Journal of Experimental Psychology:
Learning, Memory, and Cognition
1990, Vol. 16, No. 3, 484-496

0278-7393/90/$00.75

Practice Schedules and the Use of Component Skills in Problem Solving

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In motor and verbal learning, random practice schedules produce poorer acquisition performance but superior retention relative to blocked practice. We extend this contextual interference effect to the case of learning cognitive procedural skills to be used in problem solving. Subjects in three experiments practiced calculation with Boolean functions. After this acquisition phase, subjects solved problems requiring these procedures. Experiments 1 and 2 demonstrated superior transfer to problem solving for skills acquired under random schedules. In Experiment 3, subjects practiced component skills in a blocked schedule, with one of four tasks—same-different judgment, mental arithmetic, short-term memory, or long-term memory—intervening between trials. For same-different judgments and mental arithmetic, transfer performance was comparable to that found for random schedules in Experiments 1 and 2. This result suggests that the differences depend on processing rather than storage demands of intertrial activity. Implications for theories of problem solving and part-whole transfer are discussed.

Problem solving may be characterized as a process of assembling an appropriate sequence of component procedures (or operators) to accomplish a goal. Although this process may be guided by general or domain-specific problem solving strategies, the specific sequence of procedures used in a particular problem will typically be determined during the problem-solving episode. Fluent problem solving, therefore, requires efficient access and use of component skills. The issue addressed in this article is how the context in which component skills are acquired influences the ability to access and apply those skills in a problem solving context.

Practice Schedules and Component Skills

Many studies demonstrate that practice schedules can have dramatic effects on acquisition and retention when items are equated for number of practice trials (Hintzman, 1974; Shea & Zimny, 1988). For example, subjects typically show superior recall for repeated verbal items when repetitions are spaced rather than massed (e.g., Proctor, 1980). A particularly interesting example of the influence of practice schedules is the *contextual interference effect* (Battig, 1979). Increasing the similarity of items to be learned, or varying the processing requirements from trial to trial, produces a context that interferes with acquisition performance but benefits retention.

Research on the contextual interference effect in motor learning is especially relevant to understanding the effects of component practice schedules on the acquisition of cognitive procedures. The central experimental manipulation involves contrasting practice schedules that do or do not require access of different items on successive trials. The basic empirical finding is this: Relative to blocked practice of one movement at a time, practice of several movements appearing randomly from trial to trial results in poorer performance in acquisition but superior retention and transfer (e.g., Shea & Morgan, 1979). Furthermore, random practice leads to superior transfer to alternative practice schedules—subjects who learn movements under a random practice schedule have little difficulty with transfer to a blocked schedule, whereas subjects who learn under a blocked schedule have great difficulty with transfer to a random practice schedule. Many problem solving tasks require the application of different operators at each step and "on-line" access of the appropriate operators, just as a random practice schedule requires the performance of a different movement and on-line access of the appropriate movement on each trial. This work on motor learning (e.g., Shea & Morgan, 1979), therefore, provides some reason to believe that a random practice schedule during acquisition might help develop the ability to efficiently access component skills in a problem solving situation.

In previous studies of the acquisition of a procedural cognitive skill—making judgments about digital logic gates—we replicated part of these results on contextual interference (Carlson, 1989; Carlson & Schneider, 1989; Carlson, Sullivan, & Schneider, 1989b). In these studies, subjects learned to calculate Boolean logic functions. The acquisition procedure involved a moderate amount of blocked practice with each of several rules, followed by extended practice on a random schedule. Over large variations in the amount of blocked practice, we found a substantial decrement in performance

This research was supported in part by the Army Research Institute, under Contract MDA903-86-C-0149 to Walter Schneider. Portions of these data were presented at the 1988 annual meeting of the Psychonomic Society in Chicago, Illinois.

We would like to thank Rondall Khoo, Lori Forlizzi, David Lundy, Walter Schneider, John Shea, and the members of the Cognitive Research Laboratory at the Pennsylvania State University for their comments at all stages of this research. We also thank Lawrence Barsalou, David Rosenbaum, and two anonymous reviewers for their constructive comments on an earlier draft.

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associated with the transition to random practice. Although these studies did not compare practice schedules or directly address the role of component practice schedules in fluent problem solving (but see Carlson, Sullivan, & Schneider, 1989a), the results do extend previous observations of difficulty in transfer from blocked to random practice schedules (e.g., Shea & Morgan, 1979) to the domain of cognitive procedures. This suggests that the contrast between blocked and random practice may provide an important manipulation for understanding the conditions for acquiring skill in the access and use of cognitive component procedures in problem solving.

Explanations offered for the contextual interference effect and related practice schedule effects generally rely on two categories of theoretical mechanisms. The first mechanism, emphasized by Shea and his colleagues (Shea & Morgan, 1979; Shea & Zimny, 1983, 1988) focuses on the structure of the memory representation developed by practice. The central idea is that random practice (high contextual interference) encourages relational, interitem processing, which results in a richer set of retrieval cues that discriminate among the set of items to be learned. The critical feature of random practice, according to this account, is that it allows subjects to contrast the items to be learned—what we will call *interitem* or *intertrial* processing. This contrast mechanism might work in two ways. First, the item presented on trial $n - 1$ may still be present in working memory on trial n, allowing subjects to form memory structures that are explicitly relational, directly encoding similarities and differences among items to be learned. Second, the retrieval cues used to access an incorrect item will be present in working memory along with feedback concerning the correct item, allowing the subject to determine which retrieval cues fail to discriminate among items. These mechanisms presumably result in memory representations of individual items that are elaborated with distinctive cues appropriate for selecting the correct member of a set of items learned together. Anderson (1989) has offered a similar explanation for our partial replication of the contextual interference effect (Carlson et al., 1989b). The interitem processing view, then, emphasizes the organization of long-term memory structures.

The second mechanism, emphasized by Lee and Magill (1983) for motor skills and by Jacoby (1978; Cuddy & Jacoby, 1982) for verbal materials, focuses on the cognitive procedures executed within each practice trial—what we will call *intraitem* or *intratrial* processing. The central idea here is that on each trial in random practice, subjects must reconstruct a solution to the problem of producing the movement (for motor tasks) or reconstruct the item to be remembered (for verbal tasks). Because the solution or memory item can remain in working memory over trials in blocked practice, this reconstruction is not necessary. For the present case—practice and transfer of procedures for calculating Boolean functions—we assumed that loading a procedure into working memory is analogous to reconstructing a movement plan or verbal item. Because most trials require loading a procedure different from the one used on the previous trial, random practice provides more practice in this aspect of component skill. Although the development and use of distinctive retrieval cues—emphasized by the interitem processing view—may be best viewed as the acquisition of procedural knowledge (e.g., Shea & Zimny, 1983), this intraitem processing view emphasizes fluency in accessing and using component skills, rather than procedures for choosing *which* of several skills to use. The intraitem processing view thus emphasizes the efficiency of processes in working memory, rather than the structure of long-term memory representations.

The present experiments were not designed to distinguish between the interitem and intraitem processing accounts of the retention and transfer benefits of high contextual interference. Our purpose was to explore some implications of the intraitem processing view. The two accounts are not mutually exclusive. It might be that the increased opportunity for interitem processing provides the greatest benefit when it is difficult to determine which component skill is appropriate—for example, when many operators might apply at a particular problem solving step. In the present studies, we focused on a situation in which choosing the appropriate rule should not be difficult—only a few operators are available, and performance is quite accurate. In this situation, problem solvers are distinguished by the efficiency or fluency with which they access and use component skills, rather than by the accuracy or appropriateness of the components they choose. We therefore expected differences in intraitem processing produced by different practice schedules to be most important. In the General Discussion, we consider the importance of the interitem processing account and suggest that it is needed to account for some aspects of our results.

We addressed two major questions. Can the previously observed retention benefit for random practice schedules (e.g., Shea & Morgan, 1979) be generalized to the case of transferring cognitive component skills to a problem solving situation? If this transfer benefit is observed, can it be explained by an intraitem processing account that emphasizes the efficiency of processes in working memory?

