The role of input and output modality pairings in dual-task performance: Evidence for content-dependent central interference

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Abstract

Recent debate regarding dual-task performance has focused on whether costs result from limitations in central capacity, and whether central operations can be performed in parallel. While these questions are controversial, the dominant models of dual-task performance share the assumption that central operations are generic – that is, their interactions are independent of stimuli and responses modalities. To examine these issues, we conducted a series of dual-task experiments with different input and output modality pairings. One condition combined a visual-manual task with an auditory-vocal task, and the other condition reversed the input-output pairings, combining a visual-vocal task with an auditory-manual task. Input/output modality pairings proved to be a key factor; throughout practice, dual-task costs were generally more than twice as large with visual-vocal/auditory-manual tasks than with the opposite arrangement of modalities (Experiments 1 and 2). These differences could be explained neither by competition for peripheral resources nor by differences in single-task response times (Experiment 3). Moreover, the persistent dual-task costs did not appear to stem from a central bottleneck. Contrary to the dominant models of dual-task performance, we propose that central interference between tasks depends not just on the duration of central operations, nor just strategic adaptation, but also on the content of those operations. Implications for structural and strategic accounts of dual-task interference are discussed.

Keywords: Dual-Task, practice, modalities, central bottleneck

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Psychology has long sought an understanding of how humans perform concurrent tasks. Given the massive parallelism of the human brain, one might expect people to be able to perform multiple tasks concurrently with little interference. However, this expectation is not generally borne out in experimental settings, where people routinely show severe performance limitations when attempting to perform two distinct tasks simultaneously. Thousands of experiments have examined these dual-task costs, providing the basis for numerous influential theories of human cognitive architecture.

Sources of Dual-Task Interference

The extensive literature on dual-task performance describes many forms of interference between concurrent tasks (e.g., Allport, Antonis, & Reynolds, 1972; Diedrichsen, Hazeltine, Kennerley, & Ivry, 2001; Greenwald, 1972; Hirst & Kalmar, 1987; Jolicoeur, 1999; Navon & Gopher, 1979a; Pashler, 1990; Pashler & Johnston, 1989; Schumacher, Seymour, Glass, Kieras, & Meyer, 2001; Spijkers & Heuer, 1995; Tombu & Jolicoeur, 2002; Welford, 1952; Wickens, Sandry, & Vidulich, 1983). Thus, dual-task costs likely emerge from manifold sources, a subset of which may be relevant for any particular pairing of tasks. For example, the simultaneous production of motor responses with the two hands leads to bimanual interference (Franz, Zelaznik, & McCabe, 1991; Heuer, 1995; Spijkers, Heuer, Steglich, & Kleinsorge, 1997; Swinnen & Wenderoth, 2004). Bimanual interference is not present when a task requiring a manual response is paired with a task requiring a vocal response. Similarly, conceptual overlap between tasks (e.g., Hommel, 1998; Logan & Schulkind, 2000; Navon & Miller, 1987), may lead to inter-task interactions that would not have occurred with non-overlapping task pairs.

Despite evidence for multiple sources of task-specific interference, contemporary theories of dual-task performance often assume that these conditions represent special cases and that, in the absence of conceptual overlap or response overlap, meaningful generalizations about dual-task operations can be drawn across a range of task pairings. That is, it is widely accepted that the coordination of task operations is governed by a common set of general principles or control processes (e.g., a central bottleneck) assumed to apply regardless of the particular tasks currently being performed. This research strategy is supported empirically by literally hundreds of studies showing seemingly similar patterns of dual-task costs despite impressive variation in the tasks. Nevertheless, there is still considerable debate regarding the best account for these phenomena.

In the present study, we focused on three outstanding questions emerging from current theorizing about dual-task limitations. First, is the concurrent performance of two distinct tasks subject to limitations in central processing? Second, do central limitations (if any) lead to serial processing of the two tasks? Third, does central interference depend only on the duration of competition for central capacity (as often assumed), or does it also depend critically on the specific content of the two tasks? As will be seen, the answers to these questions are somewhat unexpected given the dominant models of dual-task performance, compelling us to reexamine several fundamental assumptions of those models. We first discuss each of these questions and then describe our experimental approach towards addressing them.

Does central capacity limit dual-task performance?

The dominant theories of dual-task performance differ on whether limitations on concurrent task performance reflect structural properties inherent in the cognitive architecture or a strategic adaptation to a particular set of circumstances. Structural accounts generally attribute dual-task interference to contention for limited-capacity central resources. Strategic accounts generally posit no central resource limitations, instead attributing costs to the scheduling of task operations (which may be dictated by contention for peripheral visual or motor resources).

One popular structural account, originally proposed by Welford (1952), is that the cognitive architecture of the human brain includes a single-channel central mechanism (see also, Pashler, 1984, 1994b; Pashler & Johnston, 1989, 1998; Smith, 1967; Welford, 1967). According to this "central bottleneck" theory, dual-task costs occur because central processing for one of the tasks must be delayed while the central mechanism is occupied by another task[1]. Thus, dual-task costs should be observed whenever two tasks are temporally aligned so as to simultaneously require central processing. This prediction and several others relating to the central bottleneck have been borne out in literally hundreds of studies, using a myriad of task combinations, suggesting that a central processing mechanism is required during the performance of a wide range of tasks (for a review see Lien & Proctor, 2002; Pashler & Johnston, 1998).

Central bottleneck theories have recently been challenged on the grounds that dual-task interference might represent a strategic adaptation to the demands of the experiment, not an inherent limitation of a fixed cognitive architecture, at least after sufficient practice. This interpretation is supported by recent studies showing that participants can learn to perform two tasks simultaneously as quickly and accurately as they perform each task individually. For example, Schumacher, Seymour, Glass, Kieras, and Meyer (2001) required participants to make a manual keypress based on the location of a visual stimulus and a vocal response based on the pitch of a tone. Participants performed single-task blocks using only one of the two tasks, and mixed blocks consisting of single-task trials randomly intermixed with dual-task trials in which the visual and auditory stimuli were presented simultaneously. After several sessions, performance on dual-task trials was nearly identical to performance on single-task trials. These researchers concluded that dual-task limitations can be overcome by eliminating conflict for specific peripheral resources, and by providing practice and sufficient incentives for good performance. Hazeltine, Teague, and Ivry (2002) replicated Schumacher et al.'s key findings and demonstrated that the elimination of dual-tasks interference was not due to the integration of the two tasks into a single super-task.

Despite these findings, the debate over the source of dual-task costs remains. Levy and Pashler (2001) pointed out that the visual-manual task used by Schumacher et al. was so easy that it might not have required any central processing. Furthermore, they noted that a lack of interference could be accounted for by a "latent" bottleneck, in which the central bottleneck operations on the two tasks do not overlap in time. The probability of minimal temporal overlap in central operations is increased when the when the durations of the central operations of the two tasks are short (see also, Byrne & Anderson, 2001; Ruthruff, Johnston, Van Selst, Whitsell, & Remington, 2003; Tombu & Jolicoeur, 2004), which is likely to be the case after substantial practice.

<u>Do dual-task costs arise from serial central processing?</u> The central bottleneck model and some strategic accounts of dual-task performance (e.g., EPIC, Meyer & Kieras, 1997b) assume that costs result from serial performance of certain critical operations on the two tasks. However, competition for central resources need not result in a strict processing bottleneck. An alternative theory is that two tasks can simultaneously share the central resource, so that neither task is performed as quickly as it would be performed alone (e.g., Navon & Miller, 2002; Tombu & Jolicoeur, 2002, 2003). In many cases, it might be more efficient to allocate all of the central capacity to one of the tasks until it is completed, in which case this model makes similar

predictions as the single-channel bottleneck model. However, the assumption that central capacity can be shared allows the model to account for some findings that are difficult for the single-channel bottleneck model to accommodate.

The question of whether response selection can proceed in parallel for two tasks has received a flurry of recent attention, although no consensus has yet emerged (e.g., Byrne & Anderson, 2001; Hazeltine et al., 2002; Hommel, 1998; Logan & Schulkind, 2000; Navon & Miller, 2002; Ruthruff, Pashler, & Hazeltine, 2003; Ruthruff, Pashler, & Klaassen, 2001; Schumacher et al., 1998; Schumacher et al., 2001; Tombu & Jolicoeur, 2003; Van Selst, Ruthruff, & Johnston, 1999). This unresolved issue is of critical importance for understanding the cognitive architecture underlying response selection.

<u>Is central interference content-dependent?</u> Thus far, all of the accounts we have described share the fundamental assumption (explicit or implicit) that central interference is due to generic limitations, either structural features of the architecture or strategic considerations, and that these limitations apply regardless of the pairings of stimuli and responses. Additional forms of peripheral interference may arise if both tasks use, for example, visual stimuli or manual responses. However, in the absence of conflict for peripheral resources, selecting a manual response to a visual stimulus should not result in different patterns of dual-task costs than, for instance, selecting a vocal response to a visual stimulus.

Theories that attribute dual-task costs to a structural component of the cognitive architecture (e.g., a single-channel mechanism Byrne & Anderson, 2001; McCann & Johnston, 1992; Pashler, 1994b), including capacity-sharing theories (e.g., Kahneman, 1973; Navon & Miller, 2002; Tombu & Jolicoeur, 2003), hold that central mechanisms use generic (content-independent) resources. Provided that there is no physical or conceptual overlap between the stimuli and/or responses for the two tasks (e.g., the tasks do not involve overlapping stimulus classifications), dual-task costs should be determined by the duration of central operations, not by the specific relationships between the two tasks. Hence, if participants perform an auditory-vocal task concurrently with a visual-manual task, current theories tell us that costs should arise only from competition for generic central resources, because the input and output modalities have each been separated to avoid conflict. There is no reason to suspect that we should observe any different pattern if we ask subjects to perform the same central operations but change the input-output pairings by asking them to perform the same tasks with auditory-manual and visual-vocal mappings. That is, according to content-independent theory, central operations reflect abstract conceptual choices, not influenced by the specific input or output channels. Although bottleneck and limited capacity models can be modified so that operations subject to the central limitations would depend on the composition of the tasks, such changes dramatically alter the fundamental assumption of these models that the central operations are generic.

Theories that attribute dual-task costs to voluntary sequential central processing (e.g., Meyer & Kieras, 1997b; Meyer et al., 1995) also assume that the contents of the central operations – the specific stimulus-response (S-R) mappings – do not directly affect dual-task costs. That is, although the duration of the central operations may have a powerful effect on the duration of the voluntarily imposed bottleneck delay (and hence the magnitude of dual-task costs), the S-R translation processes for the two tasks do not interact. It would be possible to expand these models to allow for direct interactions between central operations (e.g., delays might be imposed differently depending on the contents of the tasks). However, such a modification would be significant since it is not justified by the proposed cognitive architecture (which explicitly asserts that resource competition occurs only between input processors and between output processors). In essence, such changes to the models would make the claim that there are no restrictions on central processing contingent on the particular tasks being performed.

All of these accounts allow for task-specific interactions on peripheral mechanisms, such as when there is overlap between the input modalities or between the output modalities. For example, it is widely postulated that two tasks involving manual responses call upon common mechanisms that control the two hands (Heuer, 1995; Meyer & Kieras, 1997a; Pashler, 1990; Van Selst et al., 1999). Therefore, requiring two manual responses, even if made by different hands, causes competition for the manual processor and results in dual-task costs above and beyond those caused by limited central resources. Critically, the locus of the interference is assumed to be peripheral rather than central.

To summarize, each of the models, listed in the columns of Table 1, offer distinct sets of answers to our questions, listed in rows. Whereas the theories diverge regarding whether performance is constrained by central limitations and whether central operations must performed serially, there is consensus among the accounts that central limitations are not content-dependent. We now consider a class of theories that offer a different answer to this question.

Content-dependent theories. An alternative to the prevailing view, often neglected in recent work, is that central mechanisms are subject to content-dependent interference. By content-dependent interference, we refer to interactions between the specific central codes associated with the concurrently performed task. Unlike the abstract S-R codes postulated for generic central operations, content-dependent central operations reflect aspects of the underlying cognitive architecture. That is, the central operations are determined by the specific linkage between input and output modalities. Thus, the selection of a manual response to a particular stimulus will engage a set of central operations that differ from those engaged by the selection of a vocal response, and the nature of the dual-task interference will differ accordingly. We term this type of account a content-dependent theory, because the specific codes invoked by the S-R translation (the contents of the particular action required by the task on a given trial) determine the pattern of dual-task costs.

Although the content-dependence question and the serial/parallel question are logically distinct, they are not independent. Serial processing of central operations would seem to minimize the opportunity for content-dependent interference between tasks. Parallel central processing (which is supported by the data presented below), in contrast, suggests active processing of central codes on both tasks at the same time, using the same set of buffers/memory stores. Thus, the opportunity for mutual interference is greatly increased. For instance, interference might arise if both tasks rely upon similar central codes (e.g., if both use acoustic codes). As an analogy, it is much easier to simultaneously listen to a male and female voice rather than to two male voices. Note that although capacity-sharing accounts also assume interference between parallel central processes, they attribute interference to a generic resource limitation (a sort of mental “energy”) rather than to specific interactions.

We are not the first to argue that central interference depends critically on the composition of the particular tasks. Indeed, content-dependent theories have a long history (e.g., Greenwald, 1972; Hirst & Kalmar, 1987; McLeod & Posner, 1984; Navon & Miller, 1987; Pashler, 1990; Shaffer, 1975). A well-known class of content-dependent theories includes multiple-resource theory (e.g., Allport et al., 1972; Navon & Gopher, 1979a, 1979b; Wickens, 1980, 1984) in which interference is maximized when two tasks require similar pools of resources. One prominent example of multiple resource theory is the code compatibility account of Wickens, Sandry, & Vidulich (1983). These authors proposed that dual-task interference depends critically on the interaction between the type of central code (spatial versus verbal) and the type of input modality (auditory versus visual) and output modality (manual versus speech).

A second class of content-dependent theories supposes that the costs arise not from competition for a limited set of task-specific resources but instead from crosstalk between conflicting representations. Along these lines, Navon and Miller (1987) proposed that dual-task interference might result because each task produces “outputs, throughputs, or side effects that are harmful to the processing” of the other task (see also Allport, 1987; Hirst & Kalmar, 1987). If, for instance, both tasks involve the same stimulus categories, then the near-simultaneous activation of both stimulus categories can create crosstalk or a binding problem (see Logan & Gordon, 2001) and lead to decrements in performance.

These examples are typical in that it is assumed that content-dependent interference stems from overlap between salient features of the stimuli or between features of the responses for the two tasks. That is, theorizing about content-dependent interference has largely been restricted to interactions between tasks that possess overlapping features, such as when the stimuli share a semantic (e.g., Hirst & Kalmar, 1987; Logan & Schulkind, 2000; Navon & Miller, 1987) or spatial (e.g., Wickens, 1984) component, or the responses are produced in the same modality (e.g., Pashler, 1990). The experiments discussed below lead us to an alternative theory of content-dependent processing in which interference can occur even when there is no explicit overlap between the specific stimuli or responses for the two tasks, but because the stimuli activate existing linkages between input and output modalities.

Previous evidence for content-dependent central interference. Although investigations of content-dependent interactions have largely been restricted to cases when the two tasks involve conceptually overlapping S-R mappings, there is some evidence that this type of overlap is not a requirement. Levy and Pashler (2001) assessed dual-task performance after modest training (two 1-hr training sessions) with two different pairings of input and output modalities. In their Experiment 1, participants performed the Standard modality-pairings used in the Schumacher et al. (2001) study. We call these the Standard pairings, because participants responded manually to visual stimuli and vocally to auditory stimuli, which is standard in the dual-task literature. In Experiment 2, a new group of participants responded to the visual stimulus with a vocal response and to the auditory stimulus with a manual response. We call these the Nonstandard pairings. Levy and Pashler (2001) reasoned that if the use of proper instructions and moderate practice are sufficient to produce perfect time-sharing, then it should occur in both modality-pairing conditions. Contrary to this prediction, both single-task and dual-task RTs were longer with the Nonstandard pairings compared to the Standard pairings. Levy and Pashler (2001) interpreted their findings as evidence that there are structural limitations to performance. They assumed that with the Standard modality-pairings either the visual-manual task did not require central operations or that the central bottleneck was latent.

Another explanation suggested by these findings, not explicitly considered by Levy and Pashler, is that modality-pairings determine the linkage between stimulus representations and responses, and that the specifics of these linkages have a critical influence on dual-task performance. With this point in mind, we note that a very limited set of tasks has been used in previous studies showing near-perfect dual-task performance (e.g., Hazeltine et al., 2002; Schumacher et al., 2001). In particular, these studies paired a visual-manual task with an auditory-vocal task, which, according to content-dependent theories, may differ fundamentally from other pairings of input and output modalities.

Thus, the findings of Levy and Pashler (2001) provide preliminary evidence that modality pairings affect dual-task performance even when the stimulus and response modalities are distinct (although they did not interpret their results in this way). That is, when considered in tandem with other dual-task studies using similar tasks (e.g., Hazeltine et al., 2002; Schumacher et al., 2001), their findings suggest that the input-output pairings within a task play a crucial role in executive control processes. This conclusion is consistent with content-dependent accounts of response selection; reductions in dual-task costs depend on the specific S-R pairings associated with the two tasks. Such a form of content-dependent interference differs critically from previous reports of content-dependent peripheral interference (between inputs or outputs), because on the dual-task trials of Levy and Pashler the set of stimuli and responses were the same for the Standard and Nonstandard groups. Thus, the difference in dual-task costs cannot be attributed to increased competition for peripheral resources. In sum, the Levy and Pashler result is consistent with a content-dependent account if it is assumed that the Nonstandard modality-pairings (auditory-manual and visual-vocal) produce more code interference than the Standard modality pairings (auditory-vocal and visual-manual), and hence greater dual-task costs.

One factor complicating the interpretation of the Levy and Pashler (2001) findings, however, is that Nonstandard pairings happened to create less compatible tasks. Arguably, it is much easier to respond to the location of a visual stimulus by pressing a button in the corresponding position than to say the number assigned to that position. This ease of the S-R mapping is not an inherent consequence of the standard modality pairings, but rather an accidental consequence of the particular tasks being studied. Consistent with this point, the Nonstandard pairings produced longer single-task RTs in addition to the larger dual-task costs.

The pairing of particular stimuli with particular responses can shorten central operations by increasing either set-level compatibility or element-level compatibility (Kornblum, Hasbroucq, & Osman, 1990). Set-level compatibility refers to the correspondence between the relationship among the stimuli and the relationship among the responses. For example, if the leftmost stimulus location is mapped to the leftmost keypress and the rightmost stimulus location is mapped to the rightmost keypress, there is a high degree of set-level compatibility. Note that each stimulus, considered by itself, is not inherently compatible with its response; the compatibility arises only when the sets of stimuli and responses are considered. This contrasts with element-level compatibility, which relates to the compatibility of a response with a stimulus in the isolation of the other S-R pairs. For example, saying the word “red” in response to the printed word “RED” has a high-degree of element-level compatibility. Any change in the S-R pairings may affect both set-level and element-level compatibility.

An important characteristic of content-dependent interactions, as we define them, is that they are distinct from the effects of S-R compatibility. Both capacity-sharing and bottleneck models can accommodate findings in which set-level or element-level compatibility effects increase dual-task costs (Kahneman, 1973; McCann & Johnston, 1992; Navon & Miller, 2002; Tombu & Jolicoeur, 2003). According to the bottleneck model, reduced compatibility increases the duration of response selection for one of the tasks, which can in turn increase the bottleneck delay in response selection for the other task. According to capacity-sharing accounts, S-R mappings with less element-level compatibility require a greater allocation of central resources, leaving fewer resources available for any other task and increasing dual-task costs (see Navon & Gopher, 1979a; Navon & Miller, 2002). The bottom line is that if a particular manipulation of input-output pairings alters set- or element-level compatibility, it will be unclear whether any differences in dual-task costs are due to modality pairings in general or to the change in compatibility[2]. Thus, the challenge for content-dependent theories of central interference is to differentiate modality-pairing effects from S-R compatibility effects.

In addition to Levy and Pashler, a few dual-task studies have also examined the effect of modality pairings (Shaffer, 1975; Wickens, 1980, 1984; Wickens et al., 1983) on dual-task performance. Shaffer (1975), for example, had a single skilled typist type one set of words and shadow another set simultaneously. Performance was much worse when the participant had to type words presented in the auditory modality and read aloud words presented visually than when the reversed arrangement was required. Although suggestive, these studies fall far short of demonstrating a direct influence of modality-pairings on central operations. The continuous nature of the tasks and the coarse level of analysis make it possible that interference was occurring at a peripheral level. Moreover, as in the Levy and Pashler (2001) study, modality-pairings were confounded with element-level S-R compatibility. At present, we see no compelling evidence for or against the hypothesis that modality-pairings have a significant influence on dual-task performance.

The Present Study

The goal of the present study is to empirically address the three questions posed above and to lay the groundwork for an integrative theory of dual-task interference. We describe how the current approach bears upon each of these questions in turn.

