was not significant in any of these analyses, and nor were any other interactions involving the Group factor.

Secondary Task Performance  Both the dual-first and dual-second groups were generally accurate in the secondary (1-back) task. Hit rates, false alarm rates, and $d'$ values, averaged across subjects, are given in Table 6.2. Although the mean $d'$
value was higher in the dual-second than the dual-first group, this difference was not significant (t = 1.46; df = 22; p > .15).

<table>
<thead>
<tr>
<th></th>
<th>Dual-first</th>
<th>Dual-second</th>
</tr>
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<tbody>
<tr>
<td>Correct Detections (%)</td>
<td>74.6</td>
<td>84.9</td>
</tr>
<tr>
<td>False Alarms (%)</td>
<td>1.6</td>
<td>1.9</td>
</tr>
<tr>
<td>$d'$</td>
<td>3.1</td>
<td>3.5</td>
</tr>
</tbody>
</table>

Table 6.2 Mean secondary task performance in the dual-first and dual-second groups.

6.3.3 Discussion

This experiment confirmed the prediction made on the basis of the model that performing a secondary task at the same time as switching between Stroop word-reading and colour-naming tasks should affect the switch cost asymmetry, leading to an increased colour-naming switch cost. However, as predicted from the results of Experiment 1, this effect was confined to the group that had practised the Stroop tasks for some time before the secondary task was introduced. The difference between the dual-first and dual-second groups was striking. While there was no significant effect of the secondary task on task switching performance in the dual-first group, the effects on the dual-second group were large and extremely reliable.

An unexpected finding in the dual-second group was that word reading non-switch trials were slowed more by the dual-task condition than colour-naming non-switch trials. The explanation for this is not clear. It was not predicted by the model, where the only effect of the dual-task simulation on non-switch trials is a small slowing in the colour-naming task. It was also not found in Experiment 1, where the group of subjects performing the dual-task condition in even-
numbered blocks (the group of subjects most similar to the dual-second subjects in the present experiment) showed a dual-task slowing of 39 ms on word-reading non-switch trials and 41 ms on colour-naming non-switch trials. Another aspect of the data that had a relatively poor fit with the predictions of the model was the pattern of errors. The model predicts that the dual-task condition should selectively increase the error rate on colour-naming switch trials (see Figure 5.1). Empirically, although the dual-task condition did increase error rates in the dual-second group, this included both word-reading and colour-naming error rates (see Figure 6.3). However, Figure 6.3 shows that, immediately after the introduction of the secondary-task, the greatest increase in errors in the dual-second group occurs on colour-naming switch trials, as predicted by the model.

Four possible conclusions were discussed in the previous chapter in relation to the results from Experiment 1. The first two of these were that the model does not provide a satisfactory account of the cognitive processes involved in task switching, or that the assumptions used to derive predictions about dual-task performance are unsound. Since one of the experimental groups in Experiment 2 produced the pattern of results predicted by the model, it does not seem justified to draw either of these conclusions, at least on the basis of present results. The third possible conclusion discussed in Chapter 5 was that the control processes involved in dual-task performance, and those directly involved in performance of the 1-back task, are entirely separate from those involved in task switching. In this case, the pattern of results predicted by the model should never be obtained, since there would never be any interference between the control processes involved in the two tasks. Since the pattern of results predicted by the model was obtained in one group of subjects in Experiment 2, this possible conclusion can be rejected. The fourth possible conclusion discussed was that, although the control processes involved in dual-task performance may sometimes overlap and interfere with those involved in task switching, such interference is not
mandatory. This is clearly compatible with the results from Experiment 2. However, if this conclusion is drawn, an open question obviously remains: under which circumstances will interference between control processes be expected? As a first step towards answering this question, one may ask a more specific one: why did the dual-first and dual-second groups in Experiment 2 differ so dramatically? In the terminology used at the beginning of the chapter, how might their task coordination strategies have differed?

One possibility is that subjects in the dual-first group manage to avoid performing the primary (Stroop) and secondary (1-back) tasks concurrently. Since the tones are played in the intervals between trials in the Stroop tasks, it is possible that when subjects have little practice with the Stroop tasks (i.e. the dual-first group), they rapidly switch between the various tasks, rather than attempting to combine them. This would not be expected to affect the switch cost asymmetry. On the other hand, the dual-second group, having spent more time practising the Stroop tasks may attempt to perform those tasks at the same time as the 1-back task, without switching between them. In this case, an effect on the switch cost asymmetry would be expected.

Explanations of dual-task performance in terms of a high-frequency switching strategy have often been put forward by defenders of single-channel theories of attention. For example, Broadbent (1982) explains the success with which subjects in Experiment 2 of Allport et al. (1972) were able to combine sight-reading with shadowing in terms of ‘intake of large units of stimulus information well ahead of response, and the decision to output similar large units of motor sequence; which in turn can be overlapped with intake and decision for the same or another task’ (p.267). In other words, the perceptual analysis necessary for the two tasks need not take place concurrently if subjects switch attention rapidly between the two perceptual streams. However, it has been difficult to distinguish
empirically between two tasks genuinely being performed concurrently, as opposed to subjects switching very rapidly between them. It might be argued that the typical size of task-switch costs rules out extremely rapid switching between tasks (i.e. spending just tens of milliseconds on each task before switching to the next). But since the response-stimulus interval was long in Experiments 1 and 2 (1750 ms), there seems to be sufficiently little temporal overlap between the Stroop and the 1-back tasks to make a switching strategy feasible. Even if the inter-trial interval were reduced, it is not clear that this would make a switching strategy impossible. Since the Stroop stimulus remained on screen until the subject’s response, it is possible that processing in the Stroop task could await the termination of processing in the 1-back task.

An alternative to the ‘switching’ explanation of the difference between the dual-first and dual-second groups in Experiment 2 will be called the ‘top-down persistence’ hypothesis. This explanation makes two assumptions. The first assumption is that when subjects were confronted with the interfering effects of the secondary task at the beginning of Experiments 1 and 2, they were able to compensate for this so that a strong top-down input for the colour-naming task was preserved. The second assumption is that the flexibility which allows subjects to compensate for the effects of the secondary task diminishes with time. As a result, it is assumed that subjects in the dual-second group in Experiment 2, having performed several blocks of the Stroop tasks before the introduction of the secondary task, were unable to compensate for the effects of the secondary task, or were able to do so imperfectly. This would explain why it is only this group – having lost the flexibility that allows them to compensate for the secondary task – that shows the reversed asymmetry of switch costs.

This explanation of the order effect can be seen as an extension of task carryover accounts of switch costs. Such accounts, especially in the form of the TSI
hypothesis of Allport et al. (1994), explain switch costs in terms of the principle that control states can persist involuntarily from one trial to the next. The explanation of the order effect sketched above takes this further. It claims that aspects of task control such as the appropriate level of top-down input for the various experimental tasks can also persist involuntarily over time. As a result, it may be possible that the dual-second subjects in Experiment 2 do not adjust the top-down input for the colour-naming task to compensate for the effects of the secondary task, because of a persistence of the control strategy that was appropriate in the first half of the experiment.

In order to test this idea, the following experiment makes use of an experimental manipulation that is believed to have a strong effect on the top-down input for the colour-naming task. The logic of the experiment is as follows. This manipulation should be used to ensure that two groups of subjects have different colour-naming top-down inputs in the first half of the experiment. The difference in top-down inputs can be verified by the direction of the switch cost asymmetry. The subjects then perform exactly the same version of the experiment in the second half. Insofar as the top-down input persists into the second half of the experiment, it should be possible to establish group differences in the switch cost asymmetry, even though the tasks and stimuli are identical for both groups. However, if the assumption that top-down inputs show involuntary persistence is incorrect, then there should be no differences between the two groups in the second half of the experiment.

6.4 Experiment 3

The experimental manipulation that was chosen for the purpose of inducing differences in the top-down input for the colour-naming task was the stimulus onset asynchrony (SOA) between the colour and word dimensions of the Stroop
stimulus. This was chosen on the basis of an experiment of Yeung (1999, Experiment 8; see also Monsell et al., 2000), who was working with a similar theoretical framework to the one adopted here. Yeung followed the procedure of Glaser and Glaser (1982), who presented Stroop stimuli composed of colour words in black ink on coloured backgrounds. This allows the onset of the word and colour dimensions to be manipulated independently. Yeung reasoned that presenting the colour before the word on a colour-naming trial should allow the response selection process to begin before the word dimension is able to cause any interference (of course, the limiting case of this is the situation where the word dimension is presented after the response to the colour dimension has been made, making the stimulus neutral). Thus, response competition should be reduced on colour-naming trials where the onset of the word dimension is delayed. As a consequence, the top-down input required by the colour-naming task should be reduced on such trials, yielding a reversed asymmetry of switch costs.

In Yeung’s experiment, one group of subjects switched between word-reading and colour-naming tasks with no delay between the onset of the colour and word dimensions. This group showed the standard, paradoxical asymmetry of switch costs. However, a group that performed the same task with a 320 ms delay between the colour and word dimensions showed a reversed asymmetry of switch costs. A third group of subjects, who received a 160 ms delay between the colour and word dimensions, showed an intermediate pattern of results, with approximately equal switch costs for the two tasks. Thus, it is possible to infer a strong colour-naming top-down input in the 0 SOA group, an intermediate top-down input in the 160 SOA group and a low top-down input in the 320 SOA.

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14 Simulations with the model presented in Chapters 3 and 4 showed that the colour-naming error rate was reduced when the onset of the word dimension was delayed. Thus, it seems plausible that a smaller colour-naming top-down input would be required in such circumstances.
group. Similar timing parameters were adopted for the present experiment: in the first half, one group performed the Stroop tasks with a 317 ms SOA; the SOA for the other group was zero. In the second half of the experiment, both groups had an SOA of 167 ms\(^1\). The critical question was whether the groups would show a different switch cost asymmetry in the second half of the experiment. If they do, this would suggest that the top-down input persists from the first half of the experiment to the second. If the switch cost asymmetry does not differ between the groups, this would suggest that the top-down input may be adjusted flexibly, even after practice with the Stroop tasks. Since this contradicts a basic premise of the top-down persistence account, it seems that the 'switching' hypothesis described above would then offer a better explanation of the difference between the dual-first and dual-second groups in Experiment 2.

6.4.1 Method

Subjects There were 32 paid subjects, 24 female, aged 18-25 (mean: 19.8 years).

Apparatus Stimuli were presented on a 15" VGA monitor, controlled by an IBM-compatible PC. Responses were collected by means of a voicekey which was connected to the serial port of the PC.

Tasks Subjects sat approximately 60cm from the monitor, with the microphone immediately in front of them. As in Experiments 1 and 2, the monitor displayed a light grey screen, bisected horizontally by a thick black line. For half the subjects, the word 'word' was presented at the top of the screen, double-spaced horizontally, in 30 point bold black Arial type, and a similarly sized colour-spectrum was displayed at the bottom of the screen. The other subjects were

\(^1\) These values were not identical to those reported by Yeung (1999) because they were constrained to be multiples of the refresh-rate of the computer monitor.
shown the word at the bottom of the screen and the colour patch at the top. These stimuli remained on screen from the start to the end of every block of trials. There were three types of block:

**0 SOA Condition** Each trial started with the appearance of a black rectangular box, which appeared just above or just below the horizontal line (see Figure 6.4). 500 ms later, a dark grey rectangle appeared, centred within the box. Then, after 167 ms, the dark grey rectangle was replaced with a coloured rectangle. Centred within this was the name of a colour, printed in bold white 32 point Arial type. The coloured rectangle could be presented in one of the following colours: red, green, blue, yellow, brown, orange, or purple. These were also the colour names that could appear within the coloured rectangle. The position of these stimuli (above or below the horizontal line) cued the task. Subjects were instructed to read out the word if it appeared above the horizontal line, and name the colour of its background if it appeared below the line (or vice versa for those subjects who were presented with the colour patch stimulus at the top of the screen and the ‘word’ stimulus at the bottom of the screen). As soon as the voicekey detected the subject’s response, or after 3000 ms if no response was detected, the rectangular box, coloured rectangle, and colour-word were removed from the screen. There was then a 500 ms delay before the rectangular box cueing the next trial appeared. The position of the rectangle (and hence the required task) was completely predictable. It appeared twice above the line, then twice below the line, then twice above the line and so on. Stimulus presentations were synchronised with the refresh cycle of the computer monitor.

**167 and 317 SOA conditions** The 167 and 317 SOA conditions were identical to the 0 SOA condition, except that the onset of the colour-word was delayed by 167 or

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16 The initial appearance of the dark grey rectangle ensured that a solid rectangle was presented before the appearance of the word in all conditions, including the 0-SOA condition.
317 ms following the appearance of the coloured background. In addition, on colour-naming trials in these conditions, the delay between the appearance of the rectangular box cueing the task and the dark grey rectangle was extended by the same amount of time. Thus, the delay between the onset of the rectangular box cueing the task and the onset of the task-relevant stimulus was always equal for the two tasks. Reaction times were measured from the onset of the task-relevant stimulus dimension.

Figure 6.4 Sequence of events in one trial of the word-reading task in Experiment 3
**Procedure** The subjects were divided equally between a 0-SOA group and a 317- 
SOA group. After the instructions had been explained to each subject, they were 
shown examples of each of the seven colours that could appear in the experiment. They then performed 20 practice trials in the 0 SOA or 317 SOA 
condition, depending on group. Subjects then performed 6 blocks of trials in the 
same SOA condition, followed by a further 6 blocks of trials in the 167 SOA 
condition. The 42 trials in each block consisted of a pseudo-randomly ordered 
sequence of each possible incongruent combination of word and colour 
dimensions. The position of the stimulus on the first trial of the block (above or 
below the horizontal line) alternated from block to block. Subjects were 
encouraged to rest between blocks and were free to start each block of trials with 
a button-press. The accuracy of each response was coded by the experimenter.

**6.4.2 Results**

Trials where the subject failed to respond within 3000 ms were scored as errors. 
For RT analyses, error trials were excluded, as were trials immediately following 
an error, trials where the voice-key triggered inappropriately and the first trial of 
each block. 11.0% of trials were excluded for one or more of these reasons. All 
group analyses are based on mean RTs calculated in each design cell for each 
subject.

Full results are summarised in Table 6.3. The switch costs in each half of the 
experiment are graphed in Figure 6.5, and the switch cost asymmetries of the two 
groups are graphed in Figure 6.6. Reaction times were first analysed in separate 
ANOVA s for each half of the experiment, with within-subject factors of Task and 
Switch, and SOA as a between-subjects factor. In the first half of the experiment 
(where the SOA groups received different stimuli), there were significant main 
effects of Task (F = 390.0; df = 1, 30; p < .00001) and Switch (F = 112.4; df = 1, 30; p
Figure 6.5 Switch costs in the 0-SOA and 317-SOA groups in the word-reading and colour-naming tasks. Left hand bars show switch costs in the first half of the experiment and right hand bars show switch costs in the second.