Experimental Task and Overview

In the present studies, we used an equation-chaining task in which subjects calculated the value of a single variable on the basis of a set of 3–5 equations. Each equation required the application of a Boolean logic function (AND, OR, NAND, and NOR) to a pair of binary input values. Each function was represented by a typographic symbol, and input and output values were represented as 0 and 1. Subjects thus gave binary (0 or 1) responses both to individual equations and to equation-chaining problems. Figure 1 displays the symbols, rules, and a sample equation-chaining problem.

We conducted three experiments using this problem solving task. In each experiment, subjects first practiced individual logic functions and then solved equation-chaining problems. Experiment 1 examined transfer between blocked and random practice schedules, as well as transfer to problem solving. Experiment 2 provided a replication of Experiment 1, with a finer grained examination of practice during the problem solving phase. Experiment 3 examined transfer to problem solving from blocked component practice in which tasks varying in their processing requirements intervened between

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AND: X will be 1 if and only if A AND B are 1

    X = #(A,B)

OR: X will be 1 if and only if A OR B is 1

    X = $(A,B)

NAND: X will be 0 if and only if A AND B are 1

    X = *(A,B)

NOR: X will be 0 if and only if A OR B is 1

    X = ^(A,B)
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A = #(0,1)
B = ^(A,C)
C = $(1,1)
D = *(0,1)

X = #(B,D)
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Figure 1. Logic functions and symbols (top) and sample equation-chaining problem (bottom).

trials. We hoped to find one or more intervening tasks sufficient to produce a transfer benefit relative to blocked practice. The purpose of Experiment 3 was to examine our hypothesis that differences in intratrial processing can account for the differences in transfer of component skills to problem solving produced by different component practice schedules.

Experiment 1

The major purpose of Experiment 1 was to replicate the basic contextual interference effect—poorer acquisition performance but superior transfer for a random practice schedule—for the acquisition and transfer to problem solving of simple procedural cognitive skills. The acquisition phase of this experiment was essentially a replication of Shea and Morgan's study (1979), but with a cognitive rather than a motor task. Subjects practiced the Boolean logic functions described previously under blocked or random practice schedules. If the effect of practice schedules on these cognitive skills is similar to that found for motor skills, subjects in the blocked practice group should perform better during acquisition. After 48 trials per function, half of the subjects in each condition switched to the alternative practice schedule. We expected asymmetric transfer, with subjects having little difficulty in switching from random to blocked practice but great difficulty in switching from blocked to random practice.

The second phase of the experiment examined subjects' ability to transfer skill at calculating Boolean functions to the use of those functions in equation-chaining practices. If a random practice schedule results in improved ability to access and use component skills in a problem solving situation, subjects in the random-practice condition should solve equation-chaining problems more fluently than subjects in the blocked-practice condition. If differences in intraitem processing account for this effect, the difference should be greater for more difficult problems requiring the solution of more equations and the coordination of more information in working memory.

Method

Subjects. Forty-eight college students recruited from introductory psychology classes at The Pennsylvania State University participated in return for extra course credit. All subjects reported normal or corrected-to-normal vision.

Design. Subjects were randomly assigned to four experimental groups defined by the component practice schedule. Subjects in all groups practiced the rules for eight blocks of 48 trials. In *random* practice, each block of trials included 12 trials with each of the four rules appearing in random order within the block. Randomization was constrained so that the same rule could appear on no more than two consecutive trials. In *blocked* practice, each block included 48 trials with a single rule. For blocked practice, the order in which rules appeared was counterbalanced over subjects by using repeated Latin squares.

The four experimental groups were defined as follows: Subjects in the *random–random* group received eight blocks of random-schedule trials. Subjects in the *random–blocked* and *blocked–random* groups switched practice schedules after four blocks of acquisition trials, receiving four blocks of random-schedule trials followed by four blocks of blocked-schedule trials, or vice versa. Subjects in the *blocked–blocked* group received eight blocks of blocked-schedule practice, with the rule sequence of the first four blocks repeated on the second four blocks.

The problem solving transfer phase of the experiment was identical for all subjects. This phase consisted of three blocks of 30 problems each. Each block included 10 trials of each of three types of problems, as illustrated in Figure 2. *No-search* problems consisted of three equations, and the target value (X) was a function of the output values of the other two equations. *Search* problems consisted of five equations, and the target value was a function of the output values of two of the four nontarget equations. Subjects thus had to search for the appropriate equations before solving them. *Search-plus-working-memory* (*search* + *WM*) problems also consisted of five equations, but calculating the output value of the target equation required two levels of prior calculation. For example, determining the value of X in the problem at the bottom of Figure 2 requires prior calculation of B and D. Determining the value of B, in turn, requires prior calculation of A and C. Subjects thus had to search for appropriate equations and had to hold more intermediate results in working memory than they did for the simpler search problems. In both no-search and search problems, subjects had to solve three equations. Because of the need for two levels of prior calculation in search + WM problems, subjects had to solve four or five equations in these problems. The 30 problems in each block appeared in random order.

The experimental design was thus a 4 × 3 mixed factorial. The between-subjects factor was acquisition practice schedule, and the within-subjects factor was problem type.

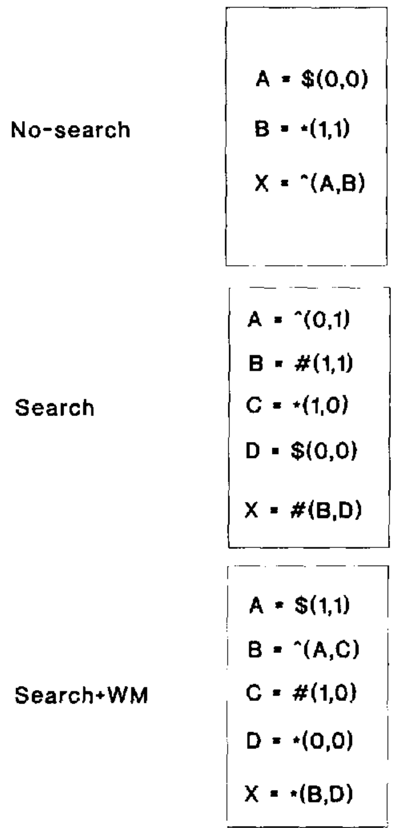

Figure 2. Examples of three problem types. (WM = working memory.)

Procedure. Each subject participated in an individual session lasting approximately 90–100 min. Presentation of the experimental tasks and collection of responses were controlled by IBM-compatible microcomputers equipped with color EGA monitors and programmed by using the MEL software system (Schneider, 1988).

After a brief introduction and an informed consent procedure, the experimenter described the acquisition task. Written instructions concerning the Boolean functions displayed the symbols along with both verbal rules (see Figure 1) and truth tables representing the functions. The instruction sheet was available throughout the acquisition procedure. Subjects were instructed to respond as quickly as possible while maintaining a high level of accuracy.

Subjects initiated each acquisition trial by pressing the space bar on the computer keyboard. After a blank delay of 500 ms, an equation appeared centered on the computer screen. The subject responded by pressing one of two adjacent keys designated 0 and 1. The computer then displayed accuracy feedback. If the answer was correct, the latency was also displayed. Feedback was displayed for 1 s, followed by a *ready* message preceding the next trial.

A brief break followed the acquisition phase. Instructions for the problem solving phase then appeared on the computer screen, describing the problem solving task and displaying a sample search + WM problem. Again, subjects received instructions to respond as quickly and accurately as possible. Subjects initiated each problem solving trial by pressing the space bar. Following a blank delay of 500 ms, the problem was displayed centered on the computer screen. As in the acquisition procedure, subjects responded by pressing one of two adjacent keys designated 0 and 1. The computer then displayed accuracy and, for correct responses, latency feedback. Feedback was displayed for 1 s, followed by a *ready* message preceding the next trial.

Results

Acquisition. Subjects responded quite accurately during acquisition, as shown in Figure 3. An analysis of variance (ANOVA) on these data showed only a marginally significant difference between practice schedules, $F(3, 44) = 2.17$, $p < .11$, $MS_e = 0.02245$. The effects of practice block, $F(7, 308) = 2.49$, $p < .02$, and the interaction of practice schedule and block, $F(21, 308) = 2.64$, $p < .01$, $MS_e = 0.0019806$, were significant. As Figure 3 shows, most of this effect was due to the drop in accuracy when the blocked–random group switched to the random practice schedule beginning with Block 5.