Do central operations limit dual-task performance? Evidence for unlimited central capacity has come from demonstrations of simultaneous task performance with minimal dual-task costs, a phenomenon termed “near-perfect time sharing” (Schumacher et al., 2001). However, the conditions under which near-perfect time sharing has been observed have involved tasks with very short response times (e.g., under 300 ms) for at least one of the two tasks. Given that short response times generally correspond to short central stages, it is difficult to distinguish between a latent bottleneck and parallel response selection (see Byrne & Anderson, 2001; Hazeltine et al., 2002; Ruthruff, Johnston et al., 2003; Ruthruff, Pashler et al., 2003). Moreover, practice and compatible S-R mapping presumably primarily shorten the central operations, leaving perceptual and motoric processes relatively unaffected (McCann & Johnston, 1992; Ruthruff, Johnston, & Van Selst, 2001; Van Selst et al., 1999). Therefore, the existing demonstrations of near-perfect time-sharing have occurred under conditions that are amenable to a latent bottleneck. Time-sharing would provide much stronger constraints to theory if it could be demonstrated with more difficult tasks that produce longer response times. Therefore, one goal of the present study is to examine dual-task performance after practice with relatively difficult tasks.

Serial or parallel response selection? Even if near-perfect time-sharing is not observed, it is possible that response selection for the two tasks occurred in parallel. The capacity-sharing theories described above (e.g., Navon & Miller, 2002; Tombu & Jolicoeur, 2002; Tombu & Jolicoeur, 2003) predict exactly this state of affairs. Dual-task costs should be distributed between the two tasks and these costs should be approximately proportional to single-task RT. Increases in the duration of central operations should lead to increases in dual-task costs according to both the central bottleneck and capacity-sharing models (see Tombu & Jolicoeur, 2003). Moreover, direct evidence for serial response selection can be obtained by manipulating the SOA and analyzing of the distribution of inter-response intervals.

Do dual-task costs reflect content-dependent interactions? Levy and Pashler (2001) provided preliminary evidence that perfect time-sharing does not occur with Nonstandard modality-pairings. However, some aspects of this study prevent the results from being conclusive in this regard. To begin, they trained participants for only two sessions rather than 6-9 sessions as in previous demonstration of near-perfect time-sharing. After this amount of practice, even the participants that performed the same task pairs as used in the Schumacher et al. (2001) study did not eliminate their dual-task costs. Therefore, it is unclear whether the performance differences observed by Levy and Pashler would persist at the level of practice where near-perfect time-sharing has previously been reported.

An even greater limitation of the conclusions regarding modality-pairings concerns the specific way the modalities were manipulated by Levy and Pashler (2001). The set-level compatibility between the stimuli and the responses was much stronger for the Standard group (see Kornblum et al., 1990), because the visual-manual task used a spatially-compatible mapping (*e.g.*, leftmost stimulus to leftmost response, and so on). The differences in the compatibility of the S-R mappings can explain why there was a considerable effect of modality-pairings on single-task RT. Such differences can also explain why there was a considerable effect of modality-pairings on dual-task RT (see Diedrichsen et al., 2001; Franz, Zelaznik, Swinnen, & Walter, 2001; Greenwald, 1972; Greenwald & Shulman, 1973; McCann & Johnston, 1992; McLeod & Posner, 1984; Wickens et al., 1983). In the present study we will evaluate whether modality-pairing effects occur even after extensive practice and even when these effects are not confounded with differences S-R compatibility.

Experiment 1a

Experiment 1a addressed the central limitation question by evaluating dual-task performance across practice for two tasks with Nonstandard pairings in a paradigm similar to that of Schumacher et al. (2001) and Hazeltine et al. (2002). Participants completed 8 sessions in which they responded manually according to the pitch of tones and vocally according to the category of the referents of visually presented words. In other words, participants practiced two tasks with the Nonstandard pairings. As in Schumacher et al. and Hazeltine et al., participants performed pure single-task blocks or mixed blocks containing some single-task trials and some dual-task trials where the stimuli for the two tasks were presented simultaneously.

Because our tasks were more difficult than those used in earlier studies, we expected to observe larger mean single-task RTs. Theories that hold that limitations in central processing lead to dual-task costs (e.g., the bottleneck model and the capacity-sharing models) predict that the longer single-task RTs should lead to increases in dual-task costs; that is, virtually perfect time-sharing should not occur, even after considerable practice. Increased dual-task costs are also consistent with content-dependent accounts, because the Nonstandard modality pairings may lead to greater conflicts between the two tasks. However, if there are no structural limitations in dual-task performance, then near-perfect time-sharing should still be observed, as in the Schumacher et al. and Hazeltine et al. studies.

Method

Participants. Four students (one male) from community colleges in the Mountain View, California area were paid to participate. Most performed one session per day and were asked to complete all the sessions within a period of about 5 weeks. All of the participants were right-handed and between the ages of 18 and 25.

Stimuli. Tone stimuli were presented at 220, 880, or 3520 Hz and lasted for 300 ms. Word stimuli, subtending 1.4° horizontally by 1.9° vertically, were white against a black background. There were four bug words (ANT, FLEA, APHID, BEETLE), four food words (EGG, SOUP, CANDY, COOKIE), and four tree words (OAK, PINE, CEDAR, SPRUCE). The words remained visible until a response was made.

Procedure. Participants responded to the 220, 880, and 3520 Hz tones by pressing the 'j', 'k', and 'l' keys and they responded to bug, food, and tree words by saying the words "bug," "food," or "tree". Prior to beginning the experiment, participants were allowed to study the words for each category. They then completed 15 blocks of 60 trials each (plus three warm-up trials). The first three blocks within each session were considered practice. There were two single-task block types (one for each task) and a Mixed block type. The single-task blocks included 60 trials of just one of the two tasks; following Schumacher et al. (2001) we refer to these conditions as homogenous single-task conditions. The Mixed blocks also contained 60 trials, consisting of 36 dual-task trials and 12 single-task trials on each task (intermixed at random). Because participants did not know which task(s) would appear on any given trial within the Mixed block, they presumably prepared for both tasks. We refer to the single-task trials embedded within the Mixed blocks as heterogeneous single-task trials.

The three practice blocks within each session consisted of one single-task block of each task followed by a Mixed block. In the subsequent 12 test blocks (which were used for data analysis), participants performed a single-task block of one task, followed by two Mixed blocks, followed by a single-task block of the other task. This cycle then repeated two more times. Thus, the test blocks consisted of 6 dual-task blocks and 6 single-task blocks (3 for each task). The order in which the two single-task block types (i.e. tone task versus word task) were performed was the same throughout each session, but was reversed for the following session. The order of the single-task blocks in the first session was counterbalanced across participants.

Participants were asked to respond quickly and accurately and to give both tasks equal emphasis. No instructions were given regarding response order in the dual-task condition; participants were allowed to respond in any order, or to group their responses together. Following Schumacher et al. (2001), participants were awarded a monetary bonus based on their speed and accuracy. Participants were not told the exact formula, only that they could maximize the payoff by being fast and accurate on both tasks. The actual formula, computed following each block (excluding the three practice blocks), involved only single-task trials within that block.[3]

Each trial began with the presentation of a fixation cross for 500 ms, then a blank field for 300 ms, followed by the tone and/or words. On dual-task trials, the word and tone were presented simultaneously; that is, the SOA was 0 ms. If an error was made, an error message was displayed for 2000 ms. The next trial began 500 ms later. At the end of each block of trials, participants received performance feedback (RT, percent correct and monetary bonus) and were allowed to take a short break.

Results and discussion

Reaction Time. Trials with an error on either task were excluded from the analysis (5.8% of trials). Also excluded were trials in which either RT fell outside the cutoff values (150 and 2000 ms for both tasks). Moreover, trials with a reaction time on either task more than 2.5 standard deviations from the mean for that replacement and that condition were also eliminated from the analysis. This RT-based trimming procedure resulted in the exclusion of an additional 1.4% of the data. Table 2 lists the mean RTs across participants for the three conditions.

Single-Task Reaction Times. RTs from the single-task blocks (Figure 1) for the visual and auditory tasks were submitted to separate repeated-measures ANOVAs, with session as the only factor. For both tasks, there were significant effects of Session [visual task: $\underline{F}(7,21) = 11.66$; $\underline{p} < .0001$; auditory task: $\underline{F}(7,21) = 9.37$; $\underline{p} < .0001$], indicating that RTs decreased with practice. Participants were able to shorten their RTs for both tasks with practice by more than 100 ms.

Mixing Costs. To assess the cost associated with having to prepare for both tasks within a block rather than preparing for just a single task, we subtracted the mean RT from homogeneous single-task trials (in the Pure single-task blocks) from the heterogeneous single-task trials (in the Mixed blocks). In the task-switching literature, this type of effect has been termed the mixing cost. This cost does not result from interference associated with performing two tasks at once (see Los, 1996, 1999; Los & Van Den Heuvel, 2001). Rather, it results from the requirement to keep the S-R mappings for both tasks in working memory and/or the inability to predict which task or tasks would appear in the upcoming trial (see Gottsdanker, 1979). It may also include a task-switching component, given that on half of the single-task trials in the Mixed blocks were task switches whereas none of the single-task trials in the single-task block were task switches (see Goschke, 2000). We combined the mixing costs across the two tasks because participants may have differed in their relative task emphasis.

The mixing costs, shown in Figure 2, demonstrated a significant effect of Session [$\underline{F}(7,21) = 9.29$; $\underline{p} < .0001$]. Although these costs diminished more rapidly than the dual-task costs (discussed below), they remained considerable on the final session (mean: 110 ms). This finding is different from that of the Schumacher et al. (2001) and Hazeltine et al. (2002) studies, where there were essentially no mixing costs after similar amounts of practice. The results, however, are consistent with those reported by Levy and Pashler (2001), despite using a slightly different method.

Dual-Task Costs. Dual-task costs were computed by subtracting the mean RTs of the single-task trials within the mixed blocks (the heterogeneous single-task condition) from the mean RTs of the dual-task trials in the mixed blocks. Rather than make assumptions regarding whether one task or the other was given priority during the dual-task trials (in fact, all participants demonstrated significant dual-task costs for both tasks), we simply summed the dual-task costs across the two tasks to form a combined dual-task cost measurement (Figure 2). Like the RTs from the single-task blocks, the combined dual-task cost decreased steadily across the 8 sessions, although the effect of session was only marginally significant [$\underline{F}(7,21) = 2.48$; $\underline{p} < .06$] in the overall analysis.

Despite this decrease across sessions, dual-task costs remained robust throughout the experiment. In fact, for the seventh and eighth sessions, the mean cost was 257 ms, with the smallest mean cost of the four participants being 161 ms. Although consistent with the report of Levy and Pashler (2001) after three sessions, this finding contrasts sharply with the reports of Schumacher et al. and Hazeltine et al., in which dual-task costs were essentially eliminated after equivalent amounts of training.

These findings suggest that the ability to eliminate dual-task costs with practice and incentives, as demonstrated by Schumacher et al. and Hazeltine et al., depends on the particular combination of tasks to be performed. This finding poses problems for theories, such as EPIC, that assume there is no central interference after moderate practice. However, the results are consistent with two classes of hypotheses described in the Introduction. First, it is possible that performance of these tasks was limited by a central bottleneck. Note that in the Hazeltine et al.

(2002) experiments, RTs for the single-task blocks of the visual task were near 250 ms when dual-task costs were near zero, whereas RTs for the single-task visual task in the present Experiment 1a averaged 495 ms. The longer RTs may have resulted from the fact that there were 12 visual stimuli rather than 3 and/or that the stimuli were not spatial. Therefore, performing the visual task may have required a greater allocation of central resources or capacity than in the Hazeltine et al. and Schumacher et al. experiments, creating competition for this limited pool of central resources. According to this view, the cause of dual-task interference was not the use of the non-standard modalities, but rather the use of a relatively difficult visual task (with more than the usual number of stimuli). Second, pairing visual stimuli with vocal responses and auditory stimuli with manual responses may have caused content-dependent interference that is not associated with task difficulty per se but instead stems from interactions between central processes for the two tasks. We investigate these two hypotheses in Experiment 1b.

Accuracy. The mean proportion of correct responses was .95 for the visual task and .96 for the auditory task. Table 2 lists the mean accuracy across participants for the three conditions. Given that dual-task costs and mixing costs were not necessarily expected to be observed in the proportions of correct responses, these data were analyzed in a slightly different manner than the RT data. The proportions of correct responses were submitted to a two-way ANOVA, with session and trial-type (Single from single-task blocks, Single from mixed blocks, and Dual). Neither factor achieved significance for either task. However, for the auditory task, there was a significant Session x Trial-type interaction [$F(14,42) = 2.17$; $p < .05$]. This interaction reflected the fact that on the initial sessions, accuracy for the dual-task trials (.91) was lower than the other trial types, but on the remaining sessions, the accuracies were nearly equivalent. These accuracy data indicate that the dual-task costs on RT did not result from a speed-accuracy tradeoff.

Experiment 1b

In Experiment 1a, dual-task costs with Nonstandard modality pairings were still robust after 8 sessions of practice. These results suggest that the pattern of dual-task costs reported by Schumacher et al. (2001) and Hazeltine et al. (2002) occurs only under restricted conditions, for instance, when one of the tasks involves spatial correspondence or when the tasks use the Standard modality-pairings. Given that the inputs and outputs of the pairings used in Experiment 1a are similar to those used in the Schumacher and Hazeltine studies, it appears that the costs are due to central limitations.

The data also appear to support the claim that central limitations are content-dependent, because the Nonstandard modality pairings produced more lasting dual-task costs than previous experiments using Standard pairings. However, before concluding that the persistent dual-task cost was due specifically to the use of Nonstandard pairings, we first test to ensure that the costs were not the result of task difficulty per se. Although the auditory task (tones) has been used in previous studies, our visual task has not. The visual task in Experiment 1a was more difficult than previously used, involving twelve different word stimuli compared to only three in previous studies. Thus, task complexity, not content-dependent interference, could have led to the persistent dual-task costs. We test the complexity hypothesis in Experiment 1b by transferring the 4 participants from Experiment 1a to the Standard modality-pairing condition, which required manual responses for the visual task and vocal responses for the auditory task. The task complexity remains unchanged, only the mapping of input and output modalities is different.

In the visual-manual task, the same twelve words used in Experiment 1a were presented, but instead of saying “bug”, “food” or “tree” participants pressed one of three keys depending on whether the word was a type of bug, food, or tree. For the auditory-task, the tones were also the same as the previous experiment, but instead of pressing keys participants said “one” to the 220 Hz tone, “two” to the 880 Hz tone, and “three” to the 3520 Hz tone, as in Schumacher et al. (2001) and Hazeltine et al. (2002). Thus, this task combination has essentially the same input and output demands as the task combination used in Experiment 1a, and the stimulus categorizations required by for each task correspond to those in the previous experiment[4]. Therefore, any differences between experiments in dual-task performance should relate to conflict for central resources rather than conflict for peripheral resources, such as visual attention or motor control. If task difficulty was the critical factor, then large dual-task costs should be observed even with the standard mappings. If, however, the persistent costs were the result of content-dependent interference induced by the Nonstandard modality pairings, then these costs should be eliminated with the Standard mappings.

The reason for not using more compatible vocal responses for the auditory task was based on concerns about differences in the element-level compatibility of the S-R mappings between the experiments. If greater element-level compatibility exists between the stimuli and responses in one modality pairing than in the other, then dual-task costs should be smaller in the former pairing (c.f., Levy & Pashler, 2001). Thus, we attempted to make the compatibility of the Nonstandard group at least as great, if not greater, than the compatibility of the Standard group. For the tone stimuli, responses (“one”, “two”, and “three” or the first, second, and third keys from left to right) were ordinally related to the pitch of the tones for both modality pairing conditions. For the word stimuli, the manual responses were mapped arbitrarily onto the categories for the standard group, whereas the vocal responses were simply the category labels themselves (“bug”, “food”, and “tree”) for the Nonstandard group. These responses keep element-level compatibility in the tone task roughly equal for Standard and Nonstandard groups, while creating somewhat higher compatibility in the word task for the Nonstandard group. If we find a detrimental effect of the Nonstandard pairs (as in Levy & Pashler, 2001), differences in element-level compatibility will not provide a likely explanation.

It was expected that these participants would initially show some negative transfer effect from having performed Experiment 1a. Because our emphasis is on performance after several sessions of practice, however, there should be sufficient time for any initial negative transfer to dissipate. While better performance in Experiment 1b may be attributed in part to the fact that these individuals have had greater exposure to the task components, our initial focus was on whether the difficult visual task could be performed in a dual-task context with little or no dual-task costs. If it can, the decrease in dual-task costs (200+ ms) would have to be much larger than what one would normally expect from practice alone, given the modest effects of practice observed in Experiment 1a (note the small decrease in dual-task costs over the last several sessions). Furthermore, concerns over the confounding of task pairings with history is addressed in Experiment 2a.

Method

Participants. The same individuals from Experiment 1a participated in Experiment 1b.

Stimuli. The stimuli were identical to those used in Experiment 1a.

Procedure. The procedure was identical to that of Experiment 1a except that participants responded manually to the visually presented words and vocally to the tones. The same three keys were used as in Experiment 1a: ‘j’, ‘k’, and ‘l’ were pressed to indicate bug, food, and tree words, respectively. The words “one”, “two”, and “three” were spoken to indicate the 200 Hz, 800 Hz, and 3520 Hz tones, respectively.

Results and discussion

Reaction Time. The RT data were analyzed in the same manner as in Experiment 1a. 7.0% of the trials were excluded because one or more of the responses were incorrect and an additional 0.7% were excluded because one of the RTs was more than 2.5 standard deviations from the mean. Table 2 lists the mean RTs across participants for the three conditions.

Single-Task Reaction Times. RTs from the single-task blocks (Figure 3) for the visual and auditory tasks were submitted to separate, identical repeated-measures ANOVAs, with session as the only factor[5]. For both tasks, there were significant effects of session [visual task: $\underline{F}(7,21) = 11.34$; $\underline{p} < .0001$; auditory task: $\underline{F}(7,21) = 15.76$; $\underline{p} < .0001$], indicating that RTs decreased during training. On its own, the meaning of the practice effect is ambiguous, because the decrease may reflect that participants learned to overcome the associations between stimuli and responses learned in Experiment 1a. In general, however, the absence of any apparent carryover effects is impressive, given the participants’ considerable exposure to the other S-R mappings in Experiment 1a. Mean RTs for both tasks were faster at the end of Experiment 1b than at the end of Experiment 1a (visual task: 448 vs. 495 ms; auditory task: 283 vs. 352 ms). Hazeltine et al. (2002) also reported small costs when, after several sessions of practice, participants switched from a spatially compatible S-R mapping for visual-manual task to an incompatible one. It may be that after moderate practice on these simple tasks, participants are able to rapidly remap the responses with only small costs to performance, making carry-over costs undetectable at a temporally coarse scale of analysis.

Mixing Costs. The mixing costs (Figure 4) were reduced significantly throughout the training sessions [$\underline{F}(7,21) = 10.58$; $\underline{p} < .0001$]. By the seventh session, the mean cost (9 ms) was no longer significantly greater than zero.

Dual-Task Costs. The dual-task costs were again combined across the two tasks. As shown in Figure 4, the dual-task costs on the first session of Experiment 1b were only 20 ms greater than the costs observed in the final session of Experiment 1a. The further reduction of the costs throughout training resulted in a significant effect of session [$\underline{F}(7,21) = 20.40$; $\underline{p} < .0001$]. Whereas dual-task costs decreased by only 85 ms in the 8 sessions of Experiment 1a, they decreased by 253 ms in the 8 sessions of Experiment 1b.

Despite the fact that the stimuli were identical and the responses highly similar in Experiments 1a and 1b, the magnitude of dual-task costs differed dramatically. By the eighth session, the dual-task costs were not completely eliminated (mean: 38 ms; range: 23-54 ms), but they were much smaller than those observed in Experiment 1a (mean: 271 ms; range 167-328ms). The difference in the final dual-task costs across the two experiments was statistically reliable [mean: 233 ms; $\underline{t}(3) = 6.04$; $\underline{p} < .01$]. Although the visual task was more difficult than those employed in previous demonstrations of near-perfect time-sharing, participants were eventually able to categorize the words with little apparent interference from the vocal task.

The visual-manual task in this experiment used word stimuli belonging to different semantic categories (with no ordinal relationship). This is in contrast to visual stimuli used in previous studies showing virtually no dual-task costs after practice (e.g., Hazeltine et al., 2002; Levy & Pashler, 2001; Schumacher et al., 2001). Those visual tasks required the translation of spatially distinct stimuli to spatially distinct responses. Thus, the negligible dual-task cost for the

Standard group suggests that people are able to achieve fairly small dual-task costs even with two tasks that do not involve a spatial stimulus mapped to a spatial response.

