Task integration in time production

ERICA L. WOHLDMANN

California State University, Northridge, California

AND

ALICE F. HEALY AND LYLE E. BOURNE, JR.

University of Colorado, Boulder, Colorado

Two experiments examined training on a prospective time production task. Participants produced intervals, expressed in fixed arbitrary units, while performing a concurrent secondary task. After a 15-min filled delay, the participants were retrained on the same tasks. These experiments tested whether the primary and secondary tasks would be integrated into a single task. In Experiment 1, the secondary-task requirements were manipulated, but the time production task was fixed. In Experiment 2, the time production task requirements were manipulated, but the secondary task was fixed. The results suggest that participants integrate primary- and secondary-task requirements.

To what extent is learning specific to the conditions of training or generalizable across different conditions? Numerous theoretical accounts have argued that learning is specific, starting with Thorndike's (1906) theory of identical elements (for more recent versions of this theory, see Rickard & Bourne, 1996; Singley & Anderson, 1989) and continuing with Tulving and Thomson's (1973) encoding specificity principle, Morris, Bransford, and Franks's (1977) principle of transfer-appropriate processing (see also McDaniel, Friedman, & Bourne, 1978; Roediger, Weldon, & Challis, 1989), and our own procedural reinstatement principle (Healy & Bourne, 1995). Specificity of learned skills has been found in many tasks, including those involving color naming (Clawson, King, Healy, & Ericsson, 1995), mental arithmetic (Rickard, Healy, & Bourne, 1994), movement learning (Proteau, Marteniuk, & Lévesque, 1992), and speeded aiming (Healy, Wohldmann, Sutton, & Bourne, 2006).

Recently, Healy, Wohldmann, Parker, and Bourne (2005) investigated specificity of learned skills in a new situation involving a dual task and showed that learning of the primary task was specific even to the secondary task. They used a time production task because previous work had indicated that time estimation accuracy depends importantly on what occurs during the time interval (for reviews, see Hicks, Miller, & Kinsbourne, 1976; Sawyer, 1999). Although this is an artificial laboratory task, it is relevant to everyday activities such as that occurring when a speaker who is delivering a talk needs to be sure to end on time. Unlike situations occurring outside the laboratory, however, arbitrary units of time were used in order to examine the effects of secondary-task demands on training and retention of a new skill, not one previously developed

for producing known time intervals such as seconds and minutes. Healy et al. (2005) found circumstances under which performing an irrelevant secondary task simultaneously with time production depressed time production accuracy during training but actually enhanced time production accuracy during testing, relative to performing time production alone. Specifically, participants were trained to perform a prospective time production task, in which they were given a target number of temporal units and, after a beep, were to press the space bar when the given number of temporal units had passed. Half of the participants learned this time production task in isolation, and half learned it while simultaneously performing a difficult alphabet production secondary task, in which they were given a letter cue and were told to count backward through the alphabet by threes (e.g., *m j g*) until they indicated that the time interval had elapsed. Time production accuracy was much worse with the secondary task than without it during training but improved in both cases over training trials. The level of time production accuracy during subsequent testing depended on whether or not the secondary task was present during training and testing. When the secondary-task requirements were the same during training and testing, there was no loss in performance at test. When the secondary-task requirements were switched between training and testing, primary-task performance suffered at test, so that performance was no better at the start of test than at the start of training in the same condition. This was true even when training involved the difficult secondary task and testing involved no secondary task, despite the fact that the difficult task lowered primary-task performance during training. Thus, performance on time production was actually better when it was performed

E. L. Wohldmann, erica.wohldmann@csun.edu

under dual-task conditions if it had been trained under dual-task conditions than when it was performed alone.

These counterintuitive results of Healy et al. (2005) were explained by proposing a functional task principle that applies to situations involving repeated trials. According to this principle, the functional task that the participants actually performed differed from the nominal tasks that they were given by the experimenter. Specifically, during training, the participants learned to combine procedures required to perform each of the two tasks together. In other words, the participants used the alphabet production task as a basis for the time production task—perhaps, for example, by stopping after a fixed number of letters had been counted or a given letter had been reached. Thus, the primary and secondary tasks were not separate and independent after training, or were not simply interleaved, but, rather, came to be treated as a fully integrated set of requirements (see also Hsiao & Reber, 2001; Rah, Reber, & Hsiao, 2000; Schmidtko & Heuer, 1997; but see Ruthruff, Van Selst, Johnston, & Remington, 2006). When conditions changed during test, the acquired functional task no longer applied (i.e., any strategy involving letters could not be applied if no alphabet production was required), and therefore, performance was no better than at the start of training. Because rhythmic counting has been shown to be the preferred strategy used for judgments of time (Killeen & Weiss, 1987), it might not be surprising that the participants seemed to use alphabet production as the basis for their time production. However, what is surprising is that requiring alphabet production during testing yielded better time production performance than did eliminating that requirement during testing under conditions in which alphabet production had also been required during training, even though producing the alphabet backward by every third letter cannot be easily done in rhythmic fashion and, presumably, speeds up with practice.

According to the functional task principle as applied to the time production paradigm used by Healy et al. (2005), participants use their knowledge of how many letters were uttered during an interval or what letter was said at the end of an interval to gauge the appropriate stopping point for time production. However, using the alphabet task to gauge time should be easier under some circumstances than under others. For example, if the alphabet production task was held constant—say, by starting with the same letter cue on each trial—participants could more easily use the alphabet task to gauge time than if the letter cue changed on each trial. Also, if the time intervals to be produced were repeated over trials, participants could more easily use the alphabet task to gauge when a specific time interval had elapsed. In both of these examples, the ease of using the secondary task to aid in the primary task, or the ability to integrate the two tasks, is varied, but in different ways. In the first of these examples, the secondary task is varied, whereas in the second of these examples, the primary task is varied. Such manipulations of the secondary and primary tasks should enable us to test the functional task principle by altering the extent to which the two tasks can be integrated. In the present study, such manipulations were used in two experiments, the first varying the sec-

ondary task and the second varying the primary task. By the functional task principle, performance on the primary time production task should be improved whenever task integration is optimized. However, there might be some cost to task integration if performance on the secondary task changes across trials. For example, if the same letter cue is used on each trial, participants should find it easier to count backward through the alphabet across trials, enabling them to count more quickly. Such improved secondary-task performance could mislead participants in time production if they are basing their stopping point for a given interval on the number of letters said or on when they reach a particular letter. Specifically, participants might be led to stop too early in their time production, thereby underestimating the amount of time that has actually elapsed.