< .00001). There was also a significant Task x SOA interaction (F = 27.0; df = 1, 30; p < .00002), reflecting (somewhat surprisingly) a greater benefit of the 317-SOA condition for the word-reading than the colour-naming task. Importantly, the SOA x Task x Switch interaction was also significant (F = 15.4; df = 1, 30; p < .0005) indicating that the switch cost asymmetry (i.e. Task x Switch interaction) was modulated by the SOA condition. Thus, the present experiment replicates the finding of Yeung (1999) that the 0-SOA condition is associated with a
paradoxical asymmetry of switch costs, and a SOA of approximately 320 ms is associated with a reversed asymmetry of switch costs.

Turning now to the second half of the experiment, an ANOVA was conducted with within-subjects factors Task and Switch, and SOA as a between-subjects factor. In this analysis, SOA refers to the SOA in the previous half of the experiment since all subjects received the same SOA in the second half. There were again significant main effects of Task ($F = 471.4; df = 1, 30; p < .00001$) and Switch ($F = 62.1; df = 1, 30; p < .00001$) but there was no significant Task x Switch interaction ($F < 2.5; df = 1, 30; p > .12$), indicating that the switch costs in the word-reading and colour-naming tasks did not differ significantly. More importantly, neither the main effect of SOA nor any of its interactions reached significance ($F < 1.5; df = 1, 30; p > .24$ in all cases). Thus, there was no evidence that the SOA in the first half of the experiment affected the switch cost asymmetry, or any other aspect of performance, in the second half. The 'top-
Table 6.3 Mean RTs and error rates in each condition for the three groups of subjects

down persistence' account predicts that in the second half of the experiment, the
switch cost asymmetry should be more paradoxical in the 0-SOA group than in
the 317-SOA group. However, the switch cost asymmetry in the 0-SOA group
was actually less paradoxical than that of the 317-SOA group, although this
group difference was not significant. Further ANOVAs were conducted,
separately for the 0-SOA and 317-SOA groups, with factors Half, Task, and
Switch. The three-way interaction was significant in both cases ($F > 5.9; df = 1, 15;$
$p < .05$), indicating that in both groups the switch cost asymmetry in the second
half of the experiment was significantly different from that in the first half.

**Time Course Of SOA Effects** Although there seems to be no evidence that SOA in
the first half of the experiment affects performance in the second half considered
as a whole, it is possible that a more fine-grained analysis might reveal 'top-
down carryover' effects at least at the beginning of the second half of the

<table>
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<td>554</td>
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<td></td>
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<td></td>
<td>Sw</td>
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<td>4.1</td>
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|               | 202         |
Figure 6.7 RTs and error rates for the two groups of subjects throughout the experiment. SOA is 0 or 317 ms in block-pairs 1-3, and 167 ms in block-pairs 4-6. Note NS = nonswitch; Sw = switch.
Figure 6.8 Switch cost asymmetry (word switch cost – colour switch cost) for the two groups of subjects throughout the experiment. The SOAs are different for the two groups of subjects in block-pairs 1-3 and the same in block-pairs 4-6.

In order to test this, the data from the 0-SOA and 317-SOA groups were split up into pairs of blocks across the experiment and are presented in Figures 6.7 and 6.8. It is clear that this analysis fails to support the top-down persistence hypothesis: immediately after the change in the SOA condition (i.e. block-pair 4) the 317-SOA group has a more paradoxical switch cost than the 0-SOA group, the reverse of the pattern predicted by the top-down persistence hypothesis. However, an interesting feature of the data graphed in Figure 6.8 is that the switch cost asymmetry appears to become decreasingly paradoxical throughout the second half of the experiment, by the same amount in both groups. In order to verify this statistically, the data from the second half of the experiment were split into two parts, corresponding to the third and the fourth quarter of the experiment. This was analysed in an ANOVA with within-subject factors Quarter, Task, and Switch, and a between-subjects factor of SOA (referring to SOA in the previous half of the experiment). There was a significant Quarter x Task x Switch interaction ($F = 6.7; \text{df} = 1, 30; p < .015$), indicating that the switch cost does indeed become less paradoxical towards the end of the
second half. However, neither then main effect of SOA nor any of its interactions approached significance (F < 2.4; df = 1, 30; p > .13 in each case).

Errors As with reaction times, the error rates were first analysed in separate ANOVAs for each half of the experiment, with within-subject factors of Task and Switch, and SOA as a between-subjects factor. In the first half of the experiment, there was a significant main effect of Switch (F = 10.4; df = 1, 30; p < 0.005) and a significant Task x Switch interaction (F = 7.8; df = 1, 30; p < .01). This resulted from a tendency in both groups for the switch cost to be greater in the word-reading task (especially since there was a switch benefit in the colour-naming task for the 0-SOA group). This was qualified by a significant SOA x Task x Switch interaction (F = 5.6; df = 1, 30; p < .05), resulting from a more paradoxical switch cost asymmetry in the 0-SOA group than the 317-SOA group. In an ANOVA examining the second half of the experiment, there were main effects of Task (F = 7.9; df = 1, 30; p < .01) and of Switch (F = 11.1; df = 1,30; p < .005) but no Task x Switch interaction (F < 1). Neither the main effect of SOA nor any of its interactions approached significance (F < 1.4; df = 1, 30; p > .25 in each case). Thus, the error rates broadly mirrored reaction times, and there was no evidence of any effect of SOA in the first half of the experiment on performance in the second half.

6.4.3 Discussion

The first half of this experiment replicated the findings of Yeung (1999, Experiment 8), showing that a delay of 317 ms between the onset of the colour and word dimension of a Stroop stimulus can reverse the switch cost asymmetry. An unexpected finding was that the difference in RT between the 0-SOA and 317-SOA groups, favouring the 317-SOA group, was larger for the word-reading than the colour-naming task. Since the purpose of the 317-SOA condition was to
reduce response competition in the colour-naming task, this result is somewhat perplexing. However, it can also be seen in the results from Yeung’s experiment, although no statistical test is reported. One possible explanation results from the decision to exclude congruent stimuli from the experiment. Since subjects could be sure that the colour of the background was not the correct response on word-reading trials, a 317 ms preview of the colour would allow them to inhibit that response, thereby reducing the number of possible responses to choose between and also reducing any interference from the colour dimension. An additional reason for the benefit of the 317-SOA condition on the word-reading task was that, as expected, this led to a greatly reduced word-reading switch cost, speeding overall word-reading RTs.

The pattern of results in the second half of the experiment is of greater theoretical interest. Neither visual inspection of the data nor statistical testing offered any support for the hypothesis that SOA in the first half of the experiment would affect performance in the second half, where the SOA was the same for all subjects. Thus, at least for the experimental conditions tested here, there was no evidence to support the idea that top-down control settings persist involuntarily over a time-course of minutes. Instead, they seem to be rapidly adjusted in order to meet the demands of the currently performed task, hence the absence of any differences between the 0-SOA and 317-SOA groups when they performed identical tasks. This suggests that top-down control inputs are determined ‘on-line’, presumably at least partially on the basis of some performance monitoring mechanism. A possible computational implementation of this is suggested by Botvinick, Braver, Barch, Carter, and Cohen (2001), who suggest that a role of anterior cingulate cortex is to monitor the activation level of various responses, and to increase top-down inputs when there is high response competition. Of course, such a mechanism would not have to take into account top-down inputs
minutes previously, and would therefore be able to account for the data in Experiment 3.

An unexpected finding in this experiment was that, although the previous SOA had no significant effect on the switch cost asymmetry in the second half of the experiment, both groups of subjects had a less paradoxical switch cost asymmetry at the end of the experiment than they did at the beginning of the 167-SOA condition. One possible explanation is that when the SOA condition suddenly changes, subjects respond to this with caution, employing a stronger colour-naming top-down input than they do at the end of the experiment. However, this explanation might predict an increasing colour-naming error rate during the second half of the experiment, whereas the error rate actually appears to fall slightly. Thus, the error data do not provide support for this explanation, although it need not be seen as contradicting it if one assumes that the colour-naming error rate tends to fall during the course of the experiment as a result of practice.

The argument that task history does not affect top-down control settings over a matter of minutes might seem to contradict the repeated finding in studies of task switching that the tasks performed previously can affect behaviour many minutes later (e.g. Allport et al., 1994; Allport & Wylie, 2000; Waszak et al., in press). However, these effects are rather different from the possible carryover effect investigated in Experiment 3. The long-term carryover effects previously reported in task-switching studies have either involved item-specific effects, where the task performed on a specific stimulus affects performance when that stimulus is re-presented several minutes later (e.g. Waszak et al., in press), or alternatively the effect of a task that was performed earlier but is no longer performed (e.g. Allport et al., 1994; see Allport & Wylie, 2000 for an argument that this type of effect is merely another form of item-specific priming). By contrast, in Experiment 3, the same tasks were performed in both halves of the
experiment and the same stimuli were presented. Thus, the factors that have led to long-term carryover effects in previous studies are different from those that were investigated in Experiment 3. Of course, the idea that top-down control inputs are determined dynamically, and do not persist over a time-course of minutes, is not incompatible with the presence of long-term item-specific priming effects. Consequently, there is no reason to see the results of Experiment 3 as conflicting with those of earlier studies revealing long-term carryover effects of a different sort.

Of course, the results from Experiment 3 do not rule out a persistence of top-down control inputs in all circumstances. It is quite possible that in other circumstances (for example, where it is not possible effectively to monitor performance on-line) there would be an effect of task history on top-down control input. If it could be shown that the circumstances in Experiments 2 and 3 differ in some relevant way, it might be possible to argue that the top-down persistence account applies to Experiment 2 but not to Experiment 3. It should be noted that the groups in Experiments 2 and 3 do not map precisely on to each other. The dual-second group in Experiment 2 starts with a task requiring a high top-down control input (ordinary Stroop colour-naming), and then has to perform a secondary task that places an even greater strain on this input. By contrast, the O-SOA group in Experiment 3 starts with a task that requires a similarly high top-down control input but then moves to an easier task. The 317-SOA group, like the dual-second group in Experiment 2, receives a more difficult task in the second half than the first half, but the task performed in the first half was easier than that performed by the dual-second group in Experiment 2.

Data from the 317-SOA group suggested that subjects were able to increase the colour-naming top-down input from the first to the second half of the experiment. Thus, it is possible to reject the hypothesis that, following practice,
top-down inputs may be decreased but not raised. However, it is still possible to argue that, when a top-down input is high (as in the 0-SOA group and the dual-second group in Experiment 2), it cannot be raised any further, following practice. Although this argument is somewhat unprincipled and ad-hoc, it cannot be rejected on the basis of the present data. One relevant experiment would be to repeat Experiment 2 using 317-SOA stimuli. In this case, the colour-naming top-down input should be below the supposed threshold for top-down persistence. Consequently, even dual-second subjects should be able to increase the colour-naming top-down input in the second half of the experiment and there should therefore be no effect of the 1-back task on the switch cost asymmetry if the threshold argument is correct.

6.5 General discussion

Experiment 2 supported the conclusions tentatively drawn from Experiment 1. Performance of the secondary task in the dual-second subjects clearly reversed the switch cost asymmetry, in line with the prediction of the model. Thus, the data from these subjects suggested interference between the control processes involved in switching between the Stroop tasks and performing the secondary task. However, there was no significant effect of the dual-task condition on the switch cost asymmetry in the dual-first subjects. Thus, as suggested by Experiment 1, there appear to be strong effects of single-task practice with the Stroop tasks on subsequent dual-task performance. This practice effect seems to affect the task coordination strategies employed by subjects, hence the qualitative difference between the dual-first and dual-second groups in the effect of the dual-task condition on the switch cost asymmetry.

Two explanations of this group difference were considered. The first was that subjects in the dual-first group continually switch between the Stroop and the 1-
back tasks. In this case, at any one moment only one task will be performed, and there should therefore be no interference between the control processes required in each task. According to the second explanation, the subjects in the dual-second group are unable to adapt the top-down inputs developed in the first half of the experiment, to compensate for the introduction of the secondary task. However, subjects in Experiment 3 were perfectly able to adjust top-down inputs following practice, so this explanation seems unlikely.

Thus, presently the most satisfactory account of the data seems to be that there are two alternative strategies for combining the primary and secondary tasks used in Experiments 1 and 2. The first strategy is to switch rapidly between the primary and secondary tasks, so that only one task is being performed at any one moment. Since this strategy should minimise the interference between tasks, it would not be expected to have any effect on the switch cost asymmetry. An alternative strategy would be to perform the primary and secondary tasks concurrently. This may lead to interference between the control processes involved in the primary and secondary tasks, and hence a modulation of the switch cost asymmetry by secondary task performance. The dual-first subjects in Experiment 2 may have adopted a 'switching strategy' in order to perform the primary and secondary tasks, whereas the dual-second subjects may have adopted a strategy of concurrent primary and secondary task performance.

Some converging evidence for this position comes from a study recently reported by Towse, Hitch, and Hutton (2002; see also Towse & Hitch, 1995; Towse, Hitch, & Hutton, 1998, 2000). Towse et al. (2002) distinguish between 'resource-sharing'

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17 This implies that the working memory secondary task used in Experiments 1 and 2 can be performed either by actively maintaining information from one auditory stimulus to the next (in the concurrent performance strategy) or by retrieving the previous auditory stimulus only when the next one is played (in the switching strategy). See Sakai, Passingham, and Rowe (2002) for some evidence in line with this suggestion.
models of working memory, where capacity is shared between processing and storage functions (e.g. Just & Carpenter, 1992), and a 'task-switching' model, which assumes that subjects switch rapidly between maintaining stored items in working memory and carrying out the processing involved in other tasks. In their study, children performed a variety of working memory tasks in which the presentation of items to be remembered was interpolated between trials of other tasks. Towse et al. (2002) found that memory performance was unaffected by the difficulty of the interpolated tasks, but was affected by their duration, with memory deteriorating as more time was spent on the tasks. They argue that this is incompatible with a resource sharing account, which would predict that more resources could be allocated to the storage of items in working memory when the interpolated tasks are easier. Instead, Towse et al. (2002) suggest that their results are compatible with a 'task-switching model', where memory deteriorates over intervals spent 'switched out' of storage functions. However, Towse et al. (1998) point out that such results do not necessarily rule out the availability of resource sharing strategies. For example, they consider the possibility that 'adults may engage in resource sharing strategies, while children do not do so' (p. 214). Thus, evidence from another experimental paradigm supports the idea that working memory tasks (such as the 1-back secondary task used in Experiments 1 and 2) may be performed in conjunction with other tasks in two qualitatively different ways: either concurrently (i.e. 'resource sharing') or by rapid switching.