The difference in dual-task costs between the two experiments is consistent with the content-dependent model of dual-task interference and indicates that competition for generic central resources is unlikely to be the primary limitation to dual-task performance after moderate practice. Dual-tasks cost were minimal when the two tasks involved the Standard modality pairs, demonstrating that the more difficult non-spatial categorization of the visual stimuli (12 words classified as a type of bug, food, or tree) did not, on its own, produce the costs observed in Experiment 1a. Given the similarity between the tasks across the two experiments, competition for generic central resources does not appear to constitute the critical limitation in performance under these conditions. Instead, the results that interference between response selection processes depends on the particular pairings of stimulus and response modalities. An alternative account, tested in Experiment 2a, is that participants were simply more familiar with the tasks when performing Experiment 1b.

Strategic accounts might posit that the greater dual-task costs observed in Experiment 1a stems from the participants conservatively scheduling task operations to avoid making vocal responses to the word during the presentation of the auditory stimuli. At least two aspects of the data argue against this interpretation, however. First, even if participants had showed no dual-task cost on the visual-vocal task in Experiment 1a (i.e., had their dual-task RTs been identical to their single-task RTs), the time between the tone presentation and the vocal response would have been more than 150 ms greater than time between the two events in Experiment 1b. That is, on the eighth session with the Nonstandard pairs, the interval between the offset of the tone and the onset of the vocal response averaged 501 ms on dual-task trials, whereas eighth session with the Standard pairs, this interval averaged 328 ms. Therefore, even allowing for some added cautiousness on the part of the participants given the tone and vocal responses were associated with distinct tasks, it seems unlikely that after 8 sessions of practice, such an unnecessarily conservative strategy would have been maintained. Second, the dual-task costs are consistently greater for the auditory-manual task than for the visual-vocal task (see Table 2). If the inflated dual-task costs reflected an effort to ensure that the auditory stimulus was not spoken over, the opposite pattern would be expected – that is, greater costs would be observed for the visual-vocal task.

Accuracy. The mean proportion of correct responses was .95 for both tasks. Table 2 lists the mean accuracy across participants for the three conditions. These were submitted to a two-way ANOVA with session and trial-type as factors, as in Experiment 1a. Neither factor nor than interaction was significant for either task.

Experiment 2a

It is conceivable that the differences in dual-task costs relate to the greater exposure to this dual-task method and the stimulus classifications by the participants when they performed Experiment 1b (Standard modality pairings) than when they performed Experiment 1a (Nonstandard modality pairings). For instance, if the more complicated visual task required more practice before the categories could be accessed without the bottleneck, then the lack of cost in Experiment 1b might simply reflect the eventual automatization of this task or a reduction in the amount of required central resources rather than content-dependent response selection processes associated with the distinct S-R pairings.

Although this explanation seems unlikely, it cannot be ruled out on the basis of the data from Experiment 1a. Therefore, to more rigorously assess this explanation, we tested two new groups of naïve participants in Experiment 2a. One group, which we will call the Standard group, performed the visual-manual and auditory-vocal tasks as in Experiment 1b. The other group, called the Nonstandard group, performed the visual-vocal and auditory-manual tasks, as in Experiment 1a. These individuals completed 16 sessions each, to determine whether differences between the Standard and Nonstandard pairs persist with even more extensive training. If exposure to the tasks, regardless of the modality pairings, is critical, then the dual-tasks costs should be similar for both groups. However, if visual-manual and auditory-vocal (Standard pairings) tasks produce less central interference than visual-vocal and auditory-manual (Nonstandard) tasks, as predicted by content-dependent models of dual-task performance, then the differences between the two groups should persist throughout training.

Method

Participants. Eight students from community colleges in the Mountain View, California area were paid to participate. Most performed one session per day and were asked to complete all the sessions within a period of about 5 weeks.

Stimuli. The stimuli were identical to those used in Experiments 1a and 1b.

Procedure. Two groups of 4 participants each completed 16 sessions of the experiment. One group, termed the nonstandard group, performed the same procedure as Experiment 1a; the other group, termed the standard group, performed the same procedure as Experiment 1b.

Results and discussion

Trials with an error on either task were excluded from the analysis (5.1% of trials). Also excluded were trials in which either RT fell outside the cutoff values (150 and 2000 ms for either task). Moreover, trials with a reaction time on either task more than 2.5 standard deviations from the mean for that replacement and that condition were also eliminated from the analysis. This RT-based trimming procedure resulted in the exclusion of 3.0% of the data. Table 3 lists the mean RTs across participants for the three conditions.

Single-Task Reaction Times. Reaction times from the single-task blocks for the visual and auditory tasks (Figure 5a and 5b, respectively) were submitted to separate, identical ANOVAs, with session as a within-subjects factor and group as a between-subjects factor. For both ANOVAs, there were significant effects of session [visual-task: $F(15,90) = 16.25$; $p < .0001$; MSE = 649.78; auditory-task: $F(15,90) = 25.04$; $p < .0001$; MSE = 1058.26] and a significant Session x Group interaction [visual-task: $F(15,90) = 3.95$; $p < .0001$; MSE = 649.78; auditory-task: $F(15,90) = 3.87$; $p < .0001$; MSE = 1058.26], but no main effect of group [visual-task: $F(1,6) = 2.99$; $p > .10$; auditory-task: $F(1,6) = 2.56$; $p > .15$]. RTs on both tasks improved with practice for both groups. However, the Session x Group interactions reflected different advantages for the two tasks. For the visual task, the Nonstandard group showed greater improvement across training than the Standard group. The pattern was reversed for the auditory task. In other words, both groups showed greater improvements with practice on the task with the vocal responses. Note that the pattern of single-task RTs changes dramatically across sessions, emphasizing the utility of evaluating performance over multiple sessions.

Mixing Costs. An ANOVA identical to those performed on the single-task RTs was performed on the mixing cost (Figure 6). The main effect of session [$F(15,90) = 31.01$; $p < .0001$; MSE = 957.61] and the Session x Group interaction [$F(15,90) = 6.37$; $p < .0005$; MSE = 957.61] were highly significant. These findings reflect that the mixing cost diminished rapidly over training, averaging less than 20 ms for both groups by the ninth session, and that this cost diminished much faster for the nonstandard group. Thus, it appears that after several sessions of training participants are able to hold the rules for both tasks in memory and switch between tasks with little performance cost.

After the initial session, the mixing costs observed for first 8 sessions performed by the Nonstandard group were considerably smaller than the costs observed in Experiment 1a, despite the fact that experimental procedures were identical. This difference, although unexplained, is statistically reliable for all of the sessions, largely due to the high variability of the mixing costs for participants in Experiment 1a. The individuals in Experiment 1a also showed a less marked decline in their dual-task costs, although the patterns of costs in relation to the modality pairings is otherwise very similar across Experiments 1a, 1b, and 2a. Because these groups performed the same tasks in the same design (differing only in the total number of sessions), any differences are presumably due to variation among the participants. In any case, as will be seen, both groups of participants performing the Nonstandard modality pairings produced much worse performance than the groups performing the Standard pairings.

Dual-Task Costs. As in Experiments 1a and 1b, the magnitude of the dual-task costs decreased with practice and was strongly affected by the S-R mapping (Figure 7). The costs were submitted to a two-factor mixed ANOVA identical to the one performed on the mixing costs. The ANOVA revealed a significant effect of group [$F(1,6) = 12.15$; $p < .05$; $MSE = 38521.22$] and a significant effect of session [$F(15,90) = 50.44$; $p < .0001$; $MSE = 2694.58$], but no Group x Session interaction [$F(15,90) = 1.62$; $p > .05$]. The difference between groups in dual-task costs persisted across all 16 sessions. In contrast, note that the difference between groups in mixing costs was essentially eliminated by the fourth session.

Although dual-task costs for the Standard group after 16 sessions of practice were small, they were significantly greater than zero over the last 4 sessions [$M = 35$ ms; $t(3) = 5.03$; $p < .01$ (one-tailed)] and showed little evidence of decreasing over the last 7 sessions. The dual-task costs were not restricted to either the visual or the auditory task but appeared to be distributed across both tasks: the dual-task cost was 20 ms for the visual task [$t(3) = 2.13$; $p < .07$ (one-tailed)] and 15 ms for the auditory task [$t(3) = 3.03$; $p < .05$ (one-tailed)].

Dual-task costs for the Nonstandard group, also were significant and persisted over the last 4 sessions [$M = 98$ ms; $t(3) = 5.78$; $p < .01$ (one-tailed)]. As with the Standard group, the costs for this group were distributed across the two tasks: the cost was 40 ms for the visual task [$t(3) = 7.86$; $p < .005$ (one-tailed)] and 58 ms for the auditory task [$t(3) = 4.83$; $p < .01$ (one-tailed)]. We will examine the nature of these residual costs in Experiment 2b.

Considered together, the data from the two groups are consistent with the findings from Experiments 1a and 1b and indicate that dual-task costs depend on the modality pairings. Whereas the costs for the Standard group fell below 50 ms by the tenth session, the costs for the Nonstandard group remained larger than this amount even after all 16 sessions. An impressive feature of the difference in dual-task costs was its persistence: over sessions 13-16, the mean dual-task cost was almost three times as large for the Nonstandard group (98 ms) as it was for the Standard group (35 ms). This difference between conditions on the final 4 sessions [$t(6) = 3.44$; $p < .05$ (two-tailed)] was observed despite the fact that the two groups were instructed to prioritize the tasks in the same way and received equivalent levels of exposure to the two component tasks. These findings cannot readily be attributed to difficulty categorizing the stimuli. The stimuli were the same for both groups, and the Standard group performed an arbitrary S-R mapping for the visual stimuli whereas the Nonstandard group uttered a superordinate category for these words (saying “bug”, “food”, or “tree”).

The difference in dual-task costs in combination with the difference in mixing costs (over the first few sessions) underscores the powerful effects of modality pairings early in training. Even though the total single-task RTs were similar (Standard: vis 513, aud 546; Nonstandard: vis 632, aud 470) in the initial session, dual-task reaction times were about 200 ms longer for the Nonstandard group compared to the Standard group on each task (Standard: vis 817, aud 793; Nonstandard: vis 1014, aud 1002), for a combined effect across tasks of about 400 ms. Although the difference in dual-task costs between the two groups was maintained throughout training, it is apparent the effect of the modality pairing is particularly strong early in training, especially when mixing costs are included in the measure.

We propose that the increased dual-task costs for the Nonstandard group results from content-dependent interference between the central processes for the two tasks. That is, the pairing of the stimulus and response modalities for the two tasks plays a role in determining how the two tasks interact during simultaneous performance. This effect is apparently not due to competition or crosstalk relating to peripheral processes; because the two groups were presented with the same combinations of stimuli and produced similar responses on dual-task trials, the modality-pairing effects are presumably due to central processes. In the general discussion we consider several specific hypotheses of how modality pairings might affect the interference between central processes

However, before accepting this conclusion, we consider another possibility. The increased dual-task costs for the Nonstandard group might result from differences in RTs for the two tasks. Although mean RTs for the visual task over the final 4 sessions were essentially identical for the two groups [Standard: 449 ms; Nonstandard: 446 ms; $\underline{t} < 1$], mean RTs for the auditory task were 68 ms faster for the standard group [273 ms vs. 341 ms; $\underline{t}(6) = 3.35$; $\underline{p} < .05$ (two-tailed)] (Figure 5). The longer RTs for the Nonstandard group suggests that the auditory-vocal task is more difficult and therefore may place greater demands on central processes independent of any content-dependent effects. The increased central demands may be manifest in a variety of forms, depending on which content-independent account is adopted. For example, the longer RTs may reflect a longer bottleneck stage or greater demands on central resources. In short, the increased dual-task costs for these participants may result from the greater difficulty of the Nonstandard pairings rather than an increase in the susceptibility to dual-task interference. We will test this hypothesis in Experiment 3. Note that, even on this view, the increased dual-task costs are due to the modality pairings, although the effect is indirect (see Teichner & Krebs, 1974).

The small but noticeable drop in dual-task costs for the Nonstandard group over the last 8 sessions highlights the fact that we can draw no firm conclusions about participants’ eventual ability to eliminate dual-task costs. It is possible that with even greater amounts of practice dual-task and single-task performance would become similar for both groups. However, we note that differences in the dual-task costs persisted between the groups even after extensive training – over 9000 presentations of each task. Also note that the non-standard group required roughly twice as many practice sessions as the standard group to reach a given level of dual-task performance. In any case, our claim is not that eliminating dual-task costs is not possible with the nonstandard pairs. Rather, the critical claim is that dual-task interference across a wide range of practice levels depends on the particular modality combinations, even when conflict for peripheral resources is held constant.

Inter-response intervals. In addition to the mean RTs, the distributions of inter-response intervals (IRI) are also diagnostic for interactions between concurrently performed tasks (see Hazeltine et al., 2002; Pashler, 1994b; Ruthruff, Pashler et al., 2003). Because this measure is based on the temporal relationship between the two responses, the distribution of IRIs can be sensitive to changes in strategies or dependencies between the two tasks that do not strongly affect overall RT. For example, postponement resulting from a central bottleneck would tend to produce a bimodal distribution of IRIs, where a specific range of IRIs (e.g., those near 0 ms) should occur infrequently. Bimodality should be especially apparent when the dual-task costs observed in both tasks reflect competition for the bottleneck and both central processing orders are used on a substantial proportion of trials. IRIs can also be diagnostic of strategies such as grouping, in which participants withhold or speed one response so that it can be executed with the other in a grouped fashion (see Pashler & Johnston, 1989). Inspection of the distribution of IRIs can reveal such task interactions whether or not they demonstrably affect overall RT.

Figure 8 shows the distribution of IRIs in the dual-task condition for both the standard group and the nonstandard group. For comparison, we have also plotted the IRI distribution expected if the two tasks were performed independently in parallel. The latter distribution is calculated by pairing the RT from each of the correct responses from the heterogeneous visual task trials with the RT from each of the correct responses from the heterogeneous auditory task trials (separately for each participant and each session), and computing the IRI between them. As an example, if there were 250 heterogeneous auditory trials and 300 heterogeneous visual trials in a given session for a given participant, the distribution of expected IRIs would be based on 75000 simulated data points.

To summarize the findings, shown in Figure 8, the distributions of expected and observed IRIs differed considerably early in practice but became similar as training progressed for both groups. In Panel A of Figure 8, the distributions of the IRIs are shown for the Standard and Nonstandard groups on the second session of practice. For both pairs, the peak is higher and narrower than would be expected based on the single-task data, suggesting that participants adopted a response-grouping strategy early in training (which may have slowed responses for at least one of the tasks). To determine whether each pair of predicted and observed distributions differed statistically, a two-sample Kolmogorov-Smirnov test was performed. This analysis showed a highly reliable difference between the observed and expected distributions for both the Standard, $D=.140$, $D_{crit}=.049$, $p<.05$, and Nonstandard, $D=.110$, $D_{crit}=.050$, $p<.05$, groups.

On session 8 (Panel B), the peak for the observed distribution of IRIs had a smaller magnitude than the peak for the predicted distribution. However, this pattern does not necessarily indicate that participants were no longer grouping. It may reflect a combination of strategies in which the participants are grouping their responses on only some fraction of the trials. This difference between the observed and expected distributions remained reliable both the Standard, $D=.091$, $D_{crit}=.050$, $p<.05$, and Nonstandard, $D=.146$, $D_{crit}=.051$, $p<.05$, groups.

On session 16 (Panel C), the observed distribution for the Standard pairs closely matched the predicted distribution. The difference between the distributions was not statistically significant, $D=.044$, $D_{crit}=.048$, $p>.05$, suggesting that the two tasks were indeed performed simultaneously and nearly independently. The match between the predicted and observed IRIs is particularly striking when one considers that small dual-task costs persisted for this group of participants on the final sessions. In contrast, the observed distribution for the Nonstandard pairs did not match the predicted distribution, $D=.085$, $D_{crit}=.052$, $p<.05$, reinforcing the assertion that central operations for the nonstandard pairs continued to interact, even after extended practice. If the participants had performed the two tasks in a fixed order, then the bottleneck model would predict much larger dual-task costs for the task performed second. However, given that the costs were spread evenly across the two tasks, this model would need to allow that both processing orders were used. If so, the distribution of IRIs should form two components, each associated with one processing order. Importantly, there was no evidence of bimodality in the IRI distributions; these data therefore argue against a bottleneck account of the data. Instead, the IRI data suggest that task operations occurred in parallel but with interference. It appears that the persisting limitation in dual-task performance after 16 sessions of practice is best characterized either as graded sharing of processing resources or cross-talk between task processes.

Accuracy. For both groups, the mean proportions of correct trials were between .95-.97 for both tasks. The proportions were submitted to a three-way ANOVA with group, session, and trial-type as factors. For the visual task, there were no significant main effects, but significant interactions between Session x Group [$F(15,90) = 2.43$; $p < .01$; MSE = .001] and between Session x Trial-type [$F(30,180) = 1.75$; $p < .05$; MSE = .0002]. The Session x Group interaction reflected the fact that the standard group was more accurate than the nonstandard group on the first session (.98 vs .92), but not significantly different on the remaining sessions. The Session x Trial-type interaction reflected subtle trade-offs in accuracy among the three trial-types that do not appear to follow an obvious pattern. The auditory task ANOVA revealed only a significant Session x Trial-type interaction [$F(30,180) = 2.03$; $p < .005$; MSE = .0004]. As with the visual-task data, the nature of the interaction was complex, although it is apparent that on the first block, the dual-task trials produced the highest proportion of incorrect responses, where as late in training, the single-task trials in the Pure blocks produced the highest proportion of incorrect responses.

Experiment 2b

The results of Experiment 2a suggest that content-dependent interference plays a major role in dual-task performance after moderate practice, as evidenced by the larger dual-task costs associated with the Nonstandard group. However, it remains an open question as to the nature of these costs as they persist throughout practice. Although the IRI distributions in Experiment 2a suggest that the Standard and Nonstandard groups were not limited by a single-channel bottleneck, we sought converging evidence for this conclusion.

To examine the nature of the residual dual-task costs, we transferred both the Standard and Nonstandard groups to follow-up sessions of the same tasks they had practiced in Experiment 2a but with different stimulus onset asynchronies (SOAs). If a bottleneck exists for these participants, then the dual-task costs should be affected by the SOA. The reason is that the SOA change should either increase or decrease the temporal alignment of the bottleneck stages on the two tasks (see Hazeltine et al., 2002; Ruthruff, Johnston et al., 2003). Given that the auditory task was performed more quickly, a reasonable expectation from bottleneck models is that dual-task costs should increase when the stimulus for visual task is presented first (*i.e.*, is given a head start) and should decrease when the stimulus for the auditory task is presented first.

The design was nearly identical to their previous sessions except that we introduced a 50-ms SOA. We presented the visual stimulus first on half the trials and the auditory stimulus first on the other half. Participants were not told of the SOA manipulation and, in a post-experiment interview, none reported noticing it. Also, because the performance difference between single-tasks trials in the pure (homogenous) blocks and single-task trials in the mixed blocks had essentially been eliminated after practice, we used only the mixed blocks in this follow-up experiment.

Method

Participants. The 8 participants from Experiment 2a also performed Experiment 2b.

Stimuli. All stimuli were identical to those used in Experiment 2a.

Procedure. Participants were instructed that they were performing an additional session of the experiment without the single-task blocks. The dual-task blocks were identical to those performed in Experiment 2a, except that on half of the dual-task trials, the visual stimulus appeared 50 ms before the auditory stimulus (SOA = 50 ms) and on the other half of the dual-task trials, the auditory stimulus occurred 50 ms before the visual stimulus (SOA = -50 ms). As in Experiment 2a, single-task trials were intermixed with the dual-task trials in the dual-task blocks.

Results and discussion

An equipment failure for one of the participants in the nonstandard group meant that this data could not be analyzed.

Reaction Time. The same trimming procedure was used as in Experiment 1. This RT-based trimming procedure resulted in the exclusion of 4.3% of the trials. An additional 3.4% was eliminated due to errors.

Of primary interest was the effect of SOA on the dual-task trials (Figure 9). The dual-task reaction times for each task were submitted to a two-way mixed ANOVA with SOA as a within-subjects factor and group as a between-subjects factor. For the auditory task, there was a main effect of group [$F(1,5) = 15.21$; $p < .05$; MSE = 3028.17], but no main effect of SOA [$F(1,5) = 4.77$; $p > .08$; MSE = 18.57] and no interaction [$F(1,5) < 1$]. The main effect of group on auditory-task RT considered on its own is ambiguous, because it could reflect differences in the two tasks or differences in the dual-task costs.

For the visual task, there was no main effect of group [$F < 1$], but there was a significant effect of SOA [$F(1,5) = 18.49$; $p < .01$; MSE = 72.40] and a Group x SOA interaction [$F(1,5) = 18.49$; $p < .01$; MSE = 72.40]. For the Standard group, RTs for the visual task were identical at the two SOAs (448 ms), whereas for the Nonstandard group, the RTs were 40 ms slower when the visual stimulus was presented first compared to when the auditory stimulus was present first (464 vs. 423 ms).