Are there alternative theoretical accounts that are consistent with the results in Healy et al. (2005) and that might make predictions about the effects of manipulating the alphabet and time production tasks on time production performance? The classic model of attention by Posner and Boies (1971) is one relevant theoretical account. In this model, primary and secondary tasks compete for attentional resources, so that a secondary task, such as alphabet production, would siphon off cognitive resources from a primary task, such as time production. Likewise, there are attentional models of time production that are also relevant, including the attentional gate model (e.g., Zakay & Block, 1997) and the attentional allocation model (e.g., Brown, 1997), although these models were not designed to explain performance following training with feedback using unfamiliar units of time and these models make the assumption that counting is prevented or impossible during time production. In the attentional gate model, individuals divide attentional resources between the external stimuli and time. Attending to time opens an attentional gate, which allows pulses produced by a pacemaker to enter a cognitive counter that is stored in working memory. Increasing arousal leads to an increase in the number of pulses produced by the pacemaker for a given unit of time. Importantly, the attentional demands of a concurrent secondary task deplete the amount of attention allocated to temporal cues. Easy secondary tasks result in the accumulation of more pulses passing through the attentional gate than do difficult secondary tasks, so that longer, less accurate productions are expected with difficult tasks than with easy tasks. Likewise, in the attentional allocation model, processing time requires attentional resources, and the more attention devoted to time, the more temporal cues are accumulated and, thus, the longer is the perceived duration. In this model, a dual-task situation requires attentional resources to be divided between time production and the concurrent secondary task. Cues accumulated in the timer are not fully processed during a difficult secondary task, shortening perceived time and, thereby, lengthening time production and making it less accurate. Thus, despite differences in the details of these two attentional accounts of time production, both of these accounts, along with the more general attentional model of Posner and Boies, seem to predict that a difficult

secondary task will lengthen time production estimates and lower their accuracy. Lower time production accuracy was, in fact, observed by Healy et al. (2005) during training under difficult secondary-task conditions. However, none of these attentional accounts would seem to be able to explain the finding in Healy et al. (2005) that, after training with a difficult secondary task, time production accuracy was better under those same secondary-task conditions than when the secondary task was removed at testing. Instead, it would seem that in all of these attentional accounts, more attentional resources should be devoted to time production when the secondary task is removed than when it is retained, so time production accuracy should be higher at testing, as during training, under easy than under difficult secondary-task conditions.

The purpose of the present study was to provide an additional test of predictions derived from the functional task principle and from attentional models, as well as to clarify certain predictions derived from these accounts. The earlier study (Healy et al., 2005) provided only limited evidence concerning performance on the secondary task, its relationship to performance on the primary task, and the likelihood of integrating the primary and secondary tasks. In the present study, these issues were examined more carefully by manipulating separately (i.e., in different experiments) the difficulty of the two tasks and measuring performance on both tasks. Manipulating the difficulty of both primary and secondary tasks is unusual in the literature on time processing; indeed, measurements of performance on both the primary and secondary tasks have been rare in studies in which the primary task has involved time production (see Brown, 1997; Burle & Casini, 2001).

Another goal of the present study was to determine the extent to which participants would develop a strategy for accurate time production based on declarative information, as claimed by Healy et al. (2005). The use of a strategy involving declarative information (e.g., number of letters counted for each production of a particular specified interval) should be associated with more forgetting across the retention interval than should the use of a strategy based primarily on procedures (e.g., rhythmic tapping; Fetterman & Killeen, 1990), according to the procedural reinstatement principle described by Healy and Bourne (1995), by which declarative information is rapidly forgotten but procedural information is well retained.

In the present experiments, participants were required to simultaneously perform two tasks, time production and alphabet production. The use of alphabet production was selected, in part, to discourage counting of numbers as a strategy for keeping track of the amount of elapsed time, because the oral letter responses would, in effect, serve as a type of articulatory suppression, which should make subvocalization of numbers impossible. We refer to time production as the primary task and alphabet production as the secondary task. Although the participants were not specifically instructed to differentiate the two tasks, they probably viewed them in this way, because they were given feedback only on the time production task, that task was the first one described in the instructions, and they were specifically told to try to become as accurate as pos-

sible on time production, with no such admonition about alphabet production. In Experiment 1, the difficulty of the alphabet production task was manipulated, whereas the difficulty of the time production task was manipulated in Experiment 2. The functional task principle and the attentional models might make different predictions with respect to the length of the time productions. The attentional models seem to predict that a more difficult secondary task will lengthen time productions, relative to an easier secondary task. In contrast, the functional task principle predicts that time productions will get shorter as participants improve their secondary-task performance, if such improvement allows them to go through the alphabet more rapidly. Thus, participants who use the alphabet production task to gauge how much time has elapsed (say, by stopping at a given letter or going through a given number of letters) will shorten their time productions whenever they say more letters in the same amount of time.

EXPERIMENT 1

Participants were trained to produce three different intervals of time while performing one of two versions of the alphabet production task. The participants in the fixed alphabet condition were given the same fixed letter cue (*m*) at the start of each trial. In contrast, the participants in the random alphabet condition were given a different letter cue at the start of each trial. Fifteen minutes after training, the participants were retrained under the same conditions. We expected accuracy on the time production task to improve across blocks of trials, and, on the basis of our previous research with the same task (Healy et al., 2005), we also expected perfect retention for the time production task between training and retraining.

Of greater interest was the relationship between the changes in performance on the two tasks. According to the functional task principle, we expected performance on both the time production and alphabet production tasks to be better when the starting letter was fixed, rather than randomly changing from trial to trial. This expectation was based on the assumption that participants develop a strategy for time production that is based on the number of letters counted for a given interval. That strategy should be easier to employ with a fixed starting letter, because it requires the use of fewer declarative facts than when the starting letter is random. However, fixing the letter cue might also have an adverse effect on time production if that condition promotes a continuous increase across trials in the number of letters counted for a given time interval. Under those circumstances, there might be a negative bias in time production, so that the intervals produced would be shorter than those prescribed. Finding both of these specific patterns (a lower overall error rate for the fixed, relative to the random, alphabet condition, along with a negative bias for the fixed, but not the random, alphabet condition) would be evidence for the functional task principle. In contrast, negative bias would seem to be inconsistent with attentional theories, which imply that increasing the difficulty of the secondary task should lead to an increase in the length of the produced time intervals (i.e., a positive bias).

