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OBSERVATIONS

A Reminder About Procedures Needed to Reliably Produce Perfect Timesharing: Comment on Lien, McCann, Ruthruff, and Proctor (2005)

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M.-C. Lien, R. S. McCann, E. Ruthruff, and R. W. Proctor (2005) argued that simultaneous ideomotor-compatible choice tasks cannot be perfectly timeshared. Their conclusion is limited in generalizability for 2 reasons: (a) Their experiments did not include procedures that previous research has shown to be necessary for obtaining perfect timesharing (motivating subjects to perform the 2 tasks rapidly and simultaneously; homogeneous blocks of simultaneous stimuli for the 2 tasks), and (b) their experiments included a procedure that previous research has shown to interfere with perfect timesharing of simultaneous tasks (within-block variation of task interstimulus intervals). Also discussed here are problems in M.-C. Lien et al.'s (2005) analysis of slopes relating Task 2 latency to Task 1 latency and their advocacy of a central bottleneck theory that may not be disconfirmable.

This is the sixth article in a debate that started with Lien, Proctor, and Allen's (2002) nonreplication of Greenwald and Shulman's (1973) finding of perfect timesharing of two simultaneous choice tasks when both tasks were ideomotor (IM) compatible.[1] This debate has already consumed substantial space in this journal, prompting brevity of this comment and the present author's resolve to avoid further debate in the absence of findings that allow a decisive advance of theory.

Review of the Prior Debate

To save space, this article uses shorthand labels for the preceding items in this series, starting by identifying Greenwald and Shulman's (1973) original report as *G&S*, and using numerical labels for the five recent articles, starting with Lien et al. (2002; identified hereafter as *#1*). Greenwald (2003; *#2*) responded to #1 by replicating both G&S's finding and #1's nonreplication while showing that, to produce perfect timesharing, it was necessary to assure that subjects were motivated to respond both simultaneously and rapidly to the two tasks. Lien, Proctor, and Ruthruff (2003; *#3*) disagreed that #2 had demonstrated perfect timesharing, reasserting their belief that perfect timesharing of two IM-compatible tasks was not theoretically possible. Greenwald (2004a; *#4*) then reviewed the history of studies that had sought to produce perfect timesharing, concluding that perfect timesharing had been demonstrated not only by G&S but in six other studies (Allport, Antonis, & Reynolds, 1972; Greenwald, 1972; #2; Hazeltine, Teague, & Ivry, 2002; McLeod & Posner, 1984; and Schumacher et al., 2001); #4's review further (a) confirmed #2's point that motivation to respond simultaneously and rapidly was critical to producing perfect timesharing and (b) noted that, among studies that had used very high levels of practice, two studies had shown perfect timesharing when only one of two simultaneous tasks was IM compatible (Hazeltine et al., 2002; Schumacher et al., 2001), and one had shown perfect timesharing when neither task was IM compatible (Hazeltine et al., 2002).

Lien, McCann, Ruthruff, and Proctor (2005; *#5*) have now reported new findings in support of their argument that a response selection bottleneck is inevitable, even when both of two timeshared tasks are IM compatible. Illustration of this conclusion can be found in the authors' assertions that their "data suggest that even IM-compatible tasks were limited by a processing bottleneck" (p. 140) and that "these results provide no evidence that IM-compatible tasks completely bypassed the bottleneck" (p. 143).

Remarkably, #5 did not use procedures that were established by previous research to be critical to obtaining perfect timesharing. Subjects in #5 were not urged to respond simultaneously and rapidly (as in Greenwald, 1972; #2; G&S; Hazeltine et al., 2002; Schumacher et al., 2001—these five articles are referred to hereafter as the *five speed-instruction studies*). Rather, #5's subjects received a milder instruction "to respond to the stimuli for both tasks quickly and accurately" (p. 127). The authors of #5 reported that they explicitly chose not to consider the possible role of instructions (Footnote 1, p. 123). In addition, #5's procedures did not include blocks of trials in which stimuli for the two tasks were consistently presented simultaneously (a procedure that was used in all of the five speed-instruction studies). As a consequence, the new findings of #5 do not effectively address its focal concern with assessing the possibility that IM-compatible tasks may bypass a central bottleneck. By its not using conditions shown previously to

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[1] IM compatibility is defined as "the dimension denoting the extent to which a stimulus corresponds to sensory feedback from its required response" (Greenwald, 1972, p. 52).

be necessary for perfect timesharing, there was no possibility that #5 could either confirm or disconfirm the inevitability of a response selection bottleneck.

Three New Points

The remainder of this article focuses on some new issues raised by #5. These are (a) artifacts associated with dual-task procedures that use, within blocks of trials, varied interstimulus intervals (ISIs) between the two tasks; (b) problems of interpretation for a regression-slope analysis used in #5 to support the bottleneck-inevitability conclusion; and (c) the possible nondisconfirmability of #5's bottleneck-inevitability conception.