For the Standard group, the absence of an effect of SOA provides further evidence that their performance is not limited by a processing bottleneck. If dual-task costs were largely avoided by this group because the bottleneck stages for the two tasks did not overlap, then temporally shifting the relative onset of the bottleneck stages (with the SOA manipulation) should have caused them to become more closely aligned for at least one of the SOAs and therefore caused dual-task costs to increase at that SOA (see Hazeltine et al., 2002). Thus, the absence of any noticeable increase in dual-task costs at either SOA suggests that interference observed in the standard group was not due to a bottleneck. Instead, central operations appear to be occurring in parallel, and the observed interference likely stems from some other source, such as crosstalk or competition for a limited capacity. Although the present data pose difficulties for a bottleneck account, these data do not rule out the possibility that a relatively short bottleneck stage still exists along with an additional source of interference not due to a bottleneck. It is possible, despite the use of a range of SOAs between -50 and 50 ms, the bottleneck stages simply were rarely demanded at the same time. Experiment 3 will address this possibility by using standard modality-pairings but with a more difficult and time-consuming auditory task.

For the Nonstandard group, on the other hand, some aspects of the data are consistent with the presence of a processing bottleneck. Assuming that the auditory task, which had a shorter mean RT, was performed first, then dual-task costs should diminish when that task came earlier in time relative to the visual task. Such a pattern was in fact observed: dual-task costs on the visual task diminished and were essentially zero when the auditory task came 50 ms before the visual task. This finding is consistent with the proposal that both tasks compete for a single-channel mechanism that is usually accessed first by the auditory task.

However, the bottleneck account does not explain why the auditory task showed a 40 ms dual-task cost (412 ms vs. 372 ms; $\underline{t}(2) = 4.76$; $\underline{p} < .05$), that was unaffected by SOA (38 ms vs. 42 ms). Because the central stage of the auditory task was assumed to usually be processed first, little interference was expected on that task. To explain the present data, a more complicated bottleneck model would need to be proposed, perhaps involving an additional source of dual-task interference. Given that the available data comes from just three practiced participants, considerable caution is warranted when interpreting the absence of SOA effects on the auditory task. Nonetheless, the largest difference between the two SOAs for any of the participants was 8 ms, suggesting that the cost observed in the auditory task was not related to delays imposed by the processing for the visual task.

Inter-response intervals. As in Experiment 2a, the observed distribution of IRIs was compared to that expected if the two tasks were performed in parallel and independently. The results are generally consistent with the proposal that performance of the two tasks interacted only for the Nonstandard pairs. For the Standard pairs (Figure 10, Panel A), the observed distribution of IRIs for both SOAs were highly similar to the expected distribution and showed little evidence of any systematic distortion; the difference between the observed and expected distributions was not significant at the positive SOA, $\underline{D}=.044$, $\underline{D}_{crit}=.050$, $\underline{p}>.05$, although it did reach significance at the negative SOA, $\underline{D}=.054$, $\underline{D}_{crit}=.050$, $\underline{p}<.05$,. In contrast, for the Nonstandard pairs (Figure 10, Panel B), the distribution of IRIs clearly did not match the expected distribution (negative SOA: $\underline{D}=.130$, $\underline{D}_{crit}=.057$, $\underline{p}<.05$; positive SOA: $\underline{D}=.245$, $\underline{D}_{crit}=.057$, $\underline{p}<.05$). For both SOAs, the distributions appeared to deviate towards a tendency to respond to the two tasks simultaneously, especially when the tone was presented before the word.

The pattern of IRIs is inconsistent with the predictions of the bottleneck model. Although it is logically possible that a latent bottleneck of short duration still lingered, such a bottleneck does not appear responsible for the dual-task interference observed in the present study. First, there was no indication of the bimodal IRI distribution predicted by a mixture of central processing orders. Instead, the IRI distribution was highly similar to the distribution predicted from the single task trials and the assumption of independent processing. The IRIs did deviate substantially from the expected distribution when the tone stimuli are presented before the words. However, in this case, failing to match the expected distribution was associated with a reduction in dual-task costs relative to the other SOA condition and relative to session 16 of Experiment 2a, in which the expected and observed distributions were similar. Second, given that the auditory task produced shorter mean RTs, it might be expected that presenting the visual task first would create greater competition for the bottleneck. However, in this condition, the dual-task costs were essentially identical to the costs observed when the two stimuli were presented simultaneously. Thus, consistent with the RT data, it appears that central operations for the two tasks occurred in parallel.

In sum, it appears that the dual-task costs and modality pairing effects have a source other than a processing bottleneck. That is, content-dependent interference does not appear to reflect a bottleneck delay whose duration depends on the central stage durations, at least after moderate practice. We return to this issue in the General Discussion.

Accuracy. The mean proportion of correct responses for both tasks was .96. The proportions were submitted to the same ANOVAs as the RTs. Neither of these ANOVA yielded any significant effects.

Experiment 3

The results from Experiments 1 and 2 suggest that the near-elimination of dual-task costs depends on the modality pairings of the two tasks that must be performed together. Throughout 16 sessions of training, participants in the Nonstandard group showed significantly larger dual-task costs than participants in the Standard group, even though both groups used essentially the same stimuli and responses.

Along with the differences in dual-task costs, the modality pairings also caused differences in single-task RTs (see also Levy & Pashler, 2001), especially for the auditory task. It is interesting that the auditory task was more difficult, in the sense of producing much longer response times, when participants responded manually rather than vocally, even though the S-R mappings appeared to be roughly equally difficult; low, medium, and high tones were mapped onto the first, second, and third fingers of the right hand in one case and mapped onto the sounds “one”, “two” and “three” in the other case.

At present, the relationship between the modality-pairing effects on single-task RTs and dual-task costs is unclear. As noted in the discussion of Experiment 2a, that smaller dual-task costs are observed in the group with shorter single-task RTs is consistent with many content-independent models of dual-task performance. Shorter RTs usually reflect less demand on the central resources required by both tasks, so both bottleneck and capacity sharing models predict that dual-task costs should generally diminish as single-task RTs decrease (Navon & Gopher, 1979a; Navon & Miller, 2002; Pashler & Johnston, 1989; Ruthruff, Johnston et al., 2003; Schumacher et al., 1998; Tombu & Jolicoeur, 2003; Wickens, 1980). However, if there is a bottleneck is associated with response initiation (e.g., De Jong, 1993; Keele, 1973; see also Pashler, 1984) but no bottleneck associated with selection, then lengthening single-task RTs should generally either reduce or enhance dual-task costs. Some change in the costs is predicted because the degree to which the initiation stages are required at the same time should be affected as the relative difference in single-task RTs changes.

The relation between single-task RTs and dual-task costs is of relevant to theories of dual-task performance even if one assumes that performance is not governed by competition for some shared processing resource or structure after practice. For instance, it is possible that the elimination of dual-task costs requires that RTs for at least one of the tasks be sufficiently short, which may reflect the development of automatic associations between stimuli and responses that allow for selection to occur with minimal need for central resources. That is, the short RTs (e.g., RTs under 300 ms) may indicate that the task is exploiting a direct pathway between the stimuli and responses that bypasses limited-capacity central operations (see Hommel, 1998; Lien & Proctor, 2002; Logan, 1988). In principle, it is only necessary for one task to cease requiring central resources in order to eliminate the competition for resources and avoid dual-task costs.

To determine whether the increased dual-task costs associated with the Nonstandard pairs are related to the longer single-task RTs, we designed a much more difficult version of the auditory task to be paired with the same visual task used by the Standard group in Experiment 2a (visual RTs were similar for the two groups in that experiment). This new task (described below) required a comparison between several auditory sounds, with irrelevant variation in pitch and a completely arbitrary mapping of stimuli on to responses (whereas before the mapping was compatible). We refer to this group is as the Hard-Standard group, because the modality pairings were Standard pairings but the auditory task was harder than before. For purposes of comparison, the main analyses reported below compared data from this Hard-Standard group with that from the Nonstandard group of Experiment 2a. As will be seen, the difficulty manipulation successfully increased the mean single-task RTs of the Hard-Standard group, so that they were generally equal to or longer than those of the Nonstandard group.

If dual-task costs simply reflect resource demands as indexed by single-task RTs, then the dual-task costs should be roughly similar for the two groups. However, if the two groups show distinct patterns of dual-task costs despite producing similar single-task RTs, then the dominant content-independent accounts cannot succinctly describe the source of the limitation in dual-task performance. In other words, the changes in duration of central processes will not be sufficient to explain the modality-pairing effects.

Method

Participants. Four students from community colleges in the Mountain View, California area were paid to participate. Most performed one session per day and were asked to complete all the sessions within a period of about 5 weeks.

Stimuli. The word stimuli were identical to those used in Experiments 1 and 2. The auditory stimuli consisted of 9 possible sounds that differed along two dimensions. One dimension was the base pitch of 220, 880 or 3520 Hz. The other dimension, termed quality, indicated whether the tones sounded pure, trill, or chirpy. Trill tones were made by placing 6 ms silent gaps between 14 ms of contiguous tone. Chirpy tones were made by starting the tone at half its base frequency and having the frequency increase linearly throughout the 160 ms so that it ended at twice the base frequency. Pure tones were identical to those used in Experiment 1. For all three sound qualities, the base frequency was perceptually salient.

Procedure. Participants performed 16 sessions of single- and dual-task blocks in an identical manner to Experiment 2a. The visual task was the same as the one used by the standard group in Experiment 2a. In the auditory task, participants responded to the tone quality and ignored the base pitch. Participants were required to respond with the nonsense syllables rather than the words “one”, “two”, and “three”, so as to eliminate any S-R compatibility based on the ordinal relationships among the responses. The nonsense syllable “fik” was assigned to the trill tones, “dap” to the chirpy tones and “goot” to the pure tones. With the exception of the different auditory task, all other aspects of the procedure were identical to those used in Experiment 2a.

<u>Results and discussion</u>

<u>Reaction Time.</u> The data were trimmed before analysis as in Experiment 1. The RT-based trimming procedure resulted in the exclusion of 4.0% of the data. An additional 2.6% was eliminated based on accuracy. Table 3 lists the mean RTs across participants for the three conditions.

<u>Single-Task Reaction Times.</u> For the visual task (Figure 11A), mean RTs for the Hard-standard group were initially shorter than RTs for the Nonstandard group but, by the final 4

sessions of training, the RTs were similar for the two groups (Hard-Standard: 449 ms; Nonstandard: 446 ms; t(6) < 1). In fact, over the final 4 sessions, the mean visual task RTs were within 3 ms for the two groups.

For the auditory task (Figure 11B), mean RTs for the Hard-Standard group were initially much longer than RTs for the Nonstandard group (763 ms vs. 470 ms; t(6) = 4.77; p < .005). However, by the final 4 sessions of training, the difference declined to a non-significant 21 ms (362 ms vs. 341 ms; t(6) < 1). In sum, for both tasks, mean RTs in the single-task blocks were nearly identical for the two groups on the final sessions. Thus, our difficulty manipulation on the auditory-vocal task was successful in equalizing single-task response times.

Mixing Costs. The pattern of mixing costs is similar to that observed in Experiment 2a: although initially substantial (e.g., 173 ms on Session 1), the mixing cost became negligible by the 9th session (Figure 12). The increased difficulty of the Hard-Standard auditory task compared to the Easy-Standard auditory task was not apparent in the mixing cost after several sessions of practice. Across the first 4 sessions, the combined mixing costs for the two groups were similar (101 ms vs. 103 ms), as were the mixing costs for the auditory tasks alone (79 ms vs. 69 ms). In fact, with the exception of the first session, the costs for the two groups were similar throughout training. Thus, unlike dual-task costs, the mixing costs appear to be only minimally affected by task difficulty and the modality pairings after a few sessions of practice.

Dual-Task Costs. Whereas single-task RTs were similar for the hard-standard and nonstandard groups (differing by less than 25 ms on both tasks), the dual-task costs were not (Figure 13). Although dual-task costs were initially largest for the Hard-Standard group (not surprising, given that the single-task RTs were initially very long), they dropped rapidly across sessions and became statistically indistinguishable from those of the Easy-Standard Group by Session 7. Across the final four sessions, the mean dual-task cost for the Hard-Standard group was 43 ms (28, 31, 33, an 81 ms for the 4 participants, respectively), compared to 35 ms (16, 36, 39, and 49 ms) for the Easy-Standard group. In contrast, the mean dual-task cost was 98 ms (69, 69, 120, 134 ms) for the Nonstandard group. The large difference in dual-task costs between the Hard-Standard and Nonstandard groups was statistically reliable [t(6) = 2.58; p < .05 (two-tailed)]. The single-task RTs were closely matched between the Hard-Standard and Nonstandard groups (with total RTs for the Hard-Standard pairs being non-significantly larger than those for the Nonstandard pairs), and yet the dual-task costs for the Nonstandard pairs were 60 ms greater.

In sum, responding vocally to visual stimuli and manually to auditory stimuli appears to produce increased dual-task costs compared to the opposite arrangement of modalities. This result cannot be readily accommodated by accounts that assume that dual-task costs stem from competition for an undifferentiated central resource (Kahneman, 1973; Navon & Gopher, 1979a; Navon & Miller, 2002; Pashler, 1994a; Tombu & Jolicoeur, 2003; see also Wickens, 1984). To the best of our knowledge, this is the first demonstration of an effect of modality pairings on dual-task performance under conditions in which element-level S-R compatibility effects did not obviously favor the Standard modality-pairings (in fact, for both of our tasks, element-level compatibility would appear to favor the Nonstandard modality-pairings).

These findings provide direct evidence for content-dependent interference. Dual-task costs were determined by the particular S-R pairings independently of the S-R pairings' effect on single-task RT: Two tasks using distinct pairings of input and output modalities yielded similar single-task RTs but different dual-task costs. Moreover, two tasks using the same modality pairings yielded different single-task RTs but similar dual-task costs. Such a pattern is inconsistent with the view that dual-task costs stem from competition for a single undifferentiated resource. Note that in the present case, the input and output on any given dual-task trial is highly similar across conditions, indicating that peripheral sources of interference are not responsible for the modality-pairing effects. In the General Discussion section we will consider several possible reasons why the pairings of stimulus and response modalities influence central processes engaged during dual-task trials.

A comparison between the Hard-Standard and Standard groups is also informative. The Hard-Standard auditory-vocal task involved a greater number of S-R pairs than the Standard auditory-vocal task and, therefore, would presumably place greater demands on response selection processes. Such manipulations are typically assumed to lengthen the duration of the bottleneck stage thereby increasing dual-task costs (Karlin & Kestenbaum, 1968; McCann & Johnston, 1992; Pashler, 1994a). The absence of a difference in dual-task costs between the Standard and Hard-Standard groups is inconsistent with the bottleneck model unless one assumes that the response selection for visual-manual task took place before response selection for the auditory-vocal task or that the bottlenecks are sufficiently short in duration to not overlap when the SOA is zero. The former possibility is unlikely given that the auditory-vocal task produced RTs that were 50 ms shorter than the visual-manual task. The latter possibility is unlikely given that RTs for both tasks are longer than 350 ms in the Hard-Standard condition. It appears that with practice, the more difficult response selection can proceed in parallel with response selection operations for other tasks (Schumacher et al., 1998). While a latent bottleneck may persist after practice, it does not appear to be the source of the dual-task interference observed in the present experiments.

<u>Inter-response intervals.</u> The distribution of observed IRIs for the Hard-standard condition was compared to the expected distribution of IRIs based on single-task performance, as in Experiments 2a and 2b. Kolmogorov-Smirnov comparisons between the expected and observed distributions revealed statistically reliable differences for each session, all $\underline{D}$'s>.109, all $\underline{D}_{crit}$'s<.053, all $\underline{p}$'s<.05. On the second session, the observed and expected distributions were obviously dissimilar (Figure 14). Whereas a unimodal distribution centered between -100 and -150 ms (i.e., when the response to the visual task preceded the response to the auditory task) was expected, a broader, bimodal distribution with the largest peak at 0 ms was observed. The peak provides direct evidence for a grouping strategy early in practice. The bimodality is consistent with a limited-capacity central process that is dominated by one of the tasks at any given time (Ruthruff, Pashler et al., 2003; , see also Tombu & Jolicoeur, 2002). Thus, early in practice, performance appears to be constrained by executive processes that prevented the tasks from being performed independently.

In contrast, the expected and observed distributions were highly similar on the sixteenth session. Both the peak and the breadth of distribution closely match those generated from the single-task data, indicating that the dual-task costs were shared evenly between the two tasks. For bottleneck accounts to accommodate such a finding, one must assume that different tasks occupy the bottleneck first across trials, which would generate a bimodal distribution of IRIs. Clearly, there is no evidence of any such effect. Thus, these data are consistent with the mean RTs in arguing that a persistent bottleneck is not responsible for the dual-task costs. This finding is particularly impressive given the fact that the mean RTs for both tasks were over 350 ms, indicating that task operations remained non-trivial and yet could be performed in parallel. This results is the strongest evidence yet that central capacity does not provide strong limitations on dual-task performance after moderate practice and that central operations can proceed in parallel

Accuracy. The proportions of correct responses were submitted to a two-way ANOVA with trial-type and session as factors, as in Experiment 2a. For the visual task, there were no significant effects. For the auditory task, there was a significant effect of session [$F(15,45) = 2.74$; $p < .01$] and a significant Session x Trial-type interaction [$F(30,90) = 1.74$; $p < .05$]. These findings indicated that participants were more accurate on the task with practice and that this improvement was greatest for dual-task trials.

General Discussion

We identified three outstanding questions regarding the nature of dual-task costs: Are dual-task costs due to limitations in central capacity? Must central operations be performed in a serial fashion? Is central interference content-dependent? We consider each of these questions and how the present data answer them in turn.

Are dual-task costs due to limitations in central capacity? Individuals in the Standard groups were able to perform the two tasks simultaneously with minimal interference, indicating that central capacity does not always impose limits on dual-task performance, at least after moderate practice. A few recent studies (e.g., Hazeltine et al., 2002; Schumacher et al., 2001) have provided evidence for near-perfect time-sharing between speeded tasks after practice. However, these studies tested a very narrow range of conditions (e.g., where both tasks are relatively easy and one involves a spatial mapping of visual stimuli onto manual responses).

The present study extended these findings in several important ways. First, participants who performed tasks with the standard modality pairings (auditory-vocal and visual-manual) were able to achieve small dual-task costs even when neither task involved mapping the spatial properties of the stimuli to spatially differentiated responses. Importantly, analyses of SOA effects and inter-response intervals indicate that the small residual interference cannot be attributed to a bottleneck. Second, the present study extends the finding of near-perfect time-sharing to more difficult tasks. The visual-manual task, in particular, was considerably more difficult (and took more time to complete) than those used in the previous studies. Although dual-task costs were not completely eliminated, the 16 training sessions produced a 16-fold reduction in the difference between single and dual-task RT. Experiment 3 indicates that near-perfect time-sharing can occur even when both tasks require more than 350 ms to complete. Given that the longer RTs likely reflect increases in the duration of response selection, the finding provides further support for the proposal that response selection can proceed in parallel for two distinct and practiced tasks. When mean RT is short (as in previous demonstrations of virtually perfect time-sharing) the bottleneck stages are likely to also be short. Consequently, if there is even a modest asynchrony between tasks in the demand for the bottleneck stages, then these stages might rarely be called for at the same time. Thus, in the previous studies, it is possible that a processing bottleneck still existed but was “latent”, producing little or no dual-task cost (see Ruthruff, Johnston et al., 2003). Because mean RT was relatively long in the present experiment, the plausibility of a latent bottleneck is greatly reduced.

Must central operations be performed in a serial fashion? Although dual-task costs were virtually eliminated in some conditions, they remained robust in other conditions. To determine whether these costs resulted from serial central processing, we examined the distribution of inter-response intervals and manipulated the timing of the two tasks (i.e., by varying the SOA). The distribution of inter-response intervals provides an effective means of investigating this question because it can provide evidence of serial processing even when participants switch the order of task operations from trial to trial. No such evidence for serial processing was found after moderate practice, however, despite the fact that dual-task costs remained robust.

In Experiment 2b, we used an SOA manipulation to test whether competition for a central processing bottleneck could explain the residual dual-task cost. If a bottleneck were the cause of the costs, then these costs should have been affected by the SOA (i.e., should have increased when the demand for the central stages of the two tasks were better synchronized). However, for the Standard group, there was no effect of SOA (for a related finding, see Hazeltine et al., 2002). This finding supports the conclusion that the small residual dual-task costs for these individuals did not result from competition for a specific single-channel component, such as a response selector, with a short temporal duration. A more complicated pattern was observed for the Nonstandard group, but again the evidence suggested that the interference was not strictly limited to competition for a single, short-duration processing component. It is possible that a bottleneck persists in the present tasks after 16 sessions of practice, but it does not appear to be a significant source of the dual-task interference or the locus of the modality-pairing effects.

<u>Is central interference content-dependent?</u> The magnitude of the dual-task costs was strongly affected by the pairing of input and output modalities. Whereas the Standard modality-pairing condition of the present study produced near-perfect time sharing after only 8 sessions (or less), the Nonstandard modality pairing condition still produced over 200 ms of interference at this point in both Experiment 1A and Experiment 2A. In general, it took roughly twice as much practice for the group with Nonstandard modality pairings to achieve the same level of dual-task performance as the group with Standard pairings.