Method

Participants

Twenty-six undergraduate students from the University of Colorado participated, but only 24 participants were included in the analyses. The 2 participants not included did not perform the secondary task at all. Nine of the 24 participants received \$12 for completing the experiment, and 15 of them received credit in an introductory psychology course. The participants were assigned by a fixed rotation to one of the two alphabet conditions, with 12 participants in each alphabet condition.

Materials and Apparatus

Macintosh computers were used, and the experimental program was written using the REALBasic application. As previously (Healy et al., 2005), each time unit was equal to 783 msec. The intervals that were chosen were a subset of those used in the previous experiments and were intended to represent a wide range while maintaining unequal spacing between intervals. Specifically, these intervals were 21, 32, and 56 units. The intervals used during retraining were the same as those used during training. In each session, every block of six trials included two of each interval in a fixed order (ascending in terms of magnitude for training and descending for retraining in terms of each subblock of three trials). The participants thus saw one order of presentation during training, and this order was reversed during retraining.

In the fixed alphabet condition, the participants were always given the letter cue *m* to start with. In the random alphabet condition, however, the letter cue was randomly selected from the alphabet and was presented at the beginning of each training trial; the order of the letters presented was not the same for all the participants. Because each session half included 36 trials, some letters were repeated twice, but never more than twice.

Procedure

Each trial began with the simultaneous presentation of two stimuli: a number specifying the length of time (in arbitrary units) to produce for the primary time production task and a letter indicating the starting point of the alphabet secondary task. The participants were required to press the space bar when they thought that the specified number of temporal units had passed following a beep, which occurred 3 sec after the onset of the stimuli. While waiting for the number of temporal units to pass, the participants were required to count backward through the alphabet by threes, starting with the given letter cue, by both saying aloud and typing each letter. Immediately after pressing the space bar, the participants were provided with feedback (in units) as to the actual length of their produced interval and also were given feedback on the difference between their produced interval and the specified interval. The instructions for training were presented on the computer screen and were as follows:

In this experiment you will be asked to estimate a certain interval of time, expressed in fixed arbitrary "units" (not standard units like seconds or minutes). At the beginning of each trial, you will be told the number of units in the interval for you to estimate. Next you will hear a beep. Then, when you are ready, press the SPACE BAR to indicate that the specified number of units have passed since the beep. Remember, start estimating the interval after you hear the beep. And, press the SPACE BAR when you think the specified number of units have passed since the beep. There will be 6 blocks of trials, with 6 trials in each block. Before each block, you will be briefly reminded of these instructions and asked to press the NEXT button to continue. For each block, you will be given feedback at the end of each trial on the length of the interval in units that you estimated and the difference between that estimated interval and the specified interval. Your task is to try to become as accurate as you can in estimating these time intervals. If you have any questions,

please ask the experimenter before you begin. Otherwise, press the SPACE BAR to continue.

Additional instructions described the secondary task, which was not specifically referred to as secondary, although the time production task was described first in the instructions and feedback was provided only on that task. The participants were told that, along with the task of producing a certain interval of time, they were to simultaneously recite aloud and type the alphabet in backward order by every third letter, beginning from (and including) the letter cue presented to them (e.g., *s* would be followed by *p*). (Note that in the previous study by Healy et al., 2005, only oral responses, and no simultaneous typing responses, were made.) The participants were asked to press the Enter key after each letter had been typed. All the participants were further instructed that if they reached the beginning of the alphabet, they were to continue at the end of the alphabet. They were also told that this task might seem difficult but they should "take it seriously and do their best." During the experiment, no feedback was given for accuracy on the secondary task. After the participants had reviewed the instructions, a practice trial of the secondary task, starting with the letter *m*, was given to each participant to ensure that the instructions had been understood.

All the participants were individually tested in a quiet room and were required to remove their watches prior to beginning the experiment. Throughout the experiment, the participants were also monitored periodically through a window in the door to their room.

After training, the participants were given a distractor task involving letter detection, which took approximately 15 min to complete. For this task, the participants read two prose passages and were asked to circle the letter *n* each time it occurred in the passage. After reading each passage, the participants were required to answer comprehension questions. After completing the letter detection task, the participants were immediately retrained on the same time production and alphabet production tasks as those used during training. Finally, after retraining, the participants were given a questionnaire asking them how they had estimated the number of units in the experiment and what was the length of one unit in seconds. These questions were included, in part, to learn whether the participants had used any declarative information about the alphabet to help determine how many units of time had elapsed.

Design

The primary task involved learning to produce intervals of time expressed in fixed arbitrary units (with one unit equal to 783 msec). During both training and retraining, three different intervals were included, and the participants performed this task for six blocks of six trials, with two of each interval per block.

Two different training conditions were included. The participants in the fixed alphabet condition were always asked to repeat aloud and type a sequence of letters in backward order by every third letter, beginning with the same letter on each trial. Those in the random alphabet condition were given a different starting letter on each trial. Following Healy et al. (2005), performance on the time production task was assessed using two dependent measures. Specifically, the proportional absolute error is the absolute (i.e., unsigned) difference between the interval produced and the specified interval, divided by the specified interval (i.e., $|\text{produced interval} - \text{specified interval}| / \text{specified interval}$). This measure of accuracy gives a normalized assessment of error magnitude, and because it represents both over- and underestimation, it might be an especially sensitive indicator of disruptions in timing performance (e.g., Brown, 1985; Brown & Boltz, 2002). The proportional relative error is the signed difference between the produced interval and the specified interval, divided by the specified interval (i.e., a signed proportional difference), which provides an index of response bias (see, e.g., Brown & Boltz, 2002, for a similar measure termed *constant error*). A positive value of this index (positive bias) is associated with a produced time interval greater than the specified interval, and a negative value (negative bias) with a produced interval less than that specified.

For the secondary alphabet tasks, the dependent measures were the number of letters typed per second and the proportion of letters correct. The proportion of letters correct was calculated by scoring pairs of successive letters. Specifically, if the second letter typed preceded the first letter in the alphabet and if two letters separated the letters in the pair, the pair was scored as correct (e.g., *mj*).