If the data from Experiment 2 are to be explained in terms of the dual-first group adopting a switching strategy, and the dual-second group adopting a concurrent performance strategy, what might have led these groups to employ different strategies? One possibility is that the primary and secondary tasks would have caused excessive interference if they were combined early in practice. This may have led to the dual-first group to adopting a switching strategy. But the dual-second subjects, having spent more time practising the Stroop tasks, may have
been able to combine them more successfully with the secondary task, leading to concurrent primary and secondary task performance. An interesting finding in Experiment 1 was that the absolute RT cost of dual-task performance was greater in the ‘odd’ subjects (who corresponded more closely to the dual-first subjects in Experiment 2) than the ‘even’ subjects (who corresponded more closely to the dual-second subjects). However, the effect of the dual-task condition on the switch cost asymmetry was (nonsignificantly) in the direction predicted by the model for the even, but not the odd subjects. This suggests that the switching strategy allows primary and secondary tasks to be combined without interference between control processes, but at a greater overall cost to reaction time than concurrent primary and secondary task performance. The strategy of performing the primary and secondary tasks concurrently may be more efficient in terms of the overall dual-task RT cost but may lead to more interference between control processes.
Chapter 7

Further dual-task investigations of task switching: Effects of varying the primary and secondary tasks

7.1 Introduction

Chapter 6 discussed two types of control process that may be relevant to the experimental paradigm employed in Experiments 1 and 2: a) the top-down control inputs which activate the relevant task demand unit, in accordance with the currently relevant task, and b) the control processes involved in the performance of the secondary task, either through a direct involvement in the auditory 1-back task, or through a general role in the coordination of multiple simultaneous tasks. It was argued on the basis of the model presented in Chapters 3 and 4 that interference between these control processes should be revealed by a 'signature' pattern of data: a modulated asymmetry of switch costs between Stroop tasks. Since this pattern of data was obtained in the dual-second group of Experiment 2, it was argued that these control processes do interfere with each other directly, at least on some occasions.

An important characteristic of the control processes posited by many accounts of executive function is that they are potentially involved in a wide variety of tasks. For example, this is the basis of the latent variable approach taken by Miyake et al. (2000): insofar as there is a valid construct corresponding to the cognitive process of (e.g.) 'inhibition', it should be possible to extract a measure of this from scores on several tasks. To what extent are the control processes discussed with respect to Experiment 2 able to contribute to a wide variety of tasks? In order to investigate this question, the experiments reported in this chapter employ the same experimental design as Experiment 2, but require subjects to
perform different primary and secondary tasks. The rationale for these experiments is as follows. If the results from Experiment 2 reflect the operation of control processes that may be involved in a wide range of different tasks, it should be possible to obtain similar results (i.e. a modulation of the switch cost asymmetry, possibly dependent on the amount of earlier practice with the primary tasks) using different primary and secondary tasks. Alternatively, if different control processes are involved in different types of task, different results might be obtained.

7.2 Experiment 4

In order to investigate the generality of the results from Experiment 2, a different pair of primary tasks (other than Stroop word-reading and colour-naming) and a different secondary task (other than a 1-back working memory task with auditory tones) were chosen. So as to increase the difference between the tasks used in Experiments 2 and 4, different output response modalities were used. In Experiment 2, the primary (Stroop) tasks required a vocal output, while the secondary (1-back) task required a manual output. These response modalities were swapped in Experiment 4, so that the primary tasks required manual responses and the secondary task a vocal response.

In order to investigate a possible asymmetry in switch costs, a pair of primary tasks with different strengths was required. In both tasks, the same stimuli were used: a square presented on the left or the right side of the screen. It is well known from phenomena such as the Simon effect (Simon, 1969; see Lu & Proctor, 1995 for a review) that lateralised stimuli can evoke spatially compatible responses. Thus, a response to the left stimulus with a left button press, and to the right stimulus with a right button press, should be a compatible mapping, and hence a strong task. By contrast, the opposite response mappings (right
response to a left stimulus, and vice versa), should be less compatible. Thus, these two possible response mappings, with stimuli on the left or right side of the screen, were employed as the two primary tasks in Experiment 4. The compatible mapping was labelled the 'pro' task, and the incompatible mapping the 'anti' task, by analogy with the literature on the pro- and antisaccade tasks (Everling & Fischer, 1998). For the secondary task, a 1-back working memory task was used again. However, instead of auditory tones, the stimuli in Experiment 4 were spoken letters of the alphabet. In addition, vocal rather than manual responses were used for this task. Thus, both the primary and secondary tasks in Experiment 4 differed considerably from those used in Experiment 2.

7.2.1 Method

Subjects There were 16 paid subjects, 9 female, aged 19-34 (mean: 22.3).

Apparatus and stimuli Stimuli were presented on a 15” VGA monitor, controlled by an IBM-compatible PC. Responses were collected by means of a response box which was connected to the serial port of the PC.

Subjects sat approximately 60cm from the monitor, with the response box immediately in front of them. As in Experiments 1-3, the monitor displayed a light grey screen, bisected horizontally by a thick black line. For half the subjects, a green rectangle, approximately 5° wide and 0.9° high was presented at the top of the screen, centred horizontally, and a red rectangle of the same proportions was displayed at the bottom of the screen. The other subjects were shown the green rectangle at the bottom of the screen and the red rectangle at the top. These stimuli remained on screen from the start to the end of every block of trials.

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Figure 7.1 Sequence of events in one trial of the pro task in Experiment 4. If the anti task was required, the dark grey square would have appeared beneath the horizontal line.

Single-task condition  Each trial consisted of the following sequence of events. First, a dark grey square with sides approximately 2.3° long was presented either just above or just below the horizontal line, centred horizontally. The gap between the top or bottom of the square and the horizontal line was about 0.9°.
The vertical position of this square served as a cue for the forthcoming task (see below). After 150 ms, the square was removed from the screen. Then, after a 350 ms pause, a similar dark grey square was presented either on the left or the right hand side of the screen, at the same vertical position as the previous square. The edge of this square was approximately 2.3° from the edge of the display area and its horizontal position (left or right) was selected randomly on each trial. Subjects were informed that when this square was presented on the same side of the horizontal line as the green rectangle, they should press the left button on the response box if it was on the left of the screen, and the right button if it was on the right. These response mappings were reversed when the squares were presented on the same side of the horizontal line as the red rectangle. In other words, subjects performed the pro task when the squares were on the same side of the screen as the green rectangle, and the anti task when they were on the same side of the screen as the red rectangle. The left response was made by pressing a button on the left side of the response box with the left index finger, and a right response was made by pressing a button on the right side of the response box with the right index finger. Subjects were instructed to rest their index fingers on these buttons throughout each block. After the subject’s response, or after 3000 ms if no response was detected, the stimulus was removed from the screen, and the cue for the next trial was displayed after a delay of 1250 ms. As in the earlier experiments, the task sequence was entirely predictable and consisted of two trials before each switch. See Figure 7.1 for an illustration of the events of one trial.

**Dual-task condition** The procedure for the dual-task condition was identical to that for the single-task condition, with the following additions. 100 ms after each button-press response, a letter of the alphabet spoken in a female voice was played through a pair of headphones. In order to make sure that these sound samples were easily discriminable from each other, only the following 16 letters
were used: 'a', 'e', 'h', 'i', 'j', 'k', 'l', 'o', 'p', 'q', 'r', 't', 'u', 'w', 'x', 'y'. The samples varied in length from 459-597 ms (mean: 520). This resulted in a delay of at least 503 ms between the end of the sample and the cue for the next trial. They were selected pseudo-randomly, with the constraint that the same stimulus as the previous trial was selected with a probability of 10%. Subjects were instructed to say the word 'yes' if the letter they heard was the same as the one spoken on the previous trial.

**Procedure**  As in Experiment 2, subjects were split into 'dual-first' and 'dual-second' groups, to investigate possible effects of practice on dual-task performance. After the single-task instructions were explained to each subject, there was a 20-trial single-task practice block. The dual-first group then performed a 20-trial dual-task practice block, followed by 5 blocks of 48 trials in the dual-task condition and then 5 blocks of the single-task condition. The dual-second group performed 5 blocks of the single-task condition, followed by a 20-trial dual-task practice block and then 5 blocks of the dual-task condition.

Subjects were encouraged to rest between blocks, and started each new block with a button press. The vertical position of the first trial of each block (and hence the required task) alternated from block to block.

**7.2.2 Results**

Trials where the subject failed to respond within 3000 ms were scored as errors. For RT analyses, error trials were excluded, as were trials immediately following an error and trials immediately following a 'yes' response in the dual-task condition. 14% of trials were excluded for one or more of these reasons. All group RT analyses are based on mean RTs calculated in each design cell for each subject.
Figure 7.2  Mean RTs, collapsed over the dual-first and dual-second groups, in each condition of Experiment 4. Error bars show standard errors. Note NS = nonswitch; Sw = switch.

Figure 7.3  Switch costs in the two tasks in single-task and dual-task conditions of Experiment 4, collapsed over the dual-first and dual-second groups.
RTs were analysed in a repeated measures ANOVA with within-subjects factors of ‘Dualtask’ (whether the block was in the single- or dual-task condition), ‘Task’ (pro or anti), and ‘Switch’ (switch or repeat), and a between-subjects factor of ‘Group’ (dual-first or dual-second). As expected, there were significant main effects of Task (F = 11.0, df = 1, 14; p = .005), since pro trials were faster than anti trials, and Switch (F = 25.5, df = 1, 14; p < .0002), since nonswitch trials were faster than switch trials. There was also a significant Task x Switch interaction (F = 5.8, df = 1, 14; p = .03), resulting from a larger switch cost in the pro than the anti task (i.e. the switch costs were ‘paradoxically’ asymmetrical, as in Experiments 1 and 2 with Stroop tasks). There was no main effect of the Dualtask factor (F < 1), but the Dualtask x Group interaction was marginally significant (F = 4.0, df = 1, 14; p = .07). This was because the dual-first group had slower RTs in the dual-task than the single-task conditions, whereas the dual-second group had slower RTs in the single-task than the dual-task condition. This finding may reflect differences between the two groups in the effect of the dual-task condition on RTs. However, since the two groups performed the dual-task condition in different halves of the experiment, it may simply reflect differences between the two halves, independently of any effects of the dual-task condition. For example, suppose that RTs speed up by 100 ms in the second half of the experiment as a result of practice, and there is an additional, additive cost of 50 ms associated with the dual-task condition. In this case, the dual-task condition would be 150 ms slower than the single-task condition in the dual-first group, but 50 ms faster in the dual-second group, leading to a Dualtask x Group interaction, even though the dual-task cost was the same for both groups. Since there was no single-task control group in this experiment, it is not possible to know what the RTs in the two halves of the experiment would be if they were not confounded with performance of the dual-task condition. Thus, two alternative (but not mutually exclusive) accounts of the Dualtask x Group interaction are available:
the dual-task cost may be greater in the dual-first than dual-second group, and/or subjects may speed up over the course of the experiment, due to practice.

Interestingly, there was a reliable Dualtask x Switch interaction \( (F = 8.3; \ df = 1, 14; \ p = .01) \), reflecting smaller switch costs in the dual-task condition than the single-task condition. However, the Dualtask x Task x Switch interaction was not significant \( (F < 1) \), indicating that the switch cost asymmetry was not significantly affected by the dual-task condition. No other main effects or interactions were significant. Thus, the dual-task condition did not reliably slow RTs, nor did it reliably affect the switch cost asymmetry, but it did reduce switch costs. These findings are summarised in Table 7.1 and Figures 7.2 and 7.3. The results from the dual-first and dual-second groups were collapsed in order to produce the figures, since the Dualtask x Task x Switch x Group interaction was not significant \( (F = 2.6; \ df = 1, 14; \ p > .1) \). Thus, there was no reason to suspect that any effect of the dual-task condition on the switch cost asymmetry was dependent on practice. In addition, the Dualtask x Switch x Group interaction was far from significant \( (F < 1) \), indicating that the ability of the dual-task condition to reduce the switch cost did not differ reliably between groups. As Figure 7.2 shows, the dual-task condition was associated with a cost of around 20 ms on nonswitch trials, and a benefit of approximately the same magnitude on switch trials.

**Errors** The first trial of each block was excluded from the following analyses. Mean error rates in each condition were calculated for each subject and entered into a repeated measures ANOVA with within-subject factors Dualtask, Task, and Switch, and Group as a between-subjects factor. There was a significant main effect of Switch \( (F = 19.0, \ df = 1, 14; \ p < .001) \), since more errors were committed on switch than nonswitch trials. There was also a significant Group x Dualtask x Switch interaction \( (F = 5.8; \ df = 1, 14; \ p = .03) \). This could result from an ability of the dual-task condition to reduce switch costs more effectively in the dual-
Table 7.1 Mean RTs and error rates in each condition for the two groups of subjects.  
*Note NS = non-switch; Sw = switch.*

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<th>Dual-first</th>
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<td></td>
<td>RT (ms)</td>
<td>Errors (%)</td>
<td>RT (ms)</td>
<td>Errors (%)</td>
</tr>
<tr>
<td>First</td>
<td></td>
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<tr>
<td>Pro</td>
<td>NS 407</td>
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</tr>
<tr>
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</tr>
<tr>
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<tr>
<td>Sw</td>
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<td></td>
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<tr>
<td>Anti</td>
<td>NS 408</td>
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<td>389</td>
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<tr>
<td>Sw</td>
<td>483</td>
<td>10.5</td>
<td>398</td>
<td>5.7</td>
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second than the dual-first group, or a reduction in switch costs from the first to the second half of the experiment (or both). As with the Group x Dualtask interaction in the RT data, it is not possible to decide between these explanations without a single-task control group. In addition to these significant effects, there were two intriguing interactions involving the Group factor. A Task x Group interaction \( (F = 7.4, \text{df} = 1, 14; p = .016) \) reflected a greater error rate in the anti task for the dual-first group, but a greater error rate in the pro task for the dual-second group. In addition, the switch cost was greater in the anti task for the dual-first group, and the pro task for the dual-second group, leading to a significant Task x Switch x Group interaction \( (F = 4.7; \text{df} = 1, 14; p < .05) \). Thus, in the dual-first group there were more errors overall and a larger switch cost in the anti task; in the dual-second group there were more errors overall and a larger switch cost in the pro task.
Secondary Task Performance  Accuracy in the secondary (1-back) task was high in both the dual-first and dual-second groups. Hit rates, false alarm rates, and \(d'\) values, calculated individually and then averaged across subjects in each group, are given in Table 7.2. The \(d'\) values were significantly higher in the dual-second than the dual-first group (\(t = 3.3, \text{df} = 14, p = .005\)). Thus, performance in the secondary task was better when subjects had first practised the primary tasks alone.