Problematic Procedure: Mixing ISIs Within Blocks

The five speed-instruction studies all used a procedure that included blocks of trials in which stimuli for the two timeshared tasks were presented simultaneously (ISI = 0) on all trials. In contrast, #5 used a mixed-ISI procedure. In a mixed-ISI procedure, multiple ISIs occur randomly within blocks of trials; in #5, there were six different ISIs that were randomly and equiprobably mixed within trial blocks.

Among #5's justifications for its mixed-ISI procedure was that "the use of mixed [ISI]s minimizes unwanted differences in preparatory state between [ISI]s" (p. 127).[2] A problem with that justification is that it is contradicted by a great deal of published evidence, most of which originally appeared in the 1950s, 1960s, and 1970s. The relevant research was well reviewed by Niemi and Näätänen (1981), whose review focused on the effects of *foreperiod duration* on response latencies in simple reaction time (RT) tasks. In investigations of foreperiod duration, subjects encounter trials in which two signals (S1 and S2) are separated by a variable interval. The subject is instructed to give a predetermined response to S2, with S1 serving as a warning that S2 is imminent. The observed latency (RT) in response to S2 provides a measure of variations in preparation resulting from variations in foreperiod duration.

Among the findings well documented in Niemi and Näätänen's (1981) review were two that bear directly on the presence of artifact in #5's mixed-ISI dual-task procedure: (a) a *sequential effect*—"When a trial with a certain FP [foreperiod] is preceded by a trial with a longer FP, RT usually is longer than when the preceding FP is either equally long or shorter" (p. 156), and "RT is characteristically longest after the shortest FP in a block" (p. 137); and (b) a *conditional probability effect*—"The increasing conditional probability of stimulus occurrence toward the end of the trial [on which S2 has not yet occurred] reduces RT" (p. 141).

How do these effects of foreperiod duration apply to timesharing experiments? In timesharing or psychological refractory period (PRP) experiments with mixed-ISI blocks, the stimulus (S1) for Task 1 (T1) serves partly as a warning that the stimulus (S2) for Task 2 (T2) is imminent. The sequential and conditional probability effects of foreperiod variations are therefore likely to be engaged in mixed-ISI dual-task experiments such as those in #5. Given #5's use of between-task ISIs that ranged between 0 and 1,000 ms within blocks, the sequential and conditional probability effects imply, respectively, that (a) latencies should be largest for the shortest of these ISIs within each block (i.e., ISI = 0 or ISI = 50 ms), and (b) latencies should be smallest for ISIs near the long end of the range (i.e., ISI = 500 ms or ISI = 1,000 ms). The possibility of such artifacts in dual-task experiments with a mixed-ISI procedure was first pointed out by Elithorn and Lawrence (1955).

The set of equiprobable ISIs in #5's experiments had the following millisecond values: 0, 50, 150, 300, 500, and 1,000. Figure 1A shows data simulated from a simple model that included only the assumption of the sequential and conditional probability effects, using very approximate estimates of those parameters from available published data (e.g., studies included in Niemi & Näätänen's, 1981, review). Figure 1A reveals a simulated timesharing deficit at short ISI values, quite similar in shape to that reported for T2 in #5's Experiment 4 (see Figure 3 in #5). Figure 1B shows results of a data collection in which the IM-compatible T2 of #5 was used as the only task that required a response but was preceded by a warning signal corresponding to #5's S1. The procedure was modeled closely after #5's Experiment 4, which used an auditory–vocal T2; it included presentation of the left- and right-arrow S1s for #5's visual–manual T1. Subjects were instructed to respond only to S2, which was the spoken letter "A" or "B," and the required IM-compatible response was rapidly pronouncing the heard letter.[3]

The data in Figure 1B were collected from laboratory staff at the University of Washington. The results reveal an effect of ISI such that (as in the simulated data of Figure 1A) responding was slower at the two shortest ISIs than at the longer ones. Figure 1B's data had a significant quadratic trend of the ISI variation, $F(1, 4) = 25.1$, $p = .007$, with no other significant polynomial effects. For the 5 subjects, the individual data of 3 showed a significant quadratic trend ($p < .05$), with 1 other having a p value of .09. That is, only 1 of the 5 subjects appeared not to produce the data pattern represented by the mean function shown in Figure 1B. A priori simple comparisons of the mean for ISI = 0 and ISIs of 150 ms, 300 ms, 500 ms, and 1,000 ms produced, respectively, $t(4)$ values of 1.72 ($p = .16$), 2.93 ($p = .04$), 2.30 ($p = .08$), and 1.63 ($p = .18$).[4] For a comparison of the average latencies for the two shortest ISIs (0 and 50 ms) and the average of the two intermediate ISIs (150 and 300 ms), $t(4) = 3.77$, $p = .02$, whereas for the

[2] In place of the present use of *ISI*, #5 used *SOA* (for stimulus onset asynchrony) to identify the variable corresponding to interval between stimulus presentations in dual-task experiments. *ISI* is used here chiefly to maintain consistency with the extensive use of that term in past dual-task, timesharing, and psychological refractory period effect literature, dating to the 1950s.

