Journal of Experimental Psychology:
Human Perception and Performance
2005, Vol. 31, No. 1, 122–144

DOI: 10.1037/0096-1523.31.1.122

Dual-Task Performance With Ideomotor-Compatible Tasks: Is the Central Processing Bottleneck Intact, Bypassed, or Shifted in Locus?

Mei-Ching Lien, Robert S. McCann, Eric Ruthruff
National Aeronautics and Space Administration
Ames Research Center

Robert W. Proctor
Purdue University

The present study examined whether the central bottleneck, assumed to be primarily responsible for the psychological refractory period (PRP) effect, is intact, bypassed, or shifted in locus with ideomotor (IM)-compatible tasks. In 4 experiments, factorial combinations of IM- and non-IM-compatible tasks were used for Task 1 and Task 2. All experiments showed substantial PRP effects, with a strong dependency between Task 1 and Task 2 response times. These findings, along with model-based simulations, indicate that the processing bottleneck was not bypassed, even with two IM-compatible tasks. Nevertheless, systematic changes in the PRP and correspondence effects across experiments suggest that IM compatibility shifted the locus of the bottleneck. The findings favor an *engage-bottleneck-later* hypothesis, whereby parallelism between tasks occurs deeper into the processing stream for IM- than for non-IM-compatible tasks, without the bottleneck being actually eliminated.

Cognitive psychologists have long sought to understand the performance limitations that produce dual-task interference, for these limitations may offer important clues to cognitive architecture. Considerable evidence now suggests that a key source of dual-task interference is a processing bottleneck that prevents central operations from proceeding on more than one task at a time (Pashler, 1984, 1994; Welford, 1952). One approach to understanding the mechanism(s) underlying this central bottleneck is to identify its boundary conditions: When is the bottleneck present, and when is it absent? Although the vast majority of dual-task studies have found interference consistent with a central bottleneck, a few studies have found interference small enough to suggest the absence of the bottleneck. One of these rare exceptions occurred when both tasks were ideomotor (IM) compatible, which is when “a stimulus corresponds to sensory feedback from its required response” (Greenwald, 1972, p. 52). Accordingly, Greenwald and a colleague have argued that IM-compatible tasks bypass the central processing bottleneck (see, e.g., Greenwald, 2003; Greenwald & Shulman, 1973).

In contrast to Greenwald (1972), several other dual-task studies have obtained substantial interference effects with IM-compatible tasks (e.g., Brebner, 1977; Gottsdanker & Stelmach, 1971; Lien, Proctor, & Allen, 2002). Thus, the existing literature does not clearly indicate whether dual-task interference is eliminated with IM-compatible tasks. A further limitation of these previous studies is that their inferences regarding the presence or absence of the bottleneck were based primarily on the presence or absence of dual-task interference. The problem with this logic is that a lack of dual-task interference does not necessarily indicate that the central bottleneck has been bypassed (see Ruthruff, Johnston, Van Selst, Whitsell, & Remington, 2003); likewise, the mere existence of interference does not necessarily implicate a central bottleneck. The present study, therefore, aimed to advance understanding of the effects of IM compatibility on dual-task performance through the use of a more diagnostic set of analyses based on cross-task correspondence effects, response time (RT) dependency between tasks, the relationship between dual-task interference and Task 1 RT, and model-based simulations. Before discussing our approach, however, we first provide background on the psychological refractory period paradigm, which is by far the most commonly used paradigm for the study of dual-task interference.

The Psychological Refractory Period Effect

Laboratory studies of performance limitations under dual-task conditions typically present two tasks—Task 1 (T1) and Task 2 (T2)—in rapid succession. The degree of task overlap is varied by manipulating the interval between the onsets of the T1 stimulus (S1) and the T2 stimulus (S2), known as the stimulus onset asynchrony (SOA). The standard finding is that RT for T1 (RT1) is constant across SOAs, but RT for T2 (RT2) increases dramatically as SOA decreases (i.e., as task overlap increases). This slowing of RT2 is commonly known as the *psychological refractory period* (PRP) effect (Telford, 1931).

The most widely accepted account of the PRP effect is the central bottleneck model, which assumes that central operations (such as response selection) for T2 do not start until central operations for T1 have finished (e.g., Pashler, 1984; Pashler &

Mei-Ching Lien, Robert S. McCann, and Eric Ruthruff, National Aeronautics and Space Administration (NASA) Ames Research Center, Moffett Field, California; Robert W. Proctor, Department of Psychological Science, Purdue University.

This research was supported by funding from the National Research Council and the Psychological and Physiological Stressors and Factors Project of NASA’s Airspace Systems Program.

Correspondence concerning this article should be addressed to Mei-Ching Lien, who is now at the Department of Psychology, Oregon State University, 204C Moreland Hall, Corvallis, OR 97331. E-mail: mei.lien@oregonstate.edu

Johnston, 1998; Welford, 1952).[1] Figure 1 illustrates the processing assumptions underlying the central bottleneck model at a short SOA. Stages 1A, 1B, and 1C correspond to the prebottleneck, bottleneck, and postbottleneck stages, respectively, of T1. Stages 2A, 2B, and 2C correspond to the same processes for T2. Because Stage 2B does not start until Stage 1B is completed, RT2 is delayed at short SOAs, causing a PRP effect. Using this notation, the central bottleneck model makes a simple prediction for the duration of RT2. Assuming that a bottleneck delay occurs on every trial at short SOAs but never at long SOAs (Pashler & Johnston, 1989),

$$\mathrm{RT2}_{\text{long SOA}} = 2\mathrm{A} + 2\mathrm{B} + 2\mathrm{C}, \tag{1}$$

and

$$\begin{aligned}\mathrm{RT2}_{\text{short SOA}} &= 1\mathrm{A} + 1\mathrm{B} + 2\mathrm{B} + 2\mathrm{C} - \mathrm{SOA}\\ &= \mathrm{RT1} - 1\mathrm{C} + 2\mathrm{B} + 2\mathrm{C} - \mathrm{SOA}.\end{aligned} \tag{2}$$

It follows that,

$$\begin{aligned}\mathrm{PRP} = \mathrm{RT2}_{\text{short SOA}} - \mathrm{RT2}_{\text{long SOA}} &= 1\mathrm{A} + 1\mathrm{B} - 2\mathrm{A} - \mathrm{SOA}\\ &= \mathrm{RT1} - 1\mathrm{C} - 2\mathrm{A} - \mathrm{SOA}.\end{aligned} \tag{3}$$

It can be seen from these PRP equations that increasing the duration of T2 central operations (i.e., Stage 2B) should not influence the magnitude of the PRP effect. This prediction has been confirmed in several studies (e.g., McCann & Johnston, 1992; Pashler & Johnston, 1989; Van Selst & Jolicœur, 1997; Van Selst, Ruthruff, & Johnston, 1999; but see also Schumacher et al., 1999). These equations also imply that changes in the duration of T1 central operations (i.e., Stage 1B) should influence RT1 and the PRP effect by the same amount. Van Selst et al. confirmed this prediction with a T1 difficulty manipulation thought to prolong Stage 1B. They also found that T1 practice (thought primarily to reduce the duration of Stage 1B; see, e.g., Pashler & Baylis, 1991) had roughly equal effects on RT1 and the PRP effect (see also Ruthruff, Johnston, & Van Selst, 2001; Ruthruff et al., in press); specifically, the PRP effect was linearly related to RT1 across sessions, with a slope very close to 1. A related implication of the above PRP equations is that when Stage 1B is very short, the PRP effect should be very small or even approach zero (depending on the relative values of 1A and 2A; see Equation 3). Van Selst et al. (1999) estimated that, under the conditions they studied, the PRP effect would approach zero when RT1 is about 258 ms (see their Figure 5).

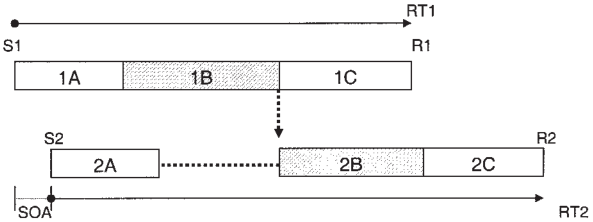

Figure 1. A bottleneck model of dual-task performance. The key assumption is that Stage 2B does not start until Stage 1B is completed. Consequently, response time for Task 2 (RT2) is delayed at short stimulus onset asynchronies (SOAs), causing the psychological refractory period effect. 1A, 1B, and 1C are the prebottleneck, bottleneck, and postbottleneck stages, respectively, of Task 1. 2A, 2B, and 2C are the corresponding processes for Task 2. S1 = stimulus for Task 1; S2 = stimulus for Task 2; R1 = response for Task 1; R2 = response for Task 2; RT1 = response time for Task 1.

The close relationship between the PRP effect and RT1 has important implications for all PRP studies in which RT1 is relatively short (including studies with IM-compatible tasks). Given that a short RT1 normally indicates a short central stage on T1 (i.e., Stage 1B), one would expect a small PRP effect even if the bottleneck has not been bypassed. The PRP effect would be reduced even further if one also placed participants under high speed stress, because this would presumably reduce RT1 even further (see Lien, Proctor, & Ruthruff, 2003). Because bottleneck models can predict small PRP effects, it follows that such findings (by themselves) cannot necessarily be taken as evidence for bottleneck bypass.

Do IM-Compatible Tasks Bypass the Central Bottleneck?

The PRP effect is very robust, having been observed even with tasks that have no input conflicts or output conflicts and even with relatively easy tasks that have high stimulus–response (S-R) compatibility (e.g., Brebner, 1977; Smith, 1967; Way & Gottsdanker, 1968). It has been proposed, however, that the PRP effect can be virtually eliminated with a certain class of very easy tasks that have a property known as *IM compatibility* (e.g., Greenwald & Shulman, 1973), even with little or no practice. One common example of an IM-compatible task is the *shadowing task*, in which participants repeat what they hear (e.g., say "A" or "B" in response to the spoken letter "A" or "B," respectively). Greenwald and Shulman (1973) proposed that because "the stimulus resembles sensory feedback from the response" (p. 70) for IM-compatible tasks, these tasks do not require any of the operations subject to the central bottleneck (i.e., response selection). According to this hypothesis, IM-compatible tasks bypass the central bottleneck altogether.

To obtain evidence for their proposal, Greenwald and Shulman (1973, Experiment 2) combined two types of T1 with two types of T2. T1 required a left or right movement to a visual stimulus, which could be a left or right arrow (IM compatible) or the word *LEFT* or *RIGHT* (non-IM compatible). T2 required a spoken response, which could be "A" or "B" (IM compatible) or "one" or "two" (non-IM compatible), to an auditory stimulus "A" or "B." When both tasks were IM compatible, and instructions emphasized

---

[1] Meyer and Kieras (1997a, 1997b) attributed PRP effects (in part) to a central bottleneck, but they proposed that the bottleneck is strategic rather than structural. This issue is still a subject of controversy. On the one hand, participants do have some control over their dual-task strategy (see, e.g., Schumacher et al., 2001, Experiment 3). On the other hand, attempts to eliminate the bottleneck by modifying PRP instructions and incentives have failed (e.g., Levy & Pashler, 2001; Ruthruff, Pashler, & Hazeltine, 2003; Ruthruff, Pashler, & Klaassen, 2001), except when they have been accompanied by the use of relatively easy tasks and considerable amounts of practice (several thousand trials; Hazeltine, Teague, & Ivry, 2002; Ruthruff, Van Selst, Johnston, & Remington, in press; Schumacher et al., 2001; but see Gottsdanker & Stelmach, 1971). In any case, the present study was not designed to address this issue.

that "most often the 2 signals [S1 and S2] on each trial would be simultaneous" (Greenwald & Shulman, 1973, p. 73), Greenwald and Shulman found virtually no PRP effect. They concluded that perfect timesharing occurred with two IM-compatible tasks, consistent with the proposal that the central operations associated with the bottleneck had been bypassed. Greenwald (2003) further proposed that response selection for IM-compatible tasks is "done in large part by a preparation process that precedes stimulus presentation" (p. 867; see also Greenwald, 2004).

Recently, the claim that IM-compatible task combinations abolish PRP effects has been challenged. Lien et al. (2002) conducted four PRP experiments using the same tasks and instructions as those in Greenwald and Shulman's (1973) Experiment 2. A significant PRP effect was observed even when both tasks were IM compatible. Furthermore, Lien, Proctor, and Ruthruff (2003) argued that the small PRP effect observed by Greenwald and Shulman, as well as by Greenwald (2003), does not necessarily indicate that IM-compatible tasks bypass the central bottleneck. They contended that even a central bottleneck would predict small PRP effects when mean RT1 is short (see also Byrne & Anderson, 2001; Ruthruff, Johnston, et al., 2003; Van Selst et al., 1999), as is typically the case when T1 is IM compatible. Consequently, Greenwald and Shulman's findings (and those of Greenwald, 2003) do not provide unambiguous support for bottleneck bypass with two IM-compatible tasks.

Whereas the central bottleneck model can account for small PRP effects with a very short RT1 (as in Greenwald, 2003; Greenwald & Shulman, 1973), it cannot easily account for small PRP effects paired with a long RT1. An example of the latter case was reported by Pashler, Carrier, and Hoffman (1993) in a series of PRP experiments using a non-IM-compatible tone T1 (producing a relative long RT1) and an eye-movement T2. The SOAs, ranging from −150 ms to 750 ms, were intermixed within blocks. In Experiment 1, T2 required participants to move their eyes to a *plus* sign presented to the left or right side of the screen, which would appear to be a clear example of an IM-compatible task. Pashler et al. found that the PRP effect (51 ms) was much smaller than the central bottleneck model would predict given the relatively long mean RT1 (500 ms). A closer examination revealed that T2 eye movements often occurred well before the T1 response was made, further suggesting that T2 eye movement may not be subject to a bottleneck delay. Pashler et al. also noted that if a central bottleneck still limited performance, there should have been a strong trial-by-trial dependency between RT1 and RT2 at short SOAs (see the present Equation 2). To test this prediction, they examined eye-movement RT2 as a function of the RT1 quintile. Specifically, they divided each participant's trials at each SOA into five bins (quintiles) on the basis of the speed of RT1 and then computed the mean RT2 for each bin. RT2 depended very little on the RT1 bin at the short SOA (see Pashler et al.'s, 1993, Figure 7), indicating that the tasks were performed more or less independently. In summary, Pashler et al.'s data provide compelling evidence that the central bottleneck had been bypassed.

Although Pashler et al. (1993) found evidence of bottleneck bypass with an eye-movement task in their Experiment 1, this finding does not indicate that all IM-compatible tasks bypass the bottleneck. Eye movements might constitute a special case and therefore not represent IM-compatible tasks in general. As Pashler et al. (1993) stated,

> People are rarely conscious of devoting much conscious mental activity towards the goal of moving the eyes per se, and thus one does not normally think of a task like reading or driving as a dual-task situation, just because it involves making eye movements. (p. 54)

It is important to note that Pashler et al. obtained a very similar pattern of results in their Experiment 2, in which participants moved their eyes to one of two objects of a prespecified color. Because evidence of bottleneck bypass was found even when the eye-movement task presumably was not IM compatible, the key to bypassing the bottleneck may have been the eye-movement responses, not IM compatibility.

Viable Models of Dual-Task Performance With IM-Compatible Tasks

In summary, there is no clear evidence from previous studies that IM-compatible tasks bypass the central bottleneck. It is entirely possible that a central bottleneck still limits dual-task performance but that the use of IM-compatible tasks shortens the duration of T1 central operations and thus reduces the PRP effect (see Equation 3). We refer to this possibility as the *central bottleneck model* of IM-compatible tasks. At the same time, there is also no clear evidence that IM-compatible tasks do not bypass the bottleneck. It is plausible that the central bottleneck is, in fact, bypassed with IM-compatible tasks on every trial; the residual PRP effect found in several previous studies could have been due to some minor source of interference that did not stem from temporal overlap between tasks (e.g., temporal uncertainty at a short SOA; Pashler et al., 1993, p. 61). We refer to this possibility as the *complete bottleneck bypass model*.