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<tr>
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<tr>
<td>Correct Detections (%)</td>
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<td>93.9</td>
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<tr>
<td>False Alarms (%)</td>
<td>0.3</td>
<td>0.3</td>
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<tr>
<td>(d')</td>
<td>3.8</td>
<td>4.5</td>
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Table 7.2  Mean secondary task performance in the dual-first and dual-second groups.

7.2.3 Discussion

The results from this experiment were strikingly different from those obtained in Experiment 2, where the dual-task condition had no reliable effect on switch costs in the dual-first group and reversed the switch cost asymmetry in the dual-second group. In the RT data from this experiment, the dual-task condition did not reliably affect the switch cost asymmetry, but did lead to reduced switch costs. The error data in the first half of the experiment was compatible with the effects of the dual-task condition on the switch cost asymmetry predicted by the model, since the subjects performing the single-task condition had a higher error switch cost in the pro task, whereas those performing the dual-task condition had a higher error switch cost in the anti task. However, each group's pattern of error data persisted into the second half of the experiment, even though the subjects switched from the single- to the dual-task condition, or vice versa. Thus, the
error data do not seem to reflect direct effects of the dual-task condition on
switch costs. Rather, they seem to reflect differences between the two groups of
subjects, caused by the circumstances at the beginning of the experiment, which
have effects throughout the experiment. These effects may be caused by strategic
differences between the groups, which are maintained through the whole
experiment. In summary, this experiment did not provide evidence for a
modulation of the switch cost asymmetry by the dual-task condition, since there
was no hint of such an effect in the RT data, and the error data, within each
group, did not differ reliably between the single-task and dual-task conditions.

Before going on to discuss the implications of this pattern of data, a basic
question will be addressed: what caused the differences between Experiment 2
and Experiment 4? Since both the primary and secondary tasks were different in
these experiments, either (or both) may be responsible for the different results.
Thus, in order to investigate whether the change in primary task alone was
responsible for the results of Experiment 4, the following experiment seeks to
replicate Experiment 4 using the same secondary task as was used in Experiment
2.

7.3 Experiment 5

7.3.1 Method

Subjects There were 16 paid subjects, 7 female, aged 17-34 (mean: 22.7).

Procedure The procedure of this experiment was the same as Experiment 4.
However, different auditory stimuli were presented in the dual-task condition,
as stimuli for the 1-back task. Rather than letters of the alphabet, the auditory
tones that were presented in Experiments 1 and 2 were used. The tones had one
of three pitches: low (80 Hz), medium (320 Hz), or high (1280 Hz) and a duration of 200 ms. Since this was shorter than the duration of the auditory stimuli in Experiment 4, the interval from the end of the auditory stimulus until the cue for the following trial was longer in Experiment 5 than it was in Experiment 4. This ensured that the timing parameters of the single task condition were identical in Experiments 4 and 5.

7.3.2 Results

The data were analysed in the same manner as Experiment 4. The RT exclusion criteria led to 13% of trials being excluded for RT analyses.

RTs were analysed in a repeated measures ANOVA with within-subjects factors Dualtask, Task, and Switch, and Group as a between-subjects factor. Unlike Experiment 4, there was a significant main effect of Dualtask ($F = 19.2$, $df = 1, 14$, $p < .001$), since RTs were slower in the dual-task than the single-task condition. There was also a significant Dualtask x Group interaction ($F = 7.1$, $df = 1, 14$, $p = .02$), reflecting either a greater dual-task cost in the dual-first subjects or a tendency for subjects to speed up in the second half of the experiment (or both). The main effect of Switch was also significant ($F = 21.7$, $df = 1, 14$, $p < .001$), switch trials being slower than nonswitch trials. As in Experiment 4, RTs in the pro task were faster than those in the anti task, which led to a marginally significant main effect of task ($F = 3.5$, $df = 1, 14$, $p = .08$). Also in accordance with Experiment 4, the switch cost was larger in the pro than the anti task, leading to a significant Task x Switch interaction ($F = 13.0$, $df = 1, 14$, $p = .003$). Crucially, in agreement with Experiment 4, the Dualtask x Switch interaction was significant ($F = 4.8$, $df = 1, 14$, $p < .05$), indicating that, once again, the dual-task condition led to reduced switch costs. Visual inspection of the data, presented in Table 7.3 and Figures 7.4 and 7.5, suggests that the dual-task condition reduced the switch
Figure 7.4  Mean RTs, collapsed over the dual-first and dual-second groups, in each condition of Experiment 5. Error bars show standard errors. Note NS = nonswitch; Sw = switch

Figure 7.5  Switch costs in the two tasks in the single-task and dual-task conditions of Experiment 5, collapsed over the dual-first and dual-second groups. Note NS = nonswitch; Sw = switch
cost more in the anti task than the pro task. However, the Dualtask x Task x Switch interaction was not significant (F = 1.3, df = 1, 14, p = .27), so the suggestion of a modulation of the switch cost asymmetry by the dual-task condition is not supported statistically. No other main effects or interactions were significant. In summary, the RT data were similar to those obtained in Experiment 4, with the exception that there was a larger cost of the dual-task condition in Experiment 5 than Experiment 4 (hence the significant main effect of Dualtask in Experiment 5, but not Experiment 4).

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<td>Sw</td>
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<td>7.9</td>
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Table 7.3 Mean RTs and error rates in each condition for the two groups of subjects. Note NS = non-switch; Sw = switch.

Errors As in Experiment 4, the first trial of each block was excluded from the error analyses. Mean error rates in each condition were calculated for each subject and entered into a repeated measures ANOVA with within-subject factors Dualtask, Task, and Switch, and a between-subjects factor of Group. The main effect of Switch was significant (F = 47.0, df = 1, 14, p < .001), since more
errors were committed on switch than nonswitch trials. Unlike Experiment 4, the Task x Group and Task x Switch x Group interactions were not significant (F = 1.5, df = 1, 14, p = .24 and F = 2.6, df = 1, 14, p = .13 respectively). Also in contrast with Experiment 4, there was no significant Dualtask x Switch x Group interaction (F < 1). Thus, Experiment 5 failed to replicate some of the unexpected results in the error data from Experiment 4. None of the other main effects or interactions were significant.

**Secondary Task Performance** Hit rates, false alarm rates, and $d'$ values for the two groups are given in Table 7.4. The $d'$ values were somewhat lower than those in Experiment 4, but similar to those in Experiments 1 and 2 which also used tones as the stimuli for the secondary task. As in Experiment 4, $d'$ values were higher in the dual-second group than the dual-first group, but this difference was not significant ($t = 1.0, df = 14, p = .34$).

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<tr>
<td>$d'$</td>
<td>3.3</td>
<td>3.7</td>
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**Table 7.4** Mean secondary task performance in the dual-first and dual-second groups.

7.3.3 **Discussion**

The results from Experiments 4 and 5 were similar in most respects. However, one difference was that, in Experiment 5 but not Experiment 4, there was a large RT cost associated with performance of the dual-task condition. This is similar to the results from Experiment 2, using the same secondary task stimuli as Experiment 5. By contrast, in Experiment 4, using spoken letters of the alphabet
instead of tones as stimuli for the secondary task, there was no reliable main effect of Dualtask. In addition, accuracy in the secondary task was greater in Experiment 4 than in Experiment 5. Thus, when combined with the primary tasks used in Experiments 4 and 5, a secondary task using spoken letters rather than tones as stimuli seems to be easier, in that it is performed more accurately, with a smaller cost to the concurrently performed primary tasks. There are several possible reasons for this. For example, since there were more possible auditory stimuli in Experiment 4 than Experiment 5, the danger of a false alarm to a stimulus presented two trials previously, rather than on the previous trial, is reduced. In addition, linguistic stimuli may be more easily maintained in working memory than tones of different pitches.

Despite these differences in the effects of the secondary tasks used in Experiments 4 and 5, the results relating to switch costs were broadly equivalent in the two experiments. In both experiments, dual-task performance led to reduced switch costs without affecting the switch cost asymmetry. Thus, this pattern of data, as opposed to the results from Experiment 2, which indicated a modulation of the switch cost asymmetry by dual-task performance in the dual-second but not the dual-first group, seems to result from the difference in primary tasks between Experiment 2 and Experiments 4-5. However, before this conclusion may be confidently drawn, one other difference between Experiment 2 and Experiments 4-5 must be addressed. The timing parameters of Experiments 4 and 5 differed from those used in Experiment 2. In Experiment 2, the auditory stimulus for the secondary task was presented 450 ms after the response to the primary (Stroop) tasks, to ensure that subjects had finished articulating their response before being presented with the auditory stimulus. Since Experiments 4 and 5 required a manual response in the primary tasks, this delay was not necessary and the auditory stimulus was presented 100 ms after each response to
the primary task. Thus, any difference between the results of Experiment 2 and those of Experiments 4-5 may result from this difference in timing parameters.

7.4 Experiment 6

In order to rule out the possibility that the effects of the secondary-task on switch costs differed between Experiment 2 and Experiments 4/5 because of a difference in timing parameters, Experiment 6 seeks to replicate Experiment 5 using the same timing parameters as Experiment 2.

7.4.1 Method

*Subjects* There were 16 paid subjects, 7 female, aged 18-44 (mean: 25.6).

*Procedure* The procedure of Experiment 6 was identical to that of Experiment 5 except for the following changes to the timing parameters in the dual-task condition. Following each response in the primary tasks, there was a 450 ms delay (instead of 100 ms in Experiment 4), after which the auditory tone, with a duration of 200 ms, was played. There was then a delay of 600 ms before the cue for the next primary-task trial. Thus, the timing parameters were identical to those used in Experiment 2.

7.4.2 Results

The data were analysed in the same manner as Experiments 4 and 5. The RT exclusion criteria led to 14% of trials being excluded from RT analyses.

RTs were analysed in a repeated measures ANOVA with within-subjects factors of Dualtask, Task, and Switch, and a between-subjects factor of Group. As in
Figure 7.6 Mean reaction times, collapsed over the dual-first and dual-second groups, in each condition of Experiment 6. Error bars show standard errors. Note NS = nonswitch; Sw = switch.

Figure 7.7 Switch costs in the two tasks in single-task and dual-task conditions of Experiment 6, collapsed over the dual-first and dual-second groups.
Experiment 5, there were significant main effects of Dualtask (F = 13.2, df = 1, 14, p = .003) and Switch (F = 27.7, df = 1, 14, p < .001). The main effect of Task, which was marginally significant in Experiment 5, was also significant (F = 6.4, df = 1, 14, p = .02). The Dualtask x Group interaction, reflecting a greater dual-task cost in the dual-first subjects, or a tendency to speed up in the second half of the experiment (or both), was marginally significant (F = 4.2, df = 1, 14, p = .06).

Although the switch cost was larger in the pro than the anti task, as in Experiments 4 and 5, the Task x Switch interaction did not reach significance (F = 1.3, df = 1, 14, p = .27). Also as in Experiments 4 and 5, the dual-task condition reduced switch costs, although the Dualtask x Switch interaction did not quite reach significance (F = 2.4, df = 1, 14, p = .07, one-tailed). The Dualtask x Task x Switch interaction was far from significant (F < 1), indicating that, as in Experiments 4 and 5, there was no tendency for the dual-task condition to modulate the switch cost asymmetry.

One difference between Experiment 6 and Experiment 5 was that there was a significant Dualtask x Switch x Group interaction (F = 9.4, df = 1, 14, p < .01). This could be interpreted either as a tendency for the dual-task condition to reduce switch costs in the dual-second but not the dual-first group, or simply as a tendency for switch costs to be smaller in the second half of the experiment, in both groups of subjects. These results are summarised in Table 7.5 and graphed in Figures 7.6 and 7.7.

Errors As in Experiments 4 and 5, the first trial of each block was excluded from the error analyses. Mean error rates in each condition were calculated for each subject and entered into a repeated measures ANOVA with within-subject factors Dualtask, Task, and Switch, and a between-subjects factor of Group. There was a significant main effect of Switch (F = 35.7, df = 1, 14, p < .001), reflecting a greater error rate on switch than nonswitch trials, and also of Task (F
Table 7.5 Mean RTs and error rates in each condition for the two groups of subjects.
Note NS = non-switch; Sw = switch.

= 6.7, df = 1, 14, p = .02), reflecting somewhat surprisingly, a greater error rate on pro than anti trials. This seems to result from a significantly larger switch cost in the pro than the anti task, the same asymmetry of switch costs observed in the RT data (Task x Switch interaction: F = 5.3, df = 1, 14, p = .04). There was also a significant Dualtask x Group interaction (F = 10.1, df = 1, 14, p < .01) which, surprisingly, resulted from a greater error rate in the dual-task condition for the dual-second group, but a greater error rate in the single-task condition for the dual-first group. In other words, error rates in the second half of the experiment were greater than those in the first half. This may reflect a speed-accuracy trade off, since RTs were faster in the second half of the experiment (hence the marginally significant Dualtask x Group interaction in the RT data).
Secondary Task Performance Hit rates, false alarm rates, and $d'$ values for the two groups are given in Table 7.6. The $d'$ values were roughly equivalent to those in Experiment 5. As in Experiment 5, $d'$ values were higher in the dual-second group than the dual-first group, but again this difference was not significant ($t = 1.6, \text{df} = 14, p = .12$).

<table>
<thead>
<tr>
<th></th>
<th>Dual-first</th>
<th>Dual-second</th>
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<tr>
<td>Correct Detections (%)</td>
<td>68.3</td>
<td>84.5</td>
</tr>
<tr>
<td>False Alarms (%)</td>
<td>1.4</td>
<td>1.5</td>
</tr>
<tr>
<td>$d'$</td>
<td>2.9</td>
<td>3.5</td>
</tr>
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</table>

Table 7.6 Mean secondary task performance in the dual-first and dual-second groups.