As expected, subjects made slower responses in the random practice schedule, confirming the occurrence of contextual interference. Figure 4 displays the mean latency for correct responses during acquisition. An analysis of variance on these data showed significant effects of condition, $F(3, 44) = 19.9$, $MS_e = 1{,}786{,}019$; block, $F(7, 308) = 70.3$, $MS_e = 95{,}921$; and their interaction, $F(21, 308) = 70.5$, $MS_e = 95{,}921$, all $ps < .001$. Responses were always slower under the random practice schedule, even after eight blocks (384 total trials) of practice.

The asymmetric transfer between practice schedules observed in previous research (Shea & Morgan, 1979) is also apparent in Figure 4. Subjects in the random–blocked group experienced little difficulty in switching from a random to a blocked practice schedule. When subjects in the blocked–random group switched from a blocked to a random schedule, however, they performed as poorly (M = 3,247 ms) as the random–random and random–blocked groups in the first block of practice (M = 3,242 ms). The performance of the blocked–random group on Blocks 5–8 (M = 2,637 ms) was not reliably different from the performance on Blocks 1–4 of

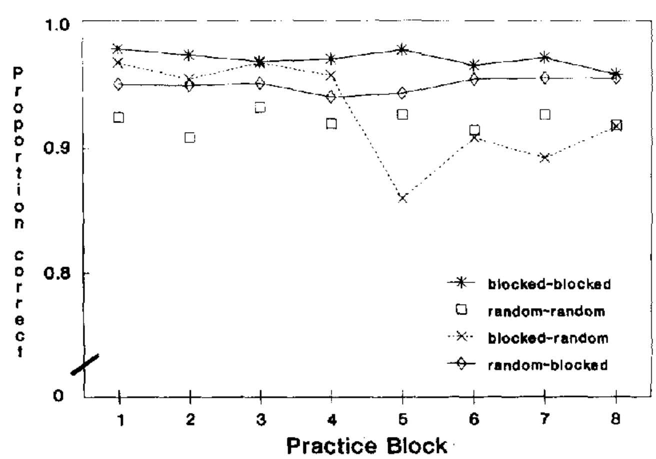

Figure 3. Proportion of correct responses during acquisition: Experiment 1.

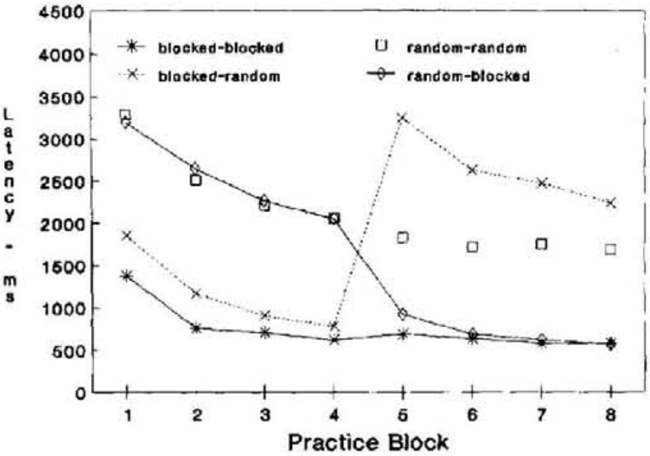

Figure 4. Latency (ms) for correct responses as a function of condition during acquisition: Experiment 1.

the random–random group (M = 2,510 ms, Bonferroni t = 0.86) or the random–blocked group (M = 2,539 ms, Bonferroni t = 0.66).

Problem solving. Overall, subjects solved 87.4% of the problems correctly. Accuracy varied as a function of problem type, $F(2, 88) = 145.5$, $MS_e = 0.01012$, $p < .001$. The mean proportion of problems solved was .94 for no-search problems, .93 for search problems, and .76 for search + WM problems. No other effects in this analysis approached significance, all $ps > .25$, except for a marginal ($p = .10$) effect of block. Accuracy improved from .86 correct on Block 1 to .88 correct on Blocks 2 and 3.

Consistent with previous research, subjects in the random–random group solved problems more quickly (M = 15.03 s) than did subjects in the blocked–blocked group (M = 17.46 s). This difference was not significant, however, $F(1, 22) = 2.43$, $MS_e = 131.6$, $p = .134$. Latency varied as a function of problem type, $F(2, 44) = 202.6$, $p < .001$, and the interaction of problem type and practice schedule, $F(2, 44) = 3.1$, $p = .055$, $MS_e = 195.1$. As shown in Figure 5, latencies were longest for search + WM problems. The longest latencies thus correspond to the lowest accuracies, indicating that speed–accuracy trade-offs are not responsible for the effects observed on latency. Subjects who learned the functions in a random practice schedule solved all types of problems more quickly, but the difference was greatest for the most difficult, search + WM problems. Planned comparisons using Bonferroni t tests showed that the difference between blocked and random groups was significant for the search + WM problems, $t = 3.57$, $p < .05$, but not for search, $t = 1.10$, or no-search, $t = 1.06$, problems.

Problem solving latency declined with practice, $F(2, 44) = 73.0$, $MS_e = 7.972$, $p < .001$, but practice and acquisition condition did not interact ($F < 1$). As shown in Figure 6, there was no evidence that performance for the random and blocked groups converged with problem solving practice. Problem type did interact with practice block, $F(4, 88) = 13.8$, $MS_e = 5.492$, $p < .001$. This interaction reflects the fact that the greater difficulty of the search + WM problems was largest early in practice.

No other effects in this analysis approached significance (all $ps > .25$). The latency analysis reported was conducted only on data from subjects who experienced the same practice schedule throughout practice (the blocked–blocked and random–random groups). Means for the mixed schedule groups (blocked–random and random–blocked) were intermediate in every case, as is consistent with previous results on motor learning (Shea, Morgan, & Ho, 1981, cited in Shea & Zimny, 1983).

Discussion

The results of Experiment 1 support our major hypotheses, extending previous findings on the contextual interference effect to the domain of cognitive skill. Acquisition performance was substantially faster in blocked than in random practice schedules, and switching from a blocked to a random schedule resulted in a large decrement in speed of performance. The difference between blocked and random practice

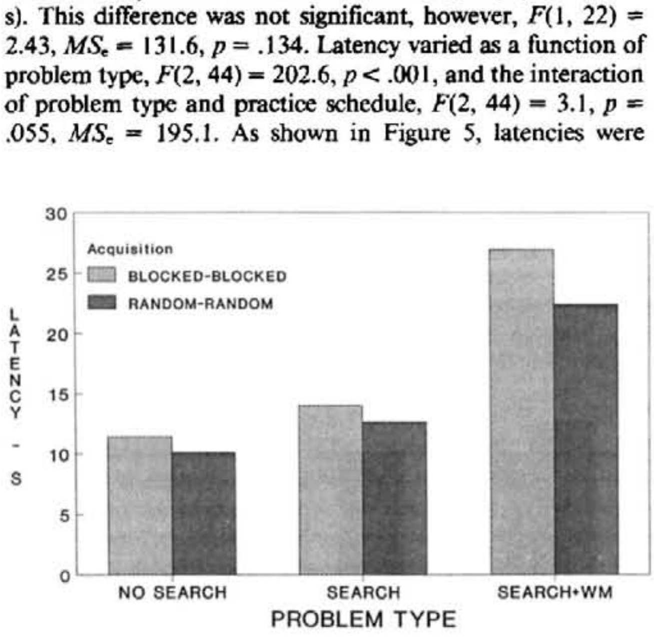

Figure 5. Latency (s) for correct responses during transfer as a function of acquisition condition and problem type: Experiment 1.