In addition to these two simple models, there are several *partial bottleneck bypass models* that can account for the small PRP effects observed with IM-compatible tasks. Table 1 summarizes these models and their key assumptions for IM-compatible tasks. According to the *intermittent bottleneck bypass model*, the bottle-

Table 1
Summary of the Models and Their Key Assumptions Regarding Ideomotor-Compatible Tasks

Model	Key assumption
No bottleneck bypass	
Central bottleneck model	No bypass, even with IM-compatible tasks
Complete bottleneck bypass	
G&S bottleneck bypass model	Bypass requires two IM-compatible tasks
Generic bottleneck bypass model	Bypass requires only one IM-compatible task
Partial bottleneck bypass	
Intermittent bottleneck bypass model	Bottleneck bypass on a proportion of trials
Release-bottleneck-earlier model	Bypass of late, but not early, bottleneck substage
Engage-bottleneck-later model	Bypass of early, but not late, bottleneck substage

Note. IM = ideomotor; G&S = Greenwald and Shulman (1973).

neck is bypassed on some trials but not others. Consequently, the small PRP effect can be primarily attributed to the reduced proportion of trials in which a bottleneck occurs. However, it is also possible that a bottleneck occurs on every trial with IM-compatible tasks but that the locus of the bottleneck shifts relative to that with non-IM-compatible tasks (see Ruthruff, Johnston, & Van Selst, 2001, for a similar model of performance with highly practiced tasks). There are two specific versions of this class of model. According to the *release-bottleneck-earlier model*, an early substage for IM-compatible tasks is still a part of the bottleneck, but a late substage is not (i.e., the bottleneck stage is "released" earlier in time). According to the *engage-bottleneck-later model*, a late substage for IM-compatible tasks is still a part of the bottleneck, but an early substage is not (i.e., the bottleneck stage is "engaged" later in time). Although these partial bottleneck bypass models are plausible and consistent with the available data, we discuss and evaluate them only after we present evidence against the simpler models described earlier (the central bottleneck model of IM-compatible tasks and the complete bottleneck bypass model).

Because all of the above hypotheses can be reconciled with the observation of small PRP effects, it is clear that some other measures are needed to definitively discriminate between them. The present study, therefore, uses a more diagnostic set of analyses, including (a) consideration of the size of the PRP effect in relation to the size of RT1, (b) measures of the dependency between RT1 and RT2 at the shortest SOA, (c) model-based simulations, and (d) cross-task correspondence effects (described in the next section).

Cross-Task Correspondence Effects

Given the ambiguity surrounding the interpretation of PRP effects (or the lack thereof) by themselves, it is important to explore sources of converging evidence regarding bottleneck bypass with IM-compatible tasks. One promising source of such evidence is cross-task correspondence effects, the modulation in RT1 and/or RT2 that occurs when there is dimensional overlap between the stimuli and/or the responses of the two tasks. Correspondence effects are widely thought to arise in central operations (e.g., Hommel, 1998; Lien & Proctor, 2000; Lien, Schweickert, & Proctor, 2003; Logan & Schulkind, 2000), the same operations that, according to Greenwald and Shulman (1973), are not needed for IM-compatible tasks.

Recent work with non-IM-compatible tasks has shown that correspondence effects are large from T1 to T2 (*forward* correspondence effects) and sometimes also occur from T2 to T1 (*backward* correspondence effects; Hommel, 1998). In Lien and Proctor (2000, Experiment 2), for instance, participants made left–right keypresses for both T1 (a tone task) and T2 (an arrow-direction task). Results showed that responses for T1 and T2 were faster when Response 1 (R1) and Response 2 (R2) corresponded (e.g., a left keypress on T1 and a left keypress on T2) than when they did not. Furthermore, the correspondence effect was larger at the shortest SOA (69 ms on T1 and 60 ms on T2) than at the longest SOA (7 ms on T1 and 15 ms on T2), suggesting that cross-task correspondence effects depend critically on the degree of temporal overlap between T1 and T2 processing. To explain the observed correspondence effects, Lien and Proctor (2000) hypothesized that because the responses for T1 and T2 were both coded within a left–right spatial-coordinate system, the response code for T1 activated the response code for T2 directly, and vice versa. Hommel (1998) and Logan and Schulkind (2000) also observed cross-task correspondence effects, but with different forms of dimensional overlap between T1 and T2.

These recent demonstrations of cross-task correspondence effects in dual-task paradigms are not the first appearances of such effects in the literature. In his seminal study of the effects of IM compatibility on dual-task interference, Greenwald (1972) found correspondence effects with two non-IM-compatible tasks but not with two IM-compatible tasks. In his experiment, two stimulus types were used. The visual stimulus was a left- or right-pointing arrow presented on the left or right side of the television monitor, respectively. The auditory stimulus "left" or "right" was presented simultaneously with the visual stimulus (i.e., 0-ms SOA). Different degrees of IM compatibility were created by pairing the two stimulus types with two response types, moving a switch left or right and saying the word "left" or "right." In the *low*-IM-compatibility condition, participants moved the switch left or right in response to the word "left" or "right," and they said "left" or "right" in response to the arrow direction. In the *high*-IM-compatibility condition, participants said "left" or "right" in response to the spoken word "left " or "right," and they moved the switch left or right in response to the arrow direction. Participants performed these tasks in three types of blocks. In the *0-decision* block, the stimuli for both tasks did not change within a block (e.g., on each trial, the right-pointing arrow was presented with the auditory word "left"); in other words, each trial contained two simple-RT tasks. In the *1-decision* block, the stimulus for one task was constant within a block, but the stimulus for the other task varied; in other words, each trial contained one simple-RT task and one choice-RT task. In the *2-decision* block, the stimuli for both tasks varied within a block; thus, each trial contained two choice-RT tasks, as in traditional dual-task studies.

One interesting comparison was between choice-RT performance in the 2-decision block and choice-RT performance in the 1-decision block (this comparison is analogous to a comparison of dual-task and single-task performance). For the low-IM-compatibility condition (which we call *non-IM compatible*), choice RT in the 2-decision blocks was substantially longer than choice RT in the 1-decision blocks (275 ms longer for the vocal task and 187 ms longer for the manual task). Furthermore, as one would expect from recent research, T1–T2 correspondence effects were substantial (139 ms for the vocal task and 118 ms for the manual task). In contrast, the high-IM-compatibility condition showed only a small increase in choice RT (10 ms for the vocal task and 63 ms for the manual task) in the 2-decision blocks relative to the 1-decision blocks, and there were only very small correspondence effects (10 ms for the vocal task and 11 ms for the manual task; see Greenwald, 1972, Table 1).

Given that correspondence effects are robust and large when two non-IM-compatible tasks with dimensional overlap are used (e.g., Hommel, 1998), the near absence of a correspondence effect between the two IM-compatible tasks in Greenwald's (1972) study is intriguing. The absence is especially striking given the high degree of dimensional overlap between the stimulus codes (left vs. right) and the response codes (left vs. right) for T1 and T2 (see Lien & Proctor, 2002, Table 2, for possible sources of correspondence effects). Given evidence that the locus of correspondence effects is in central operations (e.g., Hommel, 1998; Lien & Proctor, 2000; Lien, Schweickert, & Proctor, 2003; Logan & Schulkind, 2000), the near absence of correspondence effects in Greenwald's (1972) study suggests that IM-compatible tasks lack central operations. Thus, the correspondence data appear to represent important converging evidence that IM-compatible tasks bypass the central bottleneck.

The present study builds on Greenwald's (1972) findings by using cross-task correspondence effects to evaluate the hypothesis that IM-compatible tasks bypass central operations. According to this hypothesis, correspondence effects should be large when both tasks are non-IM compatible, but they should be very small or absent when both tasks are IM compatible. This hypothesis also predicts that correspondence effects should be very small or absent when an IM-compatible task is paired with a non-IM-compatible task. This prediction has not yet been tested—all previous studies of correspondence effects have used either two IM-compatible tasks (Greenwald, 1972) or two non-IM-compatible tasks (e.g., Hommel, 1998; Lien & Proctor, 2000; Lien, Schweickert, & Proctor, 2003; Logan & Schulkind, 2000). The present study, however, factorially manipulated IM compatibility on T1 and T2 (as explained in the next section), and therefore it provided an opportunity to test this prediction.

The Present Study

The present study was designed to examine whether the central bottleneck model can account for dual-task performance with IM-compatible tasks. More specifically, we investigated whether central operations are bypassed with IM-compatible tasks and, if so, whether bypass requires two IM-compatible tasks or just one. We conducted four PRP experiments with conceptual similarity between T1 and T2 response codes (allowing us to measure correspondence effects). Across experiments, we varied whether T1 was IM- or non-IM-compatible and whether T2 was IM- or non-IM-compatible in a factorial design (see Table 2). In Experiment 1, two non-IM-compatible tasks were used. In the subsequent experiments, we made minor modifications for T1 and/or T2 to form IM-compatible tasks. For all experiments, however, T1 involved visual stimuli and left–right joystick movements, and T2 involved auditory stimuli and "left"–"right" vocal responses. Thus, the response codes (left–right) for T1 and T2 were the same for each experiment.

According to the central bottleneck model of IM-compatible tasks, the processing of IM-compatible tasks involves central operations, and therefore the central bottleneck should occur regardless of whether one or both tasks are IM compatible. This model predicts substantial PRP and correspondence effects when both tasks are non-IM compatible. When T1 is IM compatible, the duration of Stage 1B should be reduced, resulting in a smaller PRP effect (see Equation 3). Similarly, the use of an IM-compatible T2 (which presumably requires less preparation than a non-IM-compatible task) might allow participants to better prepare for T1, thus decreasing the duration of Stage 1B and decreasing the PRP effect (see De Jong, 1995, and Gottsdanker, 1980, for discussions of preparation effects in dual-task paradigms). However, assuming that the total duration of Stages 1A and 1B still exceeds the duration of Stage 2A, the PRP effect at the 0-ms SOA should still be greater than 0 ms (see Equation 3). Most important, any delays in the completion of T1 central operations (i.e., Stage 1B) should tend to delay central operations on T2 at the 0-ms SOA by a similar amount (see Equation 2). Consequently, there should be a strong, positive dependency between RT1 and RT2 across trials at the 0-ms SOA; that is, slower responses on T1 should be associated with slower responses on T2. Finally, because the processing of IM-compatible tasks is assumed to involve central operations (presumed to be sensitive to cross-task correspondence effects), the obvious expectation is that correspondence effects should occur even when one or both tasks are IM compatible.

The predictions of the complete bottleneck bypass model depend on whether bypass is assumed to require one or two IM-compatible tasks. According to the *generic bottleneck bypass model*, IM-compatible tasks do not require central operations. Thus, the central bottleneck is bypassed when either T1 or T2 is IM compatible. Greenwald and Shulman (1973) proposed a related model in which both T1 and T2 must be IM compatible to bypass the central bottleneck (see also Greenwald, 2003). We refer to this version as the *G&S bottleneck bypass model*. Although these models disagree regarding whether one or two IM-compatible tasks are needed to bypass the bottleneck, they agree about what should happen when the bottleneck is bypassed. First, there should be little or no PRP effect. Second, even if there is some residual PRP effect (e.g., due to temporal uncertainty at short SOAs), there is no obvious reason for RT2 to depend strongly on RT1 at the shortest SOA. There may be some correlation between RT1 and RT2 as a result of trial-to-trial variation in general arousal or attentional processes. However, this correlation is likely to be very weak; for instance, at long SOAs (with which the bottleneck is rarely encountered), the correlation between RT1 and RT2 is

Table 2
Stimulus, Response, and Compatibility Manipulations, With Response Time for Task 1 (RT1) and Psychological Refractory Period (PRP) Findings, in Experiments 1–4

	Task 1: Visual–manual				Task 2: Auditory–vocal			
Exp	Stimulus	Response	Comp.	RT1 (ms)	Stimulus	Response	Comp.	PRP (ms)
1	*A/H*	left/right	non-IM	556	"one"/"two"	"left"/"right"	non-IM	228
2	←/→	left/right	IM	455	"one"/"two"	"left"/"right"	non-IM	120
3	*A/H*	left/right	non-IM	499	"left"/"right"	"left"/"right"	IM	131
4	←/→	left/right	IM	422	"left"/"right"	"left"/"right"	IM	52

Note. Exp = experiment; Comp. = compatibility; non-IM = non-ideomotor compatible; IM = ideomotor compatible.

generally close to zero. A further prediction, given the assumption that IM tasks bypass the central processing stage, is that there should be little or no correspondence effect between tasks.

Experiment 1 (Non-IM-Compatible T1, Non-IM-Compatible T2)

In Experiment 1, we measured PRP and correspondence effects with two non-IM-compatible tasks to provide a baseline against which to compare performance in the subsequent experiments with IM-compatible tasks. To avoid peripheral processing conflicts, we used different input and output modalities for T1 and T2. T1 was a visual–manual task: Participants moved a joystick to the left or right in response to a centrally located letter (*A* or *H*). T2 was an auditory–vocal task: Participants said "left" or "right" in response to the auditory stimulus "one" or "two" (see Table 2). Note that there was no conceptual similarity between the stimuli for the two tasks. However, there was conceptual similarity between the response codes, providing an opportunity to observe correspondence effects.

On each trial, one of six SOAs (0, 50, 150, 300, 500, or 1,000 ms) was selected at random, with the restriction that each SOA occur equally often. Several previous studies have shown that mixed and blocked SOAs produce similar RT2 lengthening (e.g., Bertelson, 1967; see Pashler, 1998, pp. 274–275, for detailed discussion). However, we chose to use mixed rather than blocked SOAs (e.g., Greenwald & Shulman, 1973) for the following three reasons. First, the use of mixed SOAs minimizes unwanted differences in preparatory state between SOAs; with blocked SOAs, participants can adopt a different preparatory strategy for each SOA (e.g., Gottsdanker & Stelmach, 1971). Second, response grouping (withholding R1 until R2 has also been selected), which makes it difficult to obtain a true estimate of the time to complete T1 processing, is less likely to occur with mixed SOAs than with blocked SOAs. Third, the one study using an IM-compatible task that did find clear evidence for bottleneck bypass (with saccadic eye movements; Pashler et al., 1993, Experiment 1) also mixed SOAs within blocks.

Because both tasks are non-IM compatible, all of the candidate models predict that a central bottleneck should occur in this experiment. Thus, we expected to observe a large PRP effect and a large correspondence effect at short SOAs, as in previous studies with two non-IM-compatible tasks. In addition, RT2 should depend strongly on RT1 at the shortest SOA.

Method

Participants. Sixteen participants, ranging in age from 17 to 23 years, from colleges and universities surrounding the National Aeronautics and Space Administration Ames Research Center, Moffett Field, California, participated in partial fulfillment of course requirements. All participants were required to have normal or corrected-to-normal vision.

Apparatus and stimuli. Stimulus presentation, timing, and data collection were controlled using an IBM-compatible microcomputer connected to a SONY Trinitron (640 × 800 resolution) monitor, housed in a dedicated, sound-attenuating booth. T1 involved a letter, either *A* or *H*, presented in the center of the screen. The letter measured 1.0 cm in width and 1.5 cm in height. At a viewing distance of 50 cm, each letter subtended a visual angle of 1.15° × 1.72°. All visual stimuli were presented in white on a black background. Participants were instructed to move a joystick left in response to the letter *A* and right in response to the letter *H*. The flight joystick was stabilized on the table with Velcro fastening, and participants were asked to grasp the joystick handle with their dominant hand only. T2 involved the spoken word "one" or "two," presented for 500 ms through an earphone. Participants were instructed to say "left" in response to "one" and "right" in response to "two." Vocal responses were collected by a microphone connected to a Micro Intro Voice II device for voice recognition.

Design and procedure. Each participant received one practice block of 72 trials followed by six regular blocks of 64 trials each. On each trial, a fixation point appeared in the center of the screen for 500 ms and then disappeared. The letter *A* or *H* was presented 300 ms after the offset of the fixation point and was displayed for 750 ms. After one of six SOAs (0, 50, 150, 300, 500, or 1,000 ms), the auditory stimulus "one" or "two" was presented for 500 ms. Participants were specifically asked to respond to the stimuli for both tasks quickly and accurately (to encourage equal treatment of both tasks). When participants made a correct response, the next trial began 500 ms after the response. When participants made an incorrect response, however, a feedback message (e.g., *Incorrect vocal response*) was presented in the center of the screen for 2,000 ms; the next trial began 500 ms after the offset of the feedback message. Accordingly, the interstimulus interval depended on the RT and the accuracy of the response. However, the time between the end of one trial and the start of the next trial was constant (500 ms), so participants had a constant amount of time to prepare for each upcoming trial (see also Lien, Proctor, & Ruthruff, 2003).

Results

Trials with either RT1 or RT2 of less than 100 ms or greater than 2,000 ms were excluded from the RT analyses (<2% of the trials). Trials with errors on either T1 or T2 were also excluded from the RT analyses. The proportion of errors (PE) for each task (PE1 and PE2 for T1 and T2, respectively) was determined without regard to whether the response for the other task was correct. RT and PE were analyzed as a function of SOA and correspondence (corresponding or noncorresponding) between T1 and T2. *Corresponding* trials were defined as those on which the response codes for the vocal response and the joystick movement were both left or both right, whereas *noncorresponding* trials were defined as those on which the response codes were different.