In summary, the design for training and retraining was a $2 \times 2 \times 6$ mixed factorial, with alphabet condition (fixed, random) as a between-subjects variable and session half (training, retraining) and block (1–6) as within-subjects variables. In addition to the overall set of analyses, another analysis of proportional absolute error was conducted whose aim was to examine retention only. This analysis was included to assess whether the different secondary-task requirements had differential effects on forgetting of the time production task; it was limited to the last block of training and the first block of retraining. Thus, the design for the retention analysis was the same as that for the overall analysis, but the within-subjects variable of block was eliminated.

Results

In the analyses of each task, a mixed factorial ANOVA was conducted on all blocks in both session halves as a function of alphabet condition for each dependent mea-

sure. All significant main effects and interactions ($p < .05$) are reported.

Primary Task

Proportional absolute error. A main effect of session half was found [$F(1,22) = 38.87$, $MS_e = 0.011$, $p < .001$], reflecting worse performance during training than during retraining (i.e., improvement across session halves). Furthermore, the participants in the random alphabet condition had, on average, higher proportional absolute error than did those in the fixed alphabet condition: The main effect of alphabet condition was significant [$F(1,22) = 5.84$, $MS_e = 0.026$, $p = .029$]. As is shown in the top panel of Figure 1, this difference in performance was especially evident during retraining: The interaction between session half and alphabet condition was significant [$F(1,22) = 4.42$, $MS_e = 0.011$, $p = .047$]. Performance improved systematically across blocks: The main effect of block was significant [$F(5,110) = 9.13$, $MS_e = 0.010$, $p < .001$]. There was more improvement during training than during

Figure 1. Proportional absolute error (top panel) and proportional relative error (bottom panel) for the primary task as a function of session half, block, and alphabet condition in Experiment 1. Error bars show positive and negative standard errors of the means.

retraining: The interaction of block and session half was significant [$F(5,110) = 7.37$, $MS_e = 0.009$, $p < .001$].

Because proportional absolute error provides a normalized assessment of error magnitude, it should not depend on interval length. As a post hoc verification, we calculated the mean proportional absolute error as a function of interval magnitude (excluding the first trial of each session half) and found neither a monotonic increase nor a decrease across the intervals used (21 units = .185, 32 units = .162, 56 units = .178).

Proportional relative error. As can be seen in the bottom panel of Figure 1, there was a main effect of session half [$F(1,22) = 6.60$, $MS_e = 0.035$, $p = .018$], with, overall, more positive bias during training than during retraining. Furthermore, the interaction between session half and alphabet condition was significant [$F(1,22) = 9.78$, $MS_e = 0.035$, $p = .005$]. Specifically, the participants in the fixed alphabet condition showed a negative bias during both session halves, but to a lesser extent during the second session half, whereas the participants in the random alphabet condition showed a positive bias during training and, on average, a negative bias during retraining. An analysis restricted to the second session half revealed that the two conditions showed equivalent (negative, on average) bias during retraining [$F(1,22) < 1$].

On average, the participants showed positive bias at the beginning of a given session half and negative bias thereafter: The main effect of block was significant [$F(5,110) = 2.86$, $MS_e = 0.034$, $p = .018$]. The effect of block was due primarily to the positive bias evident at the start of training, not at the start of retraining: The interaction of block and session half was significant [$F(5,110) = 3.06$, $MS_e = 0.041$, $p = .013$].

Retention

To assess retention, we examined the change in performance on proportional absolute error in the last block of training and the first block of retraining. Those results can be seen in the top panel of Figure 1.

In general, the participants in the random alphabet condition showed significantly larger proportional absolute error than did the participants in the fixed alphabet condition; the main effect of alphabet condition was significant [$F(1,22) = 6.79$, $MS_e = 0.005$, $p = .016$]. However, neither the main effect of session half nor the interaction of alphabet condition and session half was significant, suggesting that retention was perfect and that varying the secondary-task requirements did not differentially affect retention of the time production skill over the 15-min delay.

Secondary Task

Number of letters typed per second.¹ The participants typed more letters per second during retraining than during training: The main effect of session half was significant [$F(1,22) = 10.30$, $MS_e = 0.169$, $p = .004$]. On average, the participants in the random alphabet condition typed significantly fewer letters per second than did the participants in the fixed alphabet condition: The main effect of alphabet condition was significant [$F(1,22) =$

19.82, $MS_e = 0.644$, $p < .001$]. Furthermore, as is shown in the top panel of Figure 2, this difference was somewhat more evident during the retraining phase of the experiment: The interaction between session half and alphabet condition approached significance [$F(1,22) = 3.36$, $MS_e = 0.169$, $p = .081$].

The participants showed a significant increase in the number of letters typed per second across blocks: The main effect of block was significant [$F(5,110) = 13.50$, $MS_e = 0.010$, $p < .001$]. This increase can be attributed primarily to the participants in the fixed alphabet condition increasing their letter production across blocks, relative to participants in the random alphabet condition: The interaction of alphabet condition and block was significant [$F(5,110) = 6.11$, $MS_e = 0.010$, $p < .001$].

The increase across blocks in the number of letters typed per second was greater during training than during retraining: The interaction between session half and block was significant [$F(5,110) = 7.55$, $MS_e = 0.005$, $p < .001$]. Also evident in this figure is the significant three-way interaction between session half, block, and alphabet condition [$F(5,110) = 6.62$, $MS_e = 0.005$, $p < .001$], suggesting that the participants in the fixed alphabet condition had a larger increase in the number of letters typed across blocks than did the participants in the random alphabet condition during training, and this pattern was less obvious during retraining.

Proportion of letters correct. Overall, the participants in both alphabet conditions were quite accurate in their performance of the secondary task, averaging 91% correct, suggesting that they did indeed take this task seriously. There was neither a main effect of alphabet condition nor any interactions involving alphabet condition (even in an analysis restricted to retraining). However, accuracy on the secondary task improved from training to retraining—the main effect of session half was significant [$F(1,22) = 9.90$, $MS_e = 0.007$, $p = .005$ —despite the lack of feedback on the secondary task.

As can be seen in the bottom panel of Figure 2, accuracy on the secondary task increased across blocks: The main effect of block was significant [$F(5,110) = 3.20$, $MS_e = 0.004$, $p = .010$]. Also, improvement across blocks was larger during training than during retraining: The interaction of session half and block was significant [$F(5,110) = 4.21$, $MS_e = 0.004$, $p = .002$].

Retrospective Reports

The secondary alphabet task was meant to discourage the participants from using counting as a strategy for time production. Nevertheless, despite this form of articulatory suppression, it is clear from the participants' retrospective reports that they did use counting to help them keep track of the elapsed time. Although only 10 of the 24 participants (fixed alphabet condition, 5; random alphabet condition, 5) specifically used the word *count* in their explanation of how they performed concurrently the alphabet task and the time production task, an additional 10 participants (fixed alphabet condition, 7; random alphabet condition, 3) implied that they used some form of counting (e.g., counting letters or the number of times through

Figure 2. Number of letters typed per second (top panel) and proportion of letters correct (bottom panel) on the secondary task as a function of session half, block, and alphabet condition in Experiment 1. Error bars show positive and negative standard errors of the means.

the alphabet sequence). The remaining participants (random alphabet condition, 4) used a different strategy to produce each interval; 3 participants simply had a “feeling for time,” and 1 participant tapped. Altogether, 19 of the 24 participants specifically mentioned using the alphabet task to keep track of time.

On average, the participants guessed that the length of 1 unit was 1.462 sec (ranging from 0.5 to 3 sec; $SE = 0.144$), and the estimates did not differ as a function of condition. Thus, despite the fact that the participants showed significant improvement across blocks, at the end of retraining they perceived the length of a unit as being about twice as long as it actually was (1 unit = 0.783 msec).

Discussion

The secondary-task manipulation of starting letter had large effects on performance on both the primary and the secondary tasks in Experiment 1. In terms of the secondary task, the participants typed many more letters per second when the starting letter was fixed than when it was random. Also, the number of letters typed per second showed

a large, steady increase across blocks in training when the starting letter was fixed but a minimal increase when the starting letter was random. Even though the number of letters said per second during the secondary task increased more across blocks in the fixed alphabet condition than in the random alphabet condition, performance on the primary time production task showed significantly less proportional absolute error with the fixed starting letter than with the random starting letter, especially during retraining. This result can be explained by assuming that the participants counted the number of letters said in the secondary task as a means of gauging how much time had elapsed. For example, if the participants used a strategy that required them to stop either when a certain letter had been reached in the alphabet or after a certain number of letters had been counted, they would stop earlier whenever they were able to go through the alphabet more quickly. This strategy would lead the participants to produce shorter time intervals than those prescribed when the number of letters typed per second increased, resulting in a negative bias in the participants’ time judgments.

According to this reasoning, participants should show a negative bias on the primary task in terms of proportional relative error across trials to the extent that the number of letters said per second on the secondary task increased. In fact, in the fixed alphabet condition, the number of letters said per second increased during training, and there was a negative bias in time production. During retraining, the number of letters said per second did not change markedly, and there was little bias. Furthermore, during retraining, when the alphabet task had become stable, the proportional absolute error was reduced, relative to that in the random alphabet condition. Also consistent with this reasoning are the participants' retrospective reports, in which most participants described their strategy for the primary time production task as based on counting the letters said in the secondary alphabet task. By this reasoning, participants should stop at the same letter more often than would be predicted on the basis of chance alone for a given time interval. We found the letter that was used most often as the concluding letter for each participant in the fixed alphabet condition with each time interval. We then computed the proportion of times that letter was given as a concluding letter across the 12 trials involving that interval. The average proportion was .347 for the interval of 21 units, .354 for 32 units, and .313 for 56 units. The participants said, on average, 9.236 letters for 21 units, 11.875 letters for 32 units, and 18.576 letters for 56 units. On the basis of the assumption that the chance value for stopping at any given letter is $1/n$, where n = the number of letters said on average, the chance values would be .108 for 21 units, .084 for 32 units, and .054 for 56 units. It is clear, then, that the participants did end with the same letter three to six times as often as would be predicted by chance alone. In any event, the strategy used by the participants appears to have been quite durable; the participants from both conditions showed perfect retention for the time production task between the two session halves.

Can the attentional models also account for the direction of bias observed in the analyses of proportional relative error? According to the attentional models, more difficult secondary tasks should lead to more positive bias than should easier secondary tasks. This difference was, in fact, observed during training, but during retraining (when there was the greatest difference between the two secondary tasks, at least in the number of letters typed), there was no difference in bias between the two conditions.

In accordance with earlier findings that time judgments rely on counting strategies (Killeen & Weiss, 1987), retrospective reports indicated that the participants used counting strategies for the present time production task. However, instead of judging time on the basis of consistent rhythmic counting of numbers, they used the irregularly paced alphabet production task, which changed considerably across practice.

EXPERIMENT 2

There could be a simple reason why the random alphabet condition showed greater proportional absolute error than did the fixed alphabet condition in Experiment 1. Be-

cause the participants in the fixed condition were quickly able to memorize the string of letters that they were required to recite aloud, this secondary task was probably easier than the task required of the participants in the random condition. Experiment 2 tested the functional task principle in a different way, holding the secondary-task requirements constant and varying the requirements of the primary task.

All the participants were required to perform the primary time production task along with the fixed alphabet secondary task that was used in Experiment 1. Half of the participants were required to perform the time production task with 3 different intervals across all 36 trials, as in Experiment 1, and half of the participants were required to perform the time production task with 36 different intervals—a different interval for each trial. We hypothesized that when the participants had only three sets of declarative facts to hold in memory (i.e., when the participants were required to learn only three intervals), there should be good retention of those facts across the 15-min delay. In contrast, in the 36-interval condition, it would be difficult to retain the analogous facts across the same delay interval, and, so, we expected more fact forgetting for the participants who were required to learn 36 different intervals than for those who were required to learn only 3. For the 3-interval condition, as for the similar fixed condition in Experiment 1, if the participants integrated the primary and secondary tasks by using the strategy for time production of stopping at a certain letter in the alphabet or at a certain number of letters counted for a given interval, then, as in Experiment 1, they should show a negative bias as the number of letters typed per second increased. It is unlikely, however, that this same strategy could be used in the 36-interval condition. Thus, even when the number of letters typed per second increases, negative bias would not be expected in the 36-interval condition. Some positive bias might be expected in that case, according to attentional models, if the 36-interval condition were assumed to be more difficult than the 3-interval condition. However, the 36-interval condition and the 3-interval condition did not differ in the secondary task employed, and no difference in primary-task difficulty between the 3-interval and 36-interval conditions would be expected if the strategy used in time production could be maintained across time intervals, as is suggested by the findings in Fetterman and Killeen (1990).