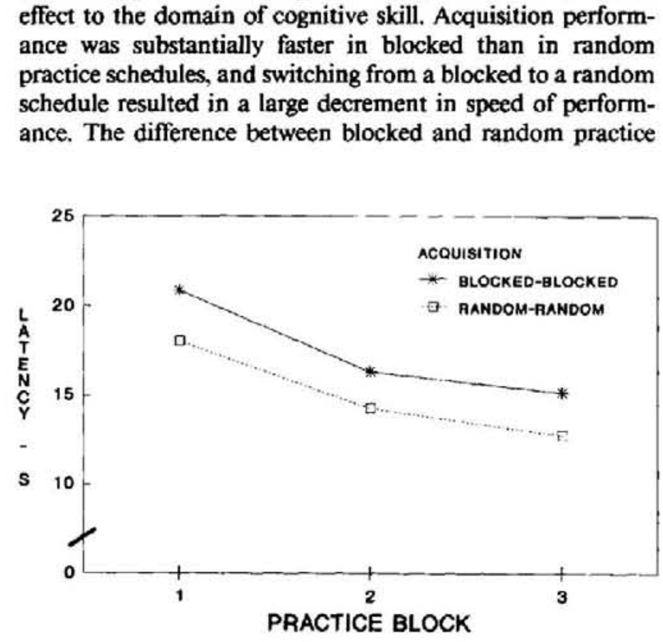

Figure 6. Latency (s) for correct responses during transfer as a function of acquisition condition and practice: Experiment 1.

groups was reversed in the problem solving phase, with the random–practice group solving problems faster than the blocked-practice group. Although the main effect of acquisition condition on problem solving latency was only marginally significant (see Figure 5), an ANOVA using phase (acquisition vs. transfer) and acquisition condition (blocked vs. random) as factors verified that the crossover interaction was significant, $F(1, 22) = 4.6$, $MS_e = 5{,}940{,}262$, $p < .05$.

The problem solving advantage of the random-practice group was greatest for the most difficult problem type, the search + WM problems, which required subjects to maintain and integrate large amounts of information in working memory. The large difference in effect size between search + WM and other problem types (see Figure 5) makes it unlikely that the interaction was due simply to the larger number of equations required for solution of the more difficult problems. If the size of the blocked versus random difference was greater for search + WM problems only because those problems required the solution of more equations, the size of the difference divided by the number of equations should be equal for the three problem types. However, the actual difference in problem solving latency for blocked- and random-practice groups was roughly 470 ms per equation in search problems but 1,004 ms per equation in search + WM problems. The advantage of random practice thus seems to be greatest for problems that required coordination of multiple working-memory representations. This result suggests that the advantage of random practice results from reductions in the working memory demands of executing component skills, as is consistent with the intratrial processing account.

The benefit of random over blocked acquisition practice for problem solving did not decline with problem solving practice. This surprising result has both theoretical and practical implications, suggesting that component practice schedules have lasting effects on the usability of component skills. Experiment 2 provides a further examination of this result.

Experiment 2

Because the main effect of practice schedule on problem solving performance in Experiment 1 was not significant and because the failure of the problem solving practice curves to converge was unexpected, we conducted a second experiment to verify these effects. Experiment 2 replicated the blocked–blocked and random–random acquisition conditions of Experiment 1 exactly. The problem solving transfer phase was modified slightly. The simplest (no-search) problems were not included, and the remaining two problem types appeared in 10 blocks of 10 trials each (5 of each problem type within each block) in order to provide a more detailed look at the problem-solving learning curve. Except for this difference in the number and distribution of problem types, the transfer procedure was identical to that in Experiment 1.

Method

Subjects. Thirty-eight college students recruited from introductory psychology classes at The Pennsylvania State University participated in return for extra course credit. All subjects reported normal or corrected-to-normal vision. Data from 5 subjects were dropped because of experimenter error or because their accuracy in the problem solving phase did not exceed the chance level of 50%. This left 17 subjects in the blocked practice condition and 16 subjects in the random condition.

Design and procedure. Subjects were randomly assigned to blocked or random practice schedules. The acquisition phase was identical to that for the blocked–blocked and random–random groups in Experiment 1. The problem solving transfer phase was similar to that in Experiment 1, except that only search and search + WM problems were presented. Each of 10 blocks included 5 trials each of the two problem types, for a total of 100 trials. In all other respects, the procedure was identical to that in Experiment 1.

Results

Acquisition. As in Experiment 1, subjects responded quite accurately during acquisition. Subjects in the blocked-practice group responded somewhat more accurately ($M = .96$ correct) than those in the random-practice group ($M = .91$ correct), $F(1, 31) = 12.1$, $MS_e = .01265$, $p < .001$. No other effects on proportion correct were significant ($p > .25$).

As in Experiment 1, the mean latency for correct responses was longer for the random-practice group ($M = 1{,}925$ ms) than for the blocked-practice group ($M = 642$ ms), $F(1, 31) = 152.5$, $MS_e = 712{,}227$, $p < .001$. There was a substantial speedup with practice, $F(7, 217) = 111.3$, $p < .001$, and an interaction of practice schedule with practice block, $F(7, 217) = 19.2$, $p < .001$; MS_e for these tests was 42,319. In the first practice block, mean latencies for the blocked and random-practice groups were 1,203 and 3,066 ms; in the last (eighth) practice block, the mean latencies were 483 and 1,581 ms.

Problem solving. As in Experiment 1, subjects maintained a high level of accuracy during the problem solving transfer phase, solving 86.4% of problems correctly. Accuracy was virtually identical for the blocked ($M = 86.2\%$) and random ($M = 86.6\%$) practice groups ($F < 1$). Subjects solved search problems more accurately than search+WM problems (Ms = .96 and .77 correct), $F(1, 31) = 210.6$, $MS_e = 0.026772$, $p < .001$. The difference in accuracy for the two types of problems was slightly smaller for the random-practice group, $F(1, 31) = 5.61$, $MS_e = 0.026772$, $p < .03$. No other effects on accuracy approached significance (all ps $> .25$).

Subjects in the random-practice group solved problems more quickly than those in the blocked-practice group (Ms = 13.96 s and 16.90 s. This difference was marginally significant, $F(1, 31) = 3.66$, $MS_e = 389.7$, $p = 0.65$. As in Experiment 1, an analysis of variance with phase (acquisition vs. transfer) and acquisition condition as factors verified the existence of a crossover interaction (slower acquisition but faster problem solving for the random-practice group), $F(1, 31) = 7.94$, $p < .01$. Problem type also affected problem solving latency, $F(1, 31) = 126.5$, $MS_e = 98.32$, $p < .001$. Mean latency was 11.13 s for search problems and 19.82 s for search+WM problems. This pattern of results is again inconsistent with a speed–accuracy trade-off, with the longest latency and lowest accuracy occurring in the same experimental condition. As in Experiment 1, the difference between problem types was greater for the blocked-practice group, but the interaction of practice schedule and problem type was not significant, $F(1, 31) = 1.72$, $MS_e = 98.32$, $p = .20$.

As shown in Figure 7, problem solving latency declined substantially with practice, $F(9, 279) = 24.9$, $MS_e = 15.2$, $p < .001$. Practice and acquisition condition did not interact, $F(9, 279) = 1.06$, $MS_e = 15.2$. This result suggests that these learning curves do not converge, or at least they converge very slowly. As a further check on this conclusion, we plotted the learning curves in log–log coordinates and calculated the slopes for the best fitting linear functions. These slopes were virtually identical, −4.70 for the blocked-practice group and −4.77 for the random-practice group, supporting the conclusion that the ability to access and use component skills is not easily acquired in the problem solving situation.

Discussion

Experiment 2 replicates the major results of Experiment 1. Again, a random practice schedule produced poorer acquisition performance but superior transfer to problem solving. Although the difference in problem solving latency between blocked- and random-practice groups was marginally significant in both experiments, an analysis combining data from comparable problem types (search and search+WM) in the two experiments verified that the difference between acquisition conditions was significant, $F(1, 55) = 5.8$, $MS_e = 295$, $p = .019$.

The results of Experiments 1 and 2 together demonstrate that the effect of acquisition practice schedules on the ability to use component skills in problem solving is relatively long lasting. This conclusion underscores the theoretical and practical importance of understanding the consequences of task demands for access and use of component skills.

The results of the two experiments are consistent with the intratrial processing account of the practice schedule effect. First, the difference between blocked- and random-practice groups in problem solving latency was greatest for problems placing the greatest load on working memory, and this difference could not be accounted for simply by differences in the

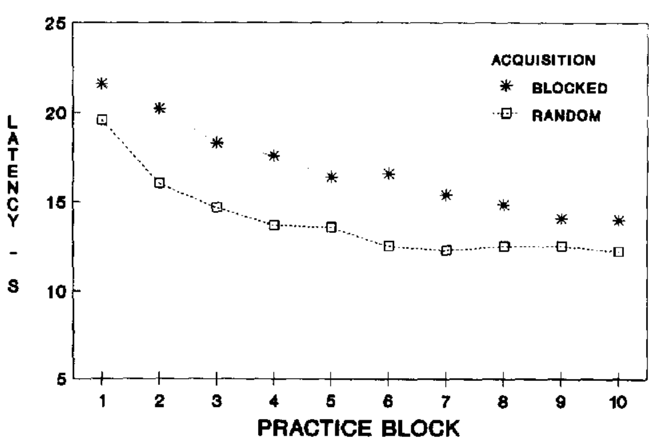

Figure 7. Latency (s) for correct responses during transfer as a function of acquisition condition and practice: Experiment 2.

number of equations to be solved. Second, the intratrial processing account also predicts that during random practice, performance will be faster on the second of two trials involving the same rule because the subject does not have to load a different procedure into working memory.[1] A comparison of latencies for the first and second member of such pairs of trials, combining the random-practice conditions of Experiments 1 and 2, confirmed this prediction. Mean latency was 2,065 ms for the first member of a pair and 1,854 ms for the second member, $F(1, 27) = 69.1$, $MS_e = 83{,}942$, $p < .001$. A similar effect was present for accuracy, with a mean proportion correct of .895 for the first member of a pair and .911 for the second member, $F(1, 27) = 6.1$, $p < .02$, $MS_e = 0.13$. These effects were apparent despite the possibility that some subjects in random-practice conditions might have adopted a strategy of "dumping" the contents of working memory between trials. The pattern of results in both transfer and acquisition thus implicates differences in the use of working memory capacity in the practice schedule effect.

Experiment 3

Experiments 1 and 2 replicate the practice-schedule effects previously observed in motor learning (Shea & Morgan, 1979; Lee & Magill, 1983), extending the scope of those observations to cognitive procedures and to transfer of component skills to problem solving. The results are also consistent with our hypothesis that the random-practice benefit in this case can be accounted for by differences in intraitem processing associated with the manipulation of practice schedules.

If the intraitem processing account is correct for the present case, it should be possible to obtain transfer benefits by introducing practice manipulations other than a random-practice schedule. In particular, other activities that force subjects to load procedures into working memory on each trial of *blocked* practice should produce transfer benefits. In this experiment, we therefore employed an intervening-task acquisition paradigm. Subjects in four experimental conditions learned the logic functions in blocked practice schedules, but with other tasks intervening between trials. Our goal was to find one or more intervening tasks sufficient to produce the transfer benefits observed for random practice in Experiments 1 and 2. The intervening tasks all had content quite dissimilar to the logic functions, but they varied in the nature of the cognitive procedures required. Previous research has demonstrated that the similarity of to-be-remembered material (Cuddy & Jacoby, 1982) and the difficulty of intervening tasks (Proctor, 1980) influence the retention of verbal material. We hypothesized that intervening tasks requiring active processing would produce transfer benefits similar to those produced by random practice.

The transfer benefit of random practice might depend not just on an intervening activity that clears each procedure from working memory between repetitions but also on the similarity of the intervening activity to the procedures being learned. In the random-practice conditions of Experiments 1 and 2,

[1] We are grateful to Lawrence Barsalou for suggesting this analysis.

for example, the activity intervening between repetitions of a single-logic function was calculation by using other logic functions. One condition in Experiment 3 therefore included an intervening task requiring processing—calculation of arithmetic functions—similar to that required by the logic-function task. If processing similarity from trial to trial is a critical characteristic of random practice in these experiments, then only this condition should produce a transfer benefit.

Because recent research on working memory (e.g., Baddeley & Hitch, 1974; Daneman & Carpenter, 1980; Klapp, Marshburn, & Lester, 1983) demonstrates that storage and processing capacities are separable aspects of working memory, we included tasks that involved primarily storage, as well as tasks requiring active processing. Our hypothesis that the critical feature of intraitem processing produced by random practice is the loading of procedures into working memory on each trial, together with the theoretical assumption that storage and processing involve separate aspects of working memory capacity, led us to the prediction that active processing, but not storage tasks, would produce a transfer benefit. In addition, the storage tasks were expected to provide a control for the increased temporal spacing of logic-function trials produced by more active intervening tasks.

Method

Subjects. Fifty-four college students recruited from introductory psychology classes at The Pennsylvania State University participated in return for extra course credit. All subjects reported normal or corrected-to-normal vision. Data from 6 subjects were eliminated because of experimenter error or because accuracy in the problem solving phase did not exceed the chance level of 50%. This left 12 subjects in each of four experimental groups.

Acquisition tasks and experimental design. All subjects in this experiment received blocked-schedule practice of the Boolean rules, with a secondary task intervening between trials. Four experimental groups were defined by the nature of the intervening task: *same–different* judgment, *math* verification, *short-term memory* (*STM*) task, and *long-term memory* task (*LTM*). Figure 8 summarizes the sequence of events on each trial for each of these tasks. Twelve subjects were randomly assigned to each of these groups. Each trial of each intervening task was designed to require approximately 7.5 s, including time for displaying stimuli, collecting responses, and providing accuracy and latency feedback.

The same–different task required subjects to judge whether two letters were the same or different. On each trial, the pair of letters was drawn randomly from the possible pairs defined by upper- and lowercase *A* and *B*. Subjects were instructed to respond on the basis of a name match; that is, to respond "yes" if the two letters had the same name, regardless of case. Each display included one upper- and one lowercase letter to ensure that responses were always based on name matches. Over trials, equal numbers of positive and negative pairs appeared. Three same–different trials appeared between each pair of logic-function trials. This task requires controlled retrieval of letter names from long-term memory and comparison of perceptual and memory representations. These processing demands should be sufficient to clear procedures from working memory between trials, so that a transfer benefit will be produced only if the random-practice advantage depends primarily on intratrial processing. If, however, that advantage depends on the similarity of the intervening processes to those required by the logic-function task, then there should be no advantage.

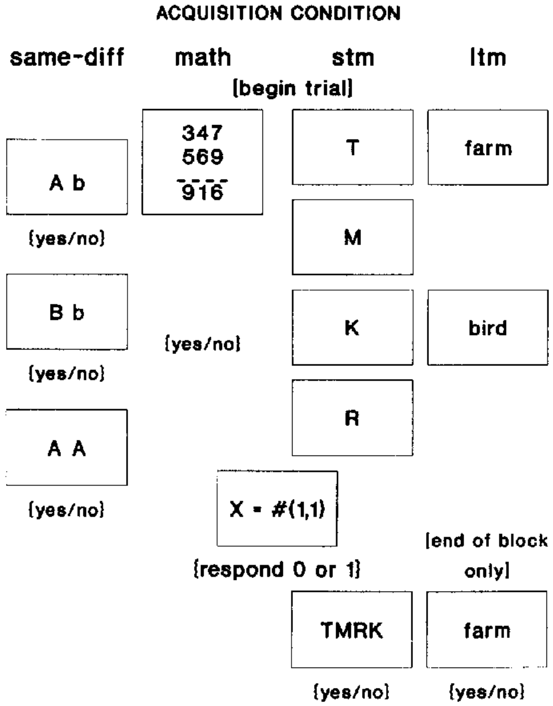

Figure 8. Intratrial sequence of events during acquisition: Experiment 3. (Time runs from top to bottom, and the four columns represent the four intervening tasks. The Boolean equation display was identical for all groups. Diff = different; stm = short-term memory; ltm = long-term memory.)

The math task required subjects to evaluate the answer to a three-digit addition problem. The addends were selected randomly, with the constraint that carrying was required for at least one column. On half of the trials, a correct sum was displayed. On the other half of trials, the displayed sum differed by plus or minus one in one column. A single addition problem was presented between each pair of trials. The processing demands of this task should be similar to those of the same–different task, with the addition of calculation procedures (arithmetic rules) similar to those needed for the logic-function task. If the random-practice advantage depends on the similarity of procedures used on successive trials, then this condition might produce a transfer benefit not present in the same–different condition.

In the STM task, a memory set consisting of four consonants was displayed. The letters in the memory set appeared sequentially, at a rate of 1 per s. After each logic-function trial, memory for the previous memory set was tested. A string consisting of the four letters from the memory set appeared, and the subject pressed one of two keys to indicate that the sequence matched or did not match the order in which the memory set was presented. On half of the trials a correct sequence was presented, and on half of the trials the sequence was randomized. Accuracy and (for correct responses) latency feedback was displayed for 1 s. The subject then initiated the display of the next memory set. Although it is obviously not correct to regard this task as requiring only storage and no active processing, it is similar to the standard tasks shown to be separable from active processing capacity (Baddeley & Hitch, 1974; Daneman & Carpenter, 1980; Klapp et al., 1983). This task therefore ought to produce a transfer benefit only if the random-practice advantage depends primarily on the storage demands of intervening trials.

In the LTM task, two common four- or five-letter words to be remembered for a later recognition test appeared on each trial. Each word was displayed for approximately 2 s, preceded by a 500-ms blank delay. A 1-s blank delay followed the second word, for a total 7.5-s interval between logic-function trials. During each block of 48 logic-function trials, a total of 32 words was presented. Each word therefore appeared three times during the block. Subjects made no responses to this task during the logic-function blocks, but they received a 10-trial recognition test at the end of each block. On each trial of the recognition test, a single word appeared, and the subject pressed a key to indicate whether the word had appeared as a study item or not. Half of the words had appeared, and half had not. This task was not expected to produce a transfer advantage because the two items appearing on each trial do not approach the limits of working memory and do not require substantial active processing. However, if the temporal spacing of practice trials is the critical factor in producing the random-practice advantage, this condition should be equivalent to the other conditions (and superior to blocked practice in Experiments 1 and 2).

Procedure. Each subject participated in an individual session lasting approximately 2 hr. After a brief introduction and an informed consent procedure, the experimenter described the acquisition and intervening tasks. Instructions stressed the importance of responding quickly and accurately to both logic-function and intervening tasks. The acquisition procedure was similar to that in the blocked-practice conditions of Experiments 1 and 2, except that the acquisition phase began with eight trials of practice on the intervening task alone. As in Experiments 1 and 2, each subject received eight blocks of practice, with 48 trials in each block. The arrangement of practice blocks was identical to that in Experiments 1 and 2, with the order of rules counterbalanced over subjects and the rule sequence of the first four blocks repeated on the second four blocks. The problem solving transfer phase was identical in all respects to that in Experiment 1.

Results and Discussion

Logic-function acquisition. Overall, subjects responded correctly on 95.0% of logic-function acquisition trials. Accuracy varied as a function of condition, $F(3, 44) = 5.04$, $MS_e = 0.008802$, $p < .01$. Mean proportion correct was .92 for the same–different group, .97 for the LTM group, .94 for the math group, and .96 for the STM group. No other effects on proportion correct approached significance (all $ps > .25$).

Latency for correct responses was almost identical for all conditions, $F(3, 44) = 0.81$, as shown in Figure 9. Latency declined substantially with practice, $F(7, 308) = 109.3$, $MS_e = 42{,}420$, $p < .001$, but practice did not interact with acquisition condition, $F(21, 308) = 1.35$, $p = .14$. The overall mean latency, 941 ms, was much closer to the mean for blocked practice in Experiments 1 and 2, 692 ms, than to the mean for random practice in those experiments, 1,771 ms.

Intervening task performance. Subjects in all groups maintained high levels of accuracy in the intervening tasks, as shown in Figure 10. There was little variability in intervening-task accuracy as a function of practice, and latency for correct responses declined slightly with practice for each task. The proportion of decline in latency ranged from .14 for the same–different task to .34 for the STM task. As expected, the longest latency was observed for the math task.

For the LTM task, which required subjects to respond to the secondary task only between blocks, the interval between logic-function trials was fixed at 7.5 s. For the remaining tasks, this interval depended on the latency of subjects' responses to the intervening tasks, and in pilot work we attempted to develop tasks that would approximately match the fixed interval of the LTM task. The observed mean intervals were 8,189 ms for the STM task, 7,573 ms for the same–different task, and 4,667 ms for the math task. The interval between logic-function trials was thus approximately equal for three of the experimental conditions but was somewhat shorter for the math condition because the subjects in this study performed the math task substantially faster than did our pilot subjects.

Problem solving transfer. Once again, problem solving was quite accurate. Overall, subjects gave correct answers to 88.6% of problems. Accuracy did vary as a function of acquisition condition, $F(3, 44) = 3.61$, $MS_e = 0.017630$, $p < .03$. Mean proportion correct was .85 for the same–different condition,

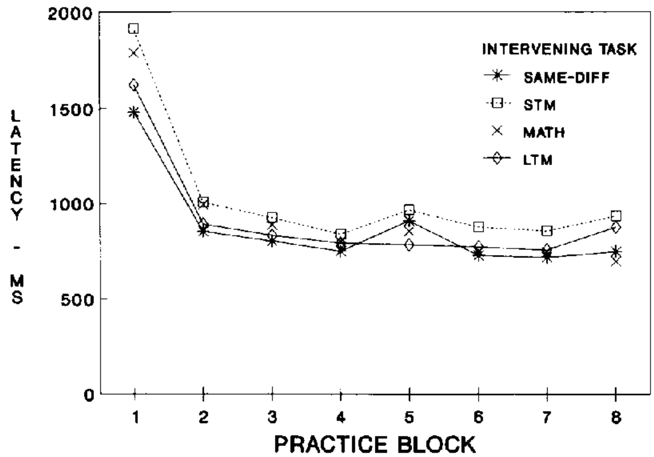

Figure 9. Latency (ms) for correct responses as a function of condition during acquisition: Experiment 3.

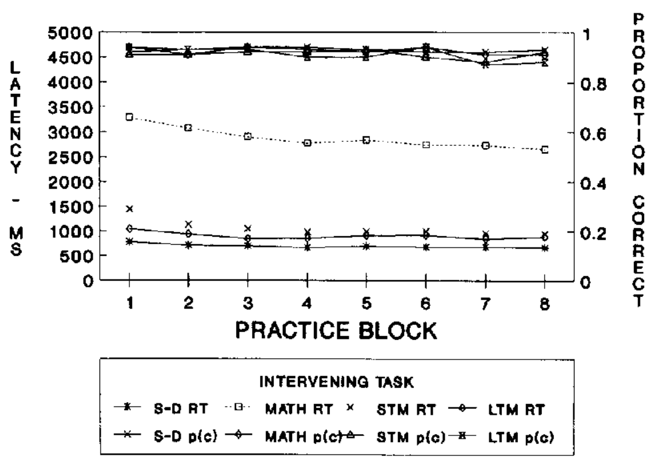

Figure 10. Latency (ms) for correct responses to intervening tasks: Experiment 3. (Latencies for the same–different task are means for individual responses, averaged over the three responses required on each trial. S = same; D = different; RT = reaction time; p(c) = percent correct; STM = short-term memory; LTM = long-term memory.)

.88 for the STM condition, .89 for the math condition, and .91 for the LTM condition. Newman-Keuls tests revealed that this effect was due to the same–different group responding with somewhat lower accuracy than the other three groups ($p < .05$), which did not differ significantly from one another. Accuracy also varied as a function of problem type, $F(2, 88) = 122.1$, $MS_e = 0.01356$, $p < .001$. Mean proportion correct was .95 for no-search and search problems and .76 for search+WM problems. No other effects on problem solving accuracy approached significance ($p > .25$).

Acquisition condition had a significant effect on problem solving latency, $F(3, 44) = 5.18$, $MS_e = 103.0$, $p < .01$. Latency declined substantially with practice, $F(2, 88) = 102.3$, $MS_e = 16.02$, $p < .001$, but practice did not interact with acquisition condition, $F(6, 88) = 1.53$, $MS_e = 16.02$, $p = .18$. These effects are displayed in Figure 11. Subjects in the same–different and math conditions solved problems faster than subjects in the STM or LTM conditions. Newman-Keuls tests confirmed that latencies for the same–different and math conditions differed significantly ($p < .05$) from those for the STM and LTM conditions and that tasks within these pairs of conditions did not differ from one another.

The blocked–blocked condition from Experiment 1 provides a comparison group for assessing the effects of intervening tasks. Subjects in that condition received practice identical in amount and distribution to that presented in Experiment 3, but without intervening tasks between logic-function trials. The number and distribution of transfer problems were also identical in the two experiments. Figure 12 illustrates this comparison. Bonferroni t tests verified that the same–different ($t = 3.48$, $p < .05$) and math ($t = 4.95$, $p < .05$) groups solved problems significantly faster than the blocked-practice group from Experiment 1. However, mean problem solving latencies for the STM ($t = 2.04$, $p > .05$) and LTM ($t = 0.12$, $p > .05$) groups did not differ significantly from that for the blocked-practice group from Experiment 1. As Figure 12 shows, the STM group was slower than the blocked-practice group, though not significantly so. Figure 12 also provides a comparison with the mean latency for the random–random group

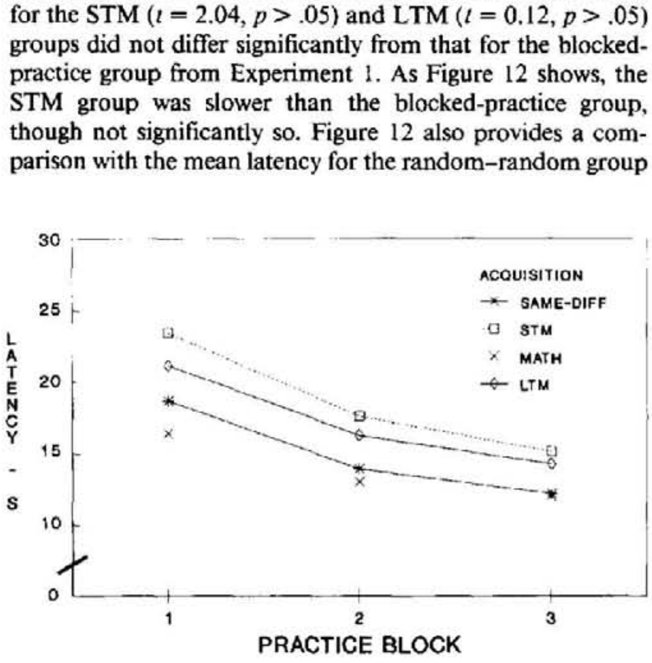

Figure 11. Latency (s) for correct responses during transfer as a function of acquisition condition and practice: Experiment 3. (Diff = different; STM = short-term memory; LTM = long-term memory.)

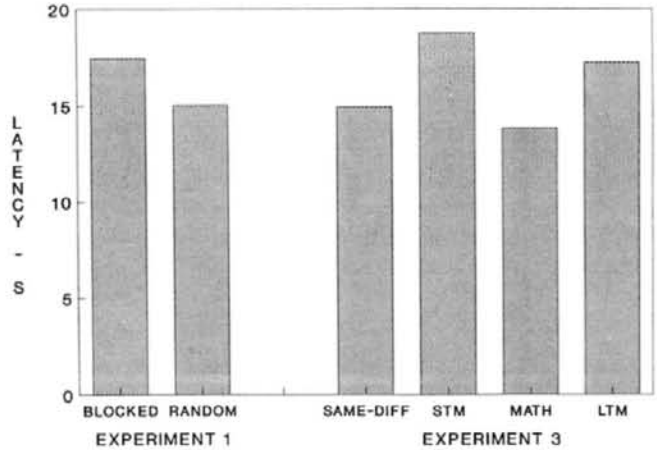

Figure 12. Latency (s) for correct responses during transfer as a function of acquisition condition, Experiment 3, compared with blocked practice acquisition, Experiment 1. (Diff = different; STM = short-term memory; LTM = long-term memory.)

from Experiment 1. Bonferroni t tests showed that the problem solving latencies for the same–different ($t = 0.37$, $p > .05$) and math ($t = 0.95$, $p > .05$) groups did not differ from those of the random-practice group, whereas the STM ($t = 5.35$, $p < .05$) and LTM ($t = 3.40$, $p < .05$) groups were significantly slower.

The present data thus demonstrate that two of the intervening tasks (same–different and math) were sufficient to produce a transfer benefit relative to a standard blocked practice schedule. Other intervening tasks (STM and LTM) that resulted in similar temporal spacing between logic-function trials were not sufficient to produce a transfer benefit. This conclusion must be qualified by noting that some of the transfer benefit observed for the same–different group might be due to a speed–accuracy trade-off; as noted above, the same–different group answered problems with somewhat lower accuracy than the other groups.

Problem type again had a substantial effect on problem solving latency, $F(2, 88) = 199.6$, $MS_e = 319.5$, $p < .001$. This effect interacted with acquisition condition, $F(6, 88) = 2.55$, $MS_e = 319.5$, $p < .03$. The differences between acquisition conditions were greater for the more difficult problems, as shown in Figure 13. Bonferroni t tests showed that the acquisition conditions did not differ significantly ($p > .05$) for no-search problems, and only the fastest (math) and slowest (STM) groups differed ($p < .05$) for search problems. For search+WM problems, however, the math and same–different groups differed significantly ($p < .05$) from the STM and LTM groups, whereas the two members of each pair of groups did not differ ($p > .05$). Thus, the transfer benefit of same–different or math tasks intervening between logic-function transfer trials was apparent primarily for problems that involved a substantial working memory load. This result is important because it demonstrates that the problem solving advantage is qualitatively similar to that observed in Experiment 1. As in Experiments 1 and 2, the lowest accuracy and longest latency (for search+WM problems) corresponded, a

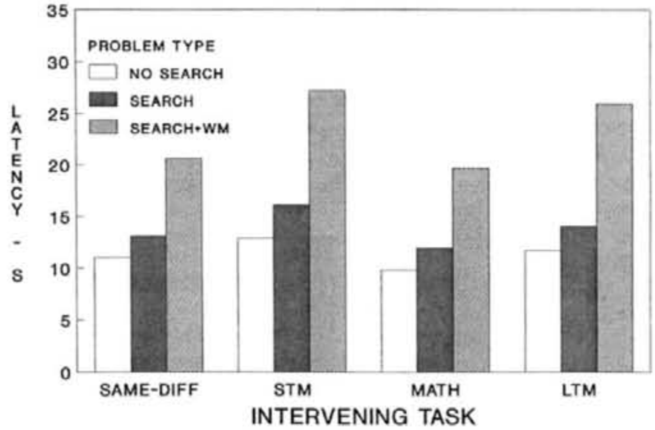

Figure 13. Latency (s) for correct responses during transfer as a function of acquisition condition and problem type: Experiment 3. (Diff = different; STM = short-term memory; LTM = long-term memory; WM = working memory.)

finding that argues against an explanation in terms of a speed–accuracy trade-off.

General Discussion

The present results replicate the effect of practice schedule (blocked vs. random) previously observed for the acquisition of motor skills (e.g., Shea & Morgan, 1979). Random practice with Boolean logic functions produced poorer acquisition performance than did blocked practice, but problem solving with those functions was faster for subjects who received random practice during acquisition. These results extend the findings of previous research in two ways. First, they demonstrate the effect of practice schedule on a procedural cognitive skill, calculation of logic functions. Second, they demonstrate that the superior transfer produced by random practice occurs in the transfer of component skills to a problem solving situation. Experiment 3 demonstated that two intervening tasks—same–different judgments and math—in blocked practice were sufficient to produce a benefit in the transfer of component skills to problem solving that was both quantitatively and qualitatively similar to the benefit produced by random practice. This result is consistent with the intraitem processing account of the practice schedule effect in this situation and with our suggestion that loading procedures into working memory is an important aspect of this processing.