RT1 and PE1. RT1 data are shown in Figure 2, and PE1 data are shown in Table 3. For RT1, no effect was statistically significant. For PE1, the main effect of SOA was significant, $F(5, 75) = 3.34$, $p < .01$, $MSE = 0.0003$. PE1s were .026, .023, .022, .011, .015, and .017 at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively. The main effect of correspondence was also significant, $F(1, 15) = 7.29$, $p < .05$, $MSE = 0.0004$. Participants committed slightly fewer errors (.008) when R1 and R2 corresponded (.015) than when they did not (.023).

RT2 and PE2. RT2 data are shown in Figure 3, and PE2 data are shown in Table 4. For RT2, the main effect of SOA was significant, $F(5, 75) = 91.05$, $p < .001$, $MSE = 2{,}863.26$; RT2 decreased as SOA increased (901, 893, 819, 768, 733, and 673 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). In a comparison of RT2 at the shortest and longest SOAs, there was a significant PRP effect of 228 ms, $F(1, 15) = 153.51$, $p < .0001$, $MSE = 5{,}404.57$. The main effect of correspondence was also significant, $F(1, 15) = 10.06$, $p < .01$, $MSE = 1{,}865.24$; overall, responses were 21 ms faster when R1 and R2 corresponded than when they did not. The SOA × Correspondence

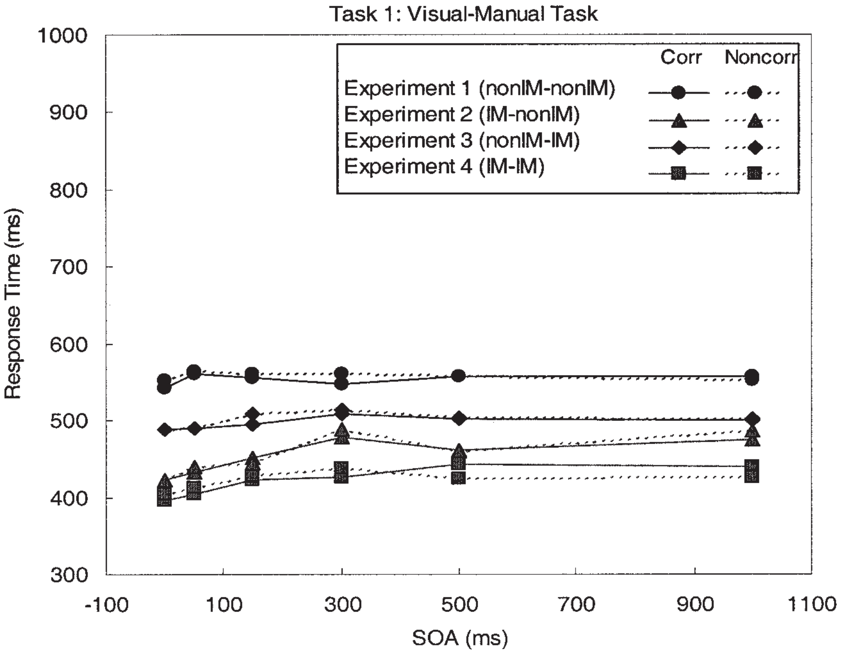

Figure 2. Mean response times for Task 1 in Experiments 1–4 as a function of correspondence and stimulus onset asynchrony (SOA; 0, 50, 150, 300, 500, or 1,000 ms). Corr = corresponding; Noncorr = noncorresponding; nonIM = nonideomotor-compatible task; IM = ideomotor-compatible task.

interaction was significant as well, $F(5, 75) = 2.39$, $p < .05$, $MSE = 989.24$; the correspondence effect was significant at the four shortest SOAs, $Fs(1, 15) \geq 6.08$, $ps < .05$, $MSEs \leq 1{,}369.79$. The correspondence effects were 26, 28, 36, 32, −5, and 1 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively.

Table 3
Proportions of Error as a Function of Stimulus Onset Asynchrony and Response Correspondence for Task 1 in Experiments 1–4

	Stimulus onset asynchrony (ms)					
Correspondence	0	50	150	300	500	1,000
	Experiment 1					
Corr	.017	.020	.015	.012	.012	.015
Noncorr	.034	.026	.030	.009	.019	.020
	Experiment 2					
Corr	.013	.005	.004	.004	.004	.007
Noncorr	.007	.007	.016	.004	.004	.006
	Experiment 3					
Corr	.008	.012	.011	.019	.011	.010
Noncorr	.009	.011	.015	.021	.012	.014
	Experiment 4					
Corr	.009	.007	.009	.009	.004	.012
Noncorr	.012	.002	.007	.002	.004	.004

Note. Corr = corresponding; Noncorr = noncorresponding.

For PE2, only the main effect of correspondence was significant, $F(1, 15) = 5.73$, $p < .05$, $MSE = 0.0011$. Participants committed slightly fewer errors (.011) when R1 and R2 corresponded (.024) than when they did not (.035).

Discussion

As expected, a relatively large PRP effect (228 ms) was obtained, similar to results from previous PRP studies using two non-IM-compatible tasks. Is this observed PRP effect largely a result of the postponement of central operations, as suggested by the central bottleneck hypothesis? If so, delays in the completion of T1 central operations (Stage 1B) should tend to delay T2 central operations at short SOAs. The result would be a positive correlation between RT2 and RT1. The strength of this relationship depends in large part on how much RT1 variation is due to variation in Stages 1A and 1B (which gets passed on to RT2) rather than variation in Stage 1C (which does not get passed on to RT2). Given the widely held assumption that central stages (e.g., 1B) are the most time-consuming and the most variable in standard RT tasks (see Pashler & Baylis, 1991; Van Selst et al., 1999), one would expect a very strong correlation between RT1 and RT2 at short SOAs (as observed in previous dual-task studies). Later, we supplement these informal arguments regarding RT2–RT1 correlations with a formal, quantitative simulation of the bottleneck model.

To examine the empirical relationship between RT2 and RT1, we measured RT2 as a function of the RT1 quintile (following the procedure of Pashler, 1993, 1994; Pashler et al., 1993; Pashler & O'Brien, 1993). This analysis was conducted using only the 0-ms

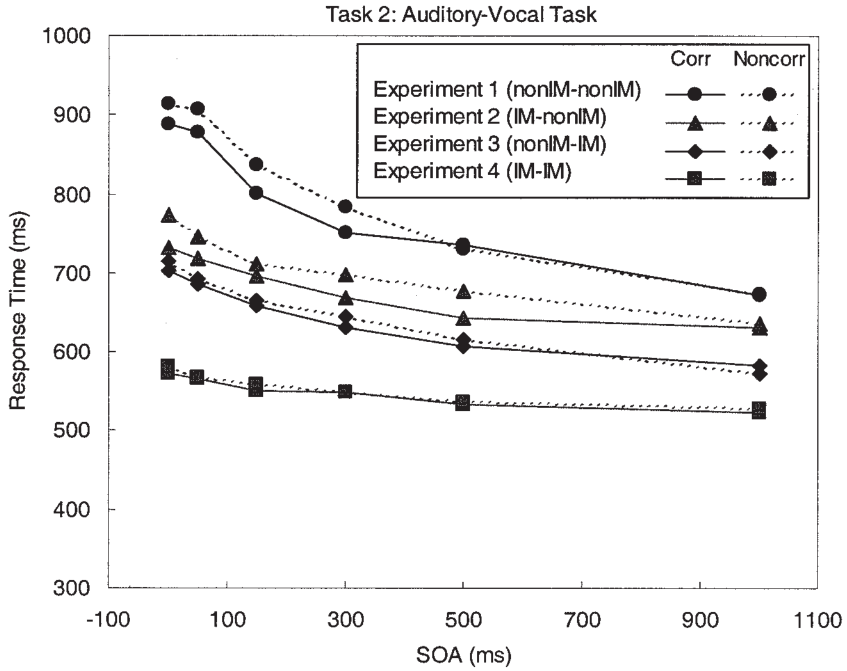

Figure 3. Mean response times for Task 2 in Experiments 1–4 as a function of correspondence and stimulus onset asynchrony (SOA; 0, 50, 150, 300, 500, or 1,000 ms). Corr = corresponding; Noncorr = noncorresponding; nonIM = nonideomotor-compatible task; IM = ideomotor-compatible task.

SOA, at which bottleneck-related delays were most likely to occur. First, we divided trials from each block of each participant into five bins (quintiles) on the basis of the length of RT1. Then we computed the mean RT2 for each RT1 bin. The results of this analysis, averaged across blocks and across participants, are shown in Figure 4. As can be seen, RT2 increased strongly and monotonically as RT1 increased; RT2 was approximately 779 ms when RT1 was relatively short (about 419 ms), but it was 1,048 ms when RT1 was relatively long (about 738 ms). The slope relating RT2 and RT1 was approximately .84. This strong dependency between RT2 and RT1 is consistent with the hypothesis that a central bottleneck limits performance with two non-IM-compatible tasks.

Table 4
Proportions of Error as a Function of Stimulus Onset Asynchrony and Response Correspondence for Task 2 in Experiments 1–4

	Stimulus onset asynchrony (ms)					
Correspondence	0	50	150	300	500	1,000
	Experiment 1					
Corr	.031	.030	.023	.020	.013	.024
Noncorr	.041	.039	.051	.030	.027	.023
	Experiment 2					
Corr	.018	.023	.030	.023	.035	.028
Noncorr	.054	.050	.058	.045	.049	.013
	Experiment 3					
Corr	.001	.004	.000	.005	.003	.004
Noncorr	.005	.007	.009	.0004	.004	.001
	Experiment 4					
Corr	.021	.033	.028	.018	.014	.023
Noncorr	.030	.019	.023	.018	.018	.017

Note. Corr = corresponding; Noncorr = noncorresponding.

Experiment 1 also revealed a correspondence effect on RT2, suggesting that T1 response activation influenced T2 response activation. Numerically, the effect averaged 30 ms at the three shortest SOAs (at which the temporal overlap between T1 and T2 was high). Although the correspondence effect on RT2 was substantial (replicating previous studies; e.g., Hommel, 1998; Lien & Proctor, 2000; Lien, Schweickert, & Proctor, 2003; Logan & Schulkind, 2000), the effect was not as large as those observed previously (e.g., 60 ms at the shortest SOA in Lien & Proctor, 2000). The key cause of the relatively small correspondence effect in Experiment 1 might be the separation of input modalities and output modalities between tasks. Whereas the present Experiment 1 used different input and output modalities for T1 and T2, previous studies generally have used the same input modalities (e.g., visual S1 and S2 in Hommel, 1998) and/or the same output modalities (e.g., manual R1 and R2 in Lien & Proctor, 2000, and Logan & Schulkind, 2000) for both tasks. In any case, having demonstrated a substantial correspondence effect with two non-IM-compatible tasks in Experiment 1, in subsequent experiments we examined whether this effect would be eliminated when one or both tasks were IM compatible.

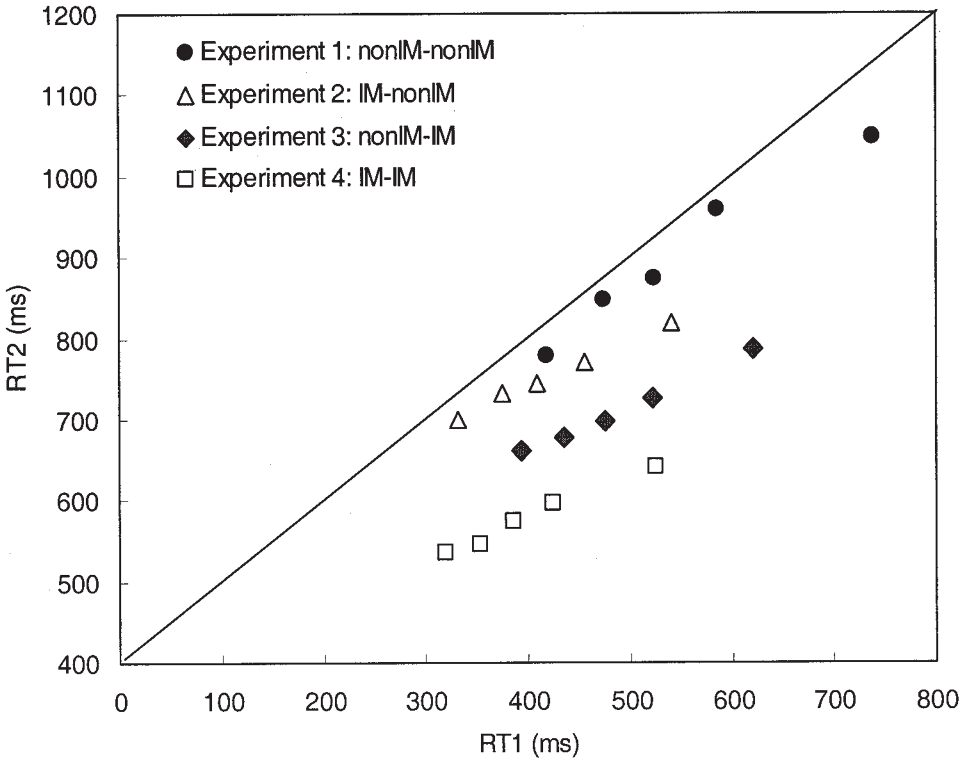

Figure 4. Task 2 response time (RT2) as a function of Task 1 response time (RT1) at the 0-ms SOA in Experiments 1–4. The five data points for each experiment correspond to the five RT1 quintiles. The solid line indicates a slope of 1. nonIM = nonideomotor-compatible task; IM = ideomotor-compatible task.

Experiment 2 (IM-Compatible T1, Non-IM-Compatible T2)

The purpose of Experiment 2 was to determine whether the central bottleneck would also occur when one of the two tasks was IM compatible. As a first step, we modified T1 to form an IM-compatible task, leaving T2 unchanged (see Table 2). More specifically, we replaced the S1 letters *A* and *H* with left- and right-pointing arrows; participants moved the joystick to the left in response to a left-pointing arrow and to the right in response to a right-pointing arrow. This was the same IM-compatible task used by Greenwald (1972). Because T1 is IM compatible, RT1 should be much shorter than it was in Experiment 1. Furthermore, the relative ease of T1 should allow participants to prepare better for T2 than was possible in Experiment 1 (see De Jong, 1995, for a detailed discussion of preparatory control of T1 and T2 in the PRP paradigm). Thus, RT2 should also be somewhat shorter than in Experiment 1, even though T2 was the same in both cases.

According to the generic bottleneck bypass model, bypass is possible with only one IM-compatible task. This model therefore predicts that there should be very little PRP effect and very little correspondence effect in this experiment. Also, this model provides no reason for RT2 to depend strongly on RT1. According to the G&S bottleneck bypass model, however, bypass requires two IM-compatible tasks. This model (along with the central bottleneck model of IM-compatible tasks) predicts that the bottleneck should still be present in this experiment, producing substantial PRP and correspondence effects. Of course, one would expect a reduction in the PRP effect relative to that observed in Experiment 1 because the IM-compatible T1 is likely to shorten Stage 1B (see Equation 3). Furthermore, RT2 should depend strongly on the duration of RT1 across trials at the 0-ms SOA.

Method

Participants. There were 16 participants in this experiment. These participants were recruited from the same population as those in Experiment 1, but none had participated in that experiment.

Apparatus, stimuli, and procedure. The only change from Experiment 1 was that a left- or right-pointing arrow, presented correspondingly to the left or right side of the fixation point, was used as S1. Participants responded to the direction of the arrow by moving the joystick in the same direction (e.g., to left in response to a left-pointing arrow). The arrow measured 2.3 cm in width and 0.5 cm in height, and as in Greenwald's (1972) study, it was displayed 3.0 cm horizontally from center of the screen in the left or right location corresponding to the arrow direction. At a viewing distance of 50 cm, each arrow subtended a visual angle of 2.39° × 0.57°. In all other respects, the method was the same as that of Experiment 1.

Results

As in Experiment 1, correct trails with either RT1 or RT2 of less than 100 ms or greater than 2,000 ms were excluded from the RT analyses. A total of 2% of the trials were omitted because they fell outside these RT cutoffs.

RT1 and PE1. RT1 data are shown in Figure 2, and PE1 data are shown in Table 3. As expected, mean RT1 was reduced from 556 ms in Experiment 1, in which T1 was non-IM compatible, to 455 ms in Experiment 2, in which T1 was IM compatible (see Table 2). For RT1, only the main effect of SOA was significant, $F(5, 75) = 10.17$, $p < .001$, $MSE = 1{,}833.49$; RT1 was shorter at the three shortest SOAs than it was at the longer SOAs (423, 437, 448, 483, 461, and 481 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). The main effect of correspondence and the Correspondence × SOA interaction were not significant on RT1 (*F*s < 1). For PE1, no effects were significant.