Because difference in the costs does not appear to be related to peripheral interference, we instead attribute it to content-dependent interference (see below for more details). Complicating this interpretation is the fact that modality pairings also modulated single-task RT. However, these differences in single-task RT, by themselves, cannot account for the observed difference in dual-task costs. In Experiment 3, we compensated for the greater inherent ease of the Standard pairings by making the auditory-vocal task much more difficult (we added irrelevant stimulus variation, and used an arbitrary S-R mapping instead of a compatible one). This difficulty manipulation served to roughly equate single-task RTs for the two groups. Nevertheless, dual-task costs after practice were only half as large for the Hard-Standard group compared to the Nonstandard group. This finding not only has important theoretical implications (discussed in more detail below), but also may have important practical implications as well. It suggests that complex systems should be designed so that, whenever feasible, visual inputs go with manual responses and auditory inputs go with vocal responses.

The present data suggest that central operations are not generic, drawing upon a monolithic central resource or “mental energy”. Accordingly, they challenge the dominant theories of dual-task performance, which imply that central operations are generic and that responding manually to visual stimuli is not fundamentally different than responding vocally to visual stimuli.

To the best of our knowledge, this is the first study to demonstrate that the pairings of stimulus and response modalities strongly modulate the magnitude of dual-task costs, where such effects cannot be attributed to task difficulty or competition for a peripheral resource. Indeed, a striking feature of the present experiments (especially Experiment 3) is that they demonstrate decrements in dual-task performance above and beyond what can be explained by the decrements in single-task performance. In other words, modality-pairings appear to directly affect the ability to perform multiple tasks at the same time. This finding provides direct evidence for content-dependent interference.

Previous evidence for content-dependent central operations

Although no previous studies have rigorously documented modality-pairing effects on dual-task performance, several previous studies have reported modality-pairing effects consistent with the present results. Many single-task experiments (e.g., Baldo, Shimamura, & Prinzmetal, 1998; Teichner & Krebs, 1974; Virzi & Egeth, 1985; Wang & Proctor, 1996) have suggested that certain types of information may be transferred from particular input modalities to particular output modalities more readily than other types of information. Specifically, it has been argued that specialized pathways exist between brain structures recruited for visual processing and those recruited for programming manual responses (see Milner & Goodale, 1995). Further evidence that the strength of the linkages between stimuli and responses is not equivalent for all combinations of stimuli and responses comes from single-task studies in which the magnitude of compatibility effects is contingent on the modality of the irrelevant stimulus information and the modality of the response (see above, Hazeltine, Bunge, Scanlon, & Gabrieli, 2003; Virzi & Egeth, 1985; Wang & Proctor, 1996). For example, Virzi and Egeth (1985) demonstrated that the magnitude of compatibility effects produced by irrelevant stimulus information depends on the modality of the responses, with visually-presented words producing greater compatibility effects when the responses are vocal compared to when they are manual. Similarly, visually-presented arrows produce greater compatibility effects when the responses are manual compared to when they are vocal.

In addition to these single-task studies, a few dual-task studies have reported results consistent with the hypothesis that modality pairings influence dual-task performance. Shaffer (1975), for example, found that dual-task performance was much more efficient when typing text (a visual-manual task) was combined with shadowing (auditory-vocal) than when auditory typing (auditory-manual) was combined with reading aloud (visual-vocal). In other words, performance was better with the standard modality-pairings than with nonstandard modality-pairings. These effects are certainly consistent with an effect of modality-pairings on dual-task performance, but they could instead be due to the fact that the single subject in this study a much more practice with the standard pairing. Other studies that have previously reported an effect of modality-pairings on dual-task performance (e.g., Greenwald & Shulman, 1973; Levy & Pashler, 2001) have not attempted to equate task difficulty. It is important to note, therefore, that we were careful to ensure that element-level S-R compatibility was as high for the Nonstandard modality pairings compared to the Standard modality pairings. The visual task used for the Nonstandard modality-pairings involved a highly compatible mapping of stimulus categories (bug, food, tree) onto vocal responses (“bug”, “food”, “tree”) whereas the visual task for the Standard pairing used an arbitrary mapping of these stimulus categories onto response keys. Similarly, the auditory task used for the Nonstandard pairings involved a compatible mapping of stimuli (low, medium, and high-pitched tones) onto response keys, whereas the auditory task in Experiment 3 involved a completely arbitrary mapping of three sounds (with irrelevant variation in pitch) onto three nonsensical vocal responses.

Implications for theories of dual-task performance

None of the dominant models of dual-task performance predicted the pattern of results obtained in our experiments (see Table 1). Several factors previously found to influence dual-task performance, including practice, the complexity of S-R translation, conflict between perceptual processes, and conflict between motor processes, are insufficient on their own to explain dramatic differences in dual-task costs. Persistent components of the dual-task costs cannot be attributed to conservative strategies, central bottlenecks, generic resources, or peripheral interference, because these constructs do not provide a cogent explanation for the differences between the Standard and Nonstandard modality-pairings.

While it is unreasonable to expect a theory of dual-task interference to explain performance for every possible task-pairing, our experiments are clearly within the purview of these models. All of the tasks were three-alternative forced-choice tasks, with either button-press manual responses or monosyllabic vocal responses, as is typical in dual-task experiments. Moreover, it is unlikely that they tapped specific real-world skills (e.g., musical or typing ability) to a greater extent than the tasks used in other experiments. Nonetheless, Nonstandard mappings produced dual-task costs that were much larger (at times by hundreds of milliseconds) than Standard mappings. Therefore, these findings suggest a serious shortcoming shared by the major existing accounts of dual-task performance.

This is not to say that the existing models cannot be modified to account for the present findings. However, the candidate modifications are non-trivial and, in each case, alter core tenets of the theories. We describe three possible theoretical approaches towards accommodating the observed pattern of results (automatic activation, multiple resources, and crosstalk) and explain how they alter existing assumptions about dual-task limitations.

Automatic activation. Previous research has demonstrated that response priming can occur from Task 2 to Task 1 in the PRP paradigm, suggesting that the response for Task 2 can be determined even while response selection for Task 1 is still taking place (e.g., Hommel, 1998; Logan & Schulkind, 2000). One way for the bottleneck model to allow for such findings is to assume that there is a direct route between the stimuli and responses that bypasses the central bottleneck (Hommel, 1998; Lien & Proctor, 2002). The activation of the Task 2 response provided by the direct route is (at least initially) insufficient to complete response selection, so the PRP effect remains. However, the activation for Task 2 is sufficient to produce a compatibility effect on Task 1. Along these lines, Kornblum and colleagues (1990; see also Kornblum & Lee, 1995) have proposed that compatibility effects between stimuli and responses result from the automatic activation of the responses by the stimuli via a direct route that is based on the dimensional overlap between the two.

Automatic activation may play an increasingly critical role during dual-task performance as practice continues. With repeated exposure to the S-R pairings, automatic activation might increase in strength until it becomes so strong that participants can base their responses entirely on the automatic activation and thereby avoid conflict for limited central capacity (e.g., Shiffrin & Schneider, 1977). That is, automatic activation may become more effective (see Logan, 1988), and central mechanisms no longer provide the primary means of selecting the appropriate response (Ruthruff, Johnston et al., 2001). Indeed, some researchers have pointed to reductions in dual-task costs as demonstrating that "automatized" tasks no longer demand central capacity (e.g., Bahrick & Shelly, 1958; Logan, 1979) or require attention (e.g., Kahneman & Treisman, 1984; Spelke, Hirst, & Neisser, 1976).[6]

If dual-task costs are reduced because the automatic activation of the appropriate response diminishes the role of bottleneck processes, then the effects of modality pairings may reflect differences in automatic activation between particular combinations of stimulus and response modalities. That is, the "direct route" between stimuli and responses may be easier to establish between, for example, visual stimuli and manual responses than between visual stimuli and vocal responses. The advantage for visual-manual S-R pairs might stem from the fact that we often use vision to guide our manual movements. With Nonstandard pairings, activation from the direct route is insufficient to complete response selection and limited-capacity central processes must be tapped. In this way, the difference in dual-task costs between Standard and Nonstandard pairings would be greater than the difference in single-task RTs, as observed in Experiments 2a and 3.

Multiple resources. In the 1970s and early 1980s, multiple resource theories were popular accounts of dual-task performance (Allport et al., 1972; McLeod & Posner, 1984; Navon & Gopher, 1979b; Wickens, 1980, 1984; Wickens & Liu, 1988; Wickens et al., 1983). According to multiple-resource theory, specific processes draw on distinct pools of resources. Limitations in dual-task performance result from competition between tasks for these specific resources. Limitations can be avoided if the tasks being concurrently performed do not draw heavily on the same specific resources.

Perhaps the most complete version of multiple resource theory has been articulated by Wickens and colleagues. To test the proposal that dual-task performance is determined by the extent to which the two tasks draw on the same set of processing resources, Wickens (1983) had experimental participants perform a visuomotor tracking task with a concurrent discrete task that involved either visual or auditory stimuli and required either manual or vocal responses. When the secondary task used visual stimuli or required manual responses, interferences was greater than when the alternative modality was employed. This finding is consistent with a model of dual-task performance in which the two tasks compete for resources that are specific to particular stimulus and response modalities.

A second important feature of the Wickens account in relation to the present findings is the proposal that dual-task costs are largely determined by the central codes mediating the S-R mappings. The central codes can affect dual-task performance in two ways (Wickens, 1980, 1984; see also, Wickens & Liu, 1988; Wickens et al., 1983). First, similarity between the central codes for the two tasks can be a source of overlap, meaning that the two tasks compete to a greater degree for a common resource, thereby increasing dual-task costs. Second, the codes interact with the stimuli and responses of the same task to determine the need for resources. According to the model, spatial codes are inherently more compatible with visual inputs and manual outputs than auditory inputs and vocal outputs. Verbal codes are assumed to show the opposite compatibility relationship. These relationships are termed “S-C-R [stimulus-code-response] compatibility” to underscore the importance of the mediating codes in driving the demand on specific resource pools.

Multiple resource theory anticipates many of the key features of the content-dependent account and can explain many of the same phenomena. First, the model posits that the task-dependent sources of interference can be central rather than resulting from competition for peripheral mechanisms associated with stimuli or with responses. Second, the theory makes clear that the magnitude of dual-task costs is dependent on the relationship between the two tasks. In proposing that certain configurations of stimulus, code, and response (S-C-R) modalities afford processing advantages that can result in reduced dual-task costs, Wickens et al. (1983) indicate that interactions between simultaneously performed tasks depend on the S-C-R ensemble rather than interference that it is restricted to one of these three levels in isolation. Such a framework is clearly applicable to the present results, in which the two stimuli and the two responses are identical for the Standard and Nonstandard groups, but the dual-task costs depend on how the stimuli and responses are paired within the two tasks. In fact, the present data provide the strongest evidence to date that dual-task costs cannot be predicted by considering of stimulus, central code, and response overlap separately.

However, the Wickens et al. (1983) multiple resources with code-compatibility hypothesis does not offer a complete explanation for the present data. Critically, they did not independently manipulate code compatibility, modality-pairings, input and output conflicts, and S-R compatibility. Such conditions make it difficult to attribute changes in dual-task costs to a specific source. Moreover, none of our tasks involved the preferred S-C-R combinations (Visual-Spatial-Manual or Auditory-Verbal-Vocal) as defined by Wickens et al. Thus, the code-compatibility hypothesis in its present form does not account for the differences between input and output modality-pairings observed in the present study. In this sense, a novel contribution of the present results is the demonstration that modality-pairings affect dual-task performance independently of central code compatibility.

It is possible that Wickens' taxonomy might be modified to accommodate the results from these experiments, but this approach does not seem very parsimonious and also may amount to a restatement of the crosstalk hypothesis (see below). Multiple-resource theory fell out of vogue in the 1980s, in part because researchers came to question its predictive value: claiming that two tasks shared more or less resources was indiscernible from saying that their pairing produces high dual-task costs (Navon, 1984). It is unclear whether the explanation does more than restate the data, especially when accounts such as the bottleneck model, are able to account for huge bodies of data without introducing ill-defined concepts.

Nonetheless, this criticism does not undermine the possibility that two seemingly distinct tasks requiring a specific process or resource that might not be shared by another pair of distinct tasks. Indeed, this notion is widely embraced by contemporary theorists. For example, dual-task costs are often much larger in PRP experiments when both tasks require manual responses, even when they involve distinct hands (Pashler, 1990; Ruthruff, Johnston et al., 2001). To account for this phenomenon, researchers have supposed that the control of two hands is governed by a common set of processes specific to manual responses (e.g., Meyer & Kieras, 1997b; Pashler, 1990). In effect, the manual controller adds a specific resource to these models, allowing them to accommodate task-specific interactions. One might argue that the manual controller is a peripheral resource, and, as such, differs from the sort of limitation described by multiple resource theory. However, recent evidence suggests that intermanual interactions are best characterized as central in that they are sensitive to both stimulus and response properties (Diedrichsen et al., 2001; Diedrichsen, Ivry, Hazeltine, Kennerley, & Cohen, 2003; Hazeltine, in press; Mechsner, Kerzel, Knoblich, & Prinz, 2001).

To remedy the predictive-value problem, converging evidence for a specific resource must be sought, so that, as with the manual-processor, the hypothesized resource limitation is not a post-hoc explanation for large dual-task costs. Along these lines, some evidence (McLeod & Posner, 1984) suggests that a specialized pathway might exist between brain structures recruited for auditory perception and speech production.[7] This pathway may be required during tasks involving either auditory stimuli or vocal responses, even though the auditory stimuli in the present experiments were tones rather words. With Nonstandard pairs, this pathway is required for both tasks, resulting in conflict, whereas with the Standard pairs, only the auditory-vocal tasks will occupy the pathway. Thus, the Nonstandard pairs would likely lead to greater dual-task interference.

This sort of account would fit naturally with contention-scheduling models like EPIC, which already propose that dual-task costs emerge from executive control processes imposing delays on task operations. To accommodate modality pairing effects, researchers might modify EPIC by adding a sound buffer module that is required for identifying the tones and producing the vocal responses and by making some assumptions about task-switch costs being larger when the same module most be used by two distinct tasks. Note, however, that while this change fits easily within the framework provided by EPIC, it does represent a major departure from the tenet that central operations are not subject to capacity limitations. A module used to process both inputs and outputs is by definition central, and it would have to be added to account for the dual-task costs observed in the present study.

Such a module could also be added to the response selection bottleneck and capacity sharing accounts. As with EPIC, however, adding such a component would alter fundamental tenets of the models. In particular, these models would become indistinguishable from multiple-resource theories. The essential notion that dual-task performance is limited by the availability of generic resources that are shared by all central operations would be abandoned.

Crosstalk. As pointed out by other theorists, dual-task costs may stem not only from resource competition but also from crosstalk between the tasks (Hirst & Kalmar, 1987; Logan & Gordon, 2001; Navon, 1984, 1985; Navon & Miller, 1987, 2002). The notion of crosstalk assumes that input intended for a subcomponent of one task is incorrectly transmitted to a subcomponent associated with the other task. Thus, the cost to performance does not reflect a limit in capacity, but instead reflects unwanted interactions between two processes.

Crosstalk is typically inferred when a feature of one task is manifest in the response for the other task. This sort of interference is perhaps best described in the motor control literature, where crosstalk is observed between simultaneous movements of the two hands. Under some conditions, when the two hands move along trajectories that are asymmetrical with respect to the body’s midline, the trajectories are distorted so that they are more similar to each other compared to when the trajectories are produced separately by the hands at distinct times (e.g., Spijkers, Heuer, Steglich, & Kleinsorge, 2000). The phenomenon can be very powerful, such as when one tries to trace an imaginary circle with one hand and an imaginary square with the other hand at the same time. However, crosstalk effects can also be observed in RT, especially during button-pressing tasks (e.g., Lien & Proctor, 2000; Navon & Miller, 1987; Spijkers et al., 2000). In such cases, the crosstalk is assumed to relate to peripheral codes, such as motor command for the two hands (e.g., Franz et al., 1991).

To explain the present results, the crosstalk must involve central codes, because the stimuli and responses were essentially identical for the Standard and Nonstandard pairings. In some cases, researchers have proposed crosstalk between central codes to explain dual-task costs, but this has been in cases where there is dimensional overlap between stimulus or response features on the two tasks (e.g., Navon & Miller, 1987). This was not the case in the present experiments, where we intentionally created tasks with no conceptual relationship. The form of crosstalk we propose differs from previous accounts in that it requires no conceptual relationship between the two tasks, just as cross-talk between phone lines can reduce intelligibility even when the spoken words do not overlap in meaning.

This type of crosstalk does not involve the transmission of particular features of one task to the response of the other task. For the Nonstandard pairings, there is no evidence of some aspect of the auditory-manual task being transmitted to the visual-vocal task, or vice versa. Thus, the crosstalk we propose may be thought of as the transmission of noise rather than of specific stimulus or response features. Nonetheless, it may be based on overlap between the two tasks. With the Nonstandard pairings, both tasks involved sounds, whereas with the Standard pairs, the sounds were restricted to the auditory-vocal task. The overlap in terms of sonic information may have provided a medium for crosstalk. That is, central representations of sound information activated by the stimulus may interfere with central representations of the sound information activated by the response. This problem may be minimal for the Standard pairings, because the auditory stimuli and vocal responses are perfectly correlated; each auditory stimulus always requires the same vocal response. The common medium of sound is an example of a specific linkage between auditory and vocal modalities whose hypothesized coactivation creates problems when crossed modality pairings are used. In this case, the application of abstract S-R rules is compromised by the coactivation. It is also conceivable that specific S-R rules have emerged for auditory-vocal mappings that take advantage of opportunities created by the shared representation.

Crosstalk and feature binding. One possible form of crosstalk consistent with the present data relates to the proposal that response selection is performed by binding stimulus properties to response features (e.g., Koch & Prinz, 2002; Logan & Gordon, 2001). Dual-task performance could present difficulties for the binding process because multiple stimulus features and multiple response features are activated simultaneously. Thus, rather than simply binding one stimulus with one response, binding must be performed so that the appropriate stimulus is bound to the appropriate response. Performing the binding operation in a serial fashion can alleviate this problem (see Logan & Gordon, 2001), but the present data suggest that the serial strategy is abandoned after sufficient practice. Therefore, as practice accrues, the binding operation must contend with multiple stimuli and responses at the same time.

In this respect, the binding account is very similar to the crosstalk account. For example, with the Standard pairings, the binding mechanism simply binds the two sources of sound information (e.g., the tone and the activated vocal response) together without having to differentiate the two. With the Nonstandard pairings, the binding mechanism must discriminate between the activation of the tone and the activation of the vocal response so that the tone can be bound with the manual response and the vocal response can be bound with the visual stimulus. This additional operation can be thought of as being equivalent to resolving crosstalk between the two tasks.

Why should there be greater overlap between visual stimuli and manual responses or auditory stimuli and vocal responses than between visual stimuli and vocal responses or auditory stimuli and manual responses? More theoretical work must be brought to bear on this question before a satisfactory answer can be obtained. At present, we suggest that the theory of event coding (TEC) provides a promising framework, because it holds that sensory and motor processes share a common representational medium (Hommel, Müsseler, Aschersleben, & Prinz, 2001; see also Mechsner et al., 2001; Prinz, 1990). According to the TEC, perceptions and planned actions are encoded as integrated representations termed event-codes. These event codes are composed of features (*e.g.*, red or left) that represent aspects of distal events and thus are applicable to both incoming stimuli and the environmental consequences of produced responses. For example, if a particular tone is consistently produced when a response is made, then features of that tone will become part of the representation of the response. These same features will serve as part of the stimulus representation if the tone is used to signal movements.

This theory has been applied to both single-task and dual-task studies to account for a variety of within- and between-task compatibility effects (e.g., Elsner & Hommel, 2001;

Hommel, 1993, 1997; Koch & Prinz, 2002; Stoet & Hommel, 1999). If one assumes that vocal responses are partly selected in terms of their sounds and manual responses are selected in terms of their visuospatial locations (i.e., the distal events associated with the actions), then this similarity might interfere with binding, because similar features would be associated with the different event files. Such an advantage might not be apparent under single-task conditions because the binding demands in such cases are trivial.

Crosstalk or competition? The crosstalk account, because it relates to overlap between the two tasks, is conceptually similar to the multiple resource account. Distinguishing between competition for limited, specific resource and crosstalk (or noise) between concurrently activated central representations presents an important challenge for dual-task research. At present, we propose that crosstalk may provide a more parsimonious explanation for the observed patterns of RTs in the present experiments for four reasons.