[3] There were two noteworthy differences from #5's protocol, other than the requirement of response only to T2. First, the intertrial interval was randomized among values of 0.5, 1.0, 1.5, and 2.0 s to motivate subjects to pay attention to S1 (which, otherwise, they were free to ignore). Second, the stimuli for the IM-compatible pronunciation response were changed from the words "left" and "right" to the letters "A" and "B." This was done after some initial subjects mentioned having delayed their vocal responses because of the length of the "left" and "right" word stimuli, which were about 400 ms in duration, compared with about 200 ms for the "A" and "B" letter stimuli.

[4] Even though it is possible to construct a justification for one-tailed tests for these comparisons, all p values reported in this article are two-tailed.

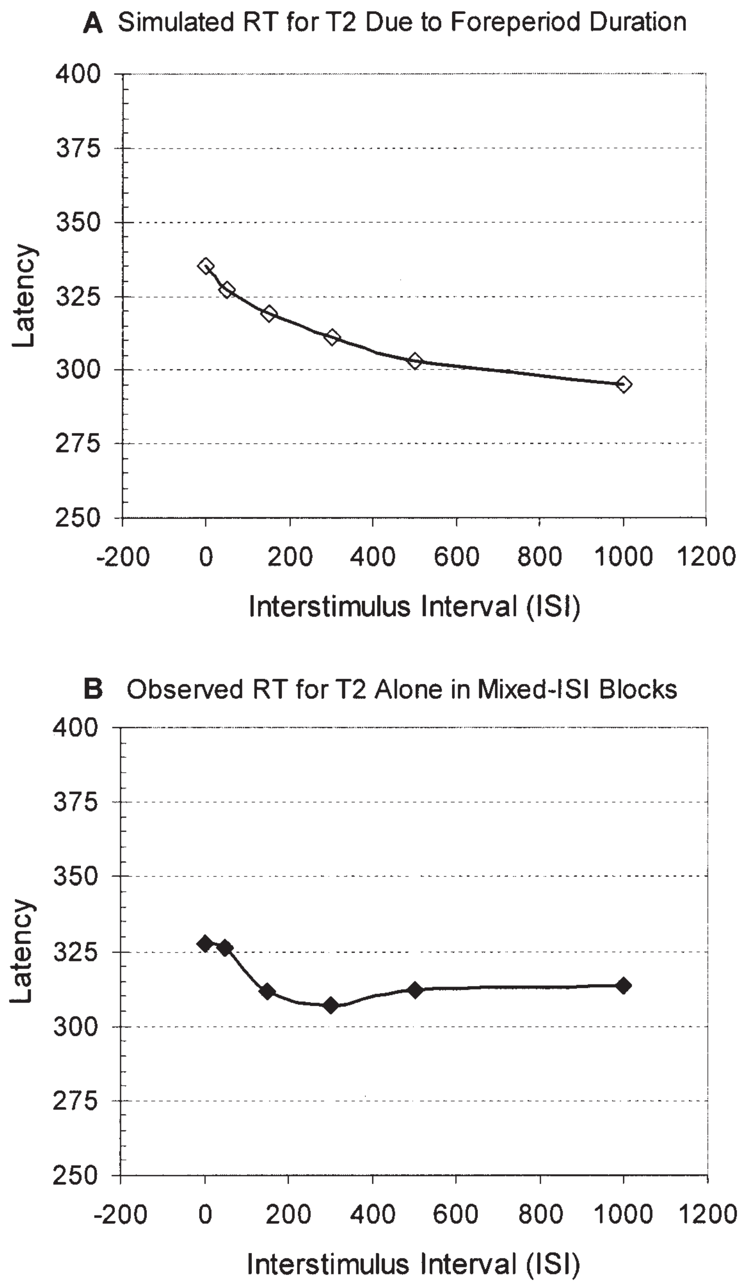

Figure 1. A: Simulated data for reaction time (RT) to T2 (Task 2) in a mixed-ISI procedure, assuming that all variation in T2's RT is due to preparation variations caused by sequential and conditional probability effects resulting from Task 1's (T1) stimuli serving as variable-foreperiod warnings for T2. The six ISIs (or foreperiods) are the values used by Lien et al. (2005). B: Observed data from an experiment (N = 5) corresponding to Panel A's simulation. Stimuli for T1 were presented at the six variable ISIs before T2, but subjects responded only to the stimuli for T2. All values represent milliseconds.

comparison of the two shortest ISIs with the two longest ISIs (500 and 1,000 ms), $t(4) = 2.15$, $p = .10$.