RT2 and PE2. RT2 data are shown in Figure 3, and PE2 data are shown in Table 4. Even though T2 was the same non-IM-compatible task in both Experiments 1 and 2, mean RT2 at the longest SOA was 40 ms shorter in Experiment 2 (633 ms) than it was in Experiment 1 (673 ms). This result was expected because the ease of T1 should have allowed participants to devote more of their pretrial preparation to T2. For RT2, the main effect of SOA was significant, $F(5, 75) = 23.39$, $p < .001$, $MSE = 2{,}696.71$; RT2 decreased as SOA increased (753, 732, 704, 684, 660, and 633 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). In a comparison of RT2 at the shortest and longest SOAs, there was a significant PRP effect of 120 ms, $F(1, 15) = 36.84$, $p < .0001$, $MSE = 6{,}165.50$. The main effect of correspondence was also significant, $F(1, 15) = 7.28$, $p < .05$, $MSE = 4{,}297.19$; RT2 was 25 ms shorter when R1 and R2 corresponded than when they did not. Although the Correspondence × SOA interaction was not significant, $F(5, 75) = 1.14$, $p = .346$, $MSE = 1{,}113.24$, the correspondence effect was numerically larger at the five shortest SOAs than it was at the longest SOA (40, 27, 17, 29, 34, and 5 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). To provide a more sensitive test for the Correspondence × SOA interaction on RT2, we conducted an additional analysis comparing just the shortest and longest SOAs; in this analysis, the interaction was significant, $F(1, 15) = 8.30$, $p < .05$, $MSE = 590.30$.

For PE2, the main effect of SOA was significant, $F(5, 75) = 2.74$, $p < .05$, $MSE = 0.0008$; PE2 was smaller at the longest SOA than at the others (.036, .037, .044, .034, .042, and .020 at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). The main effect of SOA was also significant on PE2 in a comparison of only the shortest and longest SOAs, $F(1, 15) = 9.72$, $p < .01$, $MSE = 0.0004$. Thus, we observed a significant PRP effect of .016 on PE2. The SOA × Correspondence interaction was significant, $F(5, 75) = 3.40$, $p < .01$, $MSE = 0.0008$; the correspondence effect decreased as SOA increased. The simple main analysis showed that the correspondence on PE2 was only significant for the 0-ms SOA, $F(1, 15) = 6.88$, $p < .05$, $MSE = 0.0015$. The correspondence effect was .036, .027, .027, .022, .014, and −.015 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively.

Discussion

There are several findings suggesting that a bottleneck occurred in Experiment 2 even though T1 was IM compatible. First, the PRP effect was substantial (120 ms). Second, as shown in Figure 4, RT2 again increased strongly as RT1 increased. Third, a correspondence effect on RT2 was found; in fact, the effect on RT2 averaged across the three shortest SOAs (28 ms) was about as large as that observed in Experiment 1 (30 ms). The error rates on T2 also indicated strong crosstalk from T1 to T2 when the temporal overlap between T1 and T2 was high (the correspondence effect on PE2 averaged across the three shortest SOAs was .030). Consequently, these findings argue against the generic bottleneck bypass model, which assumes that the central bottleneck is bypassed with only one IM-compatible task.

The results from Experiment 2 are instead consistent with the hypothesis that a central bottleneck limits performance even when one task is IM compatible. Although the PRP effect was reduced relative to that in Experiment 1 (with two non-IM-compatible tasks), this reduction (108 ms) was similar to the reduction in mean RT1 (101 ms). Thus, this result is consistent with the PRP equation derived from the central bottleneck model (Equation 3). The strong dependency between RT2 and RT1 at the 0-ms SOA is also consistent with the central bottleneck model. Note that the slope of the RT2 and RT1 function was .55, which is smaller than that in Experiment 1. Informally, there were two reasons to expect this reduction in slope. First, IM compatibility should greatly reduce the duration and variability of Stage 1B while leaving the duration and variability of Stage 1C (the joystick movement) unaffected. According to the central bottleneck model, therefore, the proportion of RT1 variance that can carry over to RT2 has been reduced. Second, the short T1 central stage (Stage 1B) might often be completed before Stage 2A has finished (resulting in no bottleneck delay on that trial); under these conditions, variation in RT1 should not carry over fully to RT2. Thus, the central bottleneck model naturally predicts this decrease in slope, as we later demonstrate using quantitative simulations.

Experiment 3 (Non-IM-Compatible T1, IM-Compatible T2)

Although the results of Experiment 2 are inconsistent with the generic bottleneck bypass model, they can be explained by a modified version of this model. Suppose that IM-compatible tasks do not require the limited central resource (or computational mechanism) responsible for the bottleneck but that they nevertheless recruit the central resource if it is unoccupied (Johnston & Delgado, 1993; Ruthruff et al., in press). Thus, when T1 is IM compatible, it will use the central resource, causing a bottleneck delay on a non-IM-compatible T2 (leading to a PRP effect, as found in Experiment 2). Consider, however, a case in which T1 is non-IM compatible but T2 is IM compatible (as in the present experiment). When the central resource is occupied by T1, an IM-compatible T2 will not "compete for occupancy" but will simply proceed without the central resource. Consequently, there should be no bottleneck delay on T2 and no PRP effect when T2 is IM compatible. We refer to this version of the generic bottleneck bypass model as the *greedy resource-recruitment* hypothesis.

To test the greedy resource-recruitment hypothesis, in Experiment 3 we used a combination of a non-IM-compatible T1 and an IM-compatible T2. The design was similar to that of Experiment 1, except that we replaced the S2 "one" and "two" with the spoken words "left" and "right." Thus, participants were instructed to say "left" when the spoken word was "left" and "right" when the spoken word was "right," forming the same IM-compatible task used in Greenwald (1972). Because responses for T1 and T2 were the same as in the previous experiments, the R1–R2 correspondence relation remained the same.

According to the central bottleneck model of IM-compatible tasks, the pattern of results should be similar to those for Experiments 1 and 2. However, because T2 is IM compatible in this experiment, RT2 should be much shorter than it was in Experiment 1. In addition, the ease of T2 might allow participants to prepare better for T1 than was possible in Experiment 1 (see De Jong, 1995; Gottsdanker, 1980). If so, RT1 might be somewhat shorter (and the PRP effect somewhat smaller) in this experiment than it was in Experiment 1, even though T1 is identical in both cases.

Method

Participants. There were 16 participants in this experiment. These participants were recruited from the same population as those in Experiments 1 and 2, but none of them had participated in the previous experiments.

Apparatus, stimuli, and procedure. The only change from Experiment 1 was that the T2 auditory stimuli "one" and "two" were replaced by the auditory stimuli "left" and "right." Participants were instructed to say "left" in response to the spoken word "left" and "right" in response to the spoken word "right."

Results

The same RT cutoffs as in Experiment 1 were used here, resulting in less than 3% of the trials being omitted.

RT1 and PE1. RT1 data are shown in Figure 2, and PE1 data are shown in Table 3. In agreement with our prediction that the ease of preparing for an IM-compatible T2 would benefit T1, mean RT1 was reduced from 556 ms in Experiment 1 to 499 ms in this experiment. For RT1, only the main effect of SOA was significant, $F(5, 75) = 2.91, p < .05, MSE = 790.06$. As in Experiment 2, RT1 was slightly smaller at the two shortest SOAs than it was at the others (489, 491, 502, 512, 503, and 501 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). As in previous experiments, the main effect of correspondence and the Correspondence × SOA interaction were not significant on RT1 (Fs < 1). For PE1, no effects were significant.

RT2 and PE2. RT2 data are shown in Figure 3, and PE2 data are shown in Table 4. As can been seen, mean RT2 at the longest SOA was reduced from 673 ms in Experiment 1, in which T2 was non-IM compatible, to 578 ms in this experiment, in which T2 was IM compatible. The main effect of SOA was significant, $F(5, 75) = 45.80, p < .001, MSE = 1{,}668.77$; RT2 decreased as SOA increased (709, 689, 662, 638, 612, and 578 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). In a comparison of the longest and shortest SOAs, there was a significant PRP effect on RT2 of 131 ms, $F(1, 15) = 86.98, p < .0001, MSE = 3{,}172.96$. Unlike in Experiments 1 and 2, the main effect of correspondence was not significant, $F(1, 15) = 1.24, p = .2825, MSE = 1{,}592.69$. However, the Correspondence × SOA interaction was significant, $F(5, 75) = 2.83, p < .05, MSE = 213.74$; the correspondence effect on RT2 was 12, 7, 8, 14, 8, and −10 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively. Nevertheless, in an analysis of only the three shortest SOAs, the main effect of correspondence was not significant, $F(1, 15) = 1.93, p = .1848, MSE = 980.61$. For PE2, no effects were significant.

Discussion

According to the greedy resource-recruitment hypothesis (a variant of the generic bottleneck bypass model), the central bottleneck is bypassed when T2 is IM compatible. If this is so, then little or no PRP should have been observed in Experiment 3. In contrast to this prediction, the results showed a significant PRP effect of 131 ms, very similar to the effect observed in Experiment 2 (120 ms; see Table 2). A between-experiments analysis revealed no difference in the PRP effect between Experiments 2 and 3 ($F < 1$). Furthermore, RT1 bin analyses revealed a strong relationship between RT2 and RT1 at the 0-ms SOA; RT2 increased as RT1 increased, as shown in Figure 4. Consequently, these findings argue against the greedy resource-recruitment hypothesis and the generic bottleneck bypass model in general.

However, the large PRP effect observed in the present experiment is consistent with the hypothesis that the central bottleneck occurs even when T2 is IM compatible and T1 is not. The strong dependency between RT2 and RT1 is also consistent with the central bottleneck model. The slope of the RT2–RT1 function was .56, which is almost identical to that observed in Experiment 2 (.55). Nevertheless, two additional findings from Experiment 3 are difficult to reconcile with the central bottleneck model of IM-compatible tasks. First, given that the reduction in mean RT1 relative to that found in Experiment 1 (in which both tasks were non-IM compatible) was 57 ms, the model predicts a roughly similar reduction in the magnitude of the PRP effect (see Equation 3). Instead, the reduction in the PRP effect (97 ms) was almost twice as large as the reduction in mean RT1. The second finding was that the correspondence effect averaged only 9 ms at the three shortest SOAs. This effect is less than one third of the correspondence effect obtained in Experiment 1 (30 ms). A between-experiments comparison at the three shortest SOAs showed that the difference in the magnitude of the correspondence effect between Experiments 1 and 3 was significant, $F(1, 30) = 5.00, p < .05, MSE = 1{,}067.52$.

The relatively small correspondence effect in Experiment 3 suggests that the response activation of a non-IM-compatible T1 has little influence on the response activation of an IM-compatible T2. In contrast, Experiment 2 showed that the response activation of an IM-compatible T1 has a strong influence on the response activation of a non-IM-compatible T2 (the correspondence effect averaged 28 ms across the three shortest SOAs). It appears, then, that an IM-compatible T1 can generate crosstalk on a non-IM-compatible T2, but an IM-compatible T2 is somehow insulated or protected against receiving crosstalk from a non-IM-compatible T1. Implications of these findings are considered in the General Discussion.

Experiment 4 (IM-Compatible T1, IM-Compatible T2)

The results of Experiments 2 and 3 are consistent with the hypothesis that a bottleneck occurs when one task is IM compatible and the other was not. These findings are consistent not only with the central bottleneck model but also with the G&S bottleneck bypass model. The predictions of these models differ only for the case in which both tasks are IM compatible. The G&S bottleneck bypass model predicts that the bottleneck should be bypassed in this case. The central bottleneck model of IM-compatible tasks predicts that the bottleneck should still occur. However, because both tasks are now IM compatible, mean RT1 should be relatively short, and the PRP effect should be relatively small. Assuming that RT1 is not short enough to entirely eliminate the PRP effect, the central bottleneck model predicts that PRP effects should still depend strongly on RT1 across trials (see Equation 3). In other words, there should be a strong dependency between RT2 and RT1 at the 0-ms SOA (see Equation 2). Experiment 4 tested these predictions by combining the IM-compatible T1 from Experiment 2 with the IM-compatible T2 from Experiment 3. These tasks were exactly the same as the IM-compatible T1 and T2 used in Greenwald's (1972) study, in which he observed little dual-task interference and little correspondence effect with simultaneous stimulus presentation.

Method

Participants. There were 16 participants in this experiment. These participants were recruited from the same population as those in Experiments 1–3, but none of them had participated in those experiments.

Apparatus, stimuli, and procedure. T1 was the same IM-compatible task used in Experiment 2 (moving the joystick to the left or right in the direction to which the arrow pointed), and T2 was the same IM-compatible task used in Experiment 3 (saying "left" or "right" to the spoken word "left" or "right," respectively). In all other respects, the method was the same as that of Experiment 1.

Results

As in previous experiments, correct trails with either RT1 or RT2 of less than 100 ms or greater than 2,000 ms were excluded from the RT analyses. Less than 2% of the trials were omitted because they fell outside these RT cutoffs.

RT1 and PE1. Mean RT1s are shown in Figure 2, and the corresponding PE data are shown in Table 3. Mean RT1 decreased from 556 ms in Experiment 1 to only 422 ms in Experiment 4, which primarily reflects the fact that T1 was converted from a non-IM-compatible task to an IM-compatible task (see Table 2). For RT1, the main effect of SOA was significant, $F(5, 75) = 12.52$, $p < .001$, $MSE = 529.12$, reflecting the fact that RT1 increased as SOA increased (400, 408, 425, 432, 433, and 433 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). The main effect of correspondence was not significant ($F < 1$), although the Correspondence × SOA interaction was significant, $F(5, 75) = 3.17$, $p < .05$, $MSE = 395.90$. The correspondence effect was larger at the four shortest SOAs than it was at the other two SOAs (7, 10, 6, 11, −18, and −13 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). The positive but small correspondence effect at the short SOAs was similar in magnitude to that observed with simultaneous presentation in Greenwald (1972): 10 ms. For PE1, no effects were significant.

RT2 and PE2. Mean RT2s are shown in Figure 3, and the corresponding PE data are shown in Table 4. As expected, mean RT2 at the longest SOA was reduced from 673 ms in Experiment 1 to 525 ms in Experiment 4 as a result of the conversion of both T1 and T2 from non-IM-compatible to IM-compatible tasks. For RT2, only the main effect of SOA was significant, $F(5, 75) = 6.27$, $p < .0001$, $MSE = 1{,}915.91$; RT2 decreased as SOA increased (577, 567, 555, 549, 535, and 525 ms at the 0-, 50-, 150-, 300-, 500-, and 1,000-ms SOAs, respectively). In a comparison of the shortest and longest SOAs, there was a significant PRP effect on RT2 of 52 ms, $F(1, 15) = 9.91$, $p < .01$, $MSE = 4{,}304.06$. The main effect of correspondence on RT2 was not significant, $F(1, 15) = 1.24$, $p = .2824$, $MSE = 840.07$. The Correspondence × SOA interaction on RT2 was also not significant in the omnibus analysis of variance and in a contrast comparison between the shortest and longest SOAs ($Fs < 1$). The correspondence effect averaged across the three shortest SOAs was only 8 ms for RT2. For PE2, no effects were significant.

Discussion

Experiment 4 used Greenwald's (1972) two IM-compatible tasks, but it did so within a PRP paradigm rather than a paradigm involving simultaneous presentation. We obtained a significant 52-ms PRP effect, consistent with Lien et al.'s (2002) conclusion that use of two IM-compatible tasks is not sufficient to eliminate the PRP effect. This PRP effect of 52 ms is similar to the 63-ms slowing of the visual-manual IM-compatible task in Greenwald (1972), computed by comparing mean RTs in the 2-decision and 1-decision blocks. Furthermore, the correspondence effect at the three shortest SOAs was negligible (only 8 ms) for both RT1 and RT2, which also replicates Greenwald's (1972) findings with simultaneous presentation of two IM-compatible tasks.

An important question is whether with two IM-compatible tasks, RT2 still depends on RT1 at the 0-ms SOA, as predicted by the central bottleneck model of IM-compatible tasks (see Equation 2). To examine the relationship between RT2 and RT1, we once again conducted an RT1 quintile analysis. The results of this analysis are shown in Figure 4. The strong, positive relationship observed between RT2 and RT1 is consistent with the central bottleneck model. The slope of the RT2–RT1 function was .53, similar to that in Experiment 2 (.55) and in Experiment 3 (.56). In the next section, we provide simulations of the central bottleneck model to determine whether it can account for these specific slope values.