7.4.3 Discussion

Experiment 6, using the same secondary task and timing parameters as Experiment 2, but different primary tasks, showed that the dual-task condition led to reduced RT switch costs. The dual-task condition did not reliably affect the switch cost asymmetry, and the effect of the dual-task condition on the switch cost asymmetry (or rather, the lack of one) did not differ reliably between the dual-first and dual-second groups. In these respects, the results from Experiment 6 were consistent with those from Experiments 4 and 5. These results contrast strikingly with Experiment 2, where the dual-task condition had no significant effect on switch costs in the dual-first group, and reversed the switch cost asymmetry in the dual-second group. Experiment 6 suggests that the differences between Experiment 2 and Experiments 4-6 in the effect of the dual-task condition on switch costs result from the different primary tasks used, rather than the different timing parameters used in Experiments 4 and 5.
7.5 Comparison between Experiments 4-6

Experiments 4-6 required subjects to perform the same primary tasks. Experiment 5 used a different secondary task to Experiment 4 (tones rather than letters), but had the same timing parameters. Experiment 6 used the same tasks as Experiment 5 but had different timing parameters so that it was directly comparable to Experiment 2. In order to compare reaction times across the experiments, data from all three experiments were entered into a repeated measures ANOVA with within-subject factors Dualtask, Task, and Switch, and between-subject factors Group and Experiment (4, 5, or 6). There was a significant main effect of Dualtask ($F = 27.8$, $df = 1, 42$, $p < .0001$), reflecting slower RTs in the dual-task than the single-task condition. In the analyses of individual experiments reported above, this was significant in Experiments 5 and 6, but not Experiment 4. In the present analysis, there was also a significant Dualtask x Experiment interaction ($F = 6.5$, $df = 2, 42$, $p = .003$). Thus, the different secondary tasks used led to significantly different dual-task costs: the task using spoken letters as stimuli in Experiment 4 did not produce any reliable dual-task cost, whereas there was a significant dual-task cost in Experiments 5 and 6 which used tones of different pitches. Consequently, in pairwise comparisons between the experiments, the Dualtask x Experiment interaction was significant when Experiment 4 was compared with Experiment 5 ($F = 9.9$; $df = 1, 30$; $p = .004$) and when it was compared with Experiment 6 ($F = 8.5$; $df = 1, 30$; $p = .007$), but not when Experiments 5 and 6 were compared ($F < 1$).

The Dualtask x Group interaction, which was significant in Experiment 5 and marginally significant in Experiments 4 and 6, was significant in the combined analysis of all three experiments ($F = 14.1$, $df = 1, 42$, $p = .0005$), and did not interact with the Experiment factor ($F < 1$). As discussed above, this may reflect either a greater dual-task cost in the dual-first than the dual-second group, a
general tendency for subjects to speed up in the second half of the experiment, or both. Since there were no single-task control groups in Experiments 4-6, it is not possible to decide between these accounts, because it is not possible to know to what extent subjects would speed up in the second half of the experiment if they never performed the dual-task condition.

In all three experiments, the anti task had slower RTs than the pro task, switch trials were performed more slowly than nonswitch trials, and the switch cost was larger in the pro than the anti task. Confirming these results, the combined analysis of all three experiments revealed significant main effects of Task (F = 18.1, df = 1, 42, p < .0001) and Switch (F = 73.6, df = 1, 42, p < .0001), and a significant Task x Switch interaction (F = 16.6, df = 1, 42, p = .0002). The interaction of the Experiment factor with each of these was nonsignificant (F < 1.3, df = 2, 42, p > .29).

Importantly, the combined analysis of all three experiments also revealed a significant Dualtask x Switch interaction, reflecting a reduction of the switch cost by the dual-task condition (F = 14.7, df = 1, 42, p = .0004). This was significant in Experiments 4 and 5, but only marginally significant (one-tailed) in Experiment 6. Nevertheless, the Dualtask x Switch x Experiment interaction was not significant (F = 1.6, df = 2, 42, p = .22).

There was also a significant Dualtask x Switch x Group interaction (F = 4.8, df = 1, 42, p = .03). This was significant in Experiment 6 but not Experiments 4 or 5. However, the Dualtask x Switch x Group x Experiment interaction was not significant (F = 2.0, df = 2, 42, p = .14), so there is no reason to conclude that the experiments differed in this respect. Like the Dualtask x Group interaction, the Dualtask x Switch x Group interaction could be interpreted in more than one
way. It might reflect a tendency for the dual-task condition to reduce switch costs more effectively in the dual-second than the dual-first group, a general tendency for the switch cost to become smaller in the second half of the experiment, or both of these. It is not possible to decide between these explanations since there were no single-task control groups. However, this interaction, along with the Dualtask x Group interaction, does not seem to be of great theoretical significance. The purpose of investigating separate dual-first and dual-second groups was to investigate whether any effects of the dual-task condition on the switch cost asymmetry would be dependent on practice, as in Experiment 2. In the combined analysis of the three experiments neither the Dualtask x Task x Switch nor the Dualtask x Task x Switch x Group interactions were significant (F < 2.7, df= 1, 42, p > .1), and neither of these interacted with Experiment (F < 1). Thus, there was no evidence in any experiment that the dual-task condition affected the switch cost asymmetry, and nor was there any evidence for a difference between the dual-first and dual-second group in this respect. The data from Experiments 4-6 are presented separately for the dual-first and dual-second groups in Appendix B.

Next, the error data from the three experiments were analysed in a similar manner. This revealed a significant main effect of Switch (F = 75.9, df = 1, 42, p < .0001), since more errors were committed on switch than nonswitch trials. There was also a significant Task x Switch interaction (F = 5.3, df = 1, 42, p = .03), since the switch cost was larger in the pro than the anti task. None of the other main effects or interactions (including those interactions involving the Experiment factor) were significant. Thus, although analyses of the individual experiments suggested intriguing patterns in the error data (e.g. the Task x Group interaction in Experiment 4, or the Dualtask x Group interaction in Experiment 6), these were not confirmed in the combined analysis of the experiments. Since the effects of a large number of factors and their interactions were tested in the ANOVAs reported above, the probability of type 1 errors is high (Jurlburt & Spiegel, 1976). Thus, at present it seems premature to attribute any theoretical
significance to the unexpected findings reported in the analyses of the error data from Experiments 4-6 that were not confirmed by the combined analysis.

In a final comparison between the experiments, each subject's $d'$ value, reflecting accuracy in the secondary task, was entered into a univariate ANOVA with factors Group and Experiment. This revealed a significant main effect of Group ($F = 8.1$, df = 1, 42, $p = .007$), since dual-second subjects were generally more accurate than dual-first subjects. There was also a significant main effect of Experiment ($F = 8.8$, df = 2, 42, $p = .001$). Pairwise comparisons between the experiments showed that subjects in Experiment 4 were significantly more accurate than those in Experiment 5 ($F = 9.1$, df = 1, 28, $p = .005$) and those in Experiment 6 ($F = 22.9$, df = 1, 28, $p < .0001$). There was no significant difference in accuracy between Experiments 5 and 6 ($F < 1$). Thus, subjects performing the secondary task with spoken letters outperformed each of the two groups performing the secondary task with tones, who did not differ significantly from each other. The Group x Experiment interaction was significant neither in the analysis of all three experiments nor in any of the pairwise comparisons ($F < 1$ in all cases).

7.6 Reduced switch costs in dual-task conditions: A hypothesis

The unexpected finding that the dual-task condition was able to reduce switch costs without affecting the switch cost asymmetry was consistently obtained in all three of the experiments reported in this chapter. How does this relate to the earlier experiments reported in Chapters 5 and 6? To recap, Experiments 1 and 2 sought to investigate the prediction that secondary task performance should modulate the switch cost asymmetry when subjects switch between Stroop word-reading and colour-naming tasks, so that the cost of a switch into the colour-naming task should be closer to, or larger than a switch into the word-
reading task. This prediction was confirmed in the dual-second subjects in Experiment 2, but not in the dual-first subjects. There was no significant modulation of the switch cost asymmetry by the dual-task condition in Experiment 1, where subjects alternated between single-task and dual-task blocks. Chapter 6 argued that this probably results from the majority of subjects in Experiment 1 and the dual-first subjects in Experiment 2 adopting a strategy of alternating between the primary and secondary tasks, in order to avoid interference between them. By contrast, the dual-second subjects in Experiment 2 were assumed to have performed the primary and secondary tasks concurrently, leading to a modulated switch cost asymmetry in the primary tasks.

It is worth noting that, in Experiment 1, the dual-task condition was associated with smaller switch costs than the single-task condition. In the first half of Experiment 2, switch costs were slightly smaller in dual-first subjects, performing the dual-task condition, than the single-task control subjects. However, in neither of these cases was the reduction in switch costs significant. Nevertheless, since switch costs were reduced significantly by the dual-task condition in Experiments 4 and 5, and marginally significantly in Experiment 6, it would be parsimonious to explain the reduced switch costs in each of these experiments in the same way. The results in Experiment 1, and the dual-first condition of Experiment 2, were explained in terms of a switching strategy, whereby subjects rapidly alternate between the primary and secondary tasks. Might the same explanation apply to the subjects in Experiments 4-6? In other words, is it possible that a switching strategy could account for the reduced switch costs in the dual-task condition that were obtained in Experiments 4-6, as well as (nonsignificantly) in Experiments 1 and 2?

Intuitively it seems plausible that interleaving a third task in between trials of the two primary tasks might reduce the carryover of task set between the two
primary tasks. In the model presented in Chapter 3, there are inhibitory connections between the two task demand units representing the word-reading and colour-naming tasks. This is in common with several models of the control of sequential action (e.g. Cooper & Shallice, 2000), which posit inhibitory connections between the representations of mutually incompatible behaviours. Suppose that the secondary tasks performed in Experiments 1-2 and 4-6 were represented by a third task demand unit in the model. Activating this unit in the dual-task condition might inhibit the other two units, representing the primary tasks. This might reduce the carryover of activation from one trial to the next in these task demand units, the main cause of switch costs in the model. Thus, it seems that performing a third task in the intervals between trials of the two primary tasks might reduce switch costs in this way. In order to test whether this account is plausible, it was implemented in the model described in Chapter 3.

7.7 Simulations

7.7.1 Method

In these simulations, it was assumed that the two primary tasks performed by the model could just as well represent Stroop word-reading and colour-naming, or the pro and anti tasks. Thus, no attempt was made to adjust the parameters so as to provide a precise quantitative fit to the data from the latter tasks. Rather, the purpose of these simulations was simply to show that interleaving a third task between trials of the two alternating primary tasks may reduce switch costs between the primary tasks. In order to simulate the dual-task results, the model was modified in the following ways (see Figure 3.1 for the model's basic architecture). First, an extra task demand unit, representing the secondary task, was added to the two units representing the primary tasks. A mechanism for the performance of the secondary task was not implemented, so the only connections
of the task demand unit representing the secondary task were reciprocal inhibitory connections with the other two task demand units, in accordance with the basic architecture of the model (see Chapter 3). In contrast with the majority of simulations in Chapters 3 and 4, which were run without a preparation interval, each of the simulations with the modified model were run with a 100 cycle preparation interval. This was necessary in order to simulate events relating to secondary task performance, which takes place in the intervals between trials. The duration of the simulated preparation interval corresponds to around 900 ms according to the regression equations based on the model's simulation of the Stroop data. This is approximately equal to the time available to the subjects in Experiments 4-6 to perform the secondary task during the preparation interval\(^\text{18}\).

For simplicity, and in order to ensure that the model produced large switch costs, there was no activation of the task demand units representing the primary tasks during the preparation interval. This seems appropriate since switch costs were substantial after long preparation intervals (over a second) in Experiments 1-6 above, as well as in an earlier experiment investigating the effects of preparation in switch costs with Stroop tasks (Allport et al., 1994, Experiment 5). Thus, it is doubtful whether the preparation interval is used effectively to prepare for the primary tasks in these experiments. In order to simulate the dual-task condition, the secondary-task task demand unit was activated during each preparation interval by applying an external top-down input of 6 (the same strength as the word-reading top-down input). Other than this, the single-task and dual-task conditions were identical.

\(^{18}\) The preparation interval for these experiments was 1250 ms, but the stimulus for the secondary task was not presented until 100 ms (Experiments 4-5) or 450 ms (Experiment 6) after the beginning of the preparation interval.
Figure 7.8 Simulated reaction times, in cycles, in the pro and anti tasks, in single-task and dual-task conditions. In this simulation, a strategy of alternating between the primary and secondary tasks is assumed. Note NS = nonswitch; Sw = switch

In common with the earlier simulations, the activation levels of the task demand units representing the primary tasks were squashed after the production of each response. In addition, the task demand unit representing the secondary task was squashed at the end of the preparation interval. Thus, the squashing of each task demand unit was assumed to take place at the termination of processing related to that task demand unit.

7.7.2 Results
The results from these simulations are graphed in Figures 7.8 and 7.9, with the stronger task (previously labelled 'word-reading') labelled 'pro', and the weaker task (previously labelled 'colour-naming') labelled 'anti'. As Figure 7.8 shows, the simulated dual-task condition had little effect on nonswitch trials. However, there was a small reduction in both reaction time and error rate, on both pro and anti switch trials, in the dual-task condition. Thus, the simulated dual-task condition was able to reduce the switch cost (see Figure 7.9), as in Experiments 4-6 (see e.g. Figure 7.2).

7.7.3 Concurrent primary and secondary task performance

Next, in order to show that the updated model can still simulate an effect of the dual-task condition on the switch cost asymmetry, as found in the dual-second
In Figure 7.10, simulated reaction times, in cycles, in the word-reading and colour-naming tasks, in single-task and dual-task conditions. In this simulation, a strategy of concurrent primary and secondary task performance is assumed. Note NS = nonswitch; Sw = switch.

Group in Experiment 2, concurrent performance of the primary and secondary tasks was simulated. In order to do this, the two tasks were relabelled 'word-reading' and 'colour-naming', since data from Experiment 2 were being simulated. The secondary-task task demand unit received a constant input of 6 (as above), during both primary-task performance and the interval between trials. It was assumed that this top-down input cannot be sustained at the same time as the strong top-down input required for the colour-naming task. Thus, the top-down input into the colour-naming task demand unit was reduced from 15 to 9, so that the combined top-down input during colour-naming performance
Figure 7.11 Simulated switch costs in the word-reading and colour-naming tasks, in single-task and dual-task conditions. In this simulation, a strategy of concurrent primary and secondary task performance is assumed.