There is no possibility that the slowed responding evident at the two shortest ISIs (0 and 50 ms) in Figure 1B represents a response selection bottleneck. There was no dual-task aspect to the procedure—only one response was required on each trial.

The well-accepted way to avoid artifacts of the mixed-ISI procedure, such as those shown in Figure 1, is to use constant ISIs within blocks of trials, as was done in the five speed-instruction studies. Because #5 did not use that strategy, it is highly plausible that #5's finding of less-than-perfect timesharing in the ISI = 0 condition of its Experiment 4 (in which both T1 and T2 were IM compatible) was indeed an artifact such as that shown in Figure 1.

In #5, Lien et al. cited Bertelson (1967) as having supported the point that "that mixed and blocked [ISI]s produce similar RT2 lengthening" (p. 127). What Bertelson had asserted was that PRP effects could be obtained with either mixed-ISI or blocked-ISI procedures. However, (a) Bertelson's research did not include tests in which both T1 and T2 were IM compatible and, more important, (b) Bertelson pointed out that "RT2 was slightly but systematically longer under the irregular [i.e., mixed-ISI] condition" (p. 56). In other words, Bertelson had observed an artifact of the mixed-ISI procedure. It was therefore inappropriate for #5 to cite Bertelson's report as providing justification for not being concerned about this artifact.

Problematic Analysis Method: Slope of Regression of RT2 on RT1

The conclusions of #5 relied extensively on an analysis of the regression of RT2 (latency for T2) on RT1 (latency for T1; see #5's Figures 4, 5, and 8). In #5, Lien et al. argued that a positive slope of this regression that approached 1 supported a bottleneck model. The essence of this argument is that if (a) a response selection bottleneck is assumed to be present and (b) the response selection stage of T2 is assumed to await completion of T1's response selection stage, then (c) any increase in duration of prebottleneck stages of T1 should produce both an increase in RT1 and a corresponding increase in RT2. (See the text associated with #5's Figure 1 for details of this argument.)

An unstated, additional assumption of #5's argument, based on its use of the regression-slope analysis, was that there are no other plausible interpretations of the positive slope—that is, there are no explanations other than presence of a bottleneck for the observed positive slope relating RT2 to RT1. However, there are at least two very plausible other interpretations. The first such interpretation is an effect due to subjects strategically sequencing the two tasks, which tends to occur when two tasks have a reliable sequence—this was first pointed out in 1973 by G&S (p. 73). In #5's mixed-ISI procedure, the two tasks are in the same order on all except the 16.7% of trials that have ISI = 0. With this procedure, it can be expected that most subjects will adopt a response strategy in which they produce the two responses in a reliable ordering. With this task-ordering strategy, any increase in RT1 should correspondingly lengthen RT2. This would produce a positive slope approaching 1 in the absence of a bottleneck.

The second plausible nonbottleneck interpretation of a positive slope for the regression of RT2 on RT1 is in terms of any factor extraneous to the two tasks that might affect both T1 and T2 equally. In particular, any momentary fluctuations in attention or arousal that affect the subject's general state of preparation for a trial should effectively add a constant to both RT1 and RT2, tending to produce the slope of 1.

Because there are two plausible alternative explanations for the observed positive slope in the regression of RT2 on RT1, the support provided for the bottleneck explanation by #5's regression-slope analyses is at best equivocal.

Is a Crucial Evaluation of the Bottleneck Hypothesis Possible?

The debate over inevitability of a response selection bottleneck in timeshared tasks is reminiscent of other debates that have been prolonged seemingly without end in the psychological literature (cf. Greenwald, 2004b). A characteristic of some such debates is the assertion of the universal truth of a proposition—for example, #5's implied claim that a response selection bottleneck is inevitable. A productive resolution of such debates is often to relax the universality claim and to distinguish the circumstances under which the theorized principle holds from those under which it does not. In the case of dual-task or timesharing analyses, there have been several such attempts to theoretically characterize bottleneck-avoiding conditions (e.g., Byrne & Anderson, 2001; G&S; Levy & Pashler, 2001; Schumacher et al., 2001).

The authors of #5 asserted that "the central bottleneck model predicts a small or even nonexistent PRP effect when RT1 is relatively short" (p. 139), and "our bottleneck model simulations showed that the PRP effect should approach zero when RT1 is about 300 ms or less" (p. 143). These statements reveal the authors as taking a position that treats evidence of perfect timesharing as being noncritical to establishing the presence versus absence of a response selection bottleneck. Even though the authors of #5 indicate that they "do not wish to conclude that bottleneck bypass is impossible" (p. 143), their interpretations of the existing dual-task literature and of their own experiments suggest that they are prepared to maintain the claim of bottleneck universality in the face of all data.

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