First, as noted above, the proposal that competition for a specific resource causes the persistent dual-task cost is insufficiently developed to account for the present pattern of dual-task costs. None of the existing formulations of multiple resource theory specify the resource that is required to a greater degree by both tasks for the Nonstandard pairings than for the Standard pairings, particularly when the RTs for the two pairings are equivalent. As mentioned above, one might propose that the present results reflect competition for an auditory buffer or some similar cognitive processor that is required for both classification of the tone and production of the vocal response. However, we note that as an explanation for the different dual-task costs for the two modality pairings it is incomplete in the sense that it is not obvious why this limitation should be more apparent when the auditory stimulus and vocal response belong to different tasks than when they belong to the same task. It seems likely that once the hypothesis is modified to accommodate this result, it would become isomorphic with a crosstalk account and the usefulness of the resource metaphor would be considerably diminished.

Second, the Nonstandard group had to read a word and say a semantically-related word as part of the same task. In contrast, the Standard group had to read a word (for the visual-manual task) and say an unrelated word (for the auditory-vocal task) as part of different tasks. The latter situation, where both tasks had a linguistic component, would seem much more likely to compete for verbal resources and therefore increase dual-task costs. Nonetheless the Standard group actually produced much less dual-task interference than the Nonstandard group. Thus, while it is possible for a multiple resource theory to account for this result, competition does not provide an obvious metaphor for the observed interference.

Third, it is unclear why the tasks should continue to place demands on limited-capacity resources after several sessions of practice. Most accounts of practice effects assume that changes in performance result from a transition to an automatic mode of processing that does not depend on the availability of central resources (e.g., Logan, 1988; Schneider & Shiffrin, 1977). While this transition appears to be graded rather than abrupt (Logan & Etherton, 1994), the present versions of resource theory do not address how practice changes resource demands and how these changes might be affected by modality pairings. Of course, the absence of theoretical work addressing the relationship between practice and resource demands does not preclude the possibility that the present results reflect changes in the demand for particular resources. However, given that single-task RTs are not good predictors of dual-task costs and that it is unclear how changing the mode of responding should affect central resources, limited resources models do not appear to offer a very succinct account of the present data. In contrast, crosstalk might reasonably persist for practiced tasks with short RTs.

Finally, the notion of crosstalk fits well with some neuropsychological and neuroimaging studies of dual-task performance. Holtzman and Gazzaniga (1985) showed that resection of the corpus callosum, the bundle of white fibers connecting the two cerebral hemispheres, improved dual-task performance when the two hemispheres had to encode conflicting stimulus information. Although the PRP effect appears to be preserved in split-brain individuals (Ivry & Hazeltine, 2000; Pashler et al., 1994), there is some indication that the PRP effect in this case is strategic (Ivry, Franz, Kingstone, & Johnston, 1998). There is no obvious reason why resource competition would be eliminated by splitting the brain. In contrast, that crosstalk between brain areas in different hemispheres should be reduced after callosotomy is highly plausible. Moreover, the regions of the brain most often associated with mediating dual-task performance are within prefrontal cortex (e.g., D'Esposito et al., 1995; Herath, Klingberg, Young, Amunts, & Roland, 2001; Koechlin, Gianpaolo, Pietrini, Panzer, & Grafman, 1999). Contemporary theorization about the role of the frontal lobes has focused on conflict resolution (i.e., dealing with crosstalk), whereas response selection for practiced stimuli is assumed to take place in posterior regions of the brain (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Bunge, Hazeltine, Scanlon, Rosen, & Gabrieli, 2002; Desimone & Duncan, 1995; Hazeltine, Poldrack, & Gabrieli, 2000; Konishi et al., 1999; Miller & Cohen, 2001).

For these reasons, we tentatively propose that crosstalk between the two tasks currently provides the best explanation for the differences in dual-task performance for the Standard and Nonstandard pairings (see also, Navon, 1984). Previous explanations based on crosstalk between tasks have been restricted to conditions in which the two tasks are conceptually related (e.g., Hirst & Kalmar, 1987; Hommel, 1998; Logan & Gordon, 2001; Logan & Schulkind, 2000; Navon & Miller, 1987). In contrast, we propose that crosstalk can occur even when the two tasks involve non-overlapping stimulus categories and non-overlapping response categories. Thus, the crosstalk responsible for the persistent dual-task costs in the present experiments differs from previous demonstrations of crosstalk in that it is not dependent on the particular combinations of responses (or stimuli) required by the two tasks on a given trial. Instead, the crosstalk acts generically, so that the two tasks interact regardless of the combination of particular S-R associations activated at a given time.

A general framework. Although content-dependent interference can be accounted for in a variety of ways, it is possible to extract some general features of an account for the data that are theoretically neutral regarding the underlying mechanisms. Early in practice, limitations in central processing constrain dual-task performance, as evidenced by the robust dual-task costs for all task pairings during the first few sessions. Thus, tasks with larger single-task RTs produce larger dual-task costs (Figures 11 and 13). This central limitation might be strategic or based on a bottleneck, but there is some evidence in the distribution IRIs that the central operations are initially performed in a serial fashion, although capacity-sharing cannot be ruled out.

As training progresses, it appears that central operations for the tasks are performed largely in parallel. However, while this processing approach is more efficient, it means that byproducts (e.g., activation) of the processing of both tasks are simultaneously present, making performance of the two tasks subject to content-dependent interference. That is, content-dependent interference persists long after evidence for serial response selection has greatly diminished. In short, content-dependent component of dual-task cost is more difficult to eliminate than the generic central limitations that dominate early performance. Thus, after several sessions of practice, dual-task costs are not necessarily larger for tasks with longer single-task RTs. At this phase of training, the costs are predominantly determined by direct interactions between the central operations, which vary according to the particular operations associated with the two tasks.

Summary

The present results answer three important questions about the nature of dual-task costs. First, the findings extend previous demonstrations of near-perfect time-sharing to more difficult tasks, demonstrating that central processing is not always limited in capacity. Second, the data indicate that the ability to select two responses in parallel may be achieved across a variety of conditions. However, although the responses appeared to be selected in parallel, dual-task costs remained robust after several sessions of practice with certain task pairings. That is, although we found no evidence for a central processing bottleneck, we did find evidence for some other type of central interference. Under these conditions, incentives, avoiding peripheral conflicts, and moderate amounts of practice were not sufficient to eliminate dual-task costs. This finding relates to the third question by demonstrating that dual-task interference is content-dependent. That is, dual-task costs are contingent on the specific S-R pairings across the two tasks. The answers to these three questions provide strong constraints for theories of dual-task performance.

We chose tasks that, to the best of our knowledge, did not involve specialized behaviors or unusually difficult S-R associations. Rather, our experiments simply used modality-pairings that were typical within the dual-task literature. Nonetheless, to account for these findings, the dominant models of dual-task performance must be modified in ways that alter some of their fundamental assumptions. Bottleneck and capacity-sharing models must add content-specific central resources, or allow for a direct route that bypasses central limitations and is sufficient for response selection after moderate practice. Importantly, this direct route must be more easily adapted for some modality-pairings than others. EPIC and contention-scheduling models must add capacity-limited modules that perform central operations, or assume that executive control processes are sufficiently complex to assess the particular S-R associations when starting and stopping task operations. In short, it must be assumed that central operations are not performed generically but instead are executed in way that depends on their content.

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Footnotes

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1. A related possibility is that central processes do not share a common component, but are nevertheless not allowed to operate parallel. For instance, some executive mechanism might schedule tasks in serial to reduce the potentially harmful effects of cross-talk.
2. Task difficulty is a complex concept involving multiple factors. For the present purposes we simply use single-task RT as the sole measure of task difficulty, because it can be easily measured and is clearly independent from the other factors whose effects on dual-task costs we are evaluating.
3. Target RTs were established separately for each task and for each participant. The target RT was equal to the participant’s fastest mean RT for that task in any of the previous blocks (including those from previous sessions). Participants earned a bonus following each block (whether it was single or mixed) based on how their mean RTs (always based on single-task trials only) for the two tasks compared to the target RTs. No bonus was awarded for a task if the mean RT exceeded the target RT by 100 ms. A bonus of 10 cents was awarded if the RT exceeded the target RT by 50 to 99 ms. A bonus of 25 cents was awarded if the RT exceeded the target RT by 0-49 ms. A bonus of 50 cents was awarded if the RT was less than the target RT (in which case the target RT was then adjusted to this new, lower value for the next block). In addition, 1 cent was added for each correct response and 5 cents were deducted for each incorrect response within the block.
4. The responses were not exactly the same as in Experiment 1a. If participants had said “bug”, “food”, and “tree” in response to the tones, this task might have interfered with the visual bug/food/tree classification. However, the responses we did use (“one”, “two”, and “three”) had the same number of syllables.
5. For analyses comparing RTs across the two groups, it is equally valid to compare the two manual tasks and the two vocal tasks. Although admittedly somewhat arbitrary, we have chosen the stimulus-based categorization because it minimizes RT differences between the two tasks. Note, however, that our main conclusions do not depend on whether the analyses sort tasks by stimuli or by responses.
6. With regard to the concept of automaticity, we make one additional point based on the present findings. As noted by other researchers, various operational definitions of automaticity have been proposed (see Kornblum et al., 1990; Norman & Shallice, 1986). Some researchers have pointed to reductions in RT (e.g., Logan, 1988; Logan & Etherton, 1994), whereas others have focused on reductions in dual-task costs as demonstrating that “automatized” tasks no longer demand central resources (e.g., Bahrick & Shelly, 1958; Logan, 1979; Shiffrin & Schneider, 1977) or require attention (e.g., Kahneman & Treisman, 1984; Spelke et al., 1976). The Nonstandard tasks after 16 sessions of practice may fulfill many of these criteria but still produce dual-task costs. Therefore, shortening single-task RTs, reducing demands on resources, and diminishing dual-task costs may not serve as interchangeable indices of automaticity, given the complex pattern of single-task RTs and dual-task costs in the present experiments.

7. Although possibly related, the manner of linkage described by the motor theory of speech perception (Liberman & Mattingley, 1989) is not sufficient to account for the present results, given that the auditory-vocal task required participants to say a word in response to a nonword sound.

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Figure Captions

Figure 1. Mean RTs for the two tasks across the 8 sessions in Experiment 1a.

Figure 2. Dual-Task and mixing RT costs summed across the two tasks for the 8 sessions of Experiment 1a. Dual-task costs are computed by taking the difference between RTs on dual-task trials and single-task trials within the same mixed blocks. Mixing costs are computed by taking the difference between the RTs on single-task trials in the mixed blocks and single-task trials in the Pure blocks.

Figure 3. Mean RTs for the two tasks across the 8 sessions in Experiment 1b.

Figure 4. Dual-Task and mixing RT costs summed across the two tasks for the 8 sessions of Experiment 1b.

Figure 5. A. Manual RTs across the 16 sessions for the two task-pairing groups in Experiment 2. B. Vocal RTs across the 16 sessions for the two task-pairing groups in Experiment 2.

Figure 6. Dual-task costs for the 2 task-pairing groups across the 16 sessions in Experiment 2.

Figure 7. Mixing costs for the 2 task-pairing groups across the 16 sessions in Experiment 2.

Figure 8. Distribution of expected and observed IRIs for Experiment 2a. The observed distributions are indicated by the filled squares; the expected distributions, based on performance during the heterogeneous single-task trials are indicated by the open circles. Panel A depicts the distributions for the Standard (left) and Nonstandard (right) groups from the second session of the Experiment. Panel B depicts the distributions from the eighth session, and Panel C depicts the distributions from the sixteenth session.

Figure 9. RTs for the 2 stimulus onset asynchronies and the single-task trials in Experiment 2b.

Figure 10. Distribution of expected and observed IRIs for the two SOA conditions of Experiment 2b.

Figure 11A. RTs for the visual task across the 16 sessions for the participants in Experiment 3 (Hard-Standard) shown with the visual RTs for two task-pairing groups in Experiment 2. B. RTs for the vocal task across the 16 sessions for the participants in Experiment 3 (Hard-Standard) shown with the auditory RTs for two task-pairing groups in Experiment 2.

Figure 12 Dual-task costs for the participants in Experiment 3 (Hard-Standard) shown with dual-task costs for the 2 task-pairing groups across the 16 sessions in Experiment 2.

Figure 13. Mixing costs for the participants in Experiment 3 (Hard-Standard) shown with mixing costs for the 2 task-pairing groups across the 16 sessions in Experiment 2.

Figure 14. Distributions of expected and observed IRIs for Experiment 3.

Table 1. Predictions of three accounts of dual-task performance.

	Central Bottleneck	Limited Capacity	Strategic Control
Central limitations?	Yes	Yes	No
Serial processing?	Yes	No	No
Content-dependency?	No	No	No

Table 2. Reaction Times and Accuracies for Experiments 1a and 1b.

Reaction Time

Condition	Session							
Exp. 1a	1	2	3	4	5	6	7	8
Visual Single	600	586	551	539	518	506	503	495
Visual Het.	658	608	557	532	525	510	513	504
Visual Dual	808	759	629	603	598	591	583	576
Auditory Single	463	432	397	376	361	358	347	352
Auditory Het.	748	614	568	512	488	474	467	453
Auditory Dual	954	830	790	749	684	675	640	651

	Session							
Exp. 1b	9	10	11	12	13	14	15	16
Visual Single	498	469	460	449	446	442	452	448
Visual Het.	624	519	482	474	457	455	448	457
Visual Dual	857	679	590	545	518	504	478	478
Auditory Single	451	398	356	338	313	288	294	283
Auditory Het.	612	491	432	387	345	323	306	299
Auditory Dual	671	571	518	427	369	347	320	317

Accuracy

	Session							
Exp. 1a	1	2	3	4	5	6	7	8
Visual Single	.95	.97	.97	.97	.97	.96	.95	.94
Visual Het.	.94	.97	.95	.92	.98	.96	.99	.97
Visual Dual	.89	.95	.92	.93	.96	.96	.93	.93
Auditory Single	.95	.97	.96	.96	.96	.97	.97	.96
Auditory Het.	.96	.96	.96	.96	.96	.96	.95	.95
Auditory Dual	.91	.97	.97	.96	.97	.95	.97	.95

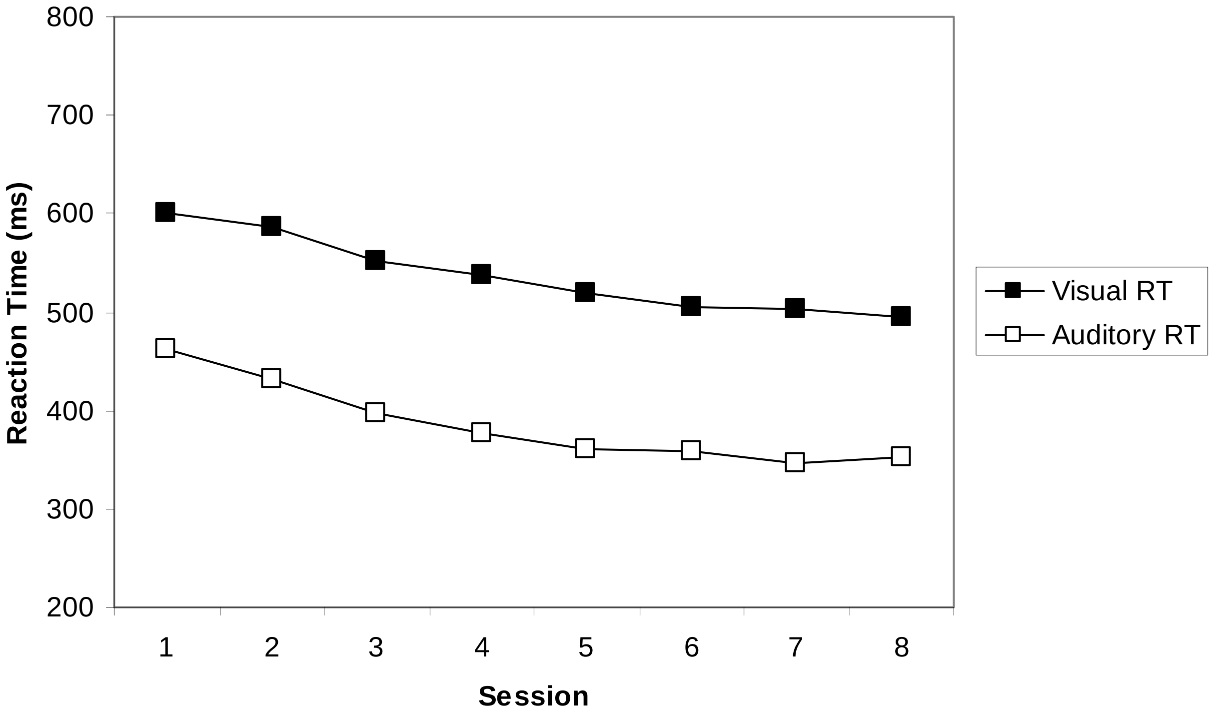
	Session							
Exp. 1b	9	10	11	12	13	14	15	16
Visual Single	.97	.96	.95	.94	.96	.95	.95	.95
Visual Het.	.97	.97	.96	.95	.95	.98	.94	.93
Visual Dual	.95	.97	.96	.97	.96	.96	.95	.94
Auditory Single	.92	.91	.93	.92	.96	.96	.96	.94
Auditory Het.	.91	.94	.91	.90	.97	.95	.96	.97
Auditory Dual	.90	.91	.92	.92	.97	.96	.96	.96

Table 3. Mean reaction times by trial-type for the sixteen sessions of Experiments 2a and 3.

		Session															
Condition	Trial-Type	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Exp. 2a Standard	Visual Single	513	500	483	473	468	460	464	459	468	455	456	447	448	453	457	438
	Visual Het.	567	531	481	481	476	462	451	458	462	462	462	460	461	438	445	437
	Visual Dual	817	663	591	529	512	491	489	503	484	490	491	498	472	467	474	448
Exp. 2a Non-Standard	Visual Single	632	587	534	544	526	513	488	490	489	474	473	468	461	452	438	434
	Visual Het.	781	632	550	553	525	499	475	473	473	462	458	451	447	432	425	414
	Visual Dual	1014	874	708	695	640	602	552	556	538	517	526	505	492	472	467	446
Exp. 3 Hard Standard	Visual Single	557	539	524	500	485	482	479	480	473	470	465	454	448	452	449	448
	Visual Het.	632	565	553	506	500	491	483	473	464	465	462	449	435	442	439	434
	Visual Dual	1127	938	889	706	643	595	544	508	492	481	477	462	452	451	443	441
Exp. 2a Standard	Auditory Single	546	455	417	397	361	343	324	299	301	288	284	288	273	273	276	272
	Auditory Het.	613	558	488	474	459	408	357	347	316	299	284	292	280	279	284	278
	Auditory Dual	793	698	625	614	550	487	420	379	365	313	299	305	292	298	298	292
Exp. 2a Non-Standard	Auditory Single	470	431	400	380	376	375	368	374	357	349	357	345	346	342	343	332
	Auditory Het.	643	537	481	458	434	416	404	407	400	379	398	371	374	359	366	352
	Auditory Dual	1002	779	716	686	616	549	534	531	514	486	503	458	437	428	429	388
Exp. 3 Hard Standard	Auditory Single	763	609	580	511	469	467	424	407	401	385	380	388	369	363	360	356
	Auditory Het.	862	685	632	563	496	487	447	430	412	389	389	388	374	377	372	372
	Auditory Dual	1067	934	827	685	631	584	543	497	472	449	446	434	416	408	403	405

Table 4. Mean accuracies by trial-type for the sixteen sessions of Experiments 2a and 3.