Method

Participants

Thirty-four undergraduate students from the University of Colorado participated for credit in an introductory psychology course. The participants were assigned by a fixed rotation to one of the two interval conditions, with 17 participants in each condition.

Materials and Apparatus

The same materials that were used in Experiment 1 were also used in Experiment 2, with one exception. The participants in the 3-interval condition were given the same intervals as those given to the participants in Experiment 1 (21, 32, and 56 units). However, the participants in the 36-interval condition were given 36 different intervals (in the following random order during training: 60, 44, 27,

62, 14, 72, 66, 16, 47, 51, 68, 38, 21, 56, 34, 20, 13, 35, 25, 55, 65, 54, 17, 42, 30, 40, 50, 33, 49, 71, 39, 46, 57, 31, 28, and 41 units), which were chosen from a range of intervals that we had used in previous experiments (Healy et al., 2005). As in Experiment 1, during retraining, the order of intervals was reversed.

Procedure

The procedure for Experiment 2 was the same as that for the participants in the fixed alphabet condition in Experiment 1.

Design

As in Experiment 1, the primary task involved learning to produce intervals of time expressed in fixed units, with 1 unit equal to 783 msec. Half of the participants were given 3 different intervals (the same intervals as those used in Experiment 1), and they performed this task for six blocks of six trials, with 2 of each interval per block. The remaining participants were given 36 different intervals (a new interval for each trial), and they also performed this task for six blocks of six trials. All the participants were given a concurrent secondary task—namely, the alphabet task that was used in the fixed alphabet condition in Experiment 1. The same dependent measures as those used in Experiment 1 were also used in Experiment 2.

In summary, the design for training and retraining was a $2 \times 2 \times 6$ mixed factorial, with interval condition (3 intervals, 36 intervals) as

a between-subjects variable and session half (training, retraining) and block (1–6) as within-subjects variables. As in Experiment 1, a retention analysis was conducted on the proportional absolute error, which was limited to the last block of training and the first block of retraining. Again, the design for this retention analysis was the same as that for the overall analysis but eliminated the within-subjects variable of block.

Results

Primary Task

Proportional absolute error. As in Experiment 1, there was a main effect of session half [$F(1,32) = 22.64$, $MS_e = 0.007$, $p < .001$], reflecting greater proportional absolute error during training than during retraining (i.e., improvement across session halves; see the top panel of Figure 3). There was also a main effect of interval condition [$F(1,32) = 4.26$, $MS_e = 0.021$, $p = .047$], such that the participants who were trained and tested on 36 different intervals had higher proportional absolute error, on average, than did the participants who were trained and tested on only 3 different intervals. The interaction of interval condition and session half was not significant;

Figure 3. Proportional absolute error (top panel) and proportional relative error (bottom panel) for the primary task as a function of session half, block, and interval condition in Experiment 2. Error bars show positive and negative standard errors of the means.

however, separate analyses of each session half yielded a main effect of interval condition only during retraining [$F(1,32) = 4.69$, $MS_e = 0.018$, $p = .038$].

There was significant improvement across blocks: The main effect of block was significant [$F(5,160) = 16.24$, $MS_e = 0.005$, $p < .001$]. Furthermore, there was more improvement across blocks during training than during retraining: The interaction of session half and block was significant [$F(5,160) = 6.13$, $MS_e = 0.006$, $p < .001$]. The interaction between block and interval condition was also significant [$F(5,160) = 2.32$, $MS_e = 0.005$, $p = .046$], suggesting that improvement across blocks depended on the number of intervals practiced. Specifically, the participants in the 36-interval condition showed more improvement across blocks than did the participants in the 3-interval condition.

Proportional relative error. The participants in the 3-interval condition showed a negative bias in their estimates, whereas those in the 36-interval condition showed a positive bias in their estimates: The main effect of interval condition was significant [$F(1,22) = 13.37$, $MS_e = 0.086$, $p = .001$; see the bottom panel of Figure 3]. In addition, the participants' estimates decreased in proportional relative error across blocks: The main effect of block was significant [$F(5,110) = 2.37$, $MS_e = 0.007$, $p = .044$]. This decrease was also found across session halves: The main effect of session half was significant [$F(1,22) = 12.03$, $MS_e = 0.006$, $p = .002$].

Retention

As in Experiment 1, the analysis for retention focused on the last block of training and the first block of retraining, and the results are presented in the top panel of Figure 3. The participants in the 36-interval condition performed worse in terms of proportional absolute error than did those in the 3-interval condition: The main effect of interval condition was significant [$F(1,32) = 5.46$, $MS_e = 0.005$, $p = .026$]. Furthermore, performance was better during training than during retraining: The main effect of session half was significant [$F(1,32) = 8.91$, $MS_e = 0.003$, $p = .005$]. This effect, however, was primarily due to the fact that only the participants in the 36-interval condition showed a dramatic increase in error across the 15-min delay: The interaction between interval condition and session half was significant [$F(1,32) = 4.65$, $MS_e = 0.003$, $p = .039$].

Secondary Task

Number of letters typed per second. Performance on the secondary task was not significantly influenced by the number of intervals to be learned; thus, there was no main effect of interval condition. However, there was a significant main effect of block [$F(5,160) = 51.74$, $MS_e = 0.016$, $p < .001$], suggesting a large increase in the number of letters typed per second across blocks (see the top panel of Figure 4).

There was a significant main effect of session half [$F(1,32) = 55.64$, $MS_e = 0.144$, $p < .001$], such that all the participants typed fewer letters per second during training than during retraining and the improvement across blocks

diminished across session halves: The interaction between block and session half was significant [$F(5,160) = 10.47$, $MS_e = 0.015$, $p < .001$]. Thus, the speedup was more pronounced for training than for retraining.

Proportion of letters correct. The results for proportion of letters correct are summarized in the bottom panel of Figure 4. The participants in the 36-interval condition were more accurate during retraining than during training, whereas the participants in the 3-interval condition showed a small difference in the opposite direction: The interaction between interval condition and session half was significant [$F(1,32) = 4.37$, $MS_e = 0.006$, $p = .045$]. There was, however, no main effect of interval condition either in the overall analysis or in analyses restricted to each session half. This finding implies that the changes in the primary task in the number of intervals produced did not affect performance on the secondary task.

Retrospective Reports

Only 1 participant from the 3-interval condition specifically used the word *count* as an explanation of how the alphabet task and the time production task were concurrently performed. However, all 33 of the remaining participants implied using some form of counting (e.g., counting letters or the number of times through the alphabet sequence). Altogether, 33 of the 34 participants specifically mentioned using the alphabet task to keep track of time. The participants did not report any other strategy for performing the two concurrent tasks, although 1 participant did report switching from a gut-feeling strategy to a counting strategy.

On average, the participants guessed that the length of 1 unit was approximately 1.677 sec (ranging from 0.45 to 4 sec; $SE = 0.162$), and the estimates did not differ as a function of condition. Again, despite the fact that significant improvement occurred across blocks, the participants perceived the length of a unit as being twice as long as it actually was.

Discussion

The number of different intervals used in the time production task should not affect time production performance if counting is regular with respect to the passage of time (Fetterman & Killeen, 1990). In contrast, the number of different intervals should affect time production performance if counting is based on the letters said in the alphabet task, as suggested by the functional task principle. In Experiment 2, time productions (in terms of proportional absolute error) were more accurate in the 3-interval condition than in the 36-interval condition. In addition, accuracy of time production declined across the 15-min retention interval to a greater extent in the 36-interval condition than in the 3-interval condition. These differences between the 3-interval and 36-interval conditions are remarkable in light of earlier findings by Fetterman and Killeen that the strategy used in time reproduction can be maintained across different time intervals.

The differences between the 3-interval and 36-interval conditions can be explained even more precisely in terms

Figure 4. Number of letters typed per second (top panel) and proportion of letters correct (bottom panel) on the secondary task as a function of session half, block, and interval condition in Experiment 2. Error bars show positive and negative standard errors of the means.

of the procedural reinstatement principle (Healy & Bourne, 1995), which asserts more rapid forgetting of declarative than of procedural information. As was expected, when there were more facts to be forgotten (the 36-interval condition), performance of the time production task declined more rapidly across the 15-min retention interval than when there were fewer (the 3-interval condition). In using the secondary task to estimate time, the participants likely went back in their memory for what they had done or for declarative facts that they had used (e.g., how many letters they had counted) in a previous encounter with the same (or a similar) interval. When there were 36 different intervals, there were too many facts to keep in memory; thus, those facts, if they were actually acquired, were forgotten across the retention interval. Furthermore, in the 3-interval condition, the information was strengthened in memory by the 12 repetitions of each interval in each session half.

Consistent with this interpretation is the fact that the participants' retrospective reports in Experiment 2 implied the use of some type of strategy based on counting letters

of the alphabet. Despite the fact that the participants in both secondary-task conditions reported using some declarative information to produce intervals of time, the strategy was both more effective and more durable for the participants who were required to produce only 3 different intervals than for those who were required to produce 36 different intervals.

In terms of proportional relative error, the participants in the 3-interval condition showed negative bias throughout training and retraining, whereas the participants in the 36-interval condition showed positive bias. The negative bias exhibited in the 3-interval condition can be understood in terms of the participants' strategy. When the number of letters typed per second increases, thereby filling up each time interval more fully, the strategy of stopping at a certain letter or after a certain number of letters should lead to a negative bias. Indeed, in this experiment, there was an increase in the number of letters typed per second throughout both training and retraining, and there was a corresponding negative bias throughout both session halves. (Recall that in Experiment 1, there was an

increase in the number of letters typed per second only during training, and there was negative bias only during training as well.) It would be unlikely that the participants in the 36-interval condition could effectively have used the same strategy for time production, because the intervals were different on every trial. Thus, it is not surprising that negative bias was not evident for that condition. Why the actual bias was positive in the 36-interval condition is not clear, although some insight into this finding can be obtained by considering predictions of attentional models in concert with the functional task principle, as elaborated in the General Discussion section.

The participants in the 3-interval condition outperformed those in the 36-interval condition with respect to the measure of proportional absolute error. This difference may have resulted from the fact that the participants were less able effectively to use declarative facts about the alphabet to produce the time intervals in the 36-interval condition than in the 3-interval condition, at least at the start of practice or after the retention interval. Despite this difference between the 36-interval and 3-interval conditions, the participants' secondary task performance did not differ significantly as a function of interval condition for the measure of letters typed per second.

The two interval conditions did, however, differ significantly in terms of the other measure of performance on the secondary task—namely, proportion of letters correct—with improvement by the participants in the 36-interval condition, but not by those in the 3-interval condition. This difference in secondary-task performance is also consistent with the acquisition over trials of a combined or functional task because, with no feedback on the secondary task, participants estimating a given interval on multiple occasions will be likely to say the same alphabet sequence on each occasion, thus giving less attention to the specific letters spoken and leading to more errors (see, e.g., Barshi & Healy, 1993).

GENERAL DISCUSSION

We investigated specificity of training for a dual-task situation in which learning of a primary time production task had been shown to be specific even to the secondary alphabet production task (Healy et al., 2005). This result had been explained in terms of a functional task principle, according to which, from the viewpoint of the participant, the primary and secondary tasks merge into a single functional task. The present study provided tests of this functional task principle by examining the effects on both primary- and secondary-task performance of changes in the demands of both the secondary task (Experiment 1) and the primary task (Experiment 2). Specifically, in Experiment 1, the secondary task used either a fixed or a random starting point, and in Experiment 2, the primary task required the production of either 3 or 36 different intervals.

The participants in the fixed (but not the random) condition in Experiment 1 showed a significant increase across blocks in the rate at which they said and typed letters of the alphabet. It might seem that increasing the number of letters said and typed would make the development of

a counting strategy for time production more difficult, yielding an increase in proportional absolute error in time productions across blocks. This, however, is not what was found. Instead, almost all the participants reported using the alphabet task as an aid for improving performance on the time production task, and yet they became more accurate with respect to proportional absolute error in time production. Furthermore, as the participants in the fixed condition said more and more letters across blocks in the secondary task, they also showed a negative bias (i.e., produced intervals that were shorter than specified) across blocks in the primary task. The speedup in letters said per second leveled off during retraining, around Block 8, and, at the same time, the negative bias in time production disappeared. Thus, the negative bias occurred only when there was an increase in the number of letters said per second, which is consistent with the hypothesis that the participants used number of letters said as a basis for time production.