According to the central bottleneck model of IM-compatible tasks, the reduction in mean RT1 across experiments should produce a similar reduction in the PRP effect ($\text{PRP} = \text{RT1} - 1\text{C} - 2\text{A} - \text{SOA}$). However, the reduction in the PRP effect relative to that in Experiment 1 (176 ms) was larger than the reduction in mean RT1 (134 ms). A similar pattern was found in Experiment 3. Furthermore, the central bottleneck model provides no clear explanation as to why the correspondence effects were greatly diminished at the three shortest SOAs (only 8 ms, compared with 30 ms in Experiment 1). At first glance, the small correspondence effect seems to be consistent with the G&S bottleneck bypass model. However, this model provides no obvious explanations as to why RT2 should depend strongly on RT1. Thus, neither the central bottleneck model of IM-compatible tasks nor the G&S bottleneck bypass model can easily account for all of the present findings.

Central Bottleneck Model Simulations

In Experiments 1–4, we observed a substantial PRP effect and a strong positive relationship between RT2 and RT1 regardless of whether one or both tasks were IM compatible. One could ask, however, whether the central bottleneck model is able to account for the specific slope values in Experiments 1–4. To answer this question, we conducted stochastic simulations of a central bottleneck model.

We assumed that each task comprises three stages: *A* (prebottleneck), *B* (bottleneck), and *C* (postbottleneck). The key assumption of the central bottleneck model is that Stage 2B does not start until Stage 1B has been completed (see Figure 1). For the purposes of these simulations, it makes no difference when Stage 1A ends and Stage 1B begins. Thus, for simplicity, we combined Stage 1A and Stage 1B into one super stage, *1AB* (the T1 stages prior to the release of the bottleneck). Similarly, we combined Stage 2B and Stage 2C into one super stage, *2BC* (the Task 2 stages after the release of the bottleneck). Thus, we modeled RT1 as the sum of two gamma-distributed stages (Stages 1AB and 1C) and RT2 as the sum of two gamma-distributed stages (Stages 2A and 2BC). Each gamma distribution has two parameters (α, β), which can be uniquely determined from the mean (μ) and the standard deviation (σ). Consequently, there were a total of eight parameters, μ and σ for each of four stages.

We assumed that IM compatibility, manipulated across experiments, would influence the parameters of the central stages but not the pre- and postbottleneck stages (i.e., 2A and 1C). Accordingly, the parameters for Stages 1C and 2A were constrained to have the same values across all four experiments.[2] Given the parameter values for Stage 1C, we determined the parameters for Stage 1AB in each experiment using Equations 4,

$$\mu_{1\mathrm{AB}} = M_{\mathrm{RT1(0\text{-}ms\ SOA)}} - \mu_{1\mathrm{C}}, \tag{4}$$

and 5,

$$\sigma_{1\mathrm{AB}} = \sqrt{[SD_{\mathrm{RT1(0\text{-}ms\ SOA)}}]^2 - (\sigma_{1\mathrm{C}})^2}, \tag{5}$$

so that the simulated RT1 values would match the observed mean and within-cell standard deviation of RT1 for that experiment at the shortest SOA. Likewise, having chosen a parameter value for Stage 2A, we determined the parameters for Stage 2BC for each experiment so that the simulated RT2 values at the longest SOA would match the observed mean and standard deviation of RT2 for that experiment. Thus, the only free parameters in this simulation were the means and standard deviations for Stages 1C and 2A.

To further reduce the search space, we simply set the mean of Stage 1C to 180 ms on the basis of a control experiment;[3] thus, in effect, there were a total of only three free parameters. We chose a range of values for the remaining free parameters for Stages 1C and 2A that seemed plausible, a priori. The mean of Stage 2A (identification of the stimulus word) was allowed to vary from 100 to 200 ms. We also allowed the coefficient of variation (defined as σ/μ) of 1C and 2A to vary from .20 and .30 (in steps of .01). For each parameter set, we simulated 1 million trials (sufficient to ensure that RTs could be estimated to within a tenth of a millisecond) in each of the four experiments. For each trial, RT1 and RT2 at the 0-ms SOA were calculated using the following equations:

$$\mathrm{RT1}_{\mathrm{0\text{-}ms\ SOA}} = \mathrm{1AB} + \mathrm{1C},$$

and

$$\mathrm{RT2}_{\mathrm{0\text{-}ms\ SOA}} = \max(\mathrm{1AB},\mathrm{2A}) + \mathrm{2BC}.$$

To assess the dependency between RT2 and RT1 at the 0-ms SOA in the simulated data, we followed the procedures used in Experiments 1–4 as closely as possible. The simulated trials for each simulated experiment were first arbitrarily subdivided into blocks so that the binning of the simulated data would be based on roughly the same number of trials as was that of the actual data. The simulated RT1 values in each block of each experiment were then rank ordered and divided into five equal-sized bins. Mean RT1 and RT2 were then computed for each bin. The simulated RT2 means (averaged over all blocks) were then compared with the observed RT2 means for the five RT1 bins in each of the four experiments (a total of 20 data points).[4] The overall goodness of fit was determined by the taking the square root of the sum of squared errors (root-mean-square error [RMSE]) between the 20 predicted and observed RT2 means.

The set of parameter values that produced the smallest RMSE is summarized in Table 5. The corresponding values for RT1 and RT2 from the simulations, along with the observed values, are plotted in Figure 5. These simulations show that a bottleneck model with plausible parameter values can in fact produce the slopes observed with IM-compatible tasks in Experiments 2–4 with a high degree of accuracy. Thus, the present data are consistent with the conclusion that a bottleneck occurs even with IM-compatible tasks.

We argue that the data are not only consistent with bottleneck models but also provide strong support for such models. We found a large set of parameter combinations within the plausible parameter space that roughly reproduced the overall observed PRP effects. Within this set of successful parameter combinations, the simulated slopes fell within a relatively narrow range (e.g., .75–.90 in Experiment 1, .40–.60 in Experiment 4). The fact that the bottleneck model naturally predicts the changes in slopes across experiments as well as the approximate slope values in each experiment adds considerably to its appeal.

---

[2] Note that all experiments used the same responses for T1 (i.e., left or right movement) and the same type of auditory stimuli for T2 (i.e., 250-ms spoken word). Therefore, if the locus of the bottleneck has not shifted, it is reasonable to assume that the durations of Stages 1C and 2A were roughly the same across experiments.

[3] To estimate the duration of Stage 1C, we conducted a simple-RT control experiment in which participants made joystick responses to an arrow that always pointed in the same direction within a block. We measured both the overall RT (from stimulus onset until the joystick had traversed 75% of the maximum displacement) and the movement time (from movement onset to the 75% threshold). We reasoned that 1C should be less than the total RT (which also includes perceptual processing) but more than just the movement time (which does not include response-initiation time). Therefore, as a rough estimate of 1C, we simply averaged these two times, which turned out to be 180 ms.

[4] Although the model parameters were constrained to exactly predict the actual mean and standard deviation of RT1 for each experiment, slight miscalculation of the RT1 value for each bin (due to sampling error and incorrect distributional assumptions) was possible. To calculate a pure measure of the error in RT2 at each bin, it was necessary to take into account any misprediction of RT1. Because the simulated RT1 and RT2 values across the five bins were linearly related (see Figure 5), our solution was to calculate the RT2–RT1 regression equation for the simulated data and then use this equation to estimate the simulated RT2 value at each observed RT1 value. We obtained nearly identical results using alternative methods for estimating RT2 error.

Table 5
Best-Fitting Parameter Values for the Central Bottleneck Model in Experiments 1–4 (RMSE = 23.78)

	1AB		1C		2A		2BC	
Exp	μ	σ	μ	σ	μ	σ	μ	σ
1	368	110.0	180	50.4	154	37.0	519	177.2
2	243	60.8	180	50.4	154	37.0	479	156.7
3	309	72.1	180	50.4	154	37.0	424	98.3
4	220	58.5	180	50.4	154	37.0	371	86.4

Note. RMSE = root-mean-square error; Exp = experiment; 1AB = prebottleneck stage for Task 1; 1C = postbottleneck stage for Task 1; 2A = prebottleneck stage for Task 2; 2BC = postbottleneck stage for Task 2; μ = mean; σ = standard deviation.

The complete bottleneck bypass model could account for a RT2–RT1 relationship across trials with the assumption that there are trial-by-trial variations in general arousal or attention.[5] However, this account provides no principled explanation for the particular slope values obtained in the present experiments, nor can it easily explain why the relationship was so strong. Furthermore, this account by itself does not predict any PRP effect; some additional assumption would therefore be needed. Accordingly, we argue that this alternative account of our data is unattractive.

Partial Bottleneck Bypass

Although there is no evidence that IM-compatible tasks completely bypassed the bottleneck, it remains possible that partial bypass occurred. Not only is partial bypass an important logical possibility, there is also some preliminary evidence for it. In particular, the reduction in the PRP effect from Experiment 1 to Experiments 3 and 4 was somewhat larger than the central bottleneck model of IM-compatible tasks would predict on the basis of the reduction in mean RT1. In the introduction, we outlined three plausible partial bypass hypotheses. In this section, we describe a series of quantitative simulations designed to evaluate them.

Bypass Bottleneck on a Stage-by-Stage Basis: A Shift in Bottleneck Locus

One plausible hypothesis is that a bottleneck occurs on every trial with IM-compatible tasks, but some substage of the bottleneck is bypassed. On this view, there is a set of bottleneck substages with non-IM-compatible tasks such that when any one of these substages is active on one task, no bottleneck substage can take place on any other task; the set of bottleneck substages, however, is assumed to be smaller for IM-compatible tasks. Consider, for example, the analogy of a grocery clerk (the "bottleneck") responsible for scanning items to determine their prices, accepting payment, and then bagging the items. One or more of these substages could be automated so that it could be performed without the clerk (i.e., partially bypassing the bottleneck).

As we described in the introduction, there are two specific versions of this partial bypass model. The *release-bottleneck-earlier model* assumes that for IM-compatible tasks, an early substage is still a part of the bottleneck but a late substage is not. This model implies that, relative to non-IM-compatible tasks, the duration of the bottleneck stage (e.g., Stage B) will decrease, causing the duration of the postbottleneck stage (e.g., Stage C) to increase correspondingly. In the grocery clerk example, releasing the bottleneck earlier could be accomplished by automating the bagging process so that the clerk can proceed to the next customer while someone (or something) else does the bagging.

Figure 6 shows this model under four combinations of IM- and non-IM-compatible T1s and T2s (corresponding to the present Experiments 1–4). Because the main goal of this figure is to demonstrate the impact of releasing the bottleneck earlier on the PRP–RT1 relationship, it is convenient to assume a constant mean RT1 across experiments (even though this is, of course, not the case). When both T1 and T2 are non-IM compatible, the usual set of substages constitutes the bottleneck (as shown in Figure 6A). When T1 is IM compatible and T2 is non-IM compatible, the bottleneck stage on T1 is released earlier in time. Thus, the duration of Stage 1B decreases, and the duration of Stage 1C increases by the same amount. This trade-off will decrease the PRP effect without changing RT1, as can be seen from a comparison of Figures 6A and 6B. When T1 is non-IM compatible and T2 is IM compatible, the earlier bottleneck release on T2 decreases the duration of Stage 2B and increases the duration of Stage 2C. Yet this trade-off does not affect the PRP–RT1 relationship, as can be seen from a comparison of Figures 6A and 6C and from the PRP equation PRP = RT1 − 1C − 2A. Next, consider the case in which both T1 and T2 are IM compatible (see Figure 6D). Although the earlier bottleneck release on T2 will have no impact on the PRP effect, the earlier bottleneck release on T1 will lead to less PRP effect for any given RT1 relative to the case with two non-IM-compatible tasks (see Figure 6D vs. Figure 6A). In summary, the release-bottleneck-earlier model suggests that the PRP–RT1 function should depend on whether T1 is IM compatible but not on whether T2 is IM compatible.

An alternative is the *engage-bottleneck-later* model. This model assumes that, for IM-compatible tasks, a late substage is still a part

---

[5] A reviewer pointed out that a positive correlation between RT2 and RT1 could also occur if occasional periods of inattention cause a delay in the processing of both T1 and T2. However, to account for the strong RT2–RT1 relationship observed in Experiment 4 (see Figure 4), the delays would need to occur frequently, often lasting more than 100 ms. Given the lack of prior evidence for such delays, it seems much more plausible that the observed RT2–RT1 relationship is a result of a processing bottleneck (for which there is prior evidence).

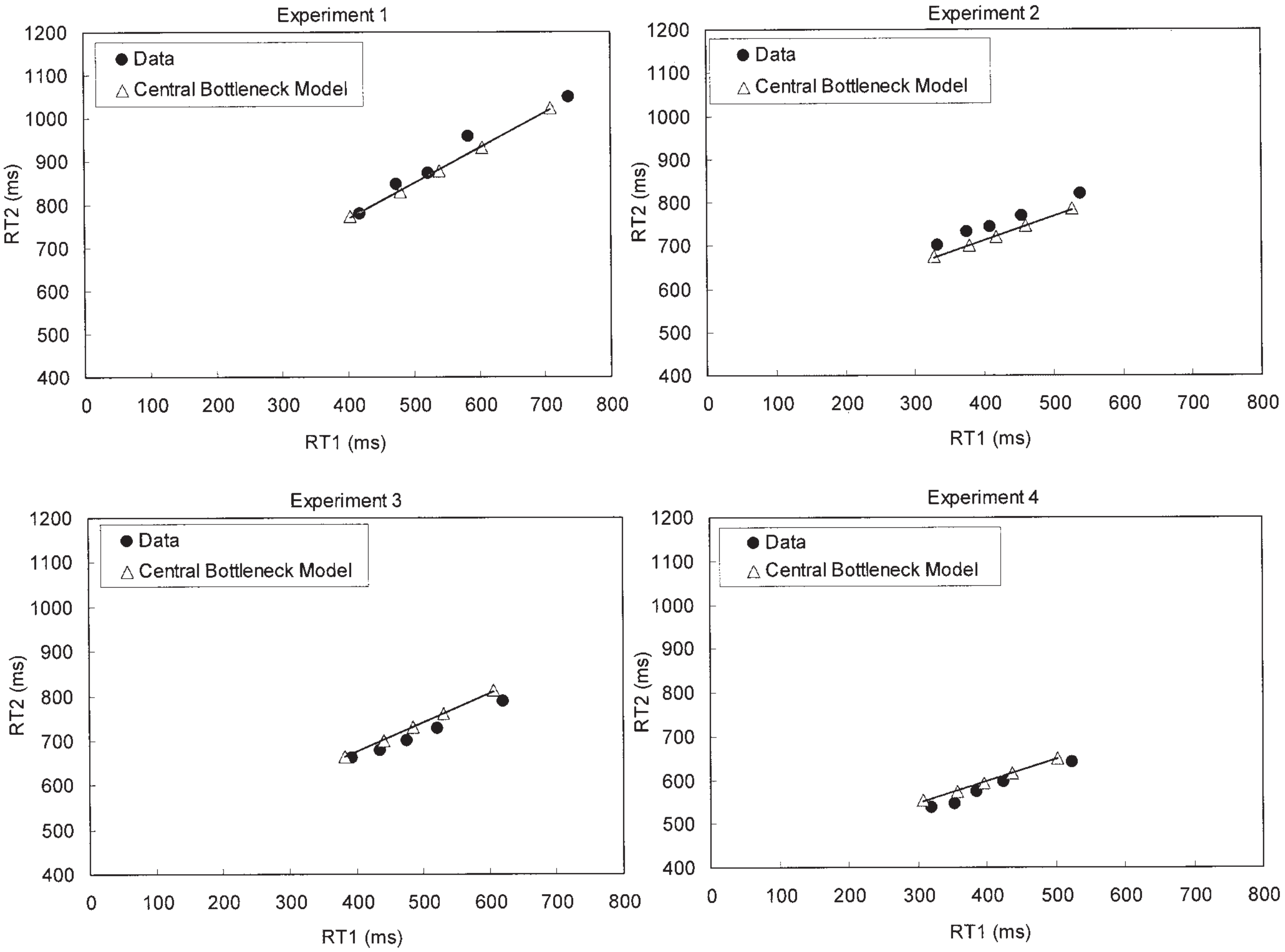

Figure 5. Task 2 response time (RT2) as a function of Task 1 response time (RT1) at the 0-ms SOA in Experiments 1–4. The filled circles represent the observed data and the open triangles represent the best-fitting values from the central bottleneck model.

of the bottleneck, but an early substage is not. Thus, relative to non-IM-compatible tasks, the duration of the bottleneck stage (e.g., Stage B) will decrease, causing the duration of the prebottleneck stage (e.g., Stage A) to increase by the same amount. In the grocery clerk example, engaging the bottleneck later could be accomplished by automating the scanning of items.