(colour-naming + secondary task) was 15. In an effort to reduce the error rate on colour-naming switch trials in the dual task condition, the noise parameter was reduced from 0.006 to 0.005 in this simulation. A possible justification for this is that data from well practised (i.e. dual-second) subjects were simulated here. Such subjects may be able to perform the tasks more efficiently, with less interference from noise. Indeed, it was argued in Chapter 6 that the dual-second subjects in Experiment 2 may have suffered less interference in the dual-task condition than the dual-first subjects, so allowing them to perform primary and secondary tasks concurrently. As Figures 7.10 and 7.11 show, in this simulation the switch cost asymmetry was reversed in the dual-task condition, as found in the dual-second subjects in Experiment 2. Thus, the model produces reduced switch costs in the dual-task condition if a switching strategy is assumed, and a reversed asymmetry of switch costs if a strategy of concurrent primary and secondary task performance is assumed.
Despite these successes, there are several shortcomings in the simulations reported above. When a switching strategy is assumed (Figures 7.8 and 7.9), error rates are lower than the equivalent empirical data (e.g. Table 7.1). In addition, the model fails to simulate any effect of dual-task performance on non-switch trials and the simulated reduction in switch costs appears smaller than the reduction obtained in Experiments 4-6. In the simulation of concurrent primary and secondary task performance (Figures 7.10 and 7.11), error rates are higher than the equivalent empirical data (see Table 6.1), and confined almost exclusively to colour-naming switch trials in the dual-task condition. Another problem is that colour-naming non-switch trials are slowed in the dual-task condition, but not word-reading non-switch trials. This contrasts with the data from the dual-second group in Experiment 2, where word-reading non-switch trials were slowed in the dual-task condition, but not colour-naming non-switch trials. In summary, although the simulations show that the two dual-task performance strategies outlined above can provide a qualitative fit to the RT switch cost data, the quantitative fit was relatively poor. Thus, further theoretical and empirical work is required, to establish whether these shortcomings would persist in a more sophisticated model and to clarify the empirical data.

7.8 General discussion

The results from Experiments 4-6 at first seemed strikingly divergent from those obtained in Experiment 2, indicating reduced switch costs in the dual-task condition rather than any effect on the switch cost asymmetry. Since the initial purpose of these experiments was to investigate whether similar control

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19 Preliminary testing of the model with longer preparation intervals or a stronger top-down input for the secondary task led to a greater reduction in switch costs in the dual-task condition. However, this reduction was greater for the pro task then the anti task. Thus, it is not clear that the model can easily simulate the equal effects of dual-task performance on pro and anti switch costs, as suggested by Experiments 4-6.
processes might be involved in a wide variety of tasks, leading to similar interactions between dual-task and task-switching performance, it may seem that entirely different control processes are implicated in the results from Experiments 1-2 and those from Experiments 4-6.

However, the simulations reported above suggested that the switching strategy that was assumed to be occurring in dual-first subjects in Experiment 2, as well as the subjects in Experiment 1, was able to provide an account of the data. Thus, a coherent account of each of the dual-task experiments (Experiments 1-2, and 4-6) is possible, if it is assumed that subjects either adopt a switching strategy, leading to reduced switch costs, or a strategy of concurrent primary and secondary task performance, affecting the switch cost asymmetry. This discussion will first consider the plausibility of this account of dual-task performance in the experiments reported above. Following this, the implications for the evaluation of the model, as well as implications for theoretical accounts of task switching will be discussed.

In arguing that subjects can adopt two alternative dual-task strategies, it is important to provide an account of the factors that may lead to the choice of one strategy or the other. In Chapter 6 it was argued that one possible reason for the choice of a switching strategy is the likelihood of excessive interference if two tasks were performed concurrently. Thus, in Experiment 2 it may not have been possible for subjects to combine the Stroop and 1-back tasks early in practice without great interference between them. This may explain why the data in the dual-first group were consistent with a switching strategy. The dual-second subjects may have been able to combine the tasks with less interference, hence the reversed asymmetry of switch costs, which was consistent with concurrent performance of primary and secondary tasks. However, both the dual-first and dual-second subjects in Experiments 4-6 produced data consistent with a
switching strategy. Thus, the question arises: why might a strategy of concurrent primary and secondary task performance be adopted by dual-second subjects in Experiment 2 but not Experiments 4-6?

One potential answer is that the primary tasks employed in Experiments 4-6, in comparison with the Stroop tasks used in Experiments 1 and 2, might have been interfered with to a greater extent by the secondary tasks if primary and secondary tasks had been performed concurrently. Consequently, both dual-first and dual-second subjects in Experiments 4-6 may have adopted a switching strategy in the dual-task condition, to prevent excessive interference. However, this explanation of the data in Experiments 4-6 is somewhat unsatisfying without an account of why Stroop tasks should suffer less interference from the secondary task than the pro and anti tasks. Without such an account this explanation is circular: 'Why did the dual-second subjects in Experiments 4-6 adopt a switching strategy? Because failure to do so would have caused excessive interference. How can we know that excessive interference would have been produced by concurrent performance of primary and secondary tasks? Because subjects opted for a switching strategy.'

An alternative account might claim that subjects in Experiments 4-6 adopted a switching strategy because it was always more efficient than concurrent performance of primary and secondary tasks. Presumably one danger of a switching strategy when the secondary task is an auditory working memory task is that information relevant to the secondary task might decay while processing for the primary tasks is taking place. In this case, a switching strategy would be less efficient if reaction times are comparatively slow, as in Experiment 2, where reaction times were much longer than those in Experiments 4-6. This may have led in Experiment 2 to subjects adopting a strategy of performing the primary and secondary tasks concurrently when it was possible, i.e. if the primary tasks
had been practised on their own before the secondary task was introduced. In Experiments 4-6, by contrast, it may have been possible to produce fast reactions to the stimuli for the primary tasks with little decay of auditory information, in which case it would not be necessary to perform the primary and secondary tasks concurrently. This would be consistent with the arguments of Towse et al. (2002), who suggested that information in working memory deteriorates over intervals spent ‘switched out’ of storage functions.

What are the implications of the results described in this chapter for the evaluation of the model presented in Chapters 3 and 4? The model gains relatively little support from its ability to simulate reduced switch costs in dual-task conditions (assuming a switching strategy) since the simulation of this pattern of data was post-hoc; it was not predicted before the experiments were run. In addition, it should be noted that the model would also have been able to simulate a general increase in switch costs (e.g. by reducing both word-reading and colour-naming top-down inputs, or adjusting the squashing parameter). Thus, it seems to have sufficient flexibility to simulate many possible patterns of data. Consequently, its ability to do so lends it little support when one of those patterns of data is obtained empirically (Roberts & Pashler, 2000).

Of course, theories gain support not only when compatible experimental evidence is obtained, but also when evidence is obtained that is incompatible with alternative theories. Thus, the ability of the model to simulate the reduced switch costs in dual-task conditions would offer stronger support if it could be shown that alternative models are unable to simulate this pattern of data. The finding of reduced switch costs in dual-task conditions does indeed appear to challenge extra process models of task switching. If the switch cost measures the duration of some executive process that reconfigures the cognitive system for the forthcoming task, why should it be possible to execute this process more quickly
when subjects have an additional task to perform at the same time? This question is especially pressing with respect to Experiment 4, where there was no significant main effect of Dualtask, and RTs on switch trials were faster in the dual-task than the single-task condition (when data were collapsed over dual-first and dual-second groups). Thus, it is not possible to explain the reduced switch costs in the dual-task condition in terms of some period of ‘slack’, where it is possible to perform control processes related to task switching whilst awaiting the completion of some other process related to dual-task performance (see e.g. Pashler & Johnston, 1989, for a discussion of the ‘locus of slack’ logic).

There seem to be two ways in which extra process accounts might explain the reduction in switch costs in the dual-task condition. First, since in Experiments 4-6 the single-task and dual-task conditions were performed in different halves of the experiment, it is possible to explain any differences in switch costs in terms of global variables such as arousal or motivation. For example, it is possible that subjects perceived the dual-task condition as more difficult, leading to greater arousal and consequently smaller switch costs. In order to rule out such an explanation, it would be necessary to design an experiment where single- and dual-task conditions could be compared within a single block (cf. Rogers & Monsell, 1995, on the advantages of the alternating runs paradigm over the comparison of pure and alternating blocks). Alternatively, it might be possible to explain the reduction in switch costs in the dual-task condition by proposing that the stimuli for the secondary task trigger an ‘exogenous control process’ that helps subjects to prepare for the forthcoming primary task. However, this account is at odds with the original formulation of the exogenous control process account. Rogers and Monsell (1995) suggest that ‘the exogenous trigger of a stimulus attributed associated with a task is needed to complete the process of reconfiguring to perform that task’ (p.226). In the present experiments the stimuli for the secondary task were not associated with either of the primary tasks, so it
is not clear why secondary-task stimuli should help subjects reconfigure for a switch into one of the primary tasks.
Chapter 8

General Discussion

8.1 Introduction

This chapter seeks to evaluate the theoretical and empirical work described in earlier chapters, and discuss the implications for the study of executive function. It will begin with a summary of the overall structure of the arguments running through the previous chapters. This will be followed by a more detailed evaluation of the computational model presented in Chapters 3 and 4, and its relationship with the empirical work presented in Chapters 5-7. Next, the implications for the study of executive function will be considered. Possible directions for further research will then be discussed.

8.2 Overview

In Chapter 1, it was argued that a difficulty in the empirical study of executive function is the scarcity of paradigms suitable for the investigation of control processes in normal subjects. Thus, although evidence for the failure of such processes has come from studies of various clinical populations, little progress has been made in specifying the computational processes that may be involved in executive function. Chapter 2 described some recent evidence from the task switching paradigm suggesting that this methodology may potentially be of relevance to the study of executive function in normal subjects. However, the relationship between task switching and executive function is unclear, largely on account of the disagreement between supporters of extra process theories, who attribute the switch cost to the duration of stage-like executive processes involved in task switching, and task carryover theories, who attribute the switch cost to an interference effect resulting from earlier performance of a different
task. These two types of theoretical account have different implications for the study of executive function with the task switching paradigm: according to extra process accounts, control processes involved in task switching can be measured relatively directly through the duration of the switch cost; carryover accounts, by contrast, imply that there can only be an indirect relationship between the operation of control processes and switch costs.

The model presented in Chapters 3 and 4 provided support for the task carryover account: a large body of empirical data was simulated with an implemented version of this account of the switch cost. By implementing this theory explicitly, it was possible to provide a more detailed account of the relationship between control processes (in the model, the top-down control inputs) and measurable behavioural phenomena (e.g. the switch cost asymmetry). One implication of this account is that direct interference between control processes might be revealed through a modulation of the switch cost asymmetry when subjects switch between tasks of different strengths, so that a paradoxical switch cost asymmetry may become less paradoxical, or even reversed, when subjects perform an additional concurrent task. Chapters 5-7 set out to test this prediction in a number of dual-task studies. The results were mixed. Evidence for a modulation of the switch cost asymmetry was obtained in only one experiment, and was confined to only one group of subjects (the 'dual-second' group in Experiment 2). In other experiments, dual-task performance led to reduced switch costs rather than any effect on the switch cost asymmetry. It was argued that one way of explaining this pattern of data is to assume the availability of two distinct strategies for dual-task performance: a) genuinely concurrent performance of the two tasks, which may lead to a modulation of the switch cost asymmetry, and b) a switching strategy, in which subjects rapidly alternate between the primary and secondary tasks, leading to reduced switch costs. This account assumes that strategy choice may be affected by the nature of
the primary tasks themselves, and also by the amount of practice that subjects have had with the primary tasks before being introduced to the additional secondary task.

8.3 Evaluation of the computational model

The breadth of the empirical data that was simulated by the model was impressive. To recap, the model simulates the facilitation and interference effects found in Stroop colour naming and word reading, with asymmetric interference between the two tasks and greater interference than facilitation, the existence of switch costs, the asymmetry of those switch costs between dominant and non-dominant tasks, the confinement of those switch costs to the first trial in a run, reverse Stroop interference and its confinement to switch trials, the effects of neutral stimuli on switch costs when they appear in the task being switched from and when they appear in the task being switched into, item-specific priming effects, and the interaction of item repetition with switch versus nonswitch trials in the word task. Each of these phenomena was robust to manipulation of the model’s parameters. When run many times with random parameter settings, the model also simulates the relatively high correlation between switch-costs across tasks, but relatively low correlations between Stroop-costs, and between switch-and Stroop-costs (Ward et al., 2001). At least two of these effects have been claimed to be problematic for task carryover accounts to explain, viz., switch costs confined to the first trial in a run (see Rogers & Monsell, 1995, Experiment 6) and nonparadoxical asymmetric switch costs (see Monsell et al., 2000).

Turning to the empirical work described in Chapters 5-7, the model predicted that the switch cost asymmetry should be modulated in conditions of concurrent secondary task performance. This pattern of data was only obtained in one experiment and in that experiment in only one group of subjects. However, the
effect was convincing in this group: the predicted pattern of data was shown by all 12 subjects, and the crucial Dualtask x Task x Switch interaction in this group was significant with a $p$ value below .001. In addition, it had already been predicted on the basis of Experiment 1 that a modulation of the switch cost asymmetry should be shown by the dual-second rather than the dual-first group.

The other pattern of data that was obtained – reduced switch costs in the dual-task condition – was also simulated, but in this case in a post-hoc manner. The model is therefore not challenged by this effect, but neither is it supported since it has the flexibility to simulate a variety of different patterns of data. To explain various patterns of data in terms of multiple ‘strategies’ that may be employed by subjects can be dangerous, in that unprincipled, post-hoc explanations can be produced more easily (cf. Newell’s [1973] remarks concerning the Atkinson & Shiffrin [1968] model of memory: ‘the control structure is completely absent and is used as a deus ex machina to concoct separate models for each task’ [p.297-8]).

In balance, it seems fair to conclude that the model is not challenged by the empirical data reported in Chapters 5-7, but is only weakly supported. However, it should also be pointed out the results obtained fit more comfortably with the theoretical position embodied by the model (i.e. the task carryover account) than with extra process accounts. It seems more plausible to explain the finding of reduced switch costs in dual-task conditions in terms of reduced interference between tasks, rather than arguing that stage-like processes involved in task set reconfiguration can be executed more rapidly when accompanied by an additional task.

The finding of a reversed switch cost asymmetry in dual-task conditions also challenges extra process accounts. Such accounts are already challenged by the paradoxical switch cost asymmetry between Stroop tasks. However, Monsell et
al. (2000) have suggested that an extra control process may be required ‘precisely in order to overcome ... interference, once it arises as the result of the stimulus retrieving a recently activated task-set or the inhibition associated with a recently suppressed task-set’ (p. 254). This may explain the paradoxical switch cost asymmetry if one assumes that word-reading is suppressed during colour-naming trials, but not vice versa. This might lead to the requirement of an extra process confined to word-reading switch trials, in order to alleviate this suppression. But in this case it is mysterious that an extra processes required on word-reading switch trials might be unaffected or even completed more quickly in dual-task conditions, when those required for colour-naming switch trials are completed more slowly. An explanation in terms of different strengths of top-down inputs, and a greater vulnerability of stronger inputs to interference from additional tasks, seems better able to account for these data.