Random Practice and Transfer to Problem Solving

It is perhaps unsurprising that a practice schedule requiring the use of component skills in random order produces superior transfer to a problem solving situation that requires access and use of those skills in varying sequences. However, it does appear paradoxical that superior transfer occurs even though acquisition performance is much poorer in a random practice schedule (Battig, 1979). Also puzzling is the very slow convergence of problem solving practice curves (Figures 6, 7, and 11) for different acquisition conditions. The problem solving task should provide component practice similar to the random acquisition schedule (because each problem requires the use of several different rules), and the total number of equations needed to solve the problems (300 in Experiments 1 and 3, 350 in Experiment 2) is a substantial fraction of the practice provided in the acquisition phase (384 total trials). Because random practice following initial blocked practice does seem to be effective in producing retention and transfer benefits (Shea & Zimny, 1988), this result suggests that other demands of problem solving somehow prevented subjects from achieving the full benefits of this additional practice.

The transfer advantage of random practice (and of the same–different and math intervening-task conditions in Experiment 3) was greatest for the most difficult problems. This effect is not due simply to the larger number of equations required in the most difficult problems because the random-practice advantage was more than doubled for the search+WM problems, even when calculated in terms of time per equation. This result suggests an interpretation of the practice-schedule effect in terms of the ability to coordinate representations and procedures in working memory (Carlson et al., 1989a; Logan, 1985). The hypothesis that the need to load procedures into working memory on each trial is an important aspect of the processing evoked in random practice is supported by the observation that when functions were repeated on consecutive trials in random practice, performance was significantly faster and more accurate on the second repetition.

Processing Demands and Part–Whole Transfer

The results of Experiment 3 provide support for the view that differences in intratrial processing are responsible for the practice-schedule effects observed in Experiments 1 and 2. Two of the intervening tasks used in blocked practice—same–different judgments about letter names and verification of addition problems—produced transfer benefits (relative to blocked practice without intervening tasks). These tasks required subjects to retrieve information from long-term memory and to coordinate it with displayed information in order to make judgments, presumably as did the processing required for logic-function trials. If close similarity of processing was critical to this effect, however, then only the math task—which, like the logic-function task, required calculation—should have produced a transfer benefit. The fact that both tasks produced transfer benefits suggests that clearing the logic-function procedure from working memory, thus requiring that it be reloaded for the next trial, was the important aspect of the same–different and math tasks. Two other intervening tasks—holding a short-term memory load during a logic-function trial and studying information for a later test of long-term memory—produced transfer performance similar to blocked-practice schedules in Experiments 1 and 2. This result is important for two reasons: First, it supports our view that active manipulation of representations in working memory, rather than more passive storage or retrieval factors, is the critical factor in the random-practice advantage. Second, it provides evidence that the temporal spacing of trials with the same function is not the cause of the random-practice advantage because the interval between logic-function trials was comparable for intervening tasks that did and did not produce transfer benefits.

These considerations suggest that the correct explanation for the present transfer results should rely more on the *intra-item processing* mechanism emphasized by Jacoby (1978) and Lee and Magill (1983) than on the relational *interitem processing* mechanism emphasized by Shea and Zimny (1983, 1988). That is, a feature of random practice sufficient for producing the transfer advantage observed here is that the procedure for applying a particular rule is cleared from working memory by intervening trials with other rules. Within each trial, then, subjects must reload into working memory the appropriate procedure for the logic function presented on that trial.

We would suggest, however, that both intraitem and interitem processing mechanisms are needed to account for the entire pattern of results observed in acquisition and transfer. The intervening-task paradigm used in Experiment 3 produced (for same–different and math intervening tasks) substantial transfer benefits without producing the large decrement in acquisition performance produced by random practice. It therefore seems unlikely that the same explanation can account for both the slower performance during random-practice acquisition and the transfer benefit of random practice. We see two possible explanations for this pattern of results. First, differences in interitem processing might account for the slower acquisition performance by random-practice groups, whereas differences in intraitem processing might account for the observed transfer benefit. In a problem solving situation that places a greater premium on choosing which rule to use at each step—perhaps problems in which a wider range of operators might apply—our intervening-task procedure might not be sufficient to produce a transfer benefit comparable to that produced by random practice. A second possibility is that we have observed transfer benefits due to two sources: (a) differences relative to blocked practice in interitem processing in Experiments 1 and 2, and (b) differences relative to blocked practice in intraitem processing in Experiment 3. Both explanations, however, depend on the assumption that differences in intraitem processing are sufficient to produce transfer benefits. These differences in intraitem processing appear to involve differences in the use of working memory. The main point of our theoretical argument, then, is that one locus of the contextual interference effect is the ability to efficiently load procedures into working memory.

The present results also contribute to the large literature on transfer of learning (Cormier & Hagman, 1987), particularly with respect to the issue of part–whole transfer (Wightman & Lintern, 1985). Although much of the research on part–whole transfer has been concerned with perceptual–motor skills and has been applied rather than theoretical in focus (Wightman & Lintern, 1985), recent work on cognitive skills provides some basis for predictions about transfer. In production-system models like Anderson's (1987) ACT*, transfer depends on the ability to use identical productions in acquisition and transfer settings. In this view, part–whole transfer depends largely on the similarity of information in working memory when productions are to be executed in acquisition and in transfer, so that the conditions of appropriate productions can be matched in both cases. Anderson (1989) explained the poorer performance during random practice by noting that random practice—unlike blocked practice—requires productions with conditions that discriminate among rules. This explanation is consistent with the relational interitem processing hypothesis, but it is not clear that this hypothesis could account for the benefit of same–different and math (but not STM or LTM) intervening tasks in blocked practice. Clearing working memory with an intervening task might require that rules be retrieved from long-term memory (which may require more discriminative cues).[2] But the STM task also ought to interfere with storage of the declarative information used to match the conditions of productions. Thus it seems that current production-system accounts of transfer should be supplemented by something like the intratrial processing mechanism described above.

The present results suggest an emphasis on the procedures used to load and apply procedures in working memory, rather than on the content of working memory. This emphasis is consistent with other recent work on working memory and on the conditions for transfer. Daneman and Carpenter (1980, 1983), for example, have demonstrated that performance on a complex task—reading comprehension—can be predicted from performance on a working memory task involving both processing and storage but cannot be predicted from short-term memory tasks involving only storage. Similarly, Klapp et al. (1983) have demonstrated that short-term storage and processing capacity can be experimentally separated. In theoretical work on transfer, Schneider and Detweiler (1988) have argued that single-task practice is often ineffective in producing transfer to dual-task situations because *compensatory activities* are needed to manage and coordinate the working memory demands of dual-task performance. These compensatory activities include efficient loading of procedures into working memory. Although the problem solving tasks in these experiments did not require concurrent processing of multiple rules, a similar analysis might be applied. Random practice or appropriate intervening tasks may benefit transfer by providing practice at loading procedures into working memory.

Cognitive and Motor Skills

One striking aspect of the present results is the similarity of the effects of practice schedules on the acquisition of motor skills (e.g., Lee & Magill, 1983; Shea & Morgan, 1979; Shea & Zimny, 1983, 1988) and the cognitive procedural skills studied here. This consistency across domains fits well with the emphasis on cognitive factors in explaining contextual interference effects in the acquisition of motor skills (e.g.,

[2] This possibility was suggested to us by Lawrence Barsalou.

Shea & Zimny, 1988). Perhaps more important, the present findings contribute to the growing evidence that skill acquisition and skilled performance in perceptual, cognitive, and motor domains share underlying mechanisms (e.g., MacKay, 1987; Rosenbaum, 1987). A general theory of human learning and skill may follow from research informed by findings that, like the present ones, generalize across domains.

Conclusion

The present study demonstrates that the processing context in which cognitive component skills are acquired is important in determining the ability to transfer those skills to problem solving situations. Our interpretation of these results has led us to focus on the ability to access and use component skills fluently, rather than on the more often studied ability to choose appropriate operators (e.g., Lewis & Anderson, 1985). Understanding the acquisition of problem solving skill will require understanding both of these aspects of cognitive skill.

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Received June 26, 1989
Revision received October 19, 1989
Accepted October 30, 1989 ■