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Original Article

How does practice reduce dual-task interference: Integration, automatization, or just stage-shortening?

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Abstract The present study assessed three hypotheses of how practice reduces dual-task interference: Practice teaches participants to efficiently integrate performance of a task pair; practice promotes automatization of individual tasks, allowing the central bottleneck to be bypassed; practice leaves the bottleneck intact but shorter in duration. These hypotheses were tested in two transfer-of-training experiments. Participants received one of three training types (Task 1 only, or Task 2 only, or dual-task), followed by dual-task test sessions. Practice effects in Experiment 1 (Task 1: auditory–vocal; Task 2: visual–manual) were fully explained by the intact bottleneck hypothesis, without task integration or automatization. This hypothesis also accounted well for the majority of participants when the task order was reversed (Experiment 2). In this case, however, there were multiple indicators that several participants had succeeded in eliminating the bottleneck by automatizing one or both tasks. Neither experiment provided any evidence that practice promotes efficient task integration.

Introduction

Although people typically have difficulty performing two choice-response tasks at the same time, this interference can be dramatically reduced with practice. Is this reduction due to a qualitative change in task performance? Specifically, does practice eliminate the central bottleneck widely believed to underlie dual-task interference? There are several ways in which this might happen. If the bottleneck occurs because central operations are carried out by scarce general-purpose resources, practice might allow people to perform tasks without those resources (task automatization). For instance, practice might produce "jumper cable" pathways directly linking stimuli and responses (Johnston & Delgado, 1993). Alternatively, practice might allow an efficient integration of the two tasks. For instance, practice might allow participants to re-organize two tasks into a single super-task, thus eliminating resource competition.

The plausibility of task automatization and task integration provides motivation to look for qualitative changes in performance with practice, but it is by no means obvious that such changes actually occur. It is possible that practice instead leaves the bottleneck intact but shorter in duration due to stage-shortening. To determine how practice reduces dual-task interference, the present study used a transfer-of-training paradigm, comparing the effects of single-task practice and dual-task practice on subsequent dual-task performance. Looking ahead, the results obtained with the task order adopted by many previous dual-task studies (Task 1: auditory–vocal; Task 2: visual–manual) can be explained entirely by the hypothesis that practice leaves the bottleneck intact but shorter in duration. With the opposite (rarely studied) task order, a substantial majority of participants again showed an intact bottleneck. However, a minority of participants succeeded in eliminating the bottleneck, automatizing one or both tasks.

Background

The psychological refractory period

Many early dual-task studies used accuracy as the primary dependent measure (e.g., Allport, Antonis, & Reynolds, 1972; Hirst, Spelke, Reaves, Caharack, & Neisser, 1980). Accuracy, however, may be insensitive to capacity limitations. Even a strict inability to perform central stages of the two tasks in parallel would not necessarily cause many errors. People might be able to buffer information on one task, switch to a second task for a period of time, and later retrieve enough of the buffered information from the first task to respond accurately (see Shaffer, 1975). Although this buffer-and-switch strategy might allow participants to avoid making overt errors, it would increase response time (RT) to one or both tasks (Pashler & Johnston, 1989). For this reason, recent dual-task research has emphasized paradigms that allow precise measurement of RT.

By far the most widely used RT paradigm in dual-task research is the psychological refractory period (PRP) paradigm, in which the key independent variable is the stimulus onset asynchrony (SOA) between the two tasks. Long SOAs, where temporal overlap between tasks is minimal, provide a baseline measurement of RT for each task performed separately. Short SOAs, where temporal overlap is high, provide an opportunity to observe whether parallel central processing has occurred. The typical PRP finding is a dramatic increase in RT to the second task (RT2) at short SOAs relative to (baseline) long SOAs. This PRP effect is remarkably robust, occurring even with very simple pairs of tasks in the absence of input or output modality conflicts (for reviews see Lien & Proctor, 2002; Pashler & Johnston, 1998).

There is substantial evidence that the primary cause of the PRP effect is a central bottleneck, requiring central stages such as response selection to proceed on only one task at a time (Davis, 1957; Welford, 1952). Figure 1 shows a generalized bottleneck model of the short SOA condition, under the assumption that Task 1 bottleneck operations are performed before those of Task 2. Each task is divided into three processing stages: Pre-bottleneck (A), bottleneck (B), and post-bottleneck (C). By hypothesis, stages A and C can proceed in parallel with any stage on another task, but stage B (the bottleneck stage) proceeds on only one task at a time. Thus, at short SOAs, the Task 2 bottleneck stage (2B) must wait for the Task 1 bottleneck stage (1B) to finish. This waiting time produces a PRP effect equal to $1A + 1B - 2A - SOA_{short}$ (see Pashler & Johnston, 1989). Because this equation does not contain a term for stage 2B, the PRP effect should not depend on the duration of the Task 2 bottleneck stage. This bottleneck model prediction and many others have been confirmed in several studies (e.g., McCann & Johnston, 1992; Pashler & Johnston, 1989).

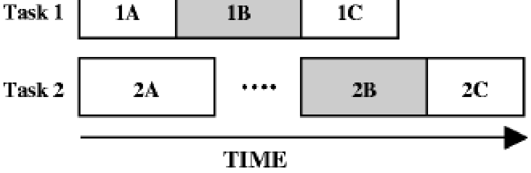

Fig. 1 Stage-time diagrams for the 0-ms stimulus onset asynchrony (SOA), according to a generalized bottleneck model. Processing is divided into three stages, arbitrarily labeled *A*, *B*, and *C*. Stages A and C of

one task can proceed in parallel with any stage of the other task. Stage B (the bottleneck stage), however, can proceed on only one task at a time.

Although it is clear that a central bottleneck occurs in the PRP paradigm, Meyer and Kieras (1997a, 1997b) have argued that the bottleneck may reflect strategic choices rather than structural limitations (see also Schumacher et al., 2001). If the bottleneck is strategic, then there should be conditions under which people choose to perform tasks in parallel. To look for such conditions, Ruthruff, Pashler, & Klaassen (2001) explicitly encouraging processing overlap, placed equal emphasis on each task, asked participants to group their responses (i.e., emit them at the same time), minimized input and output conflicts, and presented both stimuli at the same time. Despite these efforts, dual-task costs were about as large as those typically observed in the PRP paradigm. Several other recent studies have also argued that the central bottleneck is structural rather than strategic, at least at relatively low practice levels (Levy & Pashler, 2001; Pashler, 1994; Ruthruff, Miller, & Lachmann, 1995, Experiment 3; Ruthruff et al., 2001; Ruthruff, Pashler, & Hazeltine, 2003; Tombu & Jolicoeur, 2000).

Can practice eliminate the central bottleneck?

Although the central bottleneck appears to be structural, it might nevertheless be possible to overcome it with practice. Until recently, only a few PRP studies had examined the effects of extensive practice. These studies reported surprisingly little effect of practice on PRP interference (e.g., Bertelson & Tisseyre, 1969; Davis, 1956, 1957; Dutta & Walker, 1995; Hick, 1948; Karlin & Kestenbaum, 1968; Van Selst & Jolicoeur, 1997). Karlin and Kestenbaum, for instance, reported residual PRP effects greater than 200 ms, even after seven to eight practice sessions. Van Selst and Jolicoeur (1997) later replicated this aspect of Karlin and Kestenbaum’s results.

The results of these early studies suggested that PRP interference is remarkably resistant to practice. Van Selst, Ruthruff, and Johnston (1999) argued that this conclusion was premature, however, because these previous PRP studies required manual responses to both tasks. Given the likelihood of strong output interference between two manual responses (either in programming or execution; see De Jong, 1993; Keele, 1973), these studies might have provided less than ideal conditions for observing effects of practice on central interference. To address this concern, Van Selst et al. (1999) conducted a PRP practice study using task pairs that minimized output (and input) interference. One task mapped auditory stimuli to vocal responses (high- and low-pitched tones to the spoken words “high” and “low”) and the other task mapped visual stimuli to manual responses (alphanumeric characters to key presses). Contrary to previous studies, practice dramatically reduced dual-task interference, from 353 ms in the first session to only 40 ms in the 18th session. This more recent finding demonstrates that practice can indeed substantially reduce PRP interference, provided that input and output modality conflicts have been eliminated (see also Hazeltine, Teague, & Ivry, 2002; Schumacher et al., 2001).

What causes the dramatic reduction in dual-task interference with practice? In particular, does practice actually eliminate the bottleneck? Below we outline three plausible hypotheses, each of which focuses on a different consequence that practice might have. As will be seen, these models make distinct predictions about what types of practice (e.g., single-task versus dual-task) should be most effective in reducing dual-task interference.

First, dual-task practice might produce an efficient integration of the two tasks (see, e.g., Hirst et al., 1980; Spelke, Hirst, & Neisser, 1976), increasing the degree to which central stages can proceed in parallel. As an extreme example, participants might learn to combine the two tasks into a single "super-task" (Kahneman, 1973), so that a single cognitive operation jointly selects responses to both tasks at the same time. According to this *task integration* hypothesis, integration requires dual-task practice. Thus, dual-task practice should produce PRP reduction above and beyond what is possible with single-task practice.

Second, practice might automatize the performance of individual tasks. For the present purposes, we define task automatization as the elimination of demands on the limited central resources responsible for the central bottleneck. For instance, practice might allow the development of "jumper cable" pathways directly linking stimuli and responses (Johnston & Delgado, 1993). If either task is automatized, then there should be no conflict for limited central resources and hence no bottleneck. According to this *task automatization* hypothesis, therefore, complete bypass of the bottleneck (i.e., parallel processing of central operations) should occur following either single-task or dual-task practice.