In defence of extra process accounts, it might be argued that an equivalent explanation of paradoxical asymmetric switch costs is possible: the extra process(es) involved in a switch into colour-naming may be invested with greater effort or urgency than those required for a switch into word-reading. However, this argument would be unprincipled. It is possible to justify the difference in top-down inputs for the two tasks in the model presented in Chapters 3 and 4, since these inputs are present on both switch and non-switch trials, and a smaller input is required for word-reading than colour-naming in order to avoid errors. Extra process accounts assume that the stage-like control processes involved in task switching take place only on switch trials. There seems no reason to assume that these processes should be more critical, and therefore performed more effectively, in order to switch into colour-naming rather than word-reading. Thus, the combination of the modelling and empirical work described in previous chapters offers a fairly robust defence of the task
carryover account. Nevertheless, the model clearly has some important inadequacies. These will be discussed separately in the following sections.

8.3.1 Preparation

As discussed in Chapter 2, the effects of preparation (i.e. foreknowledge of the required task) on task switching are currently poorly understood. Conflicting evidence has been produced, suggesting no effect of preparation on switch costs (e.g. Rogers & Monsell, 1995, Experiment 2), a partial reduction in switch costs (e.g. Rogers & Monsell, 1995, Experiment 3), a complete elimination of switch costs (e.g. Tornay & Milán, 2001), or an advantage unrelated to the switch cost (e.g. Dreisbach et al., 2002). Studies of the effects of preparation on switch costs have most commonly obtained the second pattern of data, a partial reduction in switch costs with preparation. This has been taken as evidence for models of task switching that assume two component processes, an endogenous control process, that may take place during the preparation interval, and an exogenous control process that must await stimulus presentation (e.g. Rogers & Monsell, 1995). Alternative accounts have suggested that the imperfect reduction in switch costs obtained in most studies results from the probabilistic application of a single control process which is sufficient to eliminate the switch cost (e.g. De Jong, 2000).

The model's simulation of preparation is dependent on what is assumed about the inputs into the task demand units during the preparation interval. Strong inputs into the task demand units during the preparation interval will eliminate the switch cost after a relatively short preparation interval (in the simulation reported in Chapter 3, the equivalent of approximately 1200 ms was required). Alternatively, the model is able to simulate the residual switch cost if it is assumed that the task demand units are not activated, or are only weakly
activated, during the preparation interval. Thus, the model is able to simulate
different patterns of data in a post-hoc manner, but it does not explain why the
task demand units should be activated during the preparation interval in some
circumstances but not others. However, further studies investigating the
efficiency of preparation in different circumstances (e.g. Arbuthnott &
Woodward, 2002; Dreisbach et al., 2002; Goschke, 2000; Meiran, 2000b; Rogers &
Monsell, 1995; Tornay & Milán, 2001) may be helpful in this respect. For
example, Goschke (2000) found that speaking the name of the forthcoming task,
in comparison with unrelated words, led to a reduction in switch costs with
preparation. Thus, the availability of a linguistic representation of the
forthcoming task may be one factor that determines the activation of task
controlling representations during the preparation interval.

8.3.2 Long-term item specific effects

A number of phenomena related to the repetition of stimuli between tasks were
simulated by the model. ‘Competitor primed’ stimuli yielded slower reaction
times on word-reading switch, but not nonswitch trials; this effect was smaller in
the colour-naming task, but evident on both switch and nonswitch trials. One
consequence of this was that the paradoxical switch cost asymmetry was
enhanced when stimuli were repeated between tasks. This effect of item-
repetition on the switch cost asymmetry was recently demonstrated (using
somewhat different tasks) by Waszak et al. (in press). However, whereas the
empirical data point to a relatively long-lasting effect of item-specific priming
(e.g. a mean item-repetition lag of 96 trials in Waszak et al., in press, Experiment
3), the model, in its standard form, currently simulates item-specific priming
effects lasting for one trial only. Thus, the pairing of a particular stimulus with a
particular task on trial N will only affect the model’s behaviour on trial N+1. This
problem might be rectified, of course, by allowing the priming effects to last for
longer in the model. However, there are two potential problems with allowing this. First, if the weights between stimulus units and task demand units were allowed to persist for many trials, they might grow without bound if the same stimulus were presented on several trials. This could lead to perseverative behaviour, if the top-down inputs were not strong enough to counter this bottom-up input. Thus, a more complex associative learning rule would be required to solve this problem (see e.g. Miller & MacKay, 1994).

A second problem is that longer-lasting priming effects might challenge the model's simulation of switch costs being confined to the first trial in a run (Rogers & Monsell, 1995, Experiment 6). If such priming effects decayed at a rate that would lead to noticeably different behavioural effects from trial to trial, the model might produce switch costs that gradually dissipated over a run of trials, rather than being essentially limited to the first trial. Thus, in order to simulate long-term priming effects, it may be necessary to ensure that weights between stimulus units and task demand units decay at a sufficiently slow rate that changes should not be detectable from trial to trial.

8.3.3 Simulation of errors

Although the model generally produced good fits to RT data, its ability to simulate error rates on different types of trial was less impressive. A basic problem was that when the amount of noise was set so that the model made a realistic number of errors on colour-naming switch trials, it very rarely made errors on any other type of trial, and never made errors on word-reading non-switch trials. Additionally, in simulations of dual-task performance involving concurrent performance of primary and secondary tasks, an excessive number of errors were made on colour-naming switch trials, even after the noise parameter was reduced. In empirical data, the error rates on different types of trial tend to
be more similar. High-level factors may account for this difference. In the model, the same response threshold was used for all types of trial. However, it is possible that response thresholds can be adjusted flexibly for different types of trial (see Gopher et al., 2000; Kleinsorge, 2001). Thus, a conservative response threshold might be used for error-prone (e.g. colour-naming) trials, in order to avoid excessive numbers of errors, but not others (e.g. word-reading), in order to optimise the speed/accuracy trade-off on all types of trial. In addition, the model assumes a zero error rate in the processes that lead to the correct top-down input being activated from trial to trial. This is, of course, implausible and may account for some discrepancies between the empirical and simulated error data.

8.3.4 Simulation of nonswitch trials

The model was more successful at simulating switch costs (i.e. the difference in RT between switch and non-switch trials) in different types of conditions than the absolute non-switch reaction times. For example, in each of the dual-task experiments apart from Experiment 4 (which used a different secondary task from the others), RT was considerably slower on non-switch trials in the dual-task than the single-task condition. The model's simulated dual-task performance, while providing a good fit to the switch-cost data, only produced a small slowing on dual-task non-switch trials. Another problematic finding for the model is that, in empirical data, RTs on non-switch trials in mixed blocks (i.e. blocks involving more than one task) tend to be slower than RTs in pure blocks (see also Umilta et al., 1992 for a similar result). This slowing has been referred to as the 'mixing cost' (Meiran et al., 2000). In the model, reaction times are essentially identical on the two types of trial. Again, it is possible, although somewhat unsatisfactory, to explain these effects in terms of higher-level factors. In mixed blocks, subjects may engage in each task less fully (i.e. provide weaker top-down inputs for each task) to avoid perseveration on switch trials. This may
be accompanied by more conservative response thresholds, so as to avoid errors in mixed blocks (where there is greater interference between tasks). A similar explanation of dual-task performance is possible: if the primary and secondary tasks are performed concurrently, and there is interference between them, a more conservative response threshold may be adopted. In addition, if subjects adopt a switching strategy, processing related to the primary tasks may be postponed until the completion of processing related to the secondary task. Furthermore, in such demanding circumstances, additional executive processes may be engaged to monitor behavioural output, leading to slower responses.

8.4 Implications for the study of executive function

Chapter 1 discussed two important issues in executive function research: a) to what extent are control processes separable from lower-level processes, and b) how far is it possible to fractionate control processes into more elementary processes? These will be discussed in turn.

8.4.1 Separability of control processes from lower-level processes

As discussed in Chapter 1, control processes might be dissociable from lower-level processes in the sense that they play different computational roles, or, more strongly, in the sense that higher-level supervisory processes are involved in some types of behaviour but not others. In the model presented in Chapters 3 and 4, control processes and lower-level processes are separable in the former but not the latter sense. The task demand units and their top-down inputs play a different computational role to the other parts of the model: they are involved in biasing processing towards one of the two pathways and may operate before any stimulus has been presented. In the sense that they can modulate the pathways that process all of the possible inputs and outputs in the model, they represent
higher-level processes. Without these elements of the model, it would still be able to produce word-reading responses, but would not be able to switch between tasks or, more generally, produce colour-naming responses at all. Thus, higher and lower-level processes in the model can be distinguished, in that they are represented by different information processing units, and lower-level processes could exist in the absence of higher-level processes.

However, in the model as it is currently implemented, the higher-level processes are involved in all types of behaviour, i.e. both colour-naming and word-reading tasks, and both switch and non-switch trials. In this way, the model fails to make a clear distinction between circumstances that require the operation of higher-level processes and those that do not. But the strengths of the top-down inputs differ between the tasks, and this is crucial for the model's simulation of a number of phenomena. In addition, the model would be able to respond appropriately on word-reading non-switch trials without the operation of higher-level processes. Thus, the model is compatible with a continuum between highly stereotyped behaviours in which higher-level processes play no role, and more novel behaviours in which higher-level processes may be involved to a greater or lesser extent. Most everyday tasks, including those that would be described as 'routine', such as making a cup of tea, seem to involve ambiguous environmental cues and would therefore require some degree of higher-level control (e.g. the sight of a kettle might evoke a response to fill it with cold water at the beginning of the routine, but to pour boiling water at the end). Accordingly, evidence from action disorganisation syndrome (e.g. Schwartz et al., 1991) suggests that behaviour in such routine tasks can become disordered, even when the component actions can be performed individually. Thus, it seems that higher-level control is involved in the vast majority of behavioural output, including 'routine' behaviour. However, it is still an open question whether the type of higher-level control involved in routine behaviour is qualitatively
different from the type of higher-level control involved in non-routine behaviour (see Cooper, 2002; Cooper & Shallice, 2000 for discussion).

In common with many models of executive function (e.g. Norman & Shallice, 1986), the role of higher-level processes in the model presented in Chapters 3 and 4 is to modulate processing in lower-level stimulus-response pathways, rather than to actually generate behavioural responses. However, these higher-level processes need not be seen as part of a self-contained system. The model explicitly recognises the interrelation of higher-level and lower-level processes, since units representing stimulus features can develop connections with the higher-level task demand units. Thus, the higher-level processes that modulate lower-level stimulus-response pathways can themselves be directly modulated by lower-level processes. There is no reason to suppose that this lower-level modulation of control processes is confined to the activation of task demand units by the presentation of stimuli associated with one of the tasks. For example, in circumstances where the appropriate task is cued by stimulus position, as in the experiments presented in Chapters 5-7, one component of the 'top-down control input' might consist of connections between a representation of the position of the stimulus and the task demand units. Additionally, the role of linguistic self-cueing (e.g. in the form of internalised speech) has been emphasised in several recent studies (Baddeley et al., 2001; Emerson & Miyake, in press; Goschke, 2000; Mecklinger et al., 1999). This could be incorporated into the model by simulating linguistic representations of the required task, and their connections with the task demand units. Thus, although the task demand units in the model represent higher-level control processes, this is on account of their relationship with lower-level stimulus-response pathways, rather than their complete independence from environmental events.
This view of higher-level processes as being distinguished from lower-level ones in terms of their computational role, rather than as being part of a self-contained system, is consistent with a number of recent studies that have emphasised the role of environmental events in the control of complex behaviour (e.g. Ballard, Hayhoe, Pook, & Rao, 1997; Brooks, 1991; Clark, 1997; Hutchins, 1995; Kirsh, 1995; Kirsh & Maglio 1994). For example, Kirsh (1995) considers the use of space in order to produce goal-directed behaviours such as preparing a salad. Typically, ingredients that require the performance of different tasks (e.g. washing, chopping) are placed in different locations, and separated from those ingredients that have already had the relevant task performed. Thus, by arranging the objects in this way, the task that needs to be performed can be cued in a relatively straightforward manner by spatial location of each object. In a study by Land, Mennie, & Rusted (1999), subjects’ eye movements were recorded as they performed the sequence of actions required to make a cup of tea. It was found that subjects typically fixated the object required for the next step in the task approximately 500 ms before initiating each movement, often before completing the present step in the task. Land et al. (1999) propose that this time is used to derive information about the location of each object so that spatially accurate movements may be programmed. However, a more basic purpose of these intervals during which the subject fixates the next object but does not act on it may be to evoke the appropriate behavioural routine. In this way, complex sequential behaviour may depend strongly on environmental cueing (see Cooper & Shallice, 2000), and higher-level control in such circumstances may to a large extent be stimulus-driven.

According to this view, higher-level control results from activity in a system which is specialised for the maintenance of sustained representations which bias processing in lower-level systems. Anatomically, the prefrontal cortex (PFC) may be such a system (e.g. Fuster, 1997; Goldman-Rakic, 1987; O’Reilly, Braver, &
Cohen, 1999). Representations in this system may often be triggered by environmental events, rather than internal, cognitive events (e.g. the results of deliberate planning activity). In this way, it may be possible to 'delegate' control of one’s mental processes to basic features of one’s environment, as in Kirsh’s (1995) example of the use of spatial location to cue the appropriate task when preparing a salad. However, it is of course necessary that higher-level representations are not always triggered by environmental events: this would lead to utilisation behaviour. Thus, some means is required to distinguish between environmental events that ought to trigger sustained higher-level representations that bias lower-level processes, and those that should not.

As discussed in Chapter 4, a number of authors have recently suggested that the dopamine (DA) neurotransmitter system may play a role in regulating the sensitivity of PFC representations to external inputs, so that it is possible to switch between rapid updating of representations at some times and robust maintenance of these representations against distraction at others (e.g. Braver et al., 1999). Other accounts have emphasised the role of phasic DA activity as a reinforcement learning signal (e.g. Montague, Dayan, & Sejnowski, 1996). Recently, Braver and Cohen (2000) have suggested a synthesis of these two theoretical approaches: phasic DA activity may act as a 'gating' signal, allowing the updating of PFC representations, and also allow the information being gated to be associated with the gating signal in the future. This allows the system to learn the appropriate timing of the gating signal in complex tasks. Frank et al. (2001) have put forward a more sophisticated, hierarchically organised implementation of this hypothesis, which allows higher-level goals to be robustly maintained at the same time as gating information into lower-level representations. According to these hypotheses, learning plays an important role in higher-level control, in that it allows the system to distinguish between environmental events that should be allowed to trigger the updating of higher-
level representations (that themselves modulate lower-level ones), versus environmental events that should not affect higher-level representations. A combination of an appropriately structured environment, where different environmental cues are able to trigger a repertoire of learned behavioural responses, and learned associations between environmental cues and the need to update higher-level representations, may be sufficient to ensure coherent, goal-directed behaviour in a wide variety of circumstances.