		Session															
Condition	Trial-Type	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Exp. 2a	Visual Single	.94	.98	.98	.97	.97	.96	.97	.96	.98	.97	.97	.98	.95	.96	.96	.96
Standard	Visual Het.	.94	.99	.98	.97	.97	.97	.98	.98	.98	.98	.96	.97	.95	.96	.97	.95
	Visual Dual	.94	.97	.96	.97	.97	.96	.97	.97	.97	.96	.96	.96	.96	.95	.96	.97
Exp. 2a	Visual Single	.96	.95	.97	.96	.96	.96	.97	.97	.96	.96	.96	.96	.95	.95	.93	.95
Non-Standard	Visual Het.	.95	.97	.95	.93	.93	.93	.93	.94	.98	.94	.94	.95	.95	.94	.96	.94
	Visual Dual	.93	.95	.97	.95	.96	.95	.96	.96	.98	.97	.98	.95	.96	.96	.94	.97
Exp. 3	Visual Single	.97	.98	.97	.96	.96	.96	.96	.96	.95	.96	.95	.95	.94	.95	.97	.96
Hard Standard	Visual Het.	.98	.99	.97	.98	.97	.97	.96	.95	.96	.96	.95	.95	.94	.96	.96	.96
	Visual Dual	.94	.96	.97	.98	.96	.96	.96	.97	.97	.96	.97	.96	.96	.96	.96	.97
Exp. 2a	Auditory Single	.96	.96	.97	.97	.96	.95	.96	.95	.96	.95	.94	.95	.94	.93	.91	.95
Standard	Auditory Het.	.96	.97	.97	.95	.94	.95	.95	.94	.97	.96	.95	.96	.95	.94	.95	.95
	Auditory Dual	.94	.95	.96	.96	.96	.96	.96	.96	.97	.97	.97	.96	.96	.96	.94	.97
Exp. 2a	Auditory Single	.92	.98	.98	.98	.98	.97	.98	.98	.98	.98	.98	.98	.95	.97	.97	.97
Non-Standard	Auditory Het.	.92	.98	.99	.98	.99	.97	.99	.99	.99	.98	.98	.98	.95	.96	.99	.97
	Auditory Dual	.91	.97	.95	.96	.97	.96	.97	.97	.98	.97	.97	.96	.95	.95	.98	.97
Exp. 3	Auditory Single	.94	.98	.98	.98	.98	.97	.96	.96	.95	.98	.98	.98	.98	.99	.96	.97
Hard Standard	Auditory Het.	.93	.99	.98	.99	.98	.97	.98	.96	.98	.98	.97	.99	.98	.97	.96	.98
	Auditory Dual	.89	.95	.95	.95	.95	.94	.96	.94	.97	.96	.97	.97	.98	.99	.98	.99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600
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400
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200
Reaction Time (ms)
1
2
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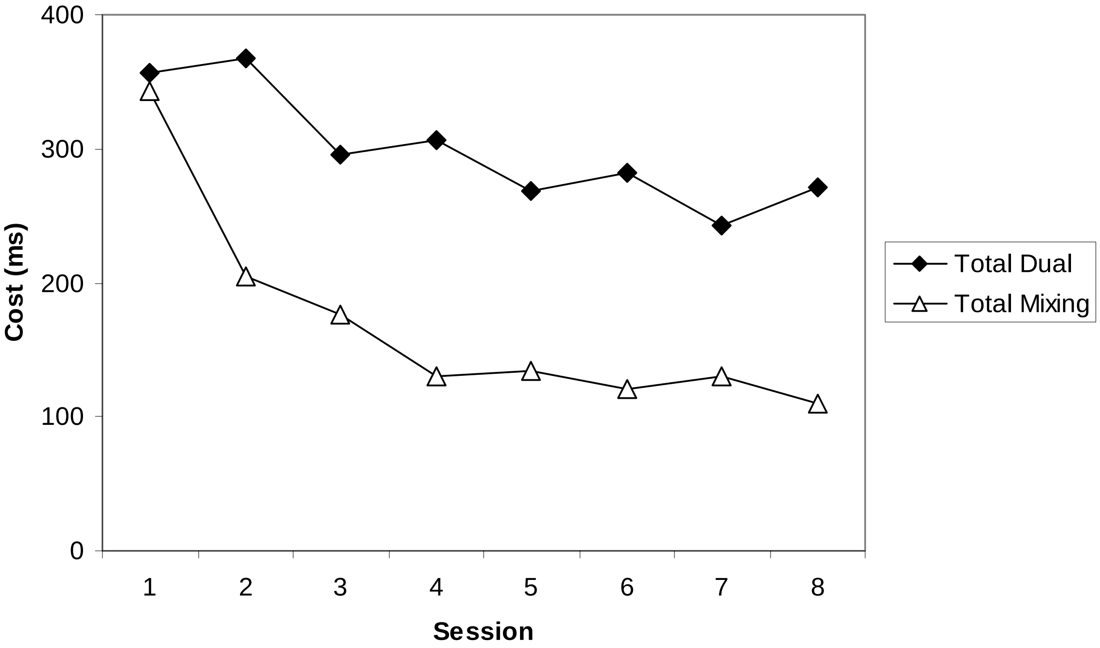
Session
■ Visual RT
□ Auditory RT

Figure 1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Cost (ms)
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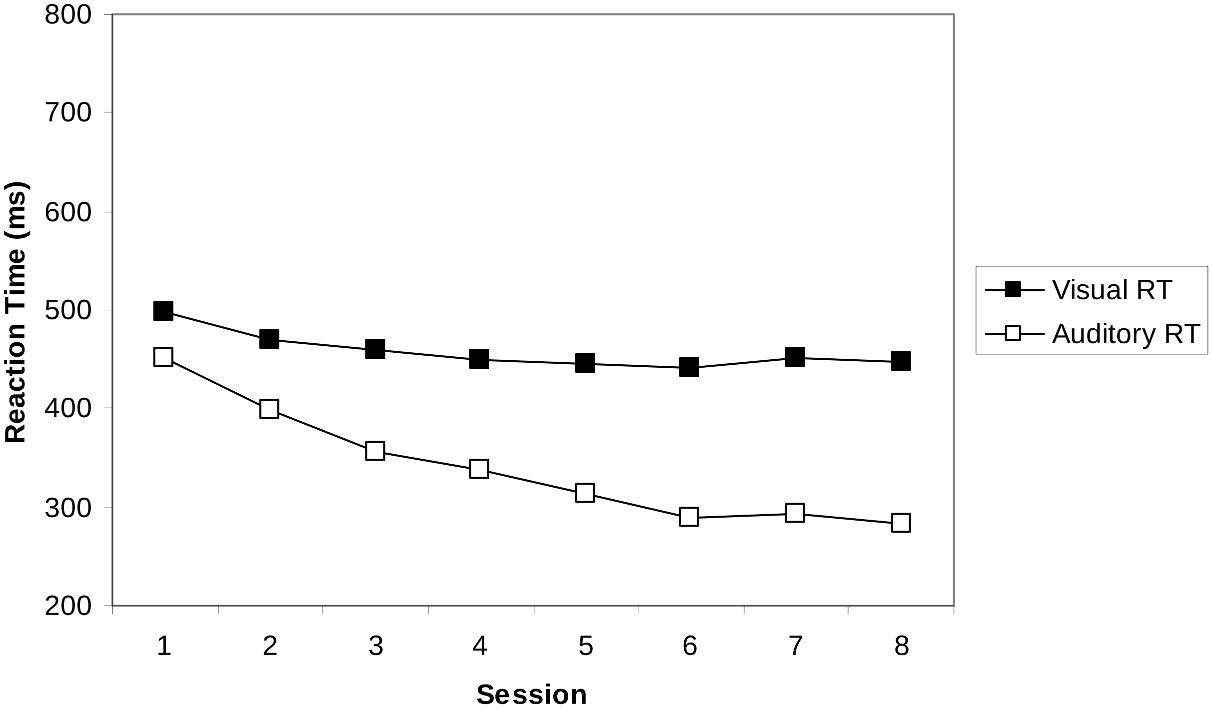
Session
Total Dual
Total Mixing

Figure 2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Session
■ Visual RT
□ Auditory RT

Figure 3

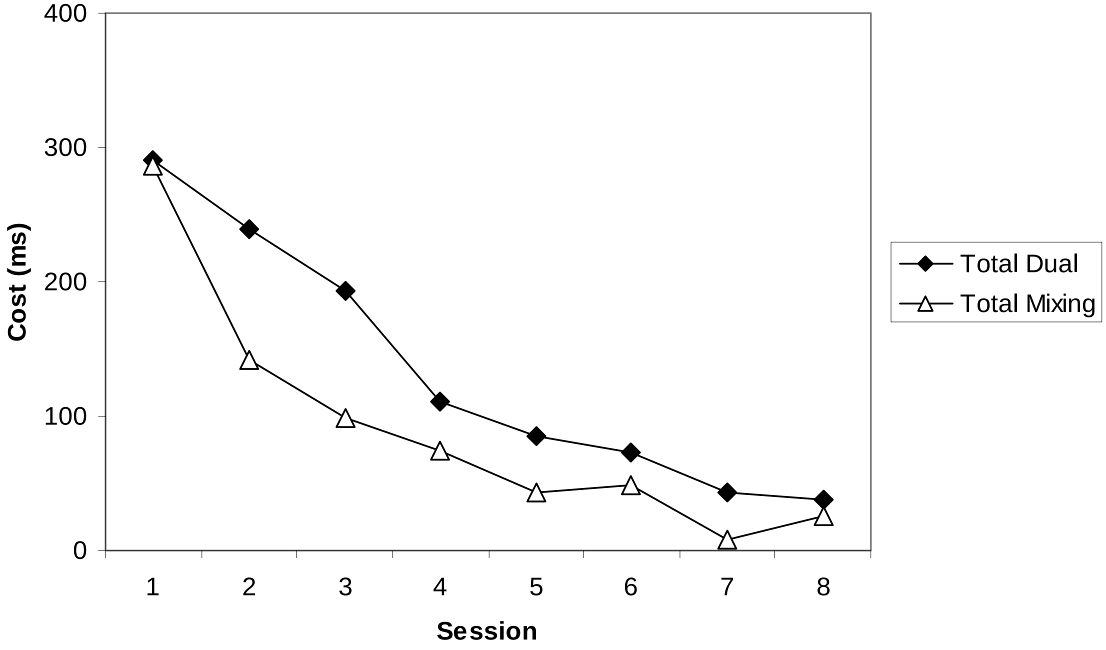

400
300
200
100
0
Cost (ms)
1
2
3
4
5
6
7
8
Session
Total Dual
Total Mixing

Figure 4

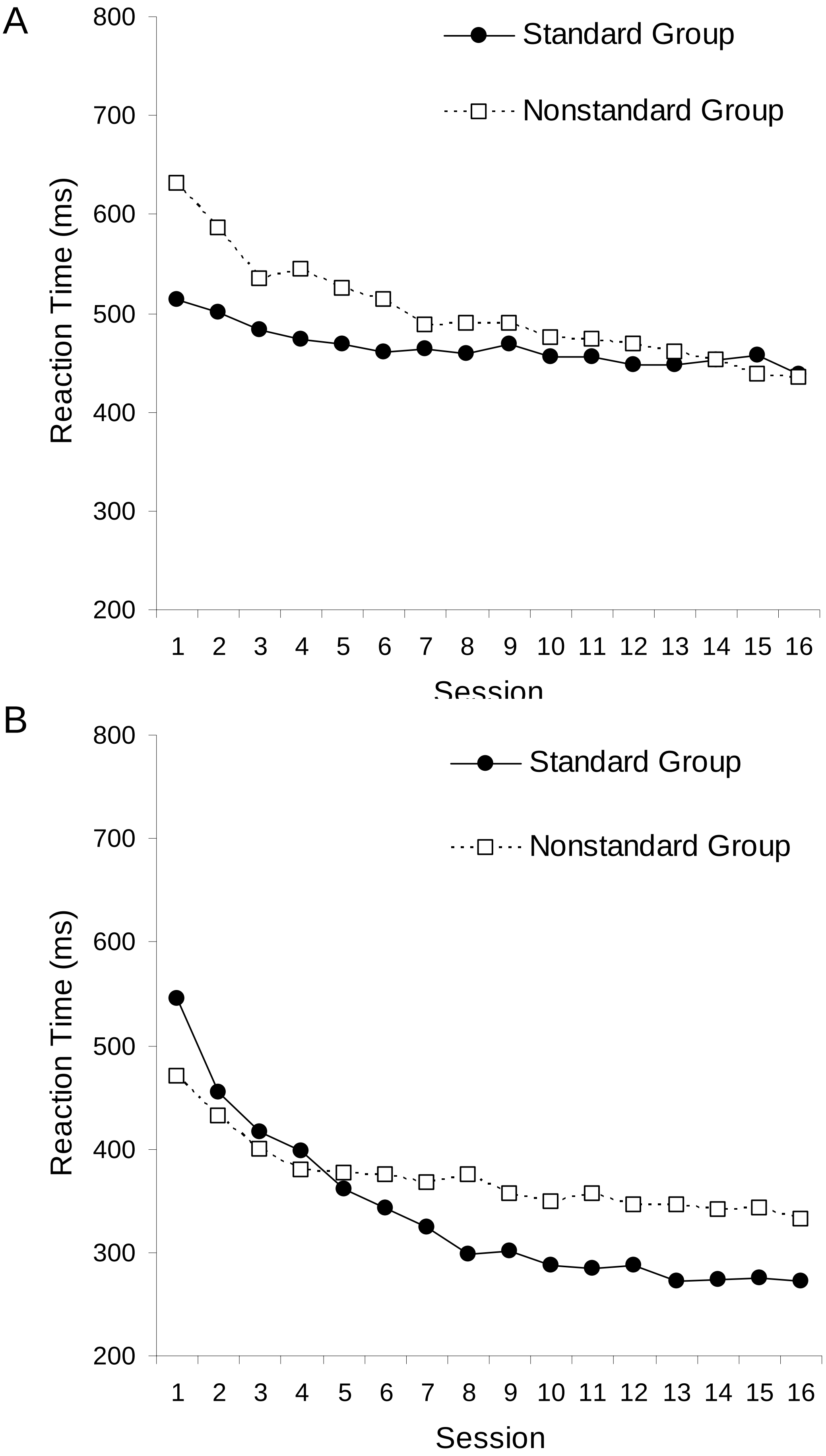

A
800
700
600
500
400
300
200
Reaction Time (ms)
Standard Group
Nonstandard Group
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
Session
B
800
700
600
500
400
300
200
Reaction Time (ms)
Standard Group
Nonstandard Group
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
Session

Figure 5

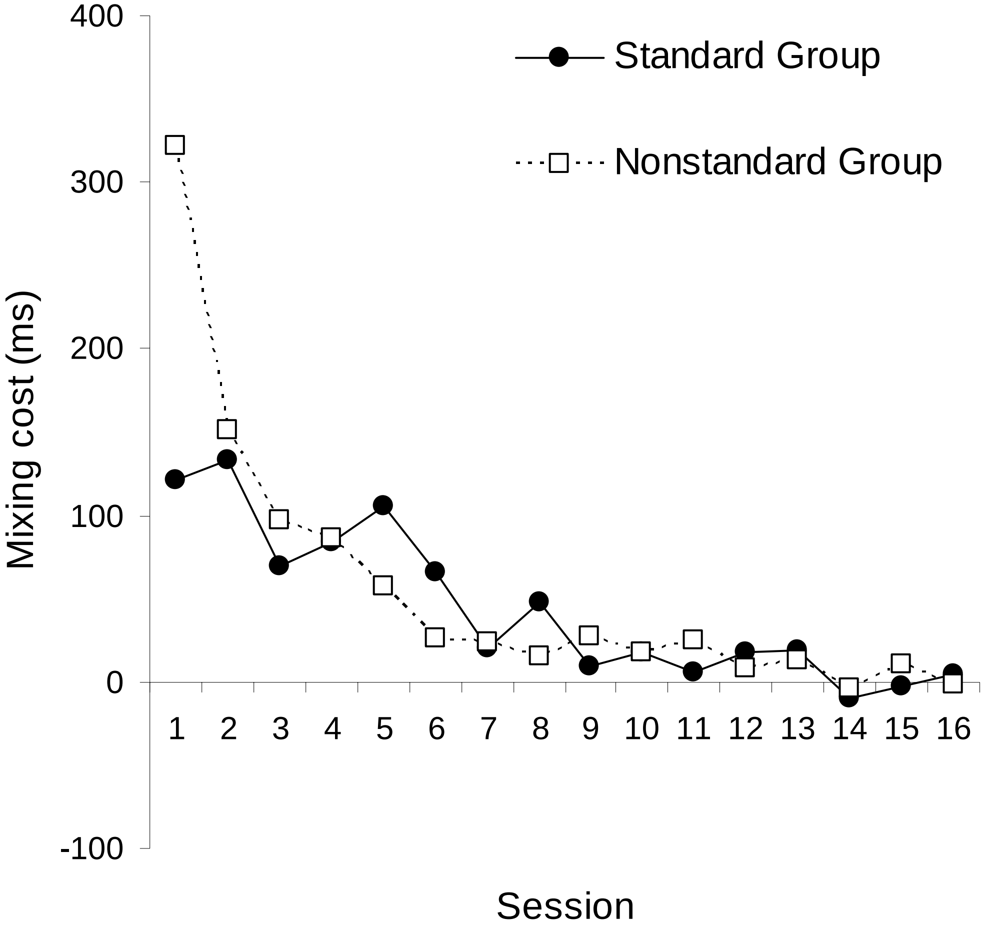

400
300
200
100
0
-100
Mixing cost (ms)
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
Session
Standard Group
Nonstandard Group

Figure 6

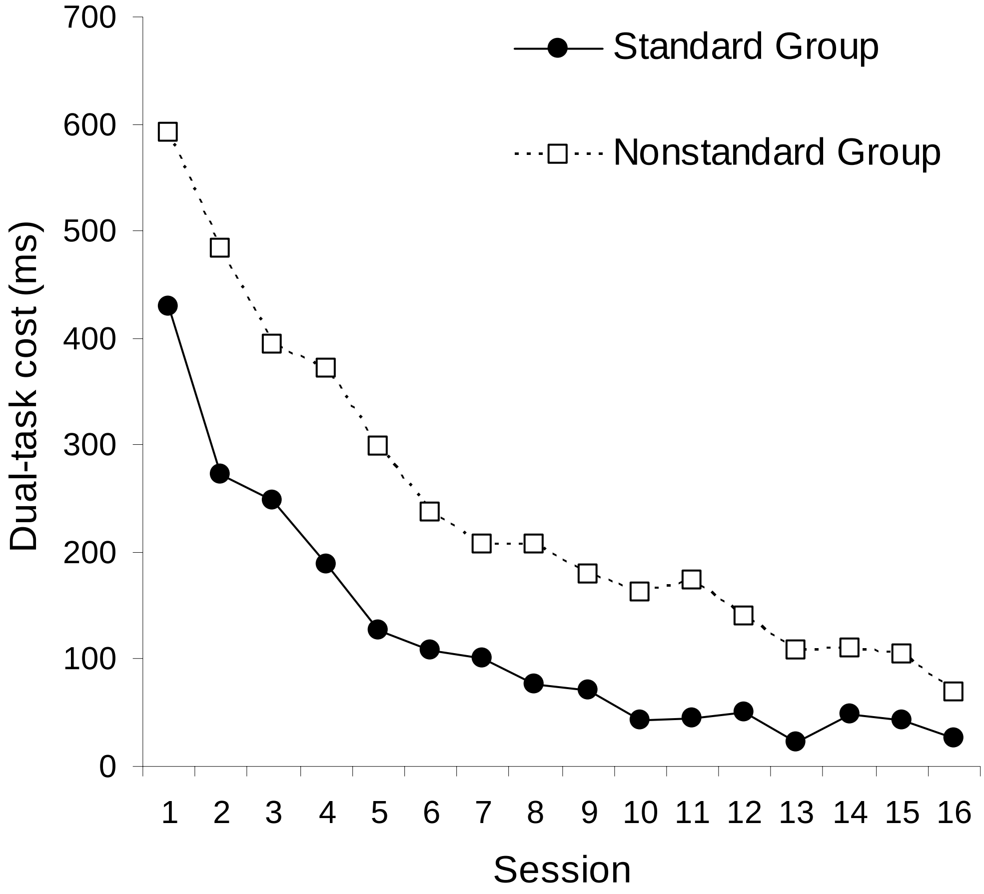

700
600
500
400
300
200
100
0
Dual-task cost (ms)
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
Session
Standard Group
Nonstandard Group