Third, practice might cause no qualitative change in performance—no task integration and no task automatization—leaving the bottleneck intact. Even in this case, practice would reduce dual-task interference by shortening stage durations. This *intact bottleneck* (with stage-shortening[1])

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[1] Stage-shortening is an inevitable consequence of task practice. It plays a critical role in the intact bottleneck hypothesis—in fact, it is the sole cause of PRP reduction. Although the task integration and task automatization hypotheses also allow for stage-shortening, it is not assumed to be the

hypothesis makes the same predictions for the PRP effect that all bottleneck models (see Fig. 1) make: PRP effect = 1A + 1B − 2A − SOA (Pashler & Johnston, 1989). This equation indicates that the PRP effect can be reduced only by shortening stages 1A or 1B, which can occur either with dual-task practice or with single-task practice on Task 1. Single-task practice on Task 2 should not reduce the PRP effect (note that reductions in 2A would actually *increase* the PRP effect).

Although the predictions of our three main hypotheses have never been tested in an appropriate transfer-of-training design, previous practice studies provide some useful clues. Van Selst et al. (1999) confirmed several bottleneck model predictions regarding factor interactions with SOA (see below for more details) after practice. Their results provide some support for the intact bottleneck hypothesis. However, Van Selst et al. did not include a single-task practice condition, so a direct test between the competing models (task integration, task automatization, intact bottleneck) was not possible.

Ruthruff, Johnston, and Van Selst (2001) transferred the highly-practiced Van Selst et al. participants to new dual-task conditions involving either the old Task 2 paired with a new Task 1 (Experiment 1), or a new Task 2 paired with the old Task 1 (Experiment 2). Although their finding of better transfer with the old Task 1 than with the old Task 2 provides some support for the intact bottleneck hypothesis, their design has serious limitations. Specifically, the two transfer conditions had neither Task 1 nor Task 2 in common. Hence, the results might be due to special properties of the test tasks (e.g., one task pair might have been more difficult than the other). This problem is an unfortunate byproduct of using the same cohort of participants throughout all the experiments (taking advantage of their heroic levels of cumulative practice). As explained in the next section, the present study avoided these problems by using different cohorts of participants for different training conditions.

The present study

The goal of the present study was to evaluate the task integration, task automatization, and intact bottleneck hypotheses of how practice reduces dual-task interference. We tested predictions of these three hypotheses using the transfer-of-training paradigm summarized in Table 1. Participants received one of three types of training:

[Table 1 will appear here. See end of document.]

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principle cause of PRP reduction (e.g., if the bottleneck is bypassed, then interference will be small regardless of stage durations).

a) Single-task practice on Task 1
b) Single-task practice on Task 2
c) Dual-task (PRP) practice

Following eight training sessions, all three groups then completed four dual-task (PRP) test sessions. Importantly, the dual-task test sessions were identical for the three training groups.

Assessing the consequences of practice

We analyzed two possible consequences of practice: Reductions in the size of the PRP effect, and elimination of the bottleneck. Assessment of PRP reduction is straightforward, but assessment of bottleneck elimination is not. Some previous dual-task studies have used the amount of dual-task cost as a proxy for bottleneck elimination, assuming that small PRP effects directly indicate the elimination of the bottleneck and large costs directly indicate the continued existence of the bottleneck. Unfortunately, the absolute amount of dual-task interference is not, by itself, a completely reliable diagnostic. Even the bottleneck model predicts that PRP effects should decline with practice. In fact, PRP effects could approach zero, if Task 1 central operations are finished very quickly, before Task 2 central operations are set to begin. In such cases, the bottleneck is said to be "latent," producing no observable RT delays (see Ruthruff, Johnston, Van Selst, Whitsell, & Remington, 2003).

We used several different measures to assess whether the bottleneck was in fact eliminated. First, as explained in more detail below, we examined whether the PRP effect was proportional to RT1 (according to the bottleneck model, PRP = RT1 − 1C − 2A − SOA). We also examined several specific bottleneck model predictions for how the effects of various task difficulty factors should interact with the effects of SOA (see below). As an additional indicator of whether a bottleneck occurred, we examined the order of responses within a trial. According to the central bottleneck model, the overt Task 1 response should precede the Task 2 response on almost every trial. But if participants perform central operations in parallel (as allowed by the task integration and task automatization hypotheses), responses might occur in either order.

Summary of key predictions

The task integration hypothesis predicts that dual-task practice should be much more effective than single-task practice, reducing and possibly even eliminating the bottleneck. The task automatization hypothesis predicts elimination of the bottleneck, with little or no residual PRP effect, following Task 1, Task 2, or dual-task practice. The intact bottleneck hypothesis predicts that the bottleneck should persist with practice (albeit shorter in duration due to stage-shortening), and that PRP effects should be much smaller following either dual-task or Task 1 practice than following Task 2 practice.

Experiment 1

As in Van Selst et al. (1999), Task 1 required participants to say “low” or “high” in response to low-pitched or high-pitched tones and Task 2 required participants to press a key corresponding to the identity of an alphanumeric character (*1*, *2*, *3*, *4*, *A*, *B*, *C*, or *D*). For half of the participants, the letters were mapped compatibly onto the four response keys and the digits were mapped incompatibly. For the other half of participants, the assignments were reversed.

Following Van Selst et al. (1999), we manipulated three task difficulty factors:

a) Tone task difficulty (whether the tone pitch was near or far from the high/low boundary)
b) Character task contrast
c) Character task stimulus–response (S–R) compatibility

If the bottleneck remains intact with practice:

a) Task 1 difficulty effects should carry over fully onto RT2 at short SOAs
b) Task 2 character intensity effects on RT2 should be smaller at short SOAs (due to “absorption” into cognitive slack) than at long SOAs
c) Task 2 S–R compatibility effects on RT2 should be roughly the same at all SOAs

For more detailed discussion of these predictions, see Van Selst et al. (1999; see also Pashler & Johnston, 1989, 1998; Schweickert, 1978).

Method

Participants

The participants were 18 students recruited from colleges and universities in the Mountain View, California, area and from work-study programs at NASA Ames Research Center. Each of the 18 participants performed 12 sessions, for a grand total of 216 sessions. They were paid for their participation.

Stimuli

The stimulus for Task 1 was one of four tones presented for 150 ms. The two tones highest in pitch (3,125 and 625 Hz) were classified as “high” tones, and the two tones lowest in pitch (400 and 80 Hz) were classified as “low” tones. The stimulus for Task 2 was a single alphanumeric character drawn at random from the set {1, 2, 3, 4, A, B, C, D}. The characters were presented in Times Roman font at a viewing distance of about 65 cm. All characters fitted into a rectangular area of 1.41° × .94° visual angle. The background was white; the characters were black (high contrast condition) on half of the trials and gray (low contrast condition) on the other half of the trials.

Apparatus

Stimulus presentation and timing were performed by IBM PC-compatible computers equipped with a Schmitt-trigger voice key for detecting speech onset and a Voice Connexion system (external) for recognizing speech.

Procedure

Participants were instructed to respond to high tones by saying “high” and to low tones by saying “low.” They were instructed to respond to the identity of the alphanumeric character by pressing the ‘h’, ‘j’, ‘k’ or ‘l’ keys on a standard keyboard, using the fingers of their right hand. For half of the participants in each training group, the letters A, B, C, and D were mapped in alphabetical order onto the four response keys from left to right (i.e., compatibly). The numbers, meanwhile, were mapped in an incompatible, non-sequential order (3, 1, 4, 2) onto the same four response keys. For the remaining participants, numbers were mapped compatibly (1, 2, 3, 4) but letters were mapped incompatibly (C, A, D, B). The instructions emphasized the importance of responding quickly and accurately.

Each participant was randomly assigned to one of the three training groups. The Task 1 training group performed only Task 1 (the tone task) during the eight training sessions and the Task 2 training group performed only Task 2 (the character task). The dual-task training group, meanwhile, performed both Task 1 and Task 2 in a PRP design. All three groups performed the same number of trials per training session (560), although the dual-task group made two responses per trial and the single-task groups made only one response per trial. Thus, the dual-task group performed Task 1 as often as the Task 1 training group and performed Task 2 as often as the Task 2 training group.

After the eight training sessions, all participants then performed four dual-task (PRP) test sessions (see Table 1). These dual-task test sessions were identical for all three groups. Because two of the training groups had to learn a new task and a new paradigm, the first test session was considered practice. Participants were instructed to respond to both tasks quickly and accurately, but to place particular emphasis on the speed of Task 1 responses.

Each session consisted of 35 warm-up trials followed by 525 experimental trials. The session was broken into seven blocks of 80 trials, separated by short breaks. During each break, the computer provided feedback on the average speed of Task 1 and the accuracy of both Task 1 and Task 2. Each trial began with the presentation of a fixation cross for 500 ms, followed by a random fore-period (100, 150, 200, or 250 ms). In the dual-task condition, the Task 1 tone sounded for 150 ms and the Task 2 alphanumeric character appeared after a variable SOA (17, 67, 150, 250, 450, or 850 ms). In the Task 1 training condition, only the tone was presented. In the Task 2 training condition, only the character was presented. The timing of the Task 2 character in this single-task condition was yoked to that of the dual-task condition: Following the random fore-period there was an additional "SOA" delay. If either response was incorrect, a message indicated the task on which the error had been made. Also, if the participant responded within 100 ms of stimulus onset, a "TOO EARLY" message was displayed. If the participant failed to respond within 2,500 ms of stimulus onset, a "TOO SLOW" message was displayed. The fixation cross for the next trial was presented 750 ms later.

Analyses

We conducted separate ANOVAs on mean RT1, RT2, Task 1 error rate, and Task 2 error rate in the test sessions (sessions 10–12), using the factors of training group (Task 1 only, Task 2 only, Dual-Task), SOA, Task 1 difficulty, Task 2 S–R compatibility, and Task 2 contrast. When assessing interactions on RT2 between task-difficulty manipulations and SOA, we relied on a separate, and more sensitive, ANOVA that included just the shortest and longest SOAs rather than all six SOAs (where adjacent SOAs tend to produce very similar performance). Before conducting RT analyses, we first eliminated trials containing an error on either task. Then, for both the error and RT analyses, we removed all trials with an RT of less than 100 ms, followed by trials in which either RT1 or RT2 was identified as an outlier (less than 5.6% of trials in each experiment) using a modified recursive outlier elimination procedure with moving criterion (described in Van Selst & Jolicoeur, 1994). All analyses used an alpha level of .05.

Results

Dual-task group only (training and test)

We first examine separately the results from the dual-task training group, which represents a replication of the Van Selst et al. (1999) experiment, albeit with fewer sessions (12 vs. 36) and a different set of six participants. The results for this group are shown in Fig. 2 (unfilled diamonds) across the 12 sessions, along with the results of Van Selst et al. (1999; filled squares). Each point represents the mean PRP effect (defined as RT2 at the shortest SOA minus RT2 at the longest SOA) for a session plotted against the mean RT1 for that session.

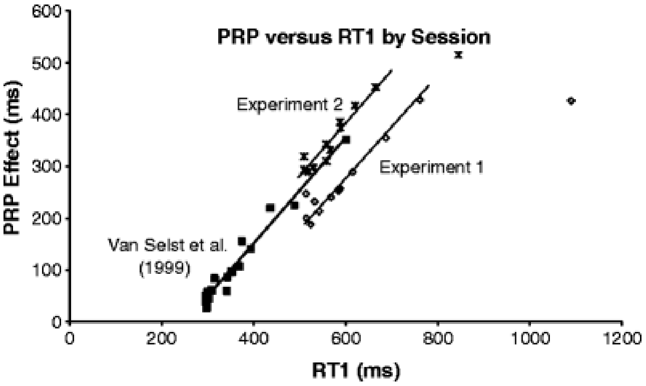

Fig. 2 Mean psychological refractory period (*PRP*) effect for each session as a function of mean Task 1 response time (*RT1*) across sessions in Van Selst et al. (1999; *filled squares*), the present Experiment 1 (*unfilled diamonds*), and the present Experiment 2 (*asterisks*). The best fitting regression line with a slope of 1.0 is shown for each experiment.

If practice primarily reduces the duration of central stages (for supporting evidence see Fletcher & Rabbitt, 1978; Mowbray & Rhoades, 1959; Pashler & Baylis, 1991; Welford, 1976), the intact bottleneck hypothesis makes an elegant prediction regarding the size of the PRP effect and RT1 across sessions. Note that RT1 equals 1A + 1B + 1C and the PRP effect equals $1A + 1B - SOA_{short} - 2A$ (assuming that a bottleneck delay always occurs at the shortest SOA but never at the longest SOA; Pashler & Johnston, 1989). The only term in each equation that, by hypothesis, can vary substantially with practice is stage 1B. Hence, practice should reduce the PRP effect and RT1 by roughly the same amount across sessions. Accordingly, a graph of the PRP effect against RT1 across sessions should have a slope of about 1.0. As shown in Fig. 2, the Van Selst et al. (1999; filled squares) data confirmed this simple prediction (the best-fitting slope was 1.02), supporting a bottleneck model with central stage shortening.

In fitting the data from the dual-task training group of the present Experiment 1, session 1 was excluded because the mean RT1 was very long (1,091 ms), indicating a clear violation of the assumption that a bottleneck delay never occurred at the longest SOA (850 ms). As shown in Fig. 2 (unfilled diamonds), declines in the PRP effect across sessions tracked declines in RT1 approximately one for one, replicating the finding of Van Selst et al. (1999). The best-fitting regression line with a slope of 1.0, shown in Fig. 2, was PRP = RT1 − 321. Allowing the slope to take on any value, the best-fitting regression line was PRP = .86 * RT1 − 249, but the additional variance accounted for (beyond the model with the fixed 1.0 slope) was not significant, $F(1,9) = 2.43$, MSE = 387.6, $p > .05$.

Although the relation between PRP and RT1 is qualitatively similar between the two studies, there are two notable quantitative differences. First, mean RT1 for a given level of practice was much shorter in Van Selst et al. than in the present study (e.g., 308 vs. 515 ms in session 12). Some of this difference can be attributed to greater motivation and/or experience of the participants in the Van Selst et al. study (1999), two of whom were co-authors of the paper. Second, the Van Selst et al. participants showed slightly more PRP effect for a given RT1, as reflected in the PRP axis intercepts (−243 vs. −321). One plausible explanation for this difference is that the highly motivated Van Selst et al. participants reduced RT1 in part by speaking the responses very abruptly. The consequent reduction in the duration of stage 1C would reduce RT1 without reducing the PRP effect, in effect producing more PRP effect for a given RT1.

Training sessions

Table 2 shows mean RT1 and RT2 across the eight training sessions for the three groups. To facilitate comparisons with the single-task groups, the data from the dual-task training group are based on only the longest SOA (the least influenced by dual-task interference). Not surprisingly, RT decreased steadily across sessions for each training group. Mean RT1 was shorter for the single-task training group than for the dual-task training group, but mean RT2 was not; as will be seen, a similar effect was observed in Experiment 2 as well. One possible explanation is that dual-task participants initially prepare for Task 1 as well as the subsequent switch to Task 2 (De Jong, 1995; Lien & Ruthruff, 2004). This initial spreading of preparation between the two tasks could slow dual-task RT1, but would not slow dual-task RT2 at the longest SOA (at this SOA, participants can prepare exclusively for Task 2).

[Table 2 will appear here. See end of document.]

Task 1 RT in test sessions

Figure 3 shows mean RT1 and RT2, averaged across the dual-task test sessions, as a function of SOA and training group. Mean RT1 was shortest for the group that had practiced Task 1 only (365.4 ms), next fastest for the group that had practiced both Task 1 and Task 2 (522 ms), and slowest for the group that had not practiced Task 1 (575 ms). These differences were marginally significant overall, $F(2,15) = 3.2$, MSE = 3,183,749.1, $.05 < p < .10$. Although mean RT1 depended very little on the SOA, the slight speed-up at the short SOAs (see Fig. 3) was significant, $F(5,75) = 3.5$, MSE = 5,160.3, $p < .01$. There was also a significant main effect of Task 1 difficulty on RT1, $F(1,15) = 21.7$, MSE = 182,472.6, $p < .001$; mean RT1 was 449 ms for the easy tones (furthest in pitch from the high/low pitch boundary) and 527 ms for the difficult tones (closest in pitch to the high/low pitch boundary).

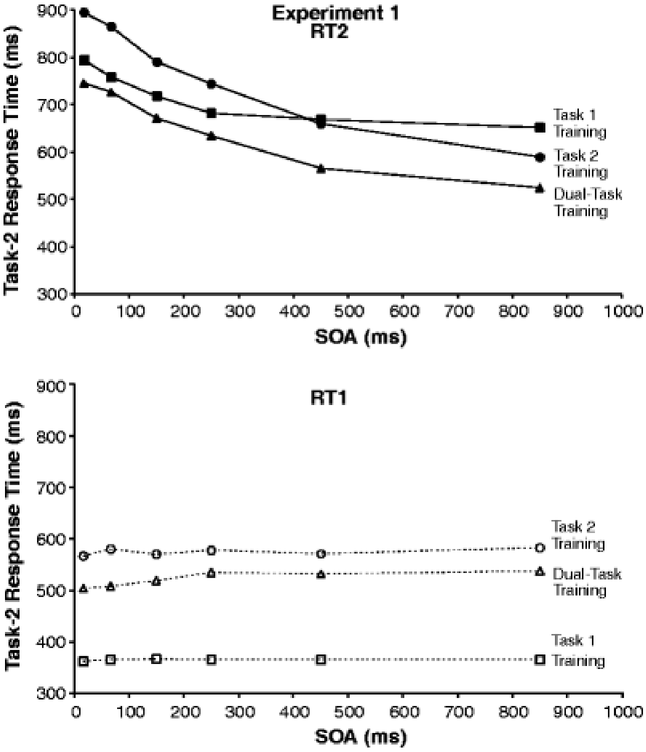

Fig. 3 Response time (*RT*) data for Task 1 and Task 2 during the dual-task test sessions of Experiment 1 as a function of SOA and training group. *Triangles* dual-task training group, *squares* Task 1 training group, *circles* Task 2 training group.

PRP effect on Task 2 in test sessions

A critical question in this experiment is how the type of training influenced the size of the PRP effect in the test sessions. The mean PRP effect was 142 ms for the Task 1 practice group, 307 ms for the Task 2 practice group, and 221 ms for the dual-task practice group. The interaction between training group and SOA was statistically significant, $F(10,75) = 2.22$, MSE = 74,977.8, $p < .05$. Planned comparisons among these three groups revealed that the PRP effect was much smaller after Task 1 practice (142 ms) than after Task 2 practice (307 ms), $t(10) = 2.13$, $p < .05$. Numerically, the PRP effect after Task 1 practice (142 ms) was even smaller than the PRP effect after dual-task practice (221 ms), although this difference was not statistically significant, $t(10) = 1.03$, $p > .1$.

PRP effect vs. RT1 for individuals

To determine whether any individuals were able to eliminate the bottleneck, we plotted the PRP effect versus RT1 for individual participants. Each point in Fig. 4 represents the mean performance for an individual participant averaged across the test sessions. According to the intact bottleneck hypothesis, there should be a strong relationship between the PRP effect and RT1. Specifically, $\text{PRP} = \text{RT1} - 1\text{C} - 2\text{A} - \text{SOA}_{\text{short}}$, assuming that a bottleneck delay always occurs at the shortest SOA but never at the longest SOA. Any individual with a very small PRP effect well below the PRP/RT1 regression line is a candidate to have eliminated the bottleneck.

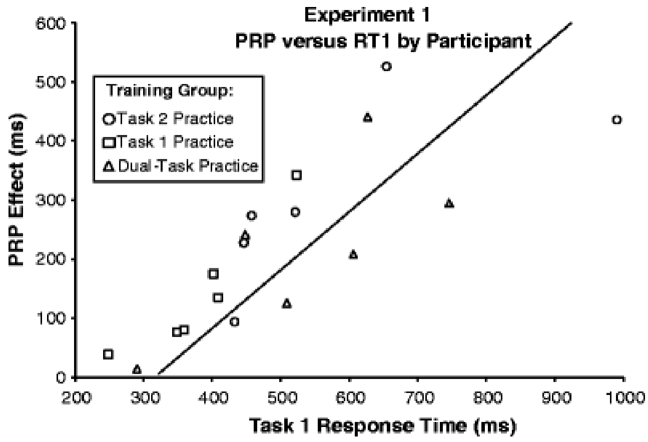

Fig. 4 Mean PRP effect versus mean RT1 for each participant in Experiment 1. *Triangles* dual-task training group, *squares* Task 1 training group, *circles* Task 2 training group.

As a baseline against which to assess the performance of individual participants, Fig. 4 shows the regression line derived from the 12 sessions of the dual-task training group ($\text{PRP} = \text{RT1} - 321$; see Fig. 2). The individual participants in all groups generally fell reasonably close to this regression line. None of the participants combined falling well below this line with a very small PRP effect. In fact, the only participant well below the line (the rightmost point) had a huge PRP effect (436 ms). Given the large observed PRP effect, it is highly unlikely that this participant eliminated the bottleneck. Furthermore, this person had a very long mean RT1 (990 ms), which is long enough to clearly violate the previously noted assumption that no bottleneck delay occurred at the long SOA (required for the linear PRP/RT1 relationship to hold). A further reason to doubt that any individual in Experiment 1 eliminated the bottleneck is the very low rate of response reversals (low single digits for each participant).

Task 1 carry-over prediction

The data confirmed the intact bottleneck prediction that Task 1 difficulty effects should “carry over” fully onto RT2 at short SOAs, but not at long SOAs. Averaged across the three training conditions, the effect on RT1 was 78 ms. This effect carried over fully onto RT2 at short SOAs (85 ms), but not at long SOAs (14 ms), *F*(5,75) = 11.0, MSE = 10,036.0, *p* < .001. Furthermore, Fig. 5 shows that Task 1 difficulty effects carried over fully onto RT2 for each of the three training groups. Because more carry-over occurred for the groups with the largest Task 1 difficulty effects (e.g., the Task 2 group, who had not practiced Task 1), there was a significant three-way interaction between training group, Task 1 difficulty, and SOA, *F*(2,15) = 5.4, MSE = 7,069, *p* < .05.

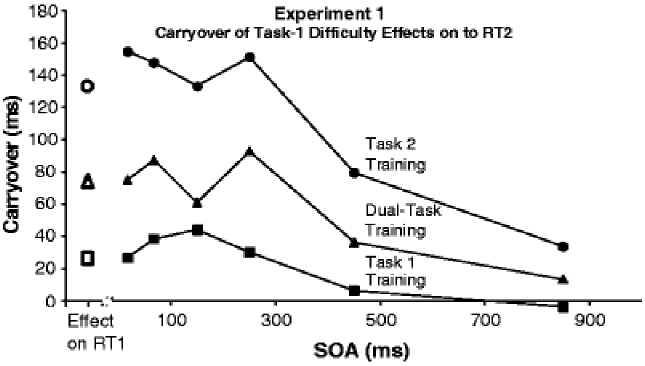

Fig. 5 Effect of Task 1 difficulty manipulation (extreme versus intermediate tone pitch) on Task 2 response time at each SOA in Experiment 1 (*filled symbols*). For comparison, the mean effect of Task 1 difficulty on Task 1 response time is also shown on the *far left* (*unfilled symbols*). *Triangles* dual-task training group, *squares* Task 1 training group, *circles* Task 2 training group.

Task 2 absorption prediction

The intact bottleneck prediction that Task 2 stimulus contrast effects should be smaller at short SOAs than at long SOAs (due to absorption into cognitive slack) was confirmed, albeit weakly. Averaged across the three training groups, the effect of Task 2 contrast was 16, 12, 22, 21, 26, and 36 ms at the 17, 67, 150, 250, 450, and 850 ms SOAs respectively. There was a nearly monotonic trend in the predicted direction (with a 56% reduction in contrast effects at the shortest SOA compared with the longest SOA); however, the decrease was only marginally significant in the ANOVA comparing just the longest and shortest SOAs, *F*(1,15) = 3.53, MSE = 6,148.1, .05 < *p* < .1. The three-way interaction between training group, Task 2 contrast, and SOA (shortest vs. longest) was not significant, *F*(2,15) = 1.26, MSE = 6,148.1, *p* > .3.

Task 2 S–R compatibility

The data generally confirmed the intact bottleneck prediction that Task 2 compatibility effects would be additive with SOA. Averaged across the three training conditions, the effect of Task 2 S–R compatibility was 103, 106, 111, 102, 122, and 127 ms at the 17, 67, 150, 250, 450, and 850 ms SOAs respectively. The small trend toward under-additivity (19% reduction at the shortest SOA compared with the longest SOA) was not significant overall, *F*(5,75) = 1.4, MSE = 8,310.5, *p* > .2, or when comparing just the shortest and longest SOAs, *F*(1,15) = 2.2, MSE = 14,089.3, *p* > .1. There was also no significant three-way interaction between training group, Task 2 S–R compatibility, and SOA, *F*(2,15) < 1.

Task 1 error rates

Participants made more errors with the difficult tones (8.4%) than with the easy tones (1.4%), *F*(1,15) = 33.5, MSE = 114,772.5, *p* < .001. This effect was stronger for the group that had not practiced Task 1 than for the groups that had practiced Task 1, leading to a two-way interaction between tone task difficulty and training group, *F*(2,15) = 5.9, MSE = 944.4, *p* < .05. There was also a main effect of SOA, *F*(5,75) = 5.6, MSE = 49.8, *p* < .001, reflecting a slight tendency to make more errors on Task 1 at short SOAs; error rates were 5.8, 5.6, 5.2, 4.0, 4.5, and 4.0% at the 17, 67, 150, 250, 450, and 850 ms SOAs respectively. In addition, the Task 1 difficulty effect was larger at the short SOAs than at long SOAs, *F*(5,75) = 4.6, MSE = 39.3, *p* < .01. This interaction was weakest for the groups that had practiced Task 1, leading to a three-way interaction between Task 1 difficulty, SOA, and training group, *F*(10,75) = 2.0, MSE = 39.3, *p* < .05.

Task 2 error rates

There was a main effect of training condition on Task 2 error rates during the test sessions, *F*(2,15) = 6.1, MSE = 1,052.7, *p* < .05; mean Task 2 error rate was 7.0% for the Task 1 training group, 3.7% for the Task 2 training group, and 1.6% for the dual-task training group. Participants also made more Task 2 errors when the S–R mapping was incompatible (6.0%) than when it was compatible (2.2%), *F*(1,15) = 28.0, MSE = 344.5, *p* < .001. This effect was more pronounced for the group that did not practice Task 2 than for the groups that did practice Task 2, leading to a two-way interaction between training group and Task 2 S–R compatibility, *F*(2,15) = 7.2, MSE = 344.5, *p* < .01.

Discussion

The results of Experiment 1 confirmed the predictions of the intact bottleneck hypothesis that practice on Task 1 alone would be more effective at reducing the PRP effect than practice on Task 2 alone. The intact bottleneck hypothesis also correctly predicted that Task 1 practice would be just as effective as dual-task practice, and that Task 1 difficulty effects would carry over fully onto RT2 at short SOAs for each training group (see Fig. 5). Furthermore, the PRP effects for individual participants (see Fig. 4) were about as large as would be expected given their mean RT1. Thus, there is no reason to suspect that any individual participant was able to eliminate the bottleneck after practice. These data, along with those of Van Selst et al. (1999), suggest that the primary effect of practice was to shorten stage durations (especially the central stages), leaving the bottleneck intact but much shorter in duration.

Whereas the results of Experiment 1 support the intact bottleneck hypothesis, they pose problems for the alternative hypotheses. The task integration hypothesis, which emphasizes the acquisition of strategies for integrating specific pairs of tasks, predicts a much smaller PRP effect following dual-task practice than following practice on Task 1 alone or Task 2 alone. The actual results, however, show no advantage for dual-task practice over Task 1 practice (in fact, the trend was in the opposite direction). The results also argue against task automatization. If the tasks had been automatized with practice, the bottleneck should have been bypassed for all three training groups, leaving little or no PRP effect. Contrary to this prediction, all groups showed large PRP effects, especially the group that practiced Task 2 alone.

Experiment 2 (reverse task order)

Because Task 1 practice was more effective than Task 2 practice, the results from Experiment 1 support the intact bottleneck hypothesis. There is, however, a confound that needs to be considered. Specifically, Task 1 always required a tone judgment and Task 2 always required a character judgment. Thus, based on Experiment 1 alone, it is impossible to determine whether PRP reduction with practice depends on the task position (Task 1 vs. Task 2) or task type (tone vs. character). In addition to this specific concern it is, of course, generically valuable to replicate results under different conditions.

To address these concerns, we reversed the task order in Experiment 2: The character task became Task 1 and the tone task became Task 2. In all other respects, the design was identical to that of Experiment 1. Note that most previous studies using auditory–vocal and visual–manual tasks have utilized the task order of Experiment 1; studies using the opposite order are relatively rare. According to the intact bottleneck hypothesis, the PRP effect should still be smaller following practice on Task 1 alone than following practice on Task 2 alone, even though the particular judgments (character vs. tone) assigned to these tasks have been exchanged. Furthermore, we should still find no evidence of the elimination of the bottleneck.

With the task order reversed, a bottleneck would produce a different pattern of interactions with SOA. The character intensity and S–R compatibility manipulation effects are now planted in Task 1 and, therefore, should carry over fully onto RT2 at short SOAs. The bottleneck model predictions for the difficulty of the tone task (now Task 2) are less clear, because the processing locus of this effect is not known. If this factor primarily influences the central stage of Task 2 (i.e., stage 2B), the effects on RT2 should be constant across SOAs. If this factor instead influences the input stage of Task 2 (i.e., stage 2A), the effects on RT2 should be smaller at short SOAs than at long SOAs (due to absorption into cognitive slack).

Method

Except for reversing the roles of the tone task (now Task 2) and the character task (now Task 1), the method was identical to that of Experiment 1. The 18 participants in this experiment did not participate in Experiment 1.

Results

Dual-task group only (training test)

We first report the results from the group in Experiment 2 that performed dual-task sessions during both the training and test phases, shown in Fig. 2 (asterisk symbols). Declines in the PRP effect tracked declines in RT1 approximately one for one across sessions, just as found in Experiment 1 and Van Selst et al. (1999). The best-fitting regression line with a slope of 1.0 was $\text{PRP} = \text{RT1} - 218$; this line is shown in Fig. 2. If we instead allow the slope to take on any value, the best fitting regression line is $\text{PRP} = 1.06 * \text{RT1} - 252.8$. Allowing the slope to vary does not account for a significant amount of variance beyond that already accounted for with a slope fixed at 1.0, *F*(1,9) < 1, MSE = 296.1, *p* > .05. These findings are consistent with the intact bottleneck hypothesis.

The points for Experiment 2 generally lie about 100 ms above those for Experiment 1. Why might this have occurred? A priori, faster input processing for Task 2 (faster stage 2A) in Experiment 2 than in Experiment 1 (that is, faster tone classification than character classification) would have been expected because auditory information should get to the cortex faster than visual information and because an easy physical discrimination (tone pitch) should be faster than one based on learned categories (character identification). Such a difference in the duration of stage 2A would increase the size of the PRP effect for a given RT1; hence, it would produce the observed differences in Fig. 2.

Training phase

Table 3 shows mean RT1 and RT2 across the eight training sessions for the three groups. Not surprisingly, RTs decreased steadily across sessions. Also, mean RT1 again tended to be longer for the dual-task training group than for the single-task training group, which might reflect a cost of having to initially prepare two tasks rather than just one.

[Table 3 will appear here. See end of document.]

Task 1 RT in test sessions

Figure 6 shows mean RT1 and RT2 as a function of SOA for each training group. Averaged across the four test sessions, mean RT1 was fastest for the groups that had practiced Task 1 in the training phase (550 ms for the Task 1 training group and 528 ms for the dual-task training group), and slowest for the group that did not (609 ms for the Task 2 training group). With only six participants per group, however, this difference did not quite reach statistical significance across participants, *F*(2,15) = 1.672350, MSE = 904,083.5, *p* = .22. A follow-up analysis revealed that the effect of previous training on Task 1 RT was large during the initial test session, but then shrank over the next few test sessions. As discussed in the “Task 1 carry-over” section below, mean RT1 depended on both Task 1 contrast, *F*(1,15) = 147.0, MSE = 4,336.3, *p* < .001, and Task 1 S–R compatibility, *F*(1,15) = 54.7, MSE = 108,520.6, *p* < .001. There was also a significant three-way interaction between these two factors and Task 2 tone difficulty, *F*(1,15) = 6.9, MSE = 2,192.2, *p* < .05; Task 2 tone difficulty had only a 1-ms effect on RT1 overall, but it ranged between −6 and +6 ms depending on the compatibility and/or contrast of Task 1. Because these effects were small we will not attempt to interpret them. These same three factors also interacted significantly with the type of training, $F(2,15) = 3.8$, $p < .05$; the effects are also small and suggest no obvious explanation.

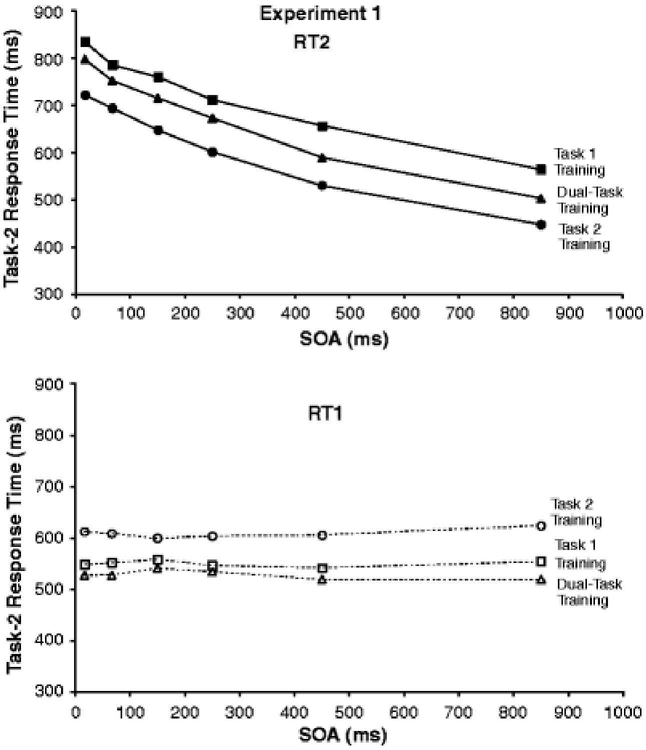

Fig. 6 Response time data for Task 1 and Task 2 during the dual-task test sessions of Experiment 2 as a function of the SOA and training group. *Triangles* dual-task training group, *squares* Task 1 training group, *circles* Task 2 training group.

PRP effect on Task 2 in test sessions

A critical question in this experiment is how the type of training influenced the size of the PRP effect in the test sessions. The mean PRP effect was 269 ms for the Task 1 practice group, 274 ms for the Task 2 practice group, and 293 ms for the dual-task practice group. Thus, in sharp contrast to Experiment 1, the type of training appeared to have little impact on the amount of PRP reduction. Indeed, the interaction between training group and SOA did not approach significance overall, $F(10,75) < 1$, or when comparing only the shortest and longest SOAs, $F(2,15) < 1$.

PRP effect vs. RT1 for individuals

Figure 7 shows the PRP effect during the test sessions as a function of RT1 for each participant in Experiment 2. As a baseline against which to assess the performance of individual participants, Fig. 7 shows the regression line (PRP = RT1 − 218) derived from the 12 sessions of the dual-task training group. Many of the individual participants fell reasonably close to this regression line, but several clearly did not. This latter subset of participants (filled symbols) shows a much smaller PRP effect than would be predicted based on their mean RT1, and therefore are candidates to have eliminated the bottleneck. We return to this issue later, after presenting additional evidence regarding factor interactions with SOA.

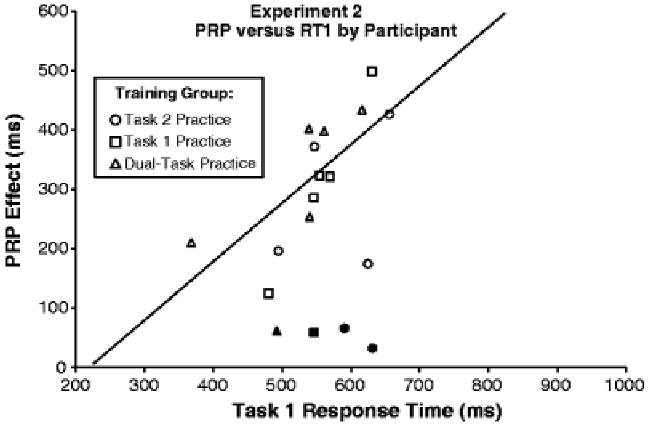

Fig. 7 Mean PRP effect versus mean RT1 for each participant in Experiment 2. *Triangles* dual-task training group, *squares* Task 1 training group, *circles* Task 2 training group. The points representing the 4 participants suspected of having bypassed the bottleneck are filled; the points representing the other 14 participants are unfilled.

Task 1 carry-over prediction

The bottleneck model predicts that manipulations of bottleneck or pre-bottleneck stages of Task 1 should carry over fully onto RT2 at short SOAs but not at long SOAs. There were two Task 1 variables in Experiment 2 that we can use to test the carry-over prediction: Contrast and S–R compatibility.

The effect of Task 1 contrast on RT to Task 1 itself was 31.4 ms and its effect on RT2 was 10.6, 20.3, 23.1, 18.0, 22.9, and 3.0 ms at the 17, 67, 150, 250, 450, or 850 ms SOAs respectively. Thus, contrary to the central bottleneck model prediction, the effect of contrast on Task 1 did not appear to carry over fully onto RT2 at the short SOAs. Furthermore, the interaction on RT2 between contrast and SOA (short vs. long) was not statistically significant, $F(1,15) < 1$. There was no three-way interaction between training group, Task 1 S–R compatibility, and SOA, $F(2,15) = 1.31$, MSE = 20,326.6, $p > .2$.

Because the effect of Task 1 S–R compatibility on RT1 (96 ms) was much larger than the effect of Task 1 contrast (31 ms), it provides an even better opportunity to evaluate the Task 1 carry-over prediction. Figure 8 shows the carry-over of Task 1 compatibility effects onto RT2 for each training group (filled symbols) as well as the mean Task 1 compatibility effect on RT1 (averaged across SOAs; unfilled symbols). Averaged across the three training groups, the effect of Task 1 compatibility was 85.2, 71.4, 61.8, 66.9, 41.8, and 10.3 ms at the 17, 67, 150, 250, 450, or 850 ms SOAs respectively. This interaction between Task 1 compatibility effects and SOA (short vs. long) was significant, $F(1,15) = 14.9$, MSE = 20,326.6, $p < .01$. Although the carry-over effect was large at the shortest SOAs, it was not quite as large as the effect on Task 1 itself (96 ms). It appears that Task 1 compatibility effects, like Task 1 contrast effects, did not carry over fully onto RT2 at the short SOAs.

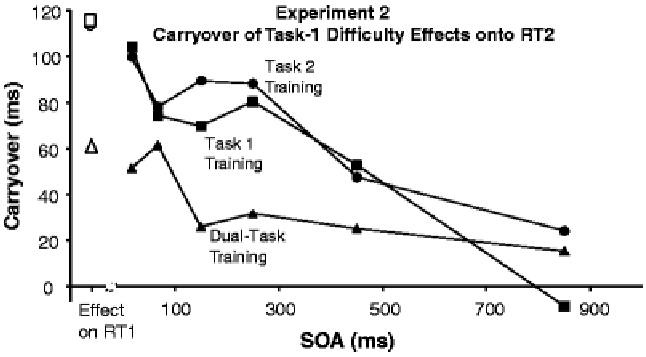

Fig. 8 Effect of Task 1 difficulty manipulation (compatible versus incompatible stimulus–response mapping) on Task 2 response time at each SOA in Experiment 2 (*filled symbols*). For comparison, the mean effect of Task 1 difficulty on Task 1 response time is also shown on the far left (*unfilled symbols*). *Triangles* dual-task training group, *squares* Task 1 training group, *circles* Task 2 training group.

Task 2 tone difficulty effects

In Experiment 2, the only Task 2 factor was tone task difficulty—whether the tones were close to or far from the boundary between high- and low-pitched tones. The effect of this variable on RT2 was 46 ms at both the shortest SOA and the longest SOA, $F(1,15) < 1$. There was also no three-way interaction between these variables and training group, $F(1,15) < 1$. According to bottleneck models, these results indicate that the Task 2 stage affected by the tone task difficulty manipulation occurred at or after the bottleneck. The most plausible conclusion is that this factor primarily influenced the duration of tone task response selection (stage 2B).

Task 1 error rates

Participants made more Task 1 errors when the S–R mapping was incompatible (5.0%) than when it was compatible (2.9%), $F(1,15) = 21.0$, MSE = 140.6, $p < .001$. The interaction between Task 1 compatibility, Task 2 tone difficulty, and training type was also significant, *F*(2,15) = 3.72, MSE = 51.8, *p* < .05; these effects were small and suggest no clear interpretation.

Task 2 error rates

Participants made more Task 2 errors with the difficult tones (6.8%) than with the easy tones (1.9%), *F*(1,15) = 16.4, MSE = 944.7, *p* < .1. They also made slightly more Task 2 errors when the Task 1 S–R mapping was incompatible (4.7%) than when it was compatible (4.0%), *F*(1,15) = 5.2, *p* < .05.

Discussion

Contrary to the results of Experiment 1, the mean PRP effect following Task 2 practice (274 ms) was similar to that obtained after an equivalent amount of Task 1 practice (269 ms) or dual-task practice (293 ms). These results do not match the prediction of the intact bottleneck hypothesis that Task 1 practice (either by itself or in the dual-task condition) should greatly reduce the PRP effect, but Task 2 practice should not. Another problem for this hypothesis is the incomplete carry-over of Task 1 factor effects onto RT2. In addition, several individual participants showed PRP effects that were hundreds of milliseconds less than the intact bottleneck hypothesis would predict based on their mean RT1.

The data also do not support the task integration hypothesis, which predicts that dual-task practice should be much more beneficial than single-task practice. In fact, dual-task practice actually produced the largest PRP effect (although the differences between training conditions were not significant). Thus, Experiment 2 again provides no support for the task integration hypothesis.

The task automatization hypothesis predicts that all forms of practice (single and dual-task) should allow bottleneck bypass and thus produce small PRP effects. Contrary to this prediction, all three groups showed large PRP effects (nearly 300 ms). Thus, the overall data do not support the task automatization hypothesis either.

In summary, each of the three main hypotheses, taken alone, provides a poor fit to the overall data from Experiment 2. How, then, can these results be explained? An examination of individual differences suggests a surprisingly simple answer: Some participants eliminated the bottleneck and others did not. In other words, there is a mixture of participants who reduced PRP effects through stage-shortening alone (leaving the bottleneck intact but shorter in duration) and participants who reduced PRP effects through task automatization.

Evidence of two distinct subgroups

Our first indication that some participants had eliminated the bottleneck came from an analysis of response order. Because the bottleneck model assumes that Task 1 central operations are performed before Task 2 central operations, the responses should be emitted in this same order; this prediction was confirmed in Experiment 1 and in many previous PRP studies. Without a bottleneck, however, it is entirely possible for Task 2 responses to be emitted before Task 1 responses at the shortest SOA; in fact, because Task 2 took less time than Task 1 (when performed alone), response reversals should occur more often than not. Indeed, four participants in Experiment 2 reversed responses on a very high proportion of trials (66–98%), often responding to Task 2 a few hundred milliseconds before Task 1. None of the other 14 participants reversed responses on more than 8% of trials. This enormous gap in the percentage of response reversals between members of the two groups strongly supports a categorical difference between these groups.

The four participants with high frequencies of response reversals also happened to show considerably smaller PRP effects (range: 33–67 ms) than any of the remaining 14 participants (range: 125–505 ms). Even more importantly, they produced smaller PRP effects than the bottleneck model would predict based on their mean RT1, by roughly 300 ms. In Fig. 7, these four participants correspond to the four data points (filled symbols) that lie the furthest below the regression line. There were a few additional participants with PRP effects below the regression line, but none of them frequently reversed the response order. Thus, there are only four participants with clear evidence of having eliminated the bottleneck. For the sake of brevity, we call these four individuals “bypassers” and the remaining 14 participants “bottleneckers.”

As an additional indicator of whether the bottleneck was bypassed, we computed the amount of Task 1 carry-over for the bypassers and bottleneckers. If the bottleneck has been eliminated, there is no obvious reason for Task 1 difficulty effects to strongly influence RT2. But if there is a bottleneck, the effects of the manipulations should carry over fully onto RT2 (as discussed above). For the bypassers, the effect of Task 1 S–R compatibility effect was 62 ms on Task 1, but only 13 ms on Task 2. For the bottleneckers, however, the Task 1 S–R compatibility effect on RT1 was 103 ms, and this effect carried over fully onto RT2 (109 ms). These findings further support our classification of participants as bypassers and bottleneckers. They also explain why the overall data showed signs of incomplete carry-over: The data represented a mixture of four participants who bypassed the bottleneck (producing very little carry-over) and 14 participants who did not (producing complete carry-over). Table 4 summarizes the differential amounts of carry-over between the two groups of participants, as well as the other prominent empirical differences mentioned above.

[Table 4 will appear here. See end of document.]

Attempts to reconcile the bottleneck model with small PRP effects

To rescue the bottleneck model, it might be argued that the bypassers performed Task 1 central operations very quickly, before Task 2 central operations were even ready to begin (i.e., the bottleneck was latent; Ruthruff, Pashler, & Hazeltine, 2003). However, this account is implausible given the relatively long mean RT1 (563 ms). Another possibility is that the bypassers generally reversed the central processing order (Task 2 before Task 1). This hypothesis correctly predicts the high frequency of response reversals and the near elimination of interference on Task 2. However, it then must predict dual-task interference on Task 1. Contrary to this prediction, mean RT1 was 565, 565, 572, 559, 551, and 564 ms at the 17, 67, 150, 250, 450, and 850 ms SOAs respectively.

It is also conceivable that the limitation underlying the bottleneck still held, but participants circumvented it by performing central operations on only one task per trial (guessing randomly on the other). This hypothesis predicts that the bypassers should show a dramatic increase in error rates. In actuality, there was only a slight trend for the bypassers to make more errors than the bottleneckers (5.3 vs. 4.0% on Task 1; 4.2 vs. 3.6% on Task 2), with considerable overlap in error rates between members of the two groups. Because we see no way to reconcile the data from these four participants with the bottleneck model, we instead conclude that they found a way to select and execute responses to both tasks at the same time.

Factors that promote bottleneck bypass

Clear evidence of bottleneck elimination is extremely rare in the PRP literature (as discussed in more detail below). It is important, therefore, to consider what factors may have facilitated bypassing the bottleneck in Experiment 2. The most obvious enabling factor is the high level practice (4,480 training trials plus 2,240 dual-task test trials per participant). Another potentially important factor is the absence of input modality conflicts and output modality conflicts. A third factor is the use of particular input/output modality pairings (auditory–vocal and visual–manual) that minimize dual-task interference (see Hazeltine, Ruthruff, & Remington, 2004; Shaffer, 1975; Van Selst & Johnston, 1997). Although these factors are likely to be important, even necessary, for eliminating the bottleneck, they clearly are not sufficient. These factors were present for all participants in all conditions of both experiments, yet bottleneck elimination occurred in only a few cases. Below we consider several other factors that might help explain when the bottleneck can be bypassed.

Training condition does not appear to be a critical factor. In Experiment 2, at least 1 out of the 6 participants in each of the three training groups appeared to bypass the bottleneck (1 in the Task 1 only group, 2 in the Task 2 only group, and 1 in the dual-task group). Although more participants eliminated the bottleneck following training on Task 2 alone, this difference was not statistically significant. Given the high costs of running these experiments (12 sessions per participant), it would be prohibitive to test much larger samples to definitively assess differential effects of training condition. However, to improve our estimate of the extent to which Task 2 training promotes bottleneck elimination, we instructed an additional six participants to perform under this condition. Only 1 of these 6 additional participants showed evidence of bottleneck elimination, as in the original Task 1 practice and dual-task practice groups. Thus, there is no evidence that any particular training condition is critical for eliminating the bottleneck.

One factor that did appear to strongly influence the likelihood of eliminating the bottleneck was task order. With the task order of Experiment 1 (Task 1: Auditory–vocal; Task 2: Visual–manual) there was no evidence that any of the 18 participants were able to eliminate the bottleneck. Furthermore, none of the six participants in Van Selst et al. (1999) were able to eliminate the bottleneck with this task order. With the opposite task order (Task 1: Visual–manual; Task 2: Auditory–vocal; Experiment 2), however, there was strong evidence that 4 out of 18 participants were able to eliminate the bottleneck. In the follow-up experiment discussed above, using the Task 2 training condition of Experiment 2, another participant (1 out of 6) also bypassed the bottleneck. Combining all of these results together, 0 out of 24 participants eliminated the bottleneck with the typical task order, but 5 out of 24 participants eliminated the bottleneck with the reverse task order.[2] This finding suggests that future studies take task order into consideration, although it is not yet clear whether this factor will be important with other task pairs.

Another critical factor might be an individual's skill level in performing Task 1 and/or Task 2, which should be reflected in the mean RTs. The mean RT1 was 563 ms for bypassers and 564 ms for bottleneckers. Thus, Task 1 skill appears not to be important. The mean RT2 at the baseline long SOA, however, was much smaller for bypassers (358 ms) than for bottleneckers (572 ms),

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[2] A chi-squared test revealed that this difference is statistically significant, $\chi^2(1) = 5.581$, $p < .05$. The difference between experiments is also significant using Fisher's exact test ($p = .0497$, two-tailed), which is more appropriate given cell frequencies less than 5.

t(16) = 3.71, $p < .01$. Thus, it appears that Task 2 (tone task) skill is crucial. As discussed below, this finding supports a variant of the task automatization hypothesis in which the tone task was automatized but the character task was not.[3]

Re-examining PRP effects as a function of training group

As noted above, the results from the 14 bottleneckers matched the predictions of the intact bottleneck hypothesis (e.g., large PRP effects and full carry-over of Task 1 difficulty effects). We now examine whether, after removing the data from the four bypassers, the intact bottleneck hypothesis also correctly predicts the pattern of PRP effects across the three training conditions. To improve the reliability of the estimates, this analysis included not only the 14 bottleneckers from the main experiment, but also the 5 bottleneckers from the follow-up experiment with Task 2 training. The resulting PRP effect was 311 ms for the Task 1 training group (n=5), 357 for the Task 2 training group (n=9), and 340 ms for the dual-task training group (n=5). Consistent with the intact bottleneck hypothesis, the groups that had practiced Task 1 tended to produce less PRP effect than the group that had not. Although the differences in PRP effects were modest, they were roughly in accord with the differences in mean RT1 among the groups (557, 617, and 523 ms respectively), as predicted by the intact bottleneck hypothesis.

General discussion

We conducted two transfer-of-training experiments to test hypotheses about why practice dramatically reduces PRP interference. In particular, we wanted to determine whether practice causes task integration, task automatization, or merely shortens the duration of component stages (leaving the processing bottleneck intact). Each experiment consisted of a training phase and a test phase. During the training phase (eight sessions), participants were assigned to one of three training conditions: Task 1 only, Task 2 only, or dual-task (in a PRP design). During the test phase, all participants performed four dual-task sessions. Experiment 1 presented an auditory–vocal task as Task 1 and a visual–manual task as Task 2. Experiment 2 used the opposite task order. The

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[3] One complication for the hypothesis that Task 2 automatization was the key to bypassing the bottleneck is that one of the bypassers had not practiced Task 2 during the training phase. Nevertheless, this individual did respond very quickly to Task 2 at the long SOA (M = 376 ms) but not to Task 1 (M = 527 ms). It is possible that this person was naturally skilled at Task 2 and was able to achieve automatization relatively quickly (e.g., during the practice session).

critical empirical questions were how the different training types would influence the PRP effect and whether they would allow participants to eliminate the processing bottleneck.

Task integration hypothesis

According to the task integration hypothesis, dual-task practice (but not single-task practice) allows participants to efficiently integrate performance of the two tasks, possibly eliminating the bottleneck. Accordingly, the dual-task training condition should produce the greatest reduction in the PRP effect. Contrary to this prediction, dual-task interference in Experiments 1 and 2 was no smaller following dual-task practice than following single-task practice on Task 1. Hence, the present data provide no evidence of task integration. It remains possible, of course, that integration occurs under different circumstances (i.e., with even higher levels of practice or with tasks closely tied to a common goal, such as braking and steering a car).

Intact bottleneck hypothesis

According to the intact bottleneck hypothesis, practice does not lead to task integration or automatization. Although the bottleneck remains intact with practice, its duration can decline (producing a smaller PRP effect) as the duration of certain Task 1 stages (1A and 1B) become shorter. Accordingly, Task 1 practice and dual-task practice should greatly reduce dual-task interference, but Task 2 practice should not. This prediction was confirmed in Experiment 1 (Task 1: Auditory–vocal; Task 2: Visual–manual). The data further confirmed the predictions that Task 1 difficulty effects should carry over fully onto Task 2 at short SOAs, and that the PRP effect and RT1 should be linearly related across practice sessions (see Fig. 2) and across individuals (see Fig. 4). There was no evidence that any individual participant in Experiment 1 was able to eliminate the bottleneck. Thus, with this task order, we conclude that practice reduced dual-task interference solely by shortening Task 1 stage durations, leaving the bottleneck intact. This conclusion is the same one reached by Van Selst et al. (1999), who also studied dual-task practice effects with the same tasks in the same order.

In Experiment 2, we used a task order opposite to that of Experiment 1. This particular task order (Task 1: Visual–manual; Task 2: Auditory–vocal) has rarely been investigated in dual-task studies, but we used it here to unconfound the effects of Task 1/Task 2 practice from the effects of tone/character task practice. On the one hand, we found evidence that for the majority of participants (14 out of 18) the bottleneck remained intact. On the other hand, we also found clear evidence that a minority of participants (4 out of 18) had completely eliminated the bottleneck.

As discussed in Experiment 2 and summarized in Table 4, this division of participants into "bypassers" and "bottleneckers" is supported by several converging measures. First, the bypassers showed very small PRP effects (only 55 ms, compared with 342 ms for the bottleneckers), which were roughly 200 ms smaller than the intact bottleneck hypothesis would have predicted based on their mean RT1 (see the four filled data points in Fig. 7). Second, each bypasser responded to the tasks in an order opposite to the presentation order (often by a few hundred milliseconds) on at least 66% of trials at the shortest SOA. Third, the bypassers showed very little carry-over of Task 1 difficulty effects onto RT2. These findings strongly suggest that, contrary to the intact bottleneck hypothesis, the bypassers were able to select responses to the two tasks virtually independently.

Task automatization hypothesis

According to the task automatization hypothesis, practice (either single-task or dual-task) allows participants to learn how to perform tasks without limited central resources and thus bypass the central bottleneck. Thus, all three types of training should produce very small PRP effects. Contrary to this prediction, we observed large mean PRP effects for all the training types in both experiments. Thus, the task automatization hypothesis, by itself, is inconsistent with the overall results. Looking at individuals, this hypothesis is inconsistent with results from all of the participants in Experiment 1 and 14 of 18 participants in Experiment 2. However, it can explain the results of the 4 participants in Experiment 2 who bypassed the bottleneck despite having practiced only one of the two tasks.

Which task was automatized?

We noted above that automatization of either task could, in principle, allow participants to bypass the bottleneck. Therefore, it is possible that the bypassers in Experiment 2 had automatized just one of the two tasks. Consistent with this possibility, the bypassers responded much more quickly than the bottleneckers to Task 2 (even at the long SOA) but not to Task 1. This observation suggests that the bypassers had automatized Task 2 (the tone task) but not Task 1 (the character task).

The tone task might have been more susceptible to automatization because it had less need of central bottleneck resources to begin with, either because the stimulus classification was based on a physical property (tone pitch) rather than an abstract, learned property (character identity), or because the S–R mapping was highly compatible (say "high" to a high tone) rather than incompatible (the visual task had an incompatible mapping for half the stimuli). A different possibility is that auditory stimuli are better than visual stimuli at gaining access to the bottleneck bypass route (e.g., a “jumper-cable” route) because auditory stimuli more readily “attract attention to themselves” (cf. Posner, Nissen, & Klein, 1976; Wickens & Liu, 1988). Further research is needed to determine which of these factors (ease of stimulus classification, S–R compatibility, difference in alerting ability) is the key to facilitating task automatization. A long-term goal is to determine how tasks can be automatized and why some people can accomplish this better than others.

What caused the effects of task order?

When we presented the auditory–vocal task first and the visual–manual task second, none of the 24 participants (pooling together Experiment 1 and Van Selst et al., 1999) produced clear evidence of having bypassed the bottleneck. Using this same task order and a sample of six younger adults and six older adults, Maquestiaux, Hartley, and Bertsch (in press) also found that participants had bypassed the bottleneck after practice. When we used the opposite task order, however, 5 out of 24 participants (pooling together Experiment 2 and the associated control experiment) produced clear evidence of having bypassed the bottleneck. As noted in the Discussion section of Experiment 2, this effect of task order was statistically significant. Interestingly, a similar effect of task order was observed in Johnston and Delgado (1993) when an unpracticed but exceptionally easy task was used as either Task 1 or Task 2 (see below for further discussion of their findings).

How can we explain the effects of task order on the probability of eliminating the bottleneck? Above we proposed that the tone task was automatized for some participants. Because automatization of either Task 1 or Task 2 should eliminate conflict for the central bottleneck resource, this hypothesis incorrectly predicts bottleneck elimination with both task orders (not just when the tone task served as Task 2). Clearly, some additional assumptions are needed to explain task order effects.

One attractive hypothesis to explain task order effects is that tasks recruit available central resources even when they are not needed to perform that task. To illustrate this *greedy resource usage hypothesis*, consider the analogy of a bank teller (a “bottleneck” mechanism that can serve only one customer at a time). Suppose that two customers arrive at the bank, one who requires the teller (e.g., to perform a wire transfer) and one who does not (e.g., to withdraw cash). If the person who does not require the teller arrives first he might, for convenience, engage the teller even if there is a nearby automated teller machine (ATM). Once this customer has engaged the teller, the other customer (who requires teller assistance) must wait (a bottleneck delay). Now consider the opposite arrival order, where the customer requiring teller assistance arrives first and engages the teller. When the other customer arrives, he cannot immediately use the teller. Instead of waiting, however, he could simply utilize the nearby ATM to withdraw cash, thus avoiding a bottleneck delay. Following this analogy, the greedy resource usage hypothesis predicts an asymmetry in the PRP paradigm, such that a bottleneck delay can be avoided only with one task order (where the task that can use the bypass route serves as Task 2).