Figure 7 illustrates this model under four combinations of IM- and non-IM-compatible T1s and T2s. When both T1 and T2 are non-IM compatible, the usual set of substages constitutes the bottleneck (see Figure 7A). When T1 is IM compatible and T2 is non-IM compatible, the bottleneck stage on T1 is engaged later in time, as indicated by the right-pointing arrow in Figure 7B. Thus, the duration of Stage 1A increases and the duration of Stage 1B decreases by the same amount. However, the trade-off between the duration of Stage 1A and Stage 1B should not affect the PRP–RT1 relationship across experiments. This prediction can be seen from the comparison of Figures 7A and 7B and also from the PRP equation for the 0-ms SOA (PRP = RT1 − 1C − 2A). When T1 is non-IM compatible and T2 is IM compatible, however, the duration of Stage 2A increases, whereas the duration of Stage 2B decreases by the same amount (see Figure 7C). Relative to the case with two non-IM-compatible tasks, the trade-off between the durations of Stage 2A and Stage 2B decreases the PRP effect without changing RT1. Thus, less PRP effect is expected for any given RT1. The same prediction also applies when both T1 and T2 are IM compatible (see Figure 7D). In summary, the engage-bottleneck-later hypothesis predicts that the PRP–RT1 function should depend on whether T2 is IM compatible but not on whether T1 is IM compatible.

Evidence for the Engage-Bottleneck-Later Model

One critical difference in predictions between the release-bottleneck-earlier model and the engage-bottleneck-later model concerns whether it is T1 or T2 IM compatibility that is predicted to alter the PRP–RT1 function. Therefore, we evaluated these models by examining whether the PRP–RT1 function changes across the four experiments, which involve different combinations of T1 and T2 IM compatibility.

According to the PRP equation derived from the central bottleneck model (PRP = RT1 − 1C − 2A), it follows that RT1 − PRP = 1C + 2A (see Equation 3). If no substages of the bottleneck have been bypassed, then there is no reason to expect any change in the durations of Stages 1C and 2A for IM-compatible tasks compared with those for non-IM-compatible tasks. Accordingly, the value of RT1–PRP should be roughly constant across experiments (ignoring sampling error).[6] However, if some substages of the bottleneck have been bypassed with IM-compatible tasks, then both versions of the partial bottleneck bypass model predict that the quantity of RT1–PRP (i.e., 1C + 2A) should increase. More specifically, the release-bottleneck-earlier model predicts that the quantity of RT1–PRP should increase only when T1 is IM compatible, whereas the engage-bottleneck-later model predicts that the quantity of RT1–PRP should increase only when T2 is IM compatible.

To test these predictions, we conducted an additional data analysis using RT1–PRP as the dependent variable; specifically, we took RT1 at the 0-ms SOA for each participant and subtracted the corresponding PRP effect. We then analyzed these data as a function of T1 compatibility (IM or non-IM) and T2 compatibility (IM or non-IM). Although there was no main effect of T1 type ($F < 1$), there was a significant main effect of T2 type, $F(1, 63) = 4.12$, $p < .05$, $MSE = 26{,}975.00$. The value of RT1–PRP was larger when T2 was IM compatible (353 ms) than when T2 was non-IM compatible (312 ms). Thus, this data analysis confirms the predictions of the engage-bottleneck-later model.

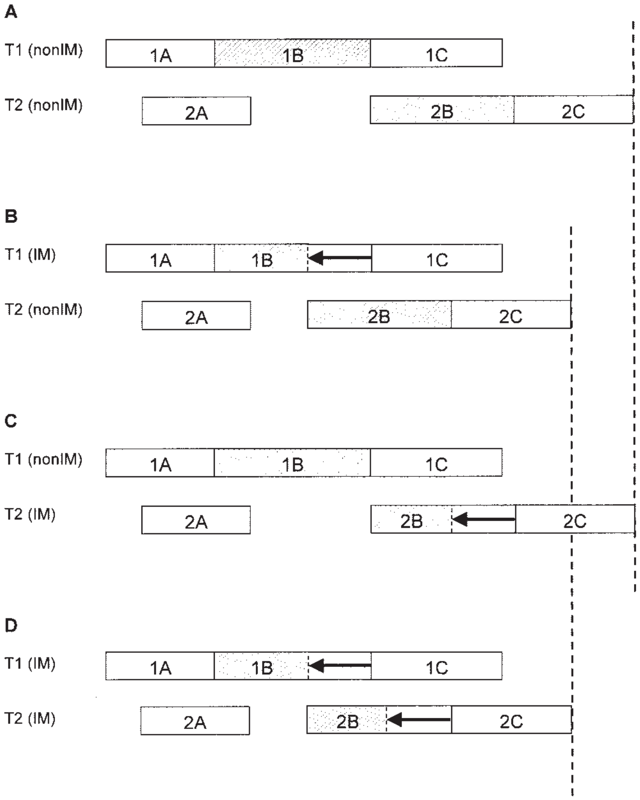

Figure 6. The release-bottleneck-earlier model under four (A–D) combinations of IM- and non-IM-compatible Task 1 (T1) and Task 2 (T2). This model assumes that a late subset of the bottleneck stages for non-IM-compatible tasks is not a part of the bottleneck stage for IM-compatible tasks. Shaded boxes indicate the bottleneck stages, and left-pointing arrows indicate that a bottleneck stage was released earlier in time. 1A, 1B, and 1C are the prebottleneck, bottleneck, and postbottleneck stages, respectively, of T1. 2A, 2B, and 2C are the corresponding stages for T2; dashed lines between panels indicate the times at which T2 processing is completed. nonIM = nonideomotor-compatible task; IM = ideomotor-compatible task.

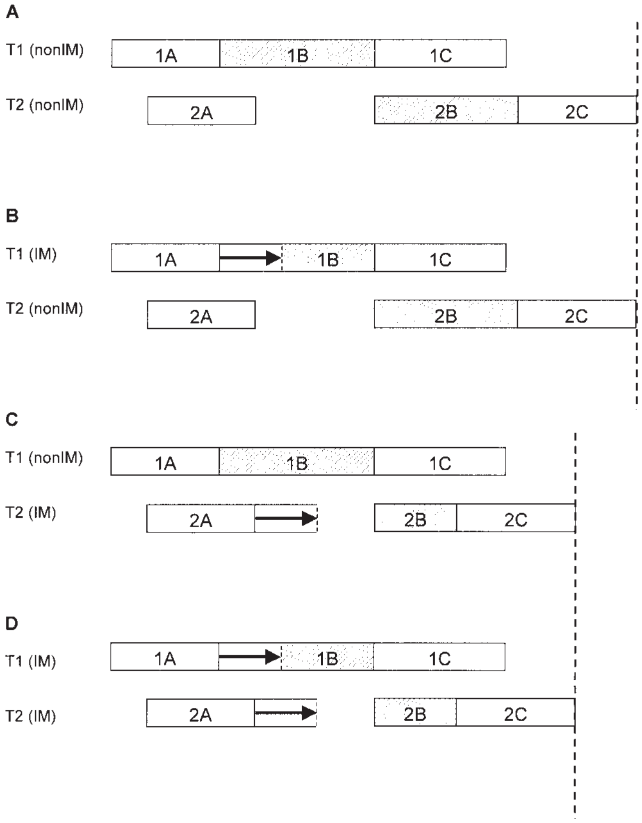

Figure 7. The engage-bottleneck-later model under four combinations (A–D) of IM- and non-IM-compatible Task 1 (T1) and Task 2 (T2). This model assumes that an early subset of the bottleneck stage for non-IM-compatible tasks is not a part of the bottleneck stage for IM-compatible tasks. Shaded boxes indicate the bottleneck stages, and right-pointing arrows indicate that a bottleneck stage was engaged later in time. 1A, 1B, and 1C are the prebottleneck, bottleneck, and postbottleneck stages, respectively, of T1. 2A, 2B, and 2C are the corresponding stages for T2; dashed lines between panels indicate the times at which T2 processing is completed. nonIM = nonideomotor-compatible task; IM = ideomotor-compatible task.

---

[6] The prediction that the quantity RT1–PRP should be constant across experiments depends on the assumptions that the bottleneck is (a) never encountered at the long SOA and (b) always encountered at the short SOA. The latter assumption is likely to be violated when T1 is IM-compatible and RT1 is very short (so that Stage 1B sometimes finishes before Stage 2A finishes), causing the quantity RT1–PRP to decrease. Note that this effect is the opposite of that predicted by partial bottleneck bypass models and opposite to the actual data pattern. Furthermore, bottleneck model simulations indicated that the decrease from Experiments 1–4 would amount to only a few milliseconds.

Table 6

Best-Fitting Parameter Values for the Engage-Bottleneck-Later and Intermittent Bottleneck Bypass Models in Experiments 1–4

	1AB		1C		2A		2BC	
Exp	μ	σ	μ	σ	μ	σ	μ	σ
Engage-bottleneck-later model (EBL = 45; RMSE = 11.16)								
1	368	110.0	180	50.4	131.5	42.4	542	176.0
2	243	60.8	180	50.4	131.5	42.4	502	155.3
3	309	72.1	180	50.4	176.5	42.4	402	96.1
4	220	58.5	180	50.4	176.5	42.4	349	83.9
Intermittent bottleneck bypass model (IBB = 0.27; RMSE = 13.40)								
1	368	110.0	180	50.4	131.5	31.6	542	178.3
2	243	60.8	180	50.4	131.5	31.6	502	157.9
3	309	72.1	180	50.4	131.5	31.6	447	100.1
4	220	58.5	180	50.4	131.5	31.6	394	88.5

Note. Exp = experiment; 1AB = prebottleneck stage for Task 1; 1C = postbottleneck stage for Task 1; 2A = prebottleneck stage for Task 2; 2BC = postbottleneck stage for Task 2; μ = mean; σ = standard deviation; EBL = engage-bottleneck-later parameter (which determines how much later the bottleneck is engaged for ideomotor-compatible tasks compared with non-ideomotor-compatible tasks); RMSE = root-mean-square error; IBB = intermittent bottleneck bypass parameter (which determines the proportion of trials on which the bottleneck is completely bypassed when Task 2 is ideomotor compatible).

Although the engage-bottleneck-later model can explain the relationship between mean PRP effects and mean RT1 across experiments, one might question whether this model can explain the RT2–RT1 dependency across trials at the 0-ms SOA in each experiment (see, e.g., Figure 4). To determine whether this model can provide a close fit, we conducted an additional set of simulations. The simulated model was identical to the general bottleneck model simulated above, except that we added a parameter (*EBL*; i.e., engage bottleneck later) that determined how much later the bottleneck was engaged for IM-compatible tasks than it was for non-IM-compatible tasks. To facilitate comparisons between these simulations and the central bottleneck model simulation described earlier, except for the impact of *EBL* we used the exact same parameter values from the general bottleneck model simulation (see Table 5).

The best-fitting value for *EBL* in these simulations was 45 ms (see Table 6), implying that IM compatibility allows participants to automatize a bottleneck substage lasting 45 ms. The corresponding values for RT1 and RT2 from the simulations, along with the observed values, are plotted in Figure 8. These simulations show that a bottleneck model with the *EBL* parameter did in fact produce a much closer fit to the data for Experiments 1–4 (RMSE = 11.16) than did the central bottleneck model without this parameter (RMSE = 23.78)[7]. Furthermore, there are no large or systematic deviations between the observed and simulated data. Thus, these simulations provide further support for the engage-bottleneck-later model.

Bottleneck Bypass on a Trial-by-Trial Basis

One might argue that partial bottleneck bypass with an IM-compatible T2 occurred not on a stage-by-stage basis, as suggested by the engage-bottleneck-later model, but rather on a trial-by-trial basis. To evaluate this *intermittent-bottleneck-bypass* model, we conducted a further simulation that was similar to our original central bottleneck model simulation but included one additional parameter (*IBB*; i.e., intermittent bottleneck bypass) corresponding to the proportion of trials on which the bottleneck is completely bypassed with an IM-compatible T2. As a starting point, we simply selected the parameter values for Stages 1C and 2A that worked well in Experiments 1 and 2 of the engage-bottleneck-later simulations (see Table 6), but we allowed *IBB* to vary from 0 to 1. On a proportion of trials equal to *IBB*, the bottleneck is completely bypassed, and thus $RT2_{0\text{-ms SOA}} = 2A + 2BC$. On the remaining proportion of trials, equal to $1 - IBB$, the bottleneck is not bypassed, and thus $RT2_{0\text{-ms SOA}} = \max(1AB, 2A) + 2BC$. The best-fitting *IBB* parameter value for the intermittent-bottleneck-bypass model was .27. We also conducted an additional set of simulations in which we searched the entire range of plausible parameter values for 1C and 2A. However, none of these combinations of parameter values produced a substantially better fit to the observed data than the combination taken from the engage-bottleneck-later simulations.

The simulated values for RT1 and RT2 for the intermittent-bottleneck-bypass model, along with the observed values, are plotted in Figure 8. These simulations show that a bottleneck model with intermittent bypass can provide a reasonable fit to the data (RMSE = 13.40). However, these fits were slightly worse

---

[7] We conducted an additional set of simulations of the engage-bottleneck-later model in which we allowed the parameters for Stages 1C and 2A to take on any values within the range of plausible values established above (e.g., allowing the mean of Stage 2A to vary between 200 and 300 ms). However, these simulations did not yield in any parameter sets that provided a noticeably better fit than did the set taken from the generic central bottleneck model simulation.

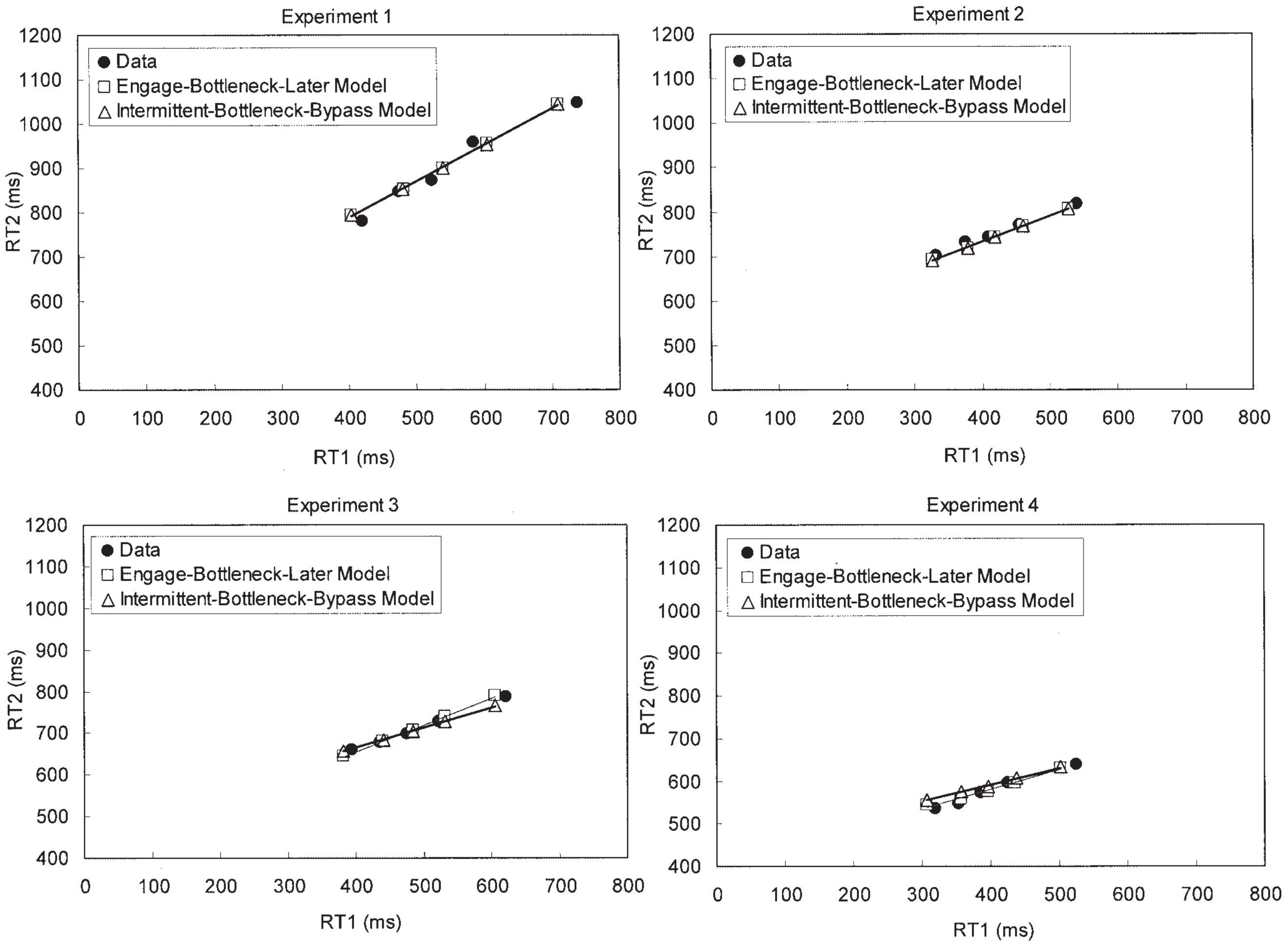

Figure 8. Task 2 response time (RT2) as a function of Task 1 response time (RT1) at the 0-ms SOA in Experiments 1–4. The filled circles represent the observed data, the open squares represent the best-fitting values from the engage-bottleneck-later model, and the open triangles represent the best-fitting values from the intermittent-bottleneck-bypass model.

than those of the engage-bottleneck-later model (RMSE = 11.16). More important, this model systematically underestimated the RT2–RT1 slopes in Experiments 3 and 4. Furthermore, this model does not provide an obvious explanation for the dramatic reduction in correspondence effects from Experiments 1 and 2 to Experiments 3 and 4. Given a bypass proportion of .27, one might naturally expect correspondence effects to decrease by only 27% (from 30 to 20 ms), whereas the actual reduction was closer to 61% (from 30 to 8 ms). Thus, although the intermittent-bottleneck-bypass model cannot be definitively ruled out, the engage-bottleneck-later model appears to provide a more attractive account.