Figure 7

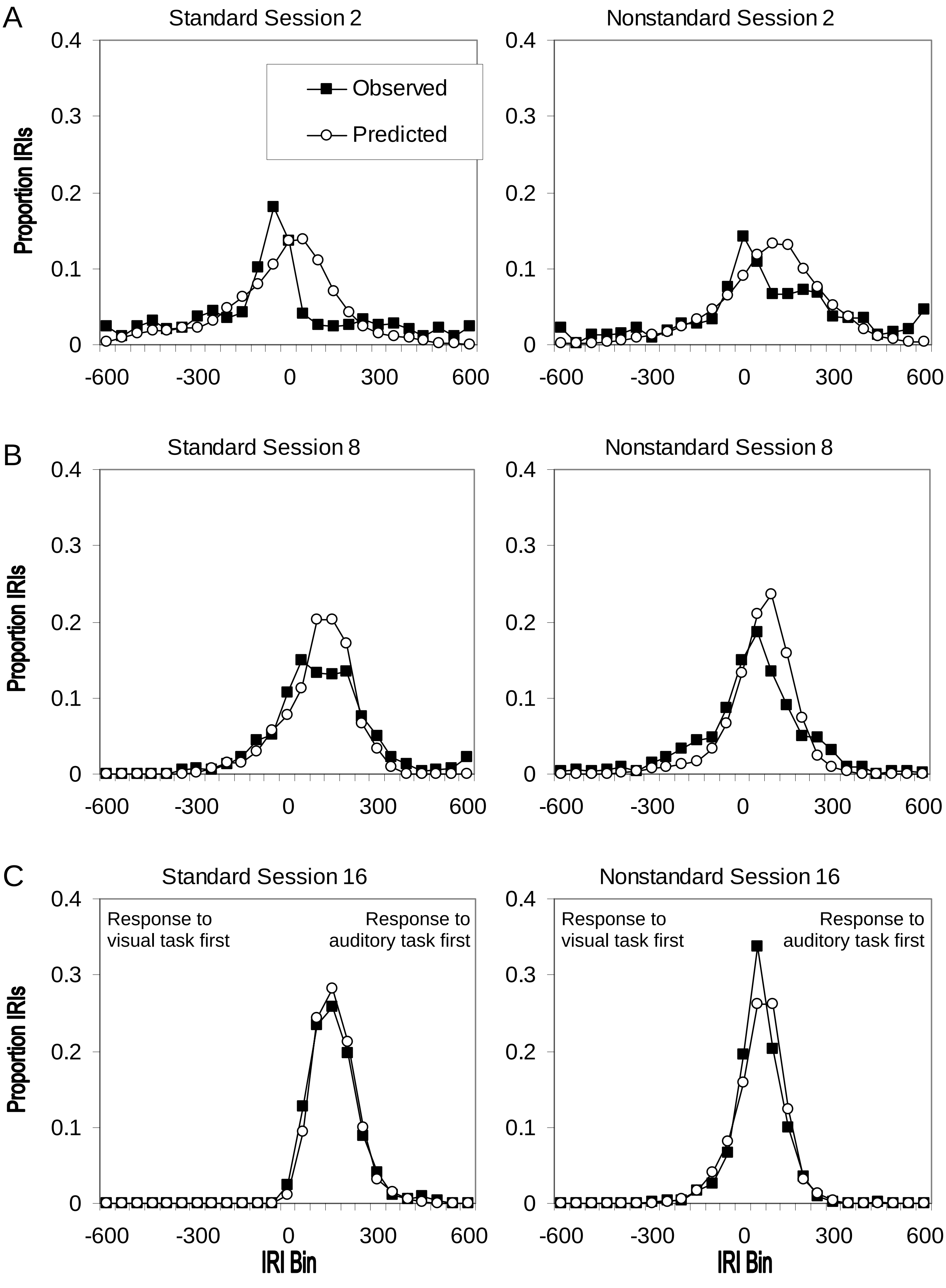
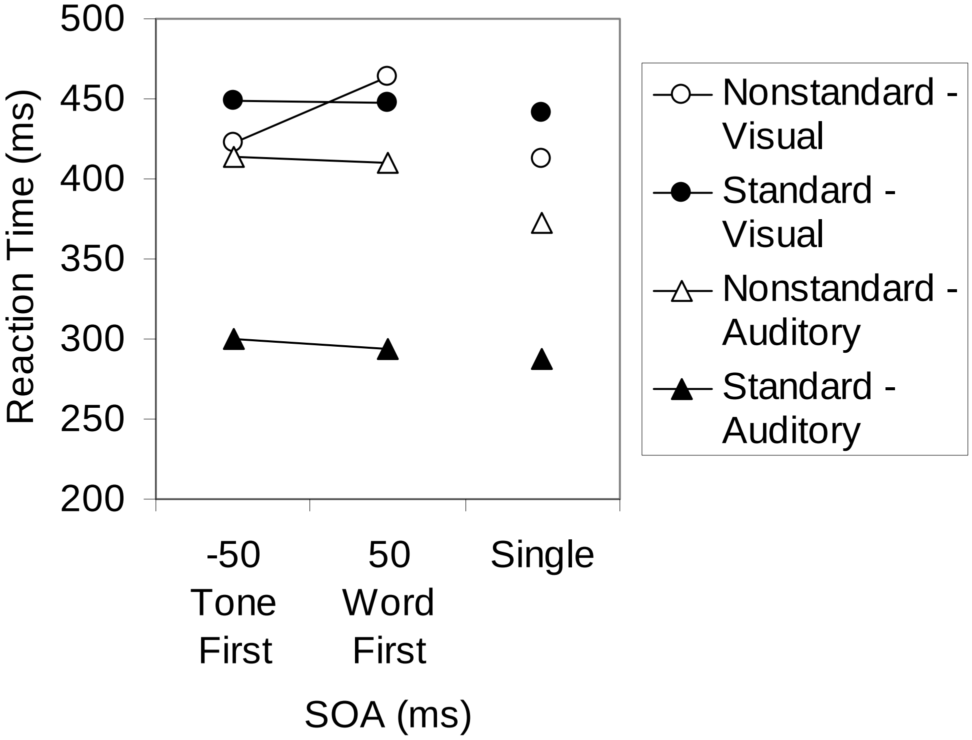

A
Standard Session 2
Nonstandard Session 2
Observed
Predicted
B
Standard Session 8
Nonstandard Session 8
C
Standard Session 16
Nonstandard Session 16
Response to visual task first
Response to auditory task first
Proportion IRIs
IRI Bin
0
0.1
0.2
0.3
0.4
-600
-300
0
300
600

Figure 8



500
450
400
350
300
250
200
Reaction Time (ms)
-50 Tone First
50 Word First
Single
SOA (ms)
Nonstandard - Visual
Standard - Visual
Nonstandard - Auditory
Standard - Auditory

Figure 9

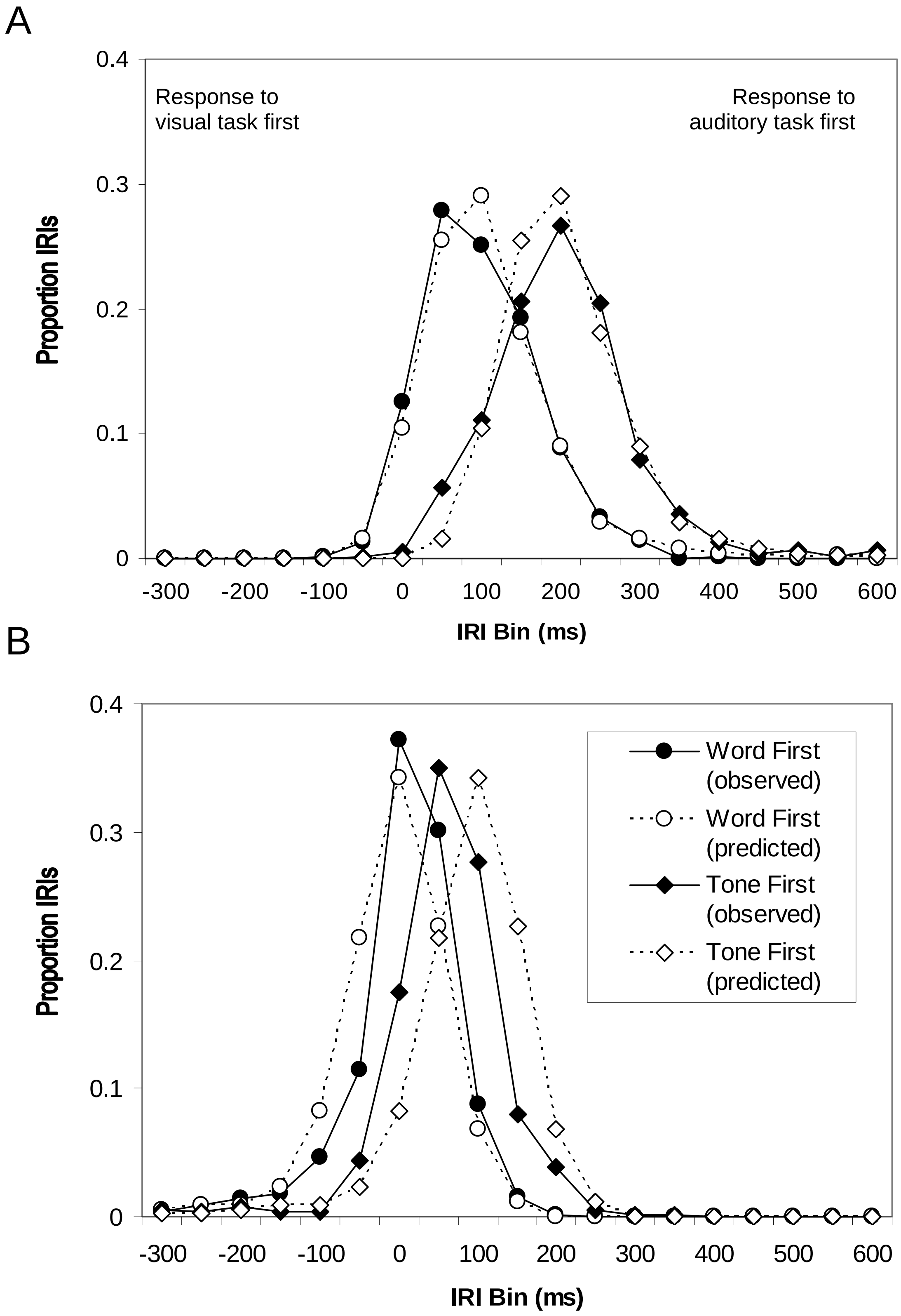

A
Proportion IRIs
0.4
0.3
0.2
0.1
0
Response to
visual task first
Response to
auditory task first
-300
-200
-100
0
100
200
300
400
500
600
IRI Bin (ms)
B
Proportion IRIs
0.4
0.3
0.2
0.1
0
Word First
(observed)
Word First
(predicted)
Tone First
(observed)
Tone First
(predicted)
-300
-200
-100
0
100
200
300
400
500
600
IRI Bin (ms)

Figure 10

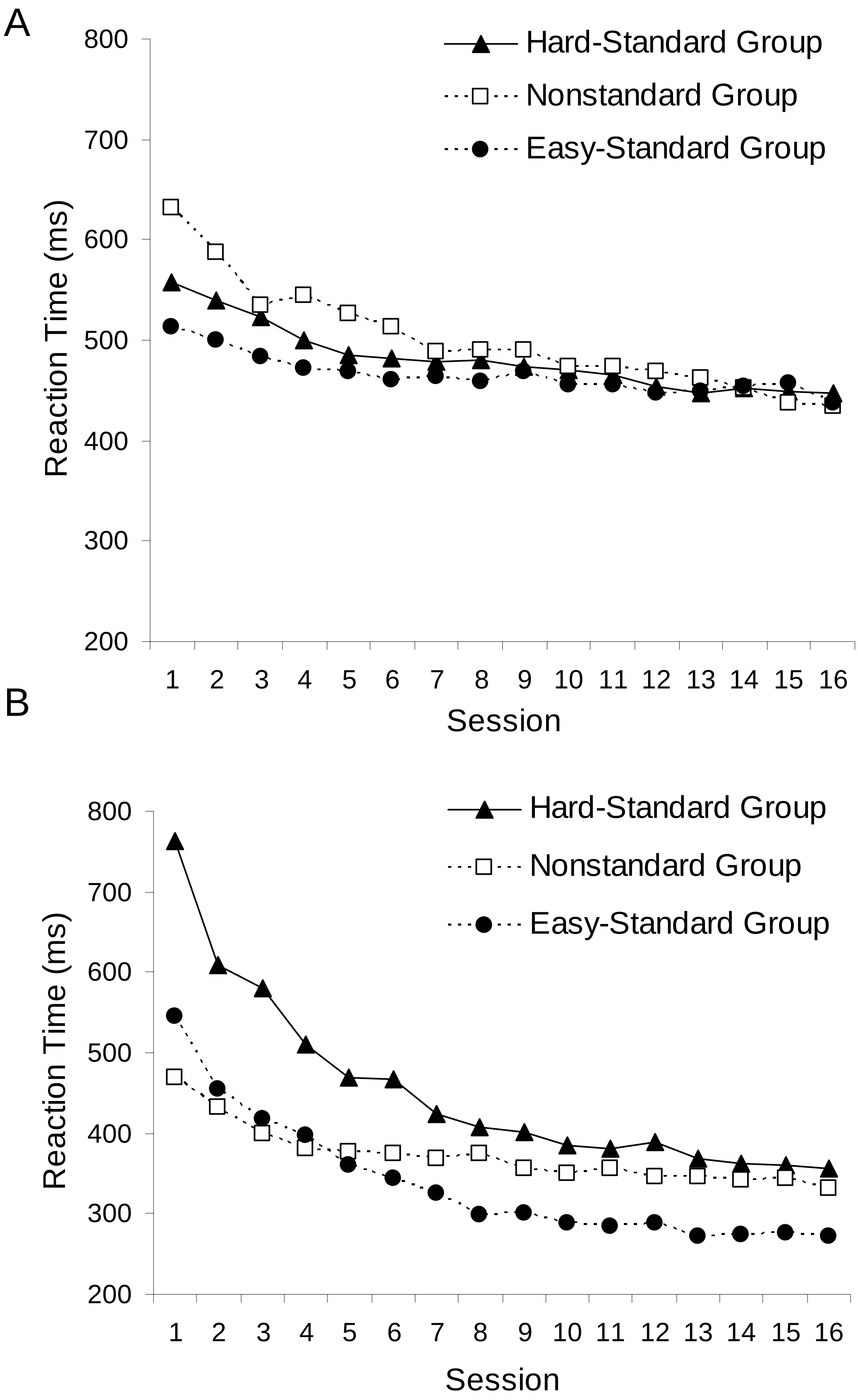

A
800
700
600
500
400
300
200
Reaction Time (ms)
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
Session
Hard-Standard Group
Nonstandard Group
Easy-Standard Group
B
800
700
600
500
400
300
200
Reaction Time (ms)
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16
Session
Hard-Standard Group
Nonstandard Group
Easy-Standard Group

Figure 11

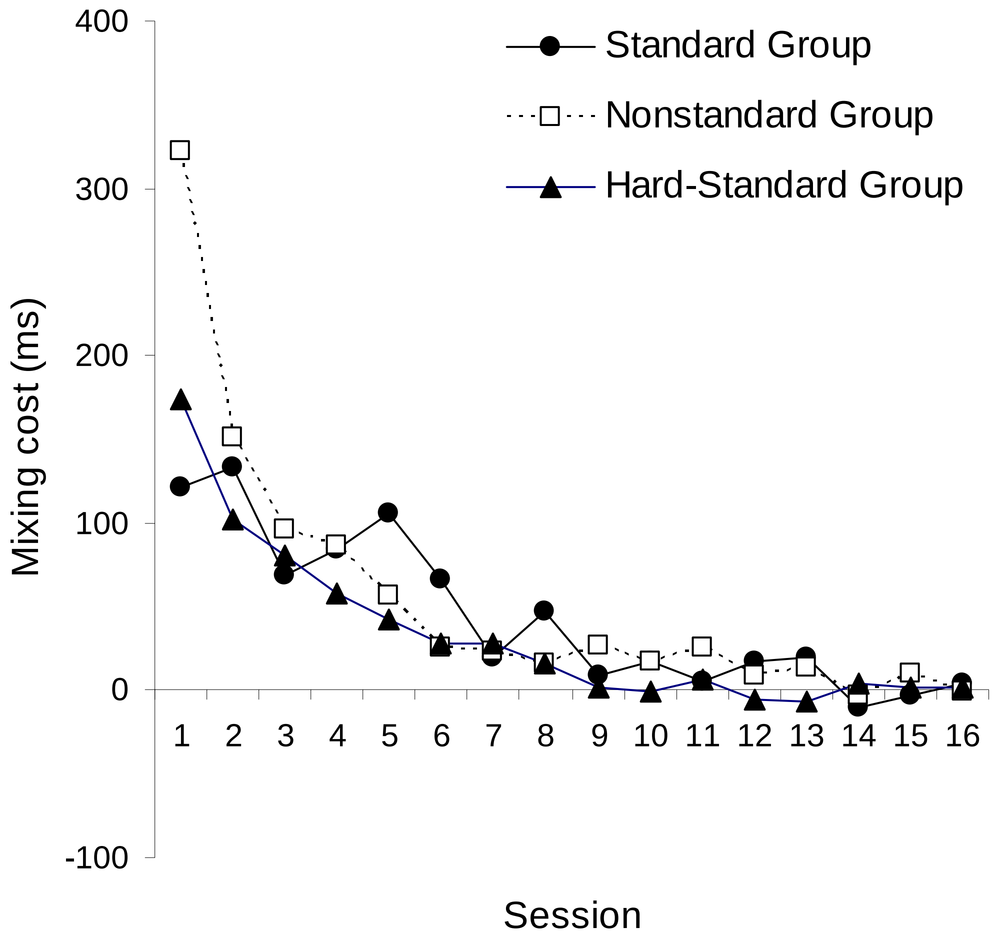
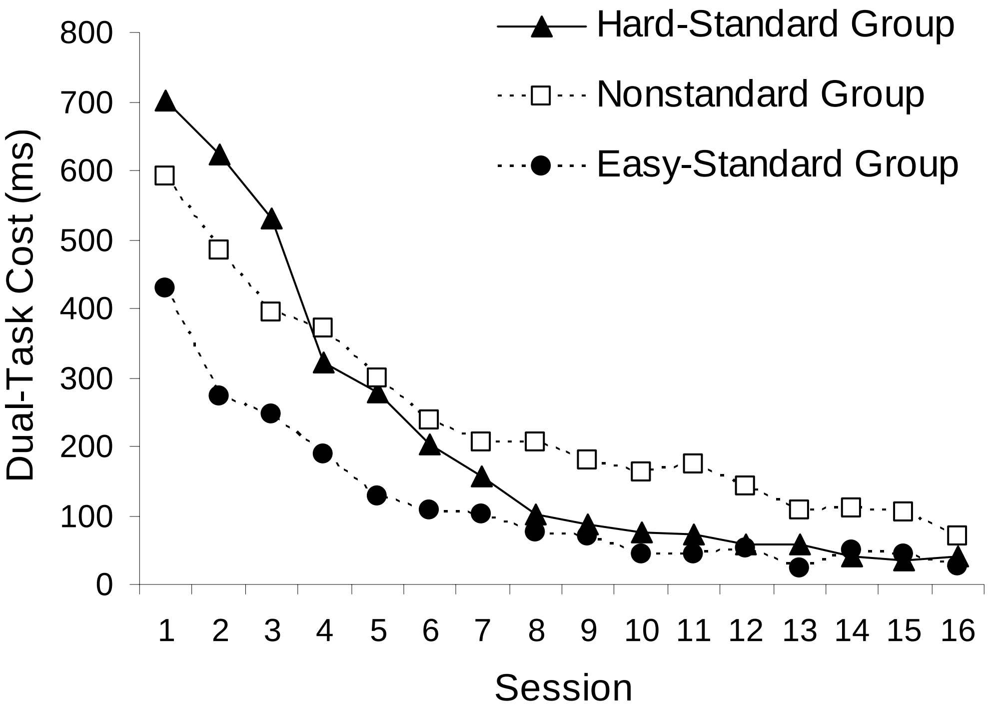

400
300
200
100
0
-100
Mixing cost (ms)
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
Session
Standard Group
Nonstandard Group
Hard-Standard Group

Figure 12



Hard-Standard Group
Nonstandard Group
Easy-Standard Group
Dual-Task Cost (ms)
0
100
200
300
400
500
600
700
800
Session
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16

Figure 13

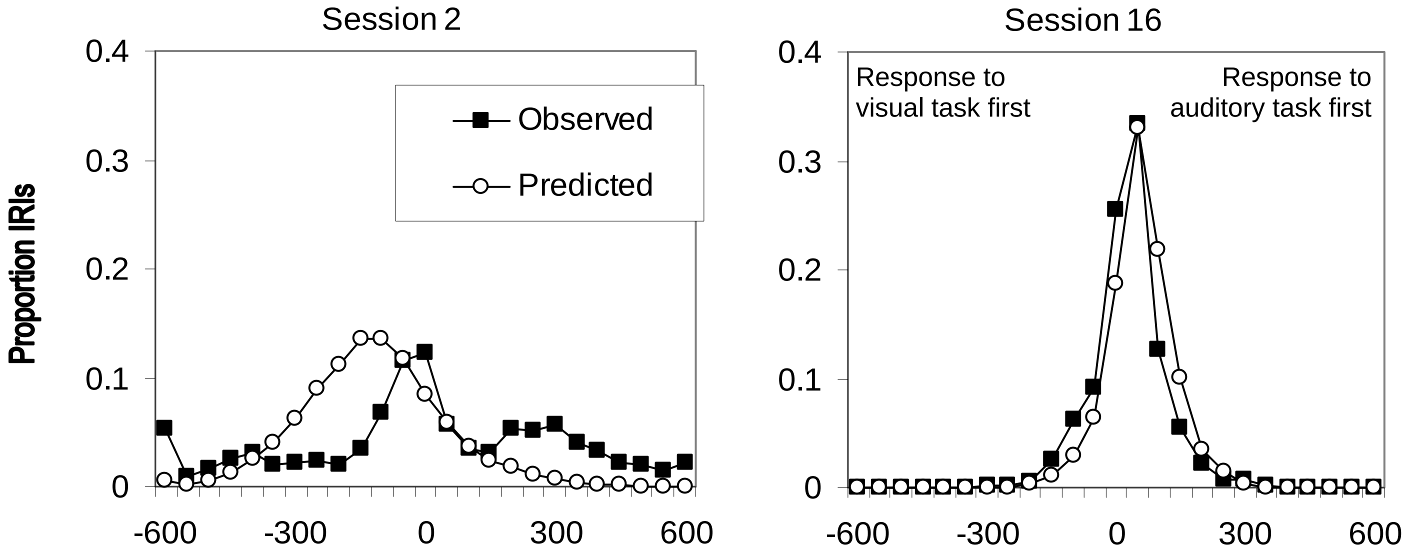

Session 2
Session 16
Proportion IRIs
0.4
0.3
0.2
0.1
0
-600
-300
0
300
600
Observed
Predicted
Response to visual task first
Response to auditory task first

Figure 14