If we now combine the greedy resource usage hypothesis with the above conclusion that the tone task is more amenable to automatization with practice than the letter task, it follows that bottleneck bypass would be more likely to occur when the tone task serves as Task 2 (as in Experiment 2) than when it serves as Task 1 (Experiment 1). Thus, this hypothesis can explain the task order effects observed in our study (and those of Johnston & Delgado, 1993). At the same time, we acknowledge that this hypothesis is post-hoc and requires further testing.

Practice can (sometimes) eliminate the bottleneck

One of the most important findings of the present study is that it is indeed possible to eliminate the central bottleneck with practice. Prior to this study, there was little published evidence that this was possible. Many early studies had found that practice had very little influence on the PRP effect. Van Selst et al. (1999) later showed that practice can dramatically reduce PRP effects when input and output conflicts were eliminated. Interestingly, one participant showed no PRP effect all. This participant's mean RT1 was so short (~250 ms), however, that even the central bottleneck could have been latent, producing little or no observable PRP interference. Ruthruff, Johnston, Van Selst, Whitsell, & Remington (2003) later confirmed the latent bottleneck account of this participant's data. When the SOA between tasks was adjusted to bring the central operations of the two tasks more closely into alignment, PRP interference returned.

Small dual-task interference effects have also been observed after practice in a non-PRP paradigm where participants perform pure blocks that contain just one task or the other and mixed blocks that contain some single-task trials and some dual-task trials with a 0-ms SOA (Hazeltine et al., 2002; Levy & Pashler, 2001; Schumacher et al., 2001). However, these small dual-task effects were obtained with an unusually easy task: Participants simply pressed a button that corresponded spatially with the location of a visual stimulus. Consequently, mean RTs were typically less than 300 ms. It is quite possible that the bottleneck was not eliminated in these studies, but instead was latent (see Hazeltine et al., 2002 for relevant discussion). Interestingly, a few studies using this dual-task paradigm but with more difficult tasks have often found longer mean RTs and larger amounts of dual-task interference (Levy & Pashler, 2001, Experiment 2; Schumacher et al., 2001, Experiment 3; but see also Hazeltine et al., 2004, Experiment 3).

In considering the *latent bottleneck* hypothesis, it is critical that the bypassers in the present Experiment 2 showed so little PRP effect despite having relatively long mean RT1s (between 492 and 631 ms). Long RT1s are usually the result of long central stage durations. Accordingly, it is implausible that interference was avoided simply because the two tasks never demanded central operations at the same time, especially given that we used multiple SOAs across a wide range (17 to 800 ms). It is also important to note that, unlike many previously published studies, we did not base our inferences solely on the magnitude of PRP effects. Instead, we also examined the PRP/RT1 relationship, several factor effects, and the frequency of response reversals. Future studies designed to look for bottleneck bypass should also use these converging indicators.

Previous evidence of the elimination of the bottleneck

Having found clear evidence of bottleneck elimination in Experiment 2, we now take a step back and consider how this finding fits in with the previous evidence (albeit scarce) of bottleneck elimination in RT paradigms. In doing so, it is helpful to classify choice RT tasks according to the compatibility of the S–R translation and, hence, their demand on central operations. The easiest tasks are *ideomotor compatible tasks*, such as shadowing (repeating aloud whatever word you hear) where the stimulus codes closely resemble the response codes (Greenwald, 1972). Intermediate in difficulty are *compatible tasks*, where the stimulus codes and response codes do not resemble each other but have some natural relationship (e.g., in our tone task participants said “low” to low-pitched tones and “high” to high-pitched tones). The next highest tier of difficulty would include *arbitrary tasks*, where there is no pre-learned association between stimuli and responses. The highest tier of difficulty includes *incompatible tasks*, where the mapping opposes some pre-learned association.

For the easiest task category (ideomotor compatible), the bottleneck might be expected to generally be bypassed. Interestingly, several studies have reached the opposite conclusion (e.g., Lien, McCann, Ruthruff, & Proctor, in press; Lien, Proctor, & Allen, 2002). Other studies have reported small PRP effects (e.g., Greenwald, 1972; Greenwald & Shulman, 1973; Greenwald, 2003), but because RTs were so short it is difficult to rule out a latent bottleneck account (see Lien, Proctor, & Ruthruff, 2003). Pashler, Carrier, and Hoffman (1993; Experiments 1 and 2), however, did find strong evidence for bottleneck bypass when Task 1 was the usual tone task and Task 2 required an eye movement to a visual stimulus (an ideomotor compatible task). Interestingly, the pattern of results was similar to that of the bypassers in Experiment 2: Although the PRP effects were greater than zero, participants generally responded to Task 2 well before Task 1 at short

SOAs. Johnston and Delgado (1993) found similar evidence of bottleneck bypass with a compatible Task 1 and an ideomotor Task 2 that required continuous tracking of a visual stimulus using a joystick. At the same time, they failed to find clear evidence of bypass with several other tracking tasks, so ideomotor compatibility is apparently not a sufficient condition for bottleneck bypass. Interestingly, Johnston and Delgado also failed to find clear evidence of bottleneck bypass when the continuous tracking task served as Task 1. This finding provides further support for our greedy resource usage hypothesis, which predicts that bottleneck bypass should occur only when the automatized task serves as Task 2.

We are aware of no previous studies with clear evidence of the elimination of the bottleneck with either compatible, arbitrary, or incompatible tasks. The present study, however, did find such evidence. Our tone task used a compatible stimulus–response mapping, whereas our character task used a compatible/incompatible mapping (depending on whether the character was a digit or a letter). It should be noted, however, that bottleneck elimination occurred only for a subset of participants, only with one task order, and only after extensive practice.

Summary

The present study provided no evidence that dual-task practice allows participants to efficiently integrate task pairs. Instead, the primary effect of practice was to shorten stages, leaving the bottleneck intact but shorter in duration. However, practice (single-task and dual-task) also allowed a few participants to automatize one or both tasks and avoid the central bottleneck.

These data represent the strongest evidence yet that practice can eliminate the bottleneck. This finding is an important lead for further research aimed at uncovering the nature of the processing bottleneck and how to circumvent it. At the same time, we should not lose sight of the fact that this intriguing finding required the combination of several favorable conditions. These conditions include extensive practice, relatively easy tasks (especially compared with many real-world tasks), an absence of input conflicts and output conflicts, and preferred modality pairings (auditory–vocal and visual–manual). Even with these favorable conditions, we found evidence of bottleneck elimination only with one of the two possible task orders. Even with the more favorable task order, it occurred only for a minority of participants (4 out of 18). Thus, at this point, elimination of the bottleneck is still a rare exception.

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Table 1 Task(s) performed in each session of Experiments 1 and 2 as a function of the training group (Single/Task 1, Single/Task 2, Dual-task). Each training condition used a different group of participants. In Experiment 1, Task 1 was the tone task (auditory–vocal) and Task 2 was the character task (visual–manual) task. In Experiment 2, the task order was reversed

Session		Training condition		
		Single/Task 1	Single/Task 2	Dual-task
Training	1	Task 1	Task 2	Task 1/Task 2
	2	Task 1	Task 2	Task 1/Task 2
	3	Task 1	Task 2	Task 1/Task 2
	4	Task 1	Task 2	Task 1/Task 2
	5	Task 1	Task 2	Task 1/Task 2
	6	Task 1	Task 2	Task 1/Task 2
	7	Task 1	Task 2	Task 1/Task 2
	8	Task 1	Task 2	Task 1/Task 2
Test	1	Task 1/Task 2	Task 1/Task 2	Task 1/Task 2
	2	Task 1/Task 2	Task 1/Task 2	Task 1/Task 2
	3	Task 1/Task 2	Task 1/Task 2	Task 1/Task 2
	4	Task 1/Task 2	Task 1/Task 2	Task 1/Task 2

Table 2 Mean response time for Task 1 (tone) and Task 2 (character) during the eight training sessions for each training group in Experiment 1. For the dual-task practice group, mean response times are based on the long stimulus onset asynchrony (*SOA*; baseline) condition only

Session	Training condition					
	Single/Task 1		Single/Task 2		Dual-task (long SOA)	
	Task 1 tone	Task 2 character	Task 1 tone	Task 2 character	Task 1 tone	Task 2 character
1	440	–	–	1,095	1,090	993
2	365	–	–	718	767	722
3	336	–	–	672	697	669
4	321	–	–	644	647	629
5	311	–	–	639	602	595
6	320	–	–	600	581	569
7	308	–	–	602	617	585
8	297	–	–	624	566	566

Table 3 Mean response time for Task 1 (character) and Task 2 (tone) during the eight training sessions for each training group in Experiment 2. For the dual-task practice group, mean response times are based on the long SOA (baseline) condition only

Session	Training condition					
	Single/Task 1		Single/Task 2		Dual-task (long SOA)	
	Task 1 character	Task 2 tone	Task 1 character	Task 2 tone	Task 1 character	Task 2 tone
1	592	–	–	597	845	726
2	554	–	–	564	664	633
3	522	–	–	549	620	556
4	501	–	–	549	588	509
5	525	–	–	595	589	500
6	459	–	–	533	567	516
7	450	–	–	522	558	509
8	424	–	–	503	558	538

Table 4 Differences in results between the two subgroups of participants in Experiment 2. *PRP* psychological refractory period, *RT2* response time to Task 2

	Bottleneck group (*n*=14)	No bottleneck group (*n*=4)
PRP effect	342 ms	55 ms
Response reversals	2%	82%
Carry-over of Task 1 compatibility effects onto RT2	106%	21%