General Discussion

The present study examined a possible boundary condition for the central bottleneck—namely, whether the central bottleneck is bypassed with IM-compatible tasks. As we noted earlier, a small PRP effect does not necessarily indicate that the central bottleneck has been bypassed. Indeed, the central bottleneck model predicts a small or even nonexistent PRP effect when RT1 is relatively short. Thus, to provide converging evidence for whether the bottleneck is bypassed with IM-compatible tasks, the present study also closely examined the dependency between RT2 and RT1 at the 0-ms SOA across trials and across experiments (see, e.g., Pashler et al., 1993) as well as cross-task correspondence effects. We also conducted model-based simulations to provide a stricter test of the candidate models.

Experiment 1 used two non-IM-compatible tasks, as in traditional PRP studies, and obtained a baseline PRP effect of 228 ms and a baseline correspondence effect of 30 ms on RT2 (averaged across the three shortest SOAs). RT2 depended strongly on RT1 across trials (with a slope of .84), consistent with the central bottleneck model. Experiments 2–4 then examined whether the bottleneck is bypassed when one or both tasks are IM compatible. To create IM-compatible tasks, we made small changes to the non-IM-compatible tasks of Experiment 1, keeping the input and output modalities the same. The responses for T1 and T2 were exactly the same in all experiments.

If central operations are not needed for IM-compatible tasks, then the bottleneck should be bypassed when either T1 or T2 is IM compatible. This generic bottleneck bypass model makes several key predictions for the present Experiments 2–4. First, this model provides no obvious reason to expect a PRP effect when either or both tasks are IM compatible. Second, there is no obvious reason to expect a strong dependency between RT2 and RT1 at the 0-ms SOA. Third, assuming that the locus of the correspondence effect is in central operations (e.g., Hommel, 1998; Lien & Proctor, 2000; Lien, Schweickert, & Proctor, 2003; Logan & Schulkind, 2000), the generic bottleneck bypass model predicts little or no correspondence effect. Contrary to these predictions, a PRP effect of 120 ms and a correspondence effect of 28 ms (averaged across the three shortest SOAs) were found when T1 was IM compatible and T2 was non-IM compatible (Experiment 2). Similarly, a PRP effect of 131 ms was obtained when T1 was non-IM compatible and T2 was IM compatible (Experiment 3). Both experiments also revealed a strong dependency between RT2 and RT1 (see Figure 4). Thus, these findings argue against the hypothesis that the central bottleneck is bypassed completely with only one IM-compatible task.

To account for the PRP effect and the strong dependency between RT1 and RT2 in Experiments 2 and 3 (in which only one task was IM compatible), one could instead propose that the central bottleneck is bypassed only when both tasks are IM compatible (e.g., the G&S bottleneck bypass model). According to this model, PRP and correspondence effects should have been very small in Experiment 4, in which both T1 and T2 were IM compatible. Similarly, there would be no reason to expect a strong dependency between RT1 and RT2 in Experiment 4. Although the correspondence effect was small in Experiment 4, there was still a significant PRP effect of 52 ms.[8] Furthermore, as in Experiments 1–3, RT2 in Experiment 4 depended strongly on RT1 (see Figure 4). This combination of results cannot easily be explained by the hypothesis that the central bottleneck is bypassed with two IM-compatible tasks.

The same evidence that is inconsistent with bottleneck bypass argues in favor of the central bottleneck model of IM-compatible tasks. Specifically, the central bottleneck model correctly predicted a substantial PRP effect in all experiments and a strong, positive dependency between RT1 and RT2 across trials at the 0-ms SOA. Furthermore, our simulations show that a central bottleneck model with plausible parameter values can in fact produce the RT2–RT1 slope values observed with IM-compatible tasks very accurately.

Although the data suggest that even IM-compatible tasks were limited by a processing bottleneck, there were several clues that the locus of the bottleneck was different for IM- and non-IM-compatible tasks. First, the simulations of the central bottleneck model showed that the model underestimated the slope relating RT1 and RT2 when T2 was non-IM compatible (Experiments 1 and 2), and it overestimated the slope when T2 was IM-compatible (Experiments 3 and 4; see Figure 5). Second, data analyses showed that the quantity of RT1–PRP was not constant across experiments—as predicted by the central bottleneck model—but, rather, depended on whether T2 was IM compatible or non-IM compatible.

To explain these findings, we proposed that some substage or substages that are part of the bottleneck for non-IM-compatible tasks are not a part of the bottleneck for IM-compatible tasks. We considered two specific bottleneck substage bypass models. Further data analyses and simulations revealed that the engage-bottleneck-later model (which assumes that a late central substage is still a part of the bottleneck with IM-compatible tasks but an early central substage is not) provides a much better account of the changes in the PRP effect across experiments than the release-bottleneck-earlier model. Specifically, the engage-bottleneck-later model correctly predicted that the PRP effect would be especially small (in relation to mean RT1) when T2 was IM compatible (see Figure 7). Furthermore, simulations of this model produced an excellent fit to the observed data (see Figure 8).

We also considered and simulated the intermittent-bottleneck-bypass model, which assumes that complete bottleneck bypass occurs on some proportion of trials when T2 is IM compatible. However, this particular model did not produce as good a fit to the present data as did the engage-bottleneck-later model.

Cross-Task Correspondence Effects: Converging Evidence for Partial Bottleneck Bypass

In each of the four experiments reported in the present study, T1 required a left or right movement of a joystick and T2 required a vocal "left" or "right" response. The fact that responses to both tasks involved a common spatial dimension provided an opportunity to measure the impact of IM compatibility on cross-task correspondence effects. Forward correspondence effects from T1 to T2 (averaged across the three shortest SOAs) were substantial in Experiment 1 (30 ms), in which neither task was IM compatible, and in Experiment 2 (28 ms), in which T1 was IM compatible but T2 was not. However, correspondence effects were reduced to a nonsignificant 9 ms in Experiment 3, in which T2 was IM compatible but T1 was not, and to 8 ms in Experiment 4, in which both tasks were IM compatible; these effect sizes are similar to those found with two IM-compatible tasks in Greenwald (1972). These results suggest that the key factor determining the size of the forward correspondence effect is T2 IM compatibility: Correspondence effects were substantial when T2 was non-IM compatible (Experiments 1 and 2), but they were very small when T2 was IM compatible (Experiments 3 and 4).

The conclusion that T2 IM compatibility determines the correspondence effect mirrors the conclusion reached above, on the basis of the PRP–RT1 relationship, that T2 IM compatibility determines whether partial bottleneck bypass occurs (as suggested by the engage-bottleneck-later model). Thus, two lines of evidence converge on the conclusion that dual-task processing is qualitatively different when T2 is IM compatible than it is when T2 is

---

[8] As we noted in the introduction, a PRP effect can occur even if the bottleneck has been bypassed. One reason is that as SOA increases, S2 onset becomes more and more predictable (reducing RT2). Although such an effect is logically possible, we are aware of no evidence for it (see Bertelson, 1967; Pashler, 1998, pp. 274–275). In addition, experiments with two IM-compatible tasks using blocked SOAs have found PRP effects of similar magnitude to that of the present Experiment 4 (Lien et al., 2002; Shin, Cho, Lien, & Proctor, 2005). More important, the S2-predictability account of the PRP effect provides no obvious explanation for the strong relationship observed between RT1 and RT2 across trials.

non-IM compatible. Earlier, we provided evidence in favor of the engage-bottleneck-later model for IM-compatible tasks. According to this model, there are multiple bottleneck substages with non-IM-compatible tasks, and the earliest such substages are bypassed when the tasks are IM compatible. Can this model also explain the virtual absence of correspondence effects in Experiments 3 and 4? For it to do so, one need only assume that cross-task correspondence effects are located in the bypassed substage(s).

A Code Translation Hypothesis for Central Operations

In this section, we describe a variant of the engage-bottleneck-later model that more clearly specifies the representations and processes involved in each substage; in particular, this variant describes how IM compatibility influences the component stages and the interference (or lack thereof) between them. Our account, illustrated in Figure 9, begins with the long-standing assumption that S-R translation in a choice-RT task involves a sequence of code translations (e.g., Hommel, Müsseler, Aschersleben, & Prinz, 2001). We further assume that the translations involve three primary codes: perceptual codes (PC), abstract response codes (ARC), and motor codes (MC).

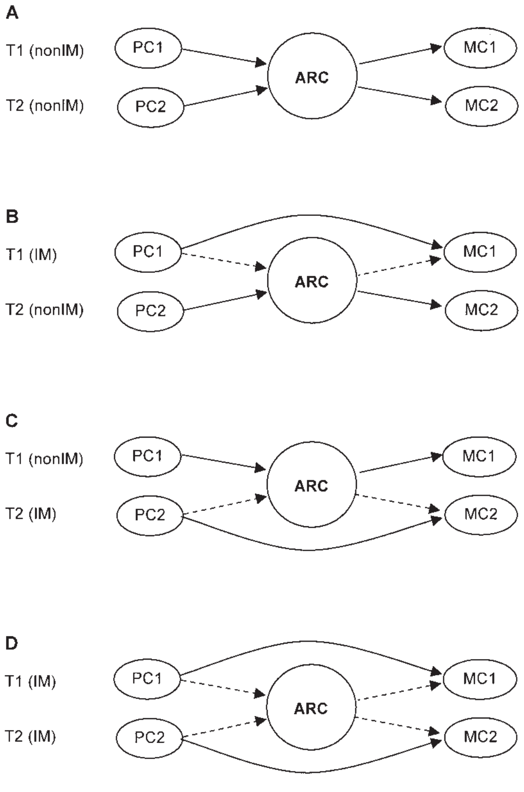

Figure 9. The code translation hypothesis for central operations under four combinations of IM and non-IM Task 1 (T1) and Task 2 (T2). Code translation from stimulus input to response output involves perceptual codes (PCs), abstract response codes (ARCs), and motor codes (MCs). IM-compatible tasks are assumed to bypass the ARC translation and directly activate MCs. The solid arrow indicates the critical pathway, and the dashed arrow indicates the incidental pathway. nonIM = nonideomotor-compatible task; IM = ideomotor-compatible task; PC1 = PC for T1; PC2 = PC for T2; MC1 = MC for T1; MC2 = MC for T2.

When a task is non-IM compatible, there is little direct featural overlap between PCs and MCs. For example, in T1 of Experiment 1, the letters *A* and *H* have no obvious conceptual similarity with effector (hand) movements coded within a left–right dimensional framework. We propose, therefore, that the translation of stimulus codes into response codes is mediated by applying the rule "if *A* then *left*, and if *H* then *right*" (Duncan, 1977; Pashler & Baylis, 1991). It is important to note that "left" and "right" are abstract representations, possibly corresponding to a pattern of activation across the same set of semantic units that are activated when people read the word *left* or *right*. Once such an ARC is sufficiently activated, an additional translation operation is required to convert it into the actual MC that specifies the appropriate effector and the appropriate action (e.g., apply pressure with the right hand to move the joystick to the left). For the present purposes, there is no need to be overly specific concerning the precise nature of the ARC and the final MC; we simply assume that the ARC is sufficiently abstract that further translation is required to activate a specific MC.

In this framework, the abstract nature of the central code is the key to producing crosstalk between tasks. Again referring to Experiment 1 (in which two non-IM-compatible tasks were used; see Figure 9A), suppose that the T1 stimuli *A* and *H* activate the abstract concepts "left" and "right." Because the stimuli for T2 (the spoken numbers "one" and "two") bear no physical resemblance to their required responses (the spoken words "left" and "right"), they also require a translation from PC to ARC. We propose that the ARC for T1 overlaps conceptually with the ARC for T2 (i.e., the ARCs for both tasks are the semantic representations for "left" and "right"). When the two tasks generate compatible ARCs, the T1 ARC preactivates (primes) the ARC for T2 and, thus, facilitates T2 processing; however, when the two tasks generate incompatible codes, ARC activation for T2 is slowed. The net result is a correspondence effect on RT2.

What happens when either or both tasks are IM compatible? Unlike non-IM-compatible tasks, IM-compatible tasks have considerable featural overlap between PCs and MCs. Following several previous proposals (e.g., Greenwald, 1972), we suggest that strong featural overlap is a sufficient condition to activate the motor code directly (although not necessarily instantly), eliminating the need for rule-based activation of an ARC. Thus, as illustrated in Figures 9B-9D, the substage of rule-based translation of a PC into an ARC is bypassed for IM-compatible tasks. Moreover, because ARCs play no direct role in IM-compatible task processing, correspondence effects are reduced or eliminated.

This ARC-bypass hypothesis for IM-compatible tasks straightforwardly explains why an IM-compatible T2 is not subject to correspondence effects. However, Experiment 2 yielded evidence that an IM-compatible T1 can still "donate" crosstalk to a non-IM-compatible T2. To account for this result, we make the additional assumption that with our particular IM-compatible tasks, IM PCs incidentally activate the associated ARCs. For example, there is evidence that simply focusing spatial attention on a spoken word such as "left" activates its meaning, even when the task (e.g., repeating the word aloud) does not require or make direct use of such a representation. When T2 is non-IM compatible and thus does make use of an ARC, it will receive crosstalk from the incidental activation by T1 of the same (or a similar) ARC.

Our account suggests that "the" dual-task bottleneck may have a variable locus, depending on which central operations are (or are not) involved in the task. When T2 is IM compatible, the bottleneck is late, involving selection among activated MCs. Why would such a late process have a bottleneck? We speculate that even if the MCs for T1 and T2 are quite distinct, there still is an MC-to-task binding problem (see Logan & Gordon, 2001) that cannot be solved without a selection process. When the tasks are non-IM compatible and thus require ARC mediation, the bottleneck also includes an earlier locus. Logically, a number of operations are candidates for the earlier bottleneck locus, including application of the S-R translation rule needed to activate the ARC, selection among activated ARCs, and/or translation of the selected ARCs into MCs. At this point, it is not necessary to commit to any one of these possibilities, although we tentatively suggest that the bottleneck includes all three operations.

The present code-translation hypothesis is closely related to, but clearly distinct from, the two-component response selection framework proposed by Hommel (1998). The two-component model assumes that S-R translation rules are applied automatically, activating response codes in parallel for both tasks. The act of selecting a final response code from the pool of activated candidates, however, is assumed to take place for only one task at a time. Our account differs from Hommel's model in several respects. First, Hommel argued for a translation from PCs to a single type of response code. In contrast, we draw a critical distinction between two different forms of response coding, ARCs and MCs. Furthermore, Hommel argued that S-R translation is automatic and thus not subject to the bottleneck. We argue that it might not be entirely automatic, at least not for non-IM-compatible tasks.

Is the Change in PRP Effects a Result of the Change in Correspondence Effects?

Because correspondence effects on RT2 are generally stronger at short SOAs than at long SOAs, they can modulate the size of the PRP effect to some extent. Assuming that these correspondence effects stem mostly from a cost on noncorresponding trials rather than a benefit on corresponding trials, it follows that correspondence effects should inflate the PRP effect. This observation raises the question of whether the reduction in correspondence effects from Experiments 1 and 2 to Experiments 3 and 4 can directly explain the concomitant reduction in the PRP effect relative to RT1. In other words, it may be possible to explain the present data without proposing a shift in bottleneck locus.