The types of higher-level control described above seem more relevant to routine than non-routine behaviour, especially since the role of learning has been emphasised. Thus, in terms of the Norman-Shallice model, these processes may be more closely related to contention scheduling rather than the supervisory system. Of course, a central assumption of this theory is a qualitative difference between processing in the contention scheduling and supervisory systems. Routine behaviour may involve the type of control discussed above, with a heavy dependence on environmental triggering. Non-routine behaviour may rely on a completely different system. However, as Rabbitt (1997b) points out, 'at least for adults, no task can be entirely novel, and so unprepared by any previous experience whatever and it is difficult to find examples of tasks that have been so highly practised that we may confidently assume that no further improvement may occur' (p.25). Thus, it may be more appropriate to conceive of higher-level control processes as involving a single hierarchically organised system (e.g. Grafman, 1989, 1995, 2002; Schwartz et al., 1991). Although learned associations between environmental cues and behavioural routines may play less of a role as one ascends this hierarchy, it is still possible that abstract knowledge of particular types of situation plays an important role even at the highest level (e.g. Grafman, 1989, 1995, 2002). Unfortunately, standard task switching paradigms do not seem likely to offer any leverage on the debate between these theoretical positions. Since task switching experiments typically involve repetitive, well-
learned tasks (at least over the course of the experiment), and the task is typically cued in a relatively straightforward manner (e.g. by visual pre-cues, or by stimulus position), the control processes involved in this type of experiment would seem to be relatively low-level (i.e. corresponding to the contention scheduling system in the Norman-Shallice theory). It is quite possible that control processes involved in less environmentally constrained task switching paradigms (e.g. Burgess et al., 2000) are qualitatively different. However, there is no reason to assume this. In conclusion, then, it seems that higher- and lower-level processes involved in the task switching paradigm are separable only in the sense that they play different information processing roles, and have different temporal characteristics, rather than in the sense that higher- and lower-level processes are involved in different types of situation. It is possible that there are additional types of higher-level process, not implicated in the task switching paradigm, that are separable from lower-level processes in this stronger sense as well.

8.4.2 Fractionation of executive function

One of the purposes of the dual-task experiments reported in Chapters 5-7 was to investigate whether there was any overlap between the control processes involved in task switching and those involved in the 1-back tasks and, more generally, in the co-ordination of dual-task performance. On the basis of the simulations reported in Chapter 5, it was assumed that any interference between control processes involved in the various tasks would reveal itself by a modulation of the switch cost asymmetry in the Stroop or pro/anti tasks. This was not the pattern of data that was generally obtained. Nevertheless, since a modulation of the switch cost asymmetry by dual-task performance was obtained in one group of subjects in one experiment, it was argued that it is possible for there to be direct interference between control processes involved in
task switching and those involved in secondary task performance. In the experiments where a different pattern of data was obtained, it was argued that this was not because the primary and secondary tasks required entirely separate control mechanisms. Rather, the most plausible explanation of the data seems to be that in cases where there was no modulation of the switch cost asymmetry by dual-task performance, this was because subjects adopted a strategy of switching between the primary and secondary tasks, so that the control processes required by each were not engaged at the same time. One of the reasons for this is that it was the dual-second rather than the dual-first group in Experiment 2 that showed a modulation of the switch cost asymmetry by the dual-task condition. Thus, the group that had practised the tasks less (i.e. dual-first), and which therefore might be expected to employ less specialised mechanisms to perform each task and suffer greater interference as a result, showed no evidence of any interference between control processes. Given that the dual-second group did show strong evidence for such interference, it seems likely that the dual-first group was able to adopt a strategy to minimise interference between control processes, rather than engaging separate control processes for the various tasks. If the dual-first group was able to deal with each task using separate control processes, it seems mysterious that the dual-second group was unable to do so.

The results from the dual-task experiments reported in previous chapters therefore suggest that a basic control process of maintaining representations of intended tasks, in order to bias lower-level processes related to those tasks, is limited in the number or strength of the representations that it can maintain, even if these representations are involved in separate tasks. However, it appears that additional control processes are able to schedule the performance of various tasks in order to prevent interference between their associated control processes. Thus, as suggested by Miyake et al. (2000), the ability to co-ordinate dual-task performance may reflect the operation of rather different control processes from
the ones involved in the performance of the tasks themselves, even if these tasks are heavily reliant on top-down control. The suggestion of an important role of scheduling processes in multiple task performance has also been made by Meyer and Kieras (1997b), in their simulation with the EPIC model of phenomena associated with the PRP paradigm. However, in their model, the scheduling processes are required in order to ensure appropriate response ordering, rather than to protect some limited capacity system.

In summary, it seems that a general control process of maintaining goal representations in order to provide top-down biasing of other processes may be relatively limited in capacity and shared between tasks. This process may be akin to the 'goal weighting' mechanism that Duncan (1995) has proposed as central to frontal lobe functions. However, other control processes (e.g. those involved in task scheduling) may be entirely separable. In addition, according to the explanation of the dual-task experiments sketched above, it seems necessary that there be some mechanism of strategy choice (e.g. to decide between concurrent performance or rapid switching strategies in dual-task performance). This may rely on a monitoring process that is entirely separable from other control processes.

8.5 Directions for further research

A theme that has emerged from recent research into task switching is the extent to which control processes, which have often been seen in the past as acting in opposition to environmental events (e.g. in inhibiting prepotent responses), may in fact be stimulus-driven (e.g. Allport & Hsieh, 2001; Allport & Wyie, 2000; Rogers & Monsell, 1995; Waszak et al., in press). One way in which this may be so is through direct associations between specific stimuli and specific tasks (e.g. Waszak et al., in press). This type of lower-level influence on higher-level
control was implemented in the model presented in Chapters 3 and 4 by allowing associative learning between stimulus input units and task demand units. However, many other types of direct cueing of higher level processes are possible. For example, in most studies of task switching the appropriate task is triggered either by the location of the stimulus (e.g. Rogers & Monsell, 1995) or an explicit cue before the stimulus is presented (e.g. Meiran, 1996). It may be informative to explicitly implement these various forms of task-cueing, especially since they have sometimes yielded different patterns of behavioural data (e.g. Tornay & Milán, 2001). It may also be interesting to implement an influence of linguistic representations on the task demand units, since the role of internalised speech has been emphasised in several recent studies of task switching (Baddeley et al., 2001; Emerson & Miyake, in press; Goschke, 2000; Mecklinger et al., 1999). In this way, the part of the model that is currently most underspecified, the 'top-down control input' could be implemented more fully.

The model's simulation of various phenomena, especially asymmetric switch costs, was dependent on a difference between the strength of the top-down inputs for the two tasks. This seems to conflict with the emphasis above on the role of environmental features in triggering activity in the task demand units. Of course, if trials for colour-naming and word-reading are presented in different spatial locations, neither location is 'stronger' than the other. Thus, there is a conflict between a) the argument that the 'top-down control input' largely reflects direct links between environmental features and the task demand units and b) the assumption that the top-down input is stronger for the colour task than the word task. One way in which this conflict could be resolved would be if the strength of the input into the task demand units were boosted or reduced as a result of a monitoring process that was sensitive to the extent of the requirement for top-down control. Botvinick et al. (2001) have recently argued that such a monitoring process takes place in anterior cingulate cortex, and
implemented this hypothesis in several models conceptually similar to the present one. This monitoring process might determine the magnitude of top-down inputs, while environmental events could determine which task demand unit(s) receive activation.

Another possible addition to the model, discussed above and also in Chapter 4, is a gating system to allow the model to switch between rapid updating of the task demand units and maintenance of the currently represented information (e.g. Braver & Cohen, 2000). This might allow the model to maintain a representation of the currently appropriate task in the task demand units without a constant top-down input. For example, in task switching paradigms where each trial is preceded by a cue informing the subject which task to perform, the model could be trained so that the information provided in cue events was gated into the task demand units, but maintained until the next cue event. Such a gating system might also allow the model to simulate the residual switch cost, if it were not possible to fully update the task demand units before the presentation of a task relevant stimulus.

Turning now to the empirical data presented in Chapters 5-7, if it is correct to explain the data from the dual-task experiments in terms of the availability of alternative strategies (concurrent task performance versus rapid switching between primary and secondary tasks), what factors affect strategy choice? Two important factors appear to be the amount of practice that subjects have had with the primary tasks (to account for the difference between the dual-first and dual-second subjects in Experiment 2) and the nature of the primary tasks themselves (to account for the difference between Experiment 2 and Experiment 6). Further

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20 This type of system would give the model certain similarities with the version of the extra process account put forward by Rogers and Monsell (1995). However, since the processes in the model would be computationally the same on nonswitch and switch trials, the only difference being the initial state of the network, it would still seem to be more appropriately classified as a version of the task carryover account.
experiments with alternative tasks will be required to investigate the effects of these factors. For example, it was argued in Chapter 7 that a possible reason for subjects adopting a switching strategy with the pro and anti tasks, but not with the Stroop tasks, was that responses could be produced more quickly in the former tasks, making it less likely that information in auditory working memory would decay during primary task performance. In this case, changing the pro and anti tasks so that response times increase, for example by increasing perceptual difficulty, might lead to a modulation of the switch cost asymmetry by the dual-task condition.

8.6 Concluding comments

Computational modelling approaches have only recently started to be applied to the domain of executive function (e.g. Braver & Cohen, 2000; Changeux & Dehaene, 2000; Cooper & Shallice, 2000; Kimberg & Farah, 1993; Logan & Gordon, 2001; Meyer & Kieras, 1997; O’Reilly, Braver, & Cohen, 1999). In the past, modelling (especially using the PDP framework) has often been thought to be better suited to the study of pattern recognition, and mnemonic processes, rather than higher-level control processes. The lack of explicitly implemented theories of executive function has led to difficulties of evaluation. First, since most theories of executive function are currently underspecified, it has been difficult to generate detailed empirical predictions. Second, since control processes are typically assumed to operate by modulating other processes, the operation of such processes are often thought to be observable indirectly through behaviour. Thus, it has been difficult to specify the precise relationship between control processes and observable behaviour. The combined modelling and empirical approach to task switching described in previous chapters has produced some promising initial results. When alternative theoretical positions (e.g. extra process accounts) are articulated in a similar fashion, it will be easier to
decide between these accounts empirically. Two steps will be required to extend the present results. First, the fit between theory and data, which is currently rather loose (especially on account of the extra degrees of freedom afforded by the assumption of multiple alternative strategies available to subjects) could be improved with further, systematic empirical studies of the phenomena simulated by the model. Such studies, combined with explicit theorising, may lead to more specific theoretical accounts. Second, the 'top-down control input', currently little more than a homunculus, requires fractionation into more tractable subprocesses, such as direct environmental inputs and inner speech, learning mechanisms to establish such inputs, and additional executive subprocesses such as monitoring and task scheduling.


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## Parameter Settings

**Table A.1** Parameters of the model, standard settings, and permitted range when set to random values

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Standard setting</th>
<th>Possible range for random values</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Weights:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stimulus input units to word output units</td>
<td>3.5</td>
<td>1.9 – 4.2</td>
</tr>
<tr>
<td>Stimulus input units to colour output units</td>
<td>1.9</td>
<td>1.5 – 4.1</td>
</tr>
<tr>
<td>Task demand units to word / colour output units</td>
<td>2.5</td>
<td>1.6 – 5.7</td>
</tr>
<tr>
<td>Word / colour output units to task demand units</td>
<td>1.0</td>
<td>0.0 – 6.7</td>
</tr>
<tr>
<td>Connections between colour and word output units</td>
<td>2.0</td>
<td>0.0 – 2.8</td>
</tr>
<tr>
<td>Within module inhibitory connections</td>
<td>-2.0</td>
<td>-5.0 – 0.0</td>
</tr>
<tr>
<td><strong>Other parameters:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bias for word / colour output units</td>
<td>-6.0</td>
<td>-11.3 – -3.5</td>
</tr>
<tr>
<td>Bias for task demand units</td>
<td>-4.0</td>
<td>-15.2 – 0.0</td>
</tr>
<tr>
<td>Top-down control input into colour task demand unit</td>
<td>15.0</td>
<td>9.0 – 21.0</td>
</tr>
<tr>
<td>Top-down control input into word task demand unit</td>
<td>6.0</td>
<td>3.2 – 21.0</td>
</tr>
<tr>
<td>Learning rate between stimulus input and task demand units</td>
<td>1.0</td>
<td>0.0 – 1.6</td>
</tr>
<tr>
<td>Response threshold</td>
<td>0.15</td>
<td>0.09 – 0.32</td>
</tr>
<tr>
<td>Minimum activation value</td>
<td>-1.0</td>
<td>-1.0</td>
</tr>
<tr>
<td>Maximum activation value</td>
<td>1.0</td>
<td>1.0</td>
</tr>
<tr>
<td>Parameter</td>
<td>Value 1</td>
<td>Value 2</td>
</tr>
<tr>
<td>-------------------------------------------------------------</td>
<td>---------</td>
<td>---------</td>
</tr>
<tr>
<td>Step size (see equation 3.1)</td>
<td>0.0015</td>
<td>0.0015</td>
</tr>
<tr>
<td>'Squashing' of task demand unit activations between trials</td>
<td>80%</td>
<td>63% - 100%</td>
</tr>
<tr>
<td>Noise (i.e. standard deviation of the distribution used to provide noise terms added to activation values)</td>
<td>0.006</td>
<td>0 - 0.006</td>
</tr>
</tbody>
</table>

*a This parameter determines both the positive connection strength from (e.g.) the word task demand unit to each of the word output units and the negative connection strength from (e.g.) the word task demand unit to each of the colour output units. Likewise, the positive and negative connection strengths for the backprojections from the colour and word output units to the task demand units are determined by a single parameter.

*b This parameter determines both the positive connection strengths between congruent words and colours, and the negative connection strengths between incongruent words and colours.

*c This parameter determines the negative connection strengths between each member of the task demand, word and colour modules.
Appendix B

**Experiment 4**

- **Dual-first**
  - Switch cost (ms)
  - Switch cost (% errors)
  - Single Task vs. Dual Task

- **Dual-second**
  - Switch cost (ms)
  - Switch cost (% errors)
  - Single Task vs. Dual Task

**Experiment 5**

- **Dual-first**
  - Switch cost (ms)
  - Switch cost (% errors)
  - Single Task vs. Dual Task

- **Dual-second**
  - Switch cost (ms)
  - Switch cost (% errors)
  - Single Task vs. Dual Task

**Experiment 6**

- **Dual-first**
  - Switch cost (ms)
  - Switch cost (% errors)
  - Single Task vs. Dual Task

- **Dual-second**
  - Switch cost (ms)
  - Switch cost (% errors)
  - Single Task vs. Dual Task

*Figure B.1* Mean RT and error switch costs in Experiments 4-6, plotted separately for dual-first and dual-second groups.