Despite the changing achievement in the secondary task with the fixed starting letter, performance on the primary time production task was more accurate, in terms of proportional absolute error, with the fixed than with the random starting letter, especially during retraining, when the negative bias in proportional relative error was eliminated. The advantage in proportional absolute error for the fixed condition can, thus, be understood in terms of the fact that participants are better able to use the secondary alphabet task to help them perform the primary time production task after secondary-task performance becomes stable with practice.

Although the results of Experiment 1 are easily understood in terms of the functional task principle, they might seem surprising from the standpoint of the literature on time production and time estimation. It has been shown that time estimation results are influenced by whether the interval to be estimated is empty (i.e., without requirements to process extra information) or filled (i.e., with requirements to process extra information, such as a secondary task) and by the amount of information that is being processed (see, e.g., Hicks et al., 1976; Sawyer, 1999). The earlier results about the impact on time production of the presence and difficulty of secondary tasks have been interpreted typically within attentional models (see, e.g., Brown, 1997; Burle & Casini, 2001; Zakay & Block, 1997). In these models, individuals in prospective time production tasks allocate more attention to temporal information when there is no concurrent task demand (i.e., when the interval is empty) than when there is a concurrent task, and they give less attention to temporal information as the difficulty of a concurrent task increases. Thus, these models, along with classic models of attention (Posner & Boies, 1971), appear to treat the primary (prospective time production) and the secondary (alphabet production) tasks as distinct and as making separate demands on attention. Our finding that performance in time production was better in the fixed than in the random alphabet condition might seem inconsistent with these models, because we showed that the interval was filled with much more alphabet production in the fixed condition than in the random

condition. Furthermore, we showed that, with practice in the fixed condition, time production performance improved as the alphabet production filled up more and more of a given time interval. However, predictions from attentional models concerning the effects of increasing task difficulty on time production can be viewed as consistent with the present results under the assumption that, with practice, secondary-task demands become undoubtedly easier and, hence, more letters can be counted.

According to the functional task principle, variation in the primary task should make it more difficult to use the secondary task to produce time intervals effectively and consistently because participants cannot use a fixed stopping point in the alphabet as a way to gauge the passage of time. In agreement with this prediction, the proportional absolute error on the primary task was significantly higher in the 36-interval condition than in the 3-interval condition in Experiment 2. Also consistent with the functional task principle is the observation in Experiment 2 that the 3-interval condition showed negative bias, which presumably resulted from increases in the number of letters said per second. The fact that the 36-interval condition showed positive bias is not yet fully understood, but some insight can be derived from attentional models, which predict positive bias for difficult secondary tasks. The secondary-task requirements are the same in the 3- and 36-interval conditions. Furthermore, on the basis of the results of Fetterman and Killeen (1990), the strategy used for time production should be maintained across time intervals, so that the primary-task requirements should be equivalent in the 3-interval and 36-interval conditions. However, the combined functional task will be more difficult in the 36-interval condition than in the 3-interval condition, because the alphabet task cannot be usefully integrated with the primary time production task when the intervals are constantly changing. That is, if participants use the letters said in the alphabet task as a way to gauge time, it would be difficult to do so if different letters are said for each trial. Thus, a combination of attentional models with the functional task principle should allow us to understand the results both when we observe negative bias (which is predicted by the functional task principle for the fixed condition in Experiment 1 and the 3-interval condition in Experiment 2) and when we observe positive bias (which is predicted by the attentional models in concert with the functional task principle for the random condition in Experiment 1 and the 36-interval condition in Experiment 2 by assuming that those conditions are difficult because it is hard to integrate the time information with the alphabet information in those cases).

The analysis of retention across the 15-min delay in the two conditions can also be understood in terms of the participants' use of the secondary alphabet task to produce time intervals. Forgetting across the retention interval was found in the 36-interval condition, but not in the 3-interval condition. This finding can be accounted for on the argument that only 3 different declarative facts about the stopping point in the sequence of letters spoken needed to be held in memory for the 3-interval condition, whereas 36 such facts would have to be retained for the 36-interval condition.

Although this study has provided support for the functional task principle, it does little to delineate the likelihood of task integration in general. In other words, the operational boundaries of the functional task principle have not yet been specified. At least on the basis of intuition, not all primary and secondary tasks can be integrated. Thus, a remaining issue of importance is to determine under what conditions and for which task combinations integration will occur. On the basis of the present findings of a clear relation between the primary and secondary tasks in Experiment 1 with the fixed alphabet condition and in Experiment 2 with the three-interval condition, we hypothesize that integration of the primary and secondary tasks is more likely to occur when there is consistency in the task requirements and in the relevant declarative information. An alternative hypothesis is that two tasks will be integrated only when such integration saves cognitive resources, such as working memory (e.g., Baddeley, 1986). Other considerations might be similar to those involving Garner's (1974) distinction between integral and separable dimensions in perceptual processes.

In any event, the support provided in this study for the development through training of a single functional task that merges primary- and secondary-task requirements has both theoretical and practical implications. In terms of theory, the principle suggests that the breakdown of a given procedure into primary and secondary tasks might not be appropriate when participants treat the two tasks as functionally merged. A theoretical description should be sensitive not only to the manipulations imposed by the experimenter, but also to the way in which participants view and perform task requirements. In terms of applications, the principle suggests that, to facilitate performance in a dual-task situation, instructors should promote the merging of primary and secondary tasks. This integration aids in performance of both tasks and in the retention of the required skills. However, as was suggested by Healy et al. (2005), this integration might come at a cost in terms of transfer from one combined task to another. Thus, if individuals are trained to perform a primary task in the context of one secondary task, they may not be able to generalize their skill to performance of the same primary task in the context of a different secondary task.

AUTHOR NOTE

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NOTE

1. In this analysis, one trial for 1 participant was unusable and, thus, was replaced with that participant's mean for the five other trials within the block.

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