One drawback of this account is that, given the assumption that there was no change in the bottleneck locus, it does not explain why correspondence effects decreased in Experiments 3 and 4. Even more seriously, this account makes the wrong prediction for the condition in which R1 and R2 correspond. In this condition, the obvious effect of correspondence would be a decrease in RT2 at short SOAs in Experiments 1 and 2 and, hence, a decrease in the PRP effect. Thus, this model would predict that the value of RT1–PRP (discussed earlier) should be larger in Experiments 1 and 2 than in Experiments 3 and 4. In fact, the actual data show the opposite pattern: When R1 and R2 corresponded, the value of RT1–PRP was only 324 ms in Experiments 1 and 2 compared with 358 ms in Experiments 3 and 4. In sum, our results do not support the hypothesis that the change in the PRP effect relative to RT1 across experiments was due entirely to the change in the correspondence effect.

Relation to Practice Studies

In the present study, we examined possible boundary conditions for the central bottleneck model with IM-compatible tasks in a PRP paradigm. Several recent PRP studies have examined a similar issue with respect to practice (i.e., does practice eliminate the bottleneck?). Van Selst et al. (1999) found a residual PRP effect even after 36 sessions of practice. On the basis of this residual PRP effect and results obtained with several within-task difficulty factors, these authors concluded that (a) there was a residual processing bottleneck, and (b) reductions in PRP interference were largely explained by practice-related reductions in Stage 1B. Nevertheless, there was some indication that extended practice improved parallel processing capabilities between T1 and T2 (see also Schumacher et al., 1999). This finding led Van Selst et al. to propose a bottleneck account similar to the engage-bottleneck-later model proposed here for IM-compatible tasks. This similarity suggests the hypothesis that practice eventually converts a non-IM-compatible task into an IM-compatible task, in the sense that practice creates direct links between the stimulus and its associated MC. If this is so, the bottleneck would shift to the later, MC-selection locus. One natural prediction of such an account is that crosstalk should be greatly reduced by practicing a non-IM-compatible T2.

Others have recently examined the effects of practice in a non-PRP paradigm in which 0-ms SOA trials were intermixed with single-task trials (Hazeltine et al., 2002; Schumacher et al., 2001). Although these studies produced very small dual-task costs after several sessions of practice, it is important to note that RT1s were generally very short. Therefore, as acknowledged by Hazeltine et al., it is difficult to rule out a latent bottleneck account of these data (see also Byrne & Anderson, 2001; Ruthruff, Johnston, et al., 2003; for an exception, see Ruthruff et al., in press).

Limitations and Directions for Further Research

One limitation of the present study is that it does not allow us to pinpoint the exact bottleneck locus with IM-compatible tasks. Although the hypothesis that the bottleneck locus for IM-compatible tasks shifts to MC selection provides a convenient account for the present PRP and correspondence data, there is no direct evidence for this particular shift in locus. Further empirical work is needed to determine the exact locus of the bottleneck with IM-compatible tasks. Similarly, further work is needed to determine whether this bottleneck is structural or strategic; if it is strategic, then bottleneck bypass should be possible with appropriate instructions and/or incentives.

Another limitation of the present study concerns the conclusion that T2 compatibility is the key determinant of the PRP–RT1 relationship and correspondence effects. Although this conclusion is consistent with our data and follows naturally from the engage-bottleneck-later model, it should be noted that a visual–manual task was always used as T1 and an auditory–vocal task was always used as T2 in all experiments. This leaves open the possibility that the critical factor is actually the specific type of IM-compatible task used for T2 (i.e., the auditory–vocal task) rather than the task order. Although there is no obvious reason why the task type should be crucial, this issue can be evaluated with further research. More generally, it would be useful to replicate the current findings with different task combinations and different task orders.

Although we have argued that IM compatibility is not sufficient, by itself, to bypass the bottleneck, we do not wish to conclude that bottleneck bypass is impossible. On the contrary, as described above, Pashler et al. (1993) provided evidence that the bottleneck is bypassed when T2 involves eye movements. A critical goal of future research is to determine what the necessary conditions for bottleneck bypass are. In this article, we have pointed out serious limitations in previous approaches, and we have proposed new techniques for addressing the issue. In brief, the key elements of our approach involve examining not just the absolute size of PRP effects but also how the PRP effect is related to RT1 (across trials and conditions).

Greenwald (2003) proposed that bottleneck bypass might be possible with IM-compatible tasks provided that SOAs are blocked and strong speed stress is provided (combined with instructions to make responses at the same time). Under these conditions, Greenwald (2003) found very small dual-task costs on RT (although there were signs of interference on error rates, as discussed in Lien, Proctor, & Ruthruff, 2003). Nevertheless, there is no direct evidence of bottleneck bypass in Greenwald's (2003) study. Not surprisingly, high speed stress combined with IM compatibility produced very short RTs (e.g., 269 ms for the visual task in the single-task condition of Greenwald, 2003, Experiment 1); accordingly, the bottleneck model would predict very little dual-task interference on RT. As a concrete example of this point, our bottleneck model simulations showed that the PRP effect should approach zero when RT1 is about 300 ms or less. Thus, Greenwald's (2003) results cannot be taken as evidence against the central bottleneck model of IM-compatible tasks. Furthermore, Lien, Proctor, and Ruthruff (2003) argued that RT results obtained with speed-stress instructions must be interpreted cautiously: "When speed is emphasized, participants may attempt to maintain a constant speed across blocks, causing any differences in difficulty between blocks to show up primarily in error rates" (p. 1270; for an example, see Experiment 2 of Greenwald, 2003). Unfortunately, we see no easy way to extend the logic behind the RT analyses reported in this article (e.g., analysis of RT2–RT1 dependency, model simulations) to a paradigm in which the PRP effect shows up on error rates.

Conclusions

Dual-task interference in the PRP paradigm provides a useful test bed within which to study limits on people's ability to perform discrete tasks in parallel. The present study demonstrates that although the PRP effect can be reduced with IM-compatible tasks, the effect is not always eliminated entirely. Furthermore, analyses of how RT2 depends on RT1 across trials and how the PRP effect depends on RT1 across experiments, along with bottleneck model simulations, support a bottleneck account of the residual interference. Therefore, these results provide no evidence that IM-compatible tasks completely bypassed the bottleneck. Given that the translation from PCs to MCs appears to be trivial for IM-compatible tasks, our findings suggest that this translation is not the sole cause of the bottleneck. Accordingly, we have proposed that the bottleneck might also include the process of MC selection.

Although the bottleneck was not bypassed entirely in this study, there were several indications (based on PRP–RT1 analyses, model-based simulations, and correspondence effects) that parallelism between tasks occurred deeper into the processing stream when the tasks were IM compatible. This shift in bottleneck locus represents a qualitative change in dual-task performance with IM-compatible tasks that is of considerable theoretical importance. At the outset of this article, we suggested that studying these special cases is a fruitful means of understanding the nature of the central bottleneck. Indeed, the present experiments have forced us to refine our models of the nature of bottleneck stages and of dual-task interference in both IM- and non-IM-compatible tasks.

References

Bertelson, P. (1967). The refractory period of choice reactions with regular and irregular interstimuli intervals. *Acta Psychologica, 27,* 45–56.

Brebner, J. (1977). The search for exceptions to the psychological refractory period. In S. Dornic (Ed.), *Attention and performance VI* (pp. 63–78). Hillsdale, NJ: Erlbaum.

Byrne, M. D., & Anderson, J. R. (2001). Serial modules in parallel: The psychological refractory period and perfect time-sharing. *Psychological Review, 108,* 847–869.

De Jong, R. (1995). The role of preparation in overlapping-task performance. *Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 48*(A), 2–25.

Duncan, J. (1977). Response selection rules in spatial choice reaction tasks. In S. Dornic (Ed.), *Attention and performance VI* (pp. 49–61). Hillside, NJ: Erlbaum.

Gottsdanker, R. (1980). The ubiquitous role of preparation. In G. E. Stelmach & J. Requin (Eds.), *Tutorials in motor behavior* (Vol. 3, pp. 355–370). Amsterdam: North-Holland.

Gottsdanker, R., & Stelmach, G. E. (1971). The persistence of psychological refractoriness. *Journal of Motor Behavior, 3,* 301–312.

Greenwald, A. G. (1972). On doing two things at once: Time sharing as a function of ideomotor compatibility. *Journal of Experimental Psychology, 94,* 52–57.

Greenwald, A. G. (2003). On doing two things at once: III. Confirmation of perfect timesharing when simultaneous tasks are ideomotor compatible. *Journal of Experimental Psychology: Human Perception and Performance, 29,* 859–868.

Greenwald, A. G. (2004). On doing two things at once: IV. Necessary and sufficient conditions: Rejoinder to Lien, Proctor, and Ruthruff (2003). *Journal of Experimental Psychology: Human Perception and Performance, 30,* 632–636.

Greenwald, A. G., & Shulman, H. G. (1973). On doing two things at once: II. Elimination of the psychological refractory period effect. *Journal of Experimental Psychology, 101,* 70–76.

Hazeltine, E., Teague, D., & Ivry, R. B. (2002). Simultaneous dual-task performance reveals parallel response selection after practice. *Journal of Experimental Psychology: Human Perception and Performance, 28,* 527–545.

Hommel, B. (1998). Automatic stimulus–response translation in dual-task performance. *Journal of Experimental Psychology: Human Perception and Performance, 24,* 1368–1384.

Hommel, B., Müsseler, J., Aschersleben, G., & Prinz, W. (2001). The theory of event coding (TEC): A framework for perception and action planning. *Behavioral and Brain Sciences, 24,* 849–937.

Johnston, J. C., & Delgado, D. F. (1993, November). *Bypassing the single-channel bottleneck in dual-task performance.* Paper presented at the 34th Annual Meeting of the Psychonomic Society, Washington, DC.

Levy, J., & Pashler, H. (2001). Is dual-task slowing instruction dependent? *Journal of Experimental Psychology: Human Perception and Performance, 27,* 862–869.

Lien, M.-C., & Proctor, R. W. (2000). Multiple spatial correspondence effects on dual-task performance. *Journal of Experimental Psychology: Human Perception and Performance, 26,* 1260–1280.

Lien, M.-C., & Proctor, R. W. (2002). Stimulus–response compatibility and psychological refractory period effects: Implications for response selection. *Psychonomic Bulletin & Review, 9,* 212–238.

Lien, M.-C., Proctor, R. W., & Allen, P. A. (2002). Ideomotor compatibility in the psychological refractory period effect: 29 years of oversimplification. *Journal of Experimental Psychology: Human Perception and Performance, 28,* 396–409.

Lien, M.-C., Proctor, R. W., & Ruthruff, E. (2003). Still no evidence for perfect timesharing with two ideomotor-compatible tasks: A reply to Greenwald (2003). *Journal of Experimental Psychology: Human Perception and Performance, 29,* 1267–1272.

Lien, M.-C., Schweickert, R., & Proctor, R. W. (2003). Task switching and response correspondence in the psychological refractory period paradigm. *Journal of Experimental Psychology: Human Perception and Performance, 29,* 692–712.

Logan G. D., & Gordon, R. D. (2001). Executive control of visual attention in dual-task situations. *Psychological Review, 108,* 393–434.

Logan, G. D., & Schulkind, M. D. (2000). Parallel memory retrieval in dual-task situations: I. Semantic memory. *Journal of Experimental Psychology: Human Perception and Performance, 26,* 1072–1090.

McCann, R. S., & Johnston, J. C. (1992). Locus of the single-channel bottleneck in dual-task interference. *Journal of Experimental Psychology: Human Perception and Performance, 18,* 471–484.

Meyer, D. E., & Kieras, D. E. (1997a). A computational theory of executive cognitive processes and multiple-task performance: Part 1. Basic mechanisms. *Psychological Review, 104,* 3–65.

Meyer, D. E., & Kieras, D. E. (1997b). A computational theory of executive cognitive processes and multiple-task performance: Part 2. Accounts of psychological refractory-period phenomena. *Psychological Review, 104,* 749–791.

Pashler, H. (1984). Processing stages in overlapping tasks: Evidence for a central bottleneck. *Journal of Experimental Psychology: Human Perception and Performance, 10,* 358–377.

Pashler, H. (1993). Dual-task interference and elementary mental mechanisms. In D. Meyer & S. Kornblum (Eds.), *Attention and performance XIV: Synergies in experimental psychology, artificial intelligence, and cognitive neuroscience* (pp. 245–264). Cambridge, MA: MIT Press.

Pashler, H. (1994). Dual-task interference in simple tasks: Data and theory. *Psychological Bulletin, 116,* 220–244.

Pashler, H. (1998). *The psychology of attention.* Cambridge, MA: MIT Press.

Pashler, H., & Baylis, G. C. (1991). Procedural learning: 2. Intertrial repetition effects in speeded-choice tasks. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 17,* 33–48.

Pashler, H., Carrier, M., & Hoffman, J. (1993). Saccadic eye movements and dual-task interference. *Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 46*(A), 51–82.

Pashler, H., & Johnston, J. C. (1989). Chronometric evidence for central postponement in temporally overlapping tasks. *Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 41*(A), 19–45.

Pashler, H., & Johnston, J. C. (1998). Attentional limitations in dual-task performance. In H. Pashler (Ed.), *Attention* (pp. 155–189). Hove, England: Psychology Press.

Pashler, H., & O'Brien, S. (1993). Dual-task interference and the cerebral hemispheres. *Journal of Experimental Psychology: Human Perception and Performance, 19,* 315–330.

Ruthruff, E., Johnston, J. C., & Van Selst, M. (2001). Why practice reduces dual-task interference. *Journal of Experimental Psychology: Human Perception and Performance, 27,* 3–21.

Ruthruff, E., Johnston, J. C., Van Selst, M., Whitsell, S., & Remington, R. (2003). Vanishing dual-task interference after practice: Has the bottleneck been eliminated or is it merely latent? *Journal of Experimental Psychology: Human Perception and Performance, 29,* 280–289.

Ruthruff, E., Pashler, H. E., & Hazeltine, E. (2003). Dual-task interference with equal task emphasis: Graded capacity-sharing or central postponement? *Perception & Psychophysics, 65,* 801–816.

Ruthruff, E., Pashler, H. E., & Klaassen, A. (2001). Processing bottlenecks in dual-task performance: Structural limitation or voluntary postponement? *Psychonomic Bulletin & Review, 8,* 73–80.

Ruthruff, E., Van Selst, M., Johnston, J. C., & Remington, R. W. (in press). How does practice reduce dual-task interference: Stage-shortening, automatization, or integration? *Psychological Research.*

Schumacher, E. H., Lauber, E. J., Glass, J. M., Zurbruggen, E. L., Gmeindl, L., Kieras, D. E., & Meyer, D. E. (1999). Concurrent response–selection processes in dual-task performance: Evidence for adaptive executive control of task scheduling. *Journal of Experimental Psychology: Human Perception and Performance, 25,* 791–814.

Schumacher, E. H., Seymour, T. L., Glass, J. M., Fencsik, D. E., Lauber, E. J., Kieras, D. E., & Meyer, D. E. (2001). Virtually perfect time sharing in dual-task performance: Uncorking the central cognitive bottleneck. *Psychological Science, 12,* 101–108.

Shin, Y.-K., Cho, Y.-S., Lien, M.-C., & Proctor, R. W. (2005). *Is the psychological refractory period effect for ideomotor compatible tasks eliminated by speed-emphasis instructions?* Manuscript submitted for publication.

Smith, M. C. (1967). Stimulus–response compatibility and parallel response selection. *Canadian Journal of Psychology, 21,* 496–503.

Telford, C. W. (1931). The refractory phase of voluntary and associative responses. *Journal of Experimental Psychology, 14,* 1–36.

Van Selst, M., & Jolicœur, P. (1997). Decision and response. *Cognitive Psychology, 33,* 266–307.

Van Selst, M., Ruthruff, E., & Johnston, J. C. (1999). Can practice eliminate the psychological refractory period effect? *Journal of Experimental Psychology: Human Perception and Performance, 25,* 1268–1283.

Way, T. C., & Gottsdanker, R. (1968). Psychological refractoriness with varying differences between tasks. *Journal of Experimental Psychology, 78,* 38–45.

Welford, A. T. (1952). The "psychological refractory period" and the timing of high-speed performance—A review and a theory. *British Journal of Psychology, 43,* 2–19.

Received August 21, 2003
Revision received July 22, 2004
Accepted July 22, 2004 ■