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High Levels of Contextual Interference Enhance Handwriting Skill Acquisition

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ABSTRACT. The authors conducted 3 experiments to examine whether introducing high levels of contextual interference is useful in handwriting skill acquisition. For all experiments, elementary school students (*N*s = 44, 50, and 78, respectively) were randomly assigned to 1 of 2 practice schedules—blocked or random practice—in the acquisition phase. In the blocked condition, each of 3 letters (*h*, *a*, and *y*) or (in Experiment 1) symbols was handwritten 24 times consecutively. In the random condition, each letter (or symbol) was practiced 24 times, but in an intermixed, unsystematic sequence. Overall, the results showed that the random practice schedule leads to enhanced retention and transfer performance of handwriting skill acquisition.

Key words: children, contextual interference, handwriting

Educators in any field are always faced with the problem of how to best facilitate learning of the skills they are responsible for teaching. Within that broad framework, there are a number of strategies that they can use to respond to the challenge, such as demonstrating the skill (see McCullagh & Weiss, 2000, for a review) or scheduling feedback in an optimal manner (Magill, 2001). The series of experiments presented in this article concerned how educators might enhance the learning of a skill by controlling the practice scheduling of the trials attempted for that skill. The particular skill of concern is that of handwriting, a language skill that relies on the integration of a number of cognitive and motor processes (Berninger, 1994). The focus in this research was on the motor aspects of handwriting skill acquisition, with a particular emphasis on enhancing the learning of handwriting skill through random practice.

The basic impetus comes from research that has shown that practice conditions that introduce high levels of contextual interference enhance the retention and transfer of motor skills. Contextual interference refers to the "interference that results from practicing various tasks or skills within the context of practice" (Magill, 2001, p. 289). One can vary the amount of contextual interference within practice by the manner in which one schedules the practice trials of various skills. For example, blocked practice, a practice schedule wherein all the trials of a particular task are practiced before another task is introduced, would have low levels of contextual interference. Conversely, high levels of contextual interference would be introduced with what has been termed a random practice schedule. Under random practice conditions, all of the tasks are practiced together in an unsystematic sequence of trials. It is important to understand that it is only the schedule of the trials of the task variations that is manipulated in contextual interference research; the number of trials practiced per task is held constant.

The basic finding of experiments in which those practice conditions have been compared is that random practice leads to better retention and transfer of skills than blocked practice does—a phenomenon termed the *contextual interference effect* (see Magill & Hall, 1990, for a review). Research on the contextual interference effect has been motivated both by theory and pedagogy. At the theoretical level, interest has been directed at the nature of the cognitive processing responsible for the benefits derived from high contextual interference conditions and the positive influence that processing has on learning the motor programming necessary to execute movement patterns. Although two different theoretical approaches have been advanced (Lee & Magill, 1985 vs. Shea & Morgan, 1979), the active involvement of cognition during skill acquisition is considered in both approaches.

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At the pedagogical level, investigators have considered how the contextual interference effect can be applied to the teaching of real-life motor and cognitive skills. Indeed, empirical support for the applications of the contextual interference effect has been obtained not only for laboratory motor tasks (Hall & Magill, 1995; Lee & Magill, 1983; Shea & Morgan, 1979; Tsutsui, Lee, & Hodges, 1998) but also for sport skills such as the badminton serve (Goode & Magill, 1986; Wrisberg, 1991), the basketball set shot (Landin & Herbert, 1997), and baseball hitting (Hall, Domingues & Casvasos, 1994). Although a number of applied settings and varying motor skills have been used in the research on the contextual interference effect, the studies have typically involved gross motor skills, with adults being the population tested. The uniqueness of our research is that we tested the effects of varying levels of contextual interference on a fine motor skill, handwriting (see also Albaret & Thon, 1998, for a drawing task), and that the population of concern was young children.

Why is it important to study the population of children? One key reason is that there are age-related differences in a number of information-processing skills (Gallagher & Thomas, 1984, 1986) that might be important for the contextual interference effect. For example, researchers have shown that children have a reduced working-memory capacity as compared with that of adults (Keogh & Sugden, 1985). According to the elaboration hypothesis of Shea and Morgan (1979), for instance, high contextual interference conditions are effective because individuals make active comparisons among the different movement patterns that are held in working memory. Thus, because of their reduced working-memory capacity, children might not be able to effectively make those comparisons or to obtain the same benefits as adults do.

In fact, the limited number of studies of child participants have yielded mixed results: High contextual interference benefits have been reported by Edwards, Elliott, and Lee (1986) and by Pollock and Lee (1997). However, Pigott and Shapiro (1984) reported no differences between high and low contextual interference groups. Delrey, Whitehurst, and Wood (1983) found a reverse effect: The blocked group attained better transfer performance than the random group did. And finally, Wegman (1999) reported varied results that depended on the skill type being practiced (i.e., open versus closed skills). Given those equivocal results among the limited studies, it seems pertinent to continue contextual interference research with a population of children. Indeed, researchers in the area of contextual interference have explicitly stated that more research with children is necessary (Landin & Hebert, 1997; Magill, 2001; Newell & McDonald, 1992).

With respect to the second unique feature, the motor skill of handwriting was selected for two reasons. First, as mentioned earlier, mainly gross motor skills have been used in contextual interference research to date. Although Albaret and Thon (1998) did use the fine motor skill task of hand-drawing, the research on fine motor skills is limited. Thus, further contextual interference research with fine motor skills is warranted.

A second, and more important, reason for the selection of handwriting is the potential impact of the research findings. The argument we propose here is that it is more significant to introduce a new pedagogical technique to a field in which that technique has not as yet been widely adopted. Indeed, in reviewing handwriting literature, there appeared to be greater emphasis on blocked than on random practice conditions. For example, traditional practices of teaching handwriting follow the blocked practice schedule (e.g., Enfiejian, Sanders, & Lindner, 1975). Even more recently, in his review of teaching strategies to improve letter handwriting, Gosselin (1991) revealed that repeated tracing and copying of a letter were common to many of the techniques introduced from 1979 through 1984 (see also a review of handwriting research by Dobbie & Askov, 1995). Similarly, the blocked style of acquisition has been adopted in recent studies that have incorporated handwriting instruction interventions. The instructional method, for example, in Graham, Harris, and Fink's (2000) research on the treatment of handwriting problems in beginning writers, required participants to practice the same letter for 10 trials in the initial stages of their instructional program. Thus, it seems that the recommendation that one should use a random practice schedule is not routinely implemented, although Berninger et al. (1997) showed in their intervention study that children benefited from practice of all 26 letters in what would be considered a randomly structured practice. However, Berninger et al. did not systematically compare blocked and random schedules; thus, they did not determine which of the practice schedules would, in fact, have benefited the participants more.

Another issue to bear in mind is that individual letter handwriting will most likely be transferred to the task of handwriting words. Handwriting a word is more akin to a random structure of presentation than to a blocked structure of presentation. That is, handwriting the individual letters *A*, *B*, and *C* could easily transfer to the task of handwriting the word *CAB*. Considering handwriting skill acquisition in that manner introduces the concepts of transfer-appropriate processing and encoding specificity. The more similar a retention test is in the processing demands that are required during the encoding, the better the retention performance (Bransford, Franks, Morris, & Stein, 1979; Stein, 1978). The importance of compatibility of the encoding and the retrieval relationship has been demonstrated in numerous studies (Tulving & Osler, 1968; Tulving & Thompson, 1973). Practicing the handwriting of letters in a random presentation, then, might more closely match the demands of handwriting words, which can be considered the real-life transfer task of handwriting skill.

In the series of experiments presented here, we examined the effects of blocked and random acquisition practice schedules on the retention and transfer of handwriting performance. Our prediction was that acquisition performance would be best under blocked conditions but that random practice schedules would lead to the best retention and transfer performance for handwriting skill; we assumed that retention and transfer performance measures are true indicators of actual learning (Schmidt & Bjork, 1992). If the results provide support for that prediction, a strong case can be made for developing new strategies for teaching the fine motor skill of handwriting.

EXPERIMENT 1

Our basic objectives in this experiment were (a) to determine whether high levels of contextual interference enhance retention of handwriting skill better than low levels of contextual interference and (b) to ascertain the importance of the retention test phase design. We considered it important to investigate the issue of the retention test phase design, given the encoding specificity findings mentioned earlier. That is, we wanted to ascertain whether transfer to a retention test schedule that is the same as the schedule used during acquisition (e.g., random acquisition–random retention) would produce different results from transfer to a retention test schedule unlike the one used in acquisition (e.g., random acquisition–blocked retention).

Method

Participants

Participants were 44 first-grade children from three Grade 1 classes, registered in two different Catholic elementary schools. That resulted in three different testing sessions for each of the class groups; each group experienced all experimental conditions. Classes 1 and 2 were students enrolled in a French-language school board, whereas Class 3 was enrolled in an English-language school board. The instructions for the groups were given in their respective languages. Twenty-one of the participants were girls (mean age = 6.30 years, SD = 0.43 years), and the remaining 23 were boys (mean age = 6.20 years, SD = 0.55 years). Consent for participation was obtained from the parents or guardians of the children through a letter circulated to them in advance of the experiment.

Materials

Sheets of paper that the symbols were to be handwritten on were provided to the children. We used three lines to designate the handwriting space of the symbols; the top and bottom lines were solid, and the middle line was dashed. To avoid previous experience with the handwriting of regular alphabet letters, we used different symbols that were considered to be representative of the movement patterns encountered in handwriting letters. One symbol was invented by the researchers and the remaining two were taken from the international phonetic alphabet; one was the symbol that represents the sound *er*, and the other was the symbol that represents the sound *th*. One diagram of each symbol was drawn onto a sheet of paper and was left on display for the children throughout the experiment.

Procedure

The experiment was divided into three phases: an acquisition phase, an interpolated phase, and a retention test phase.

Acquisition phase. This phase lasted approximately 35 min and consisted of 72 acquisition trials, with 24 trials of each symbol. Participants were randomly assigned to one of two groups (blocked or random) differing with respect to how the practice trials were scheduled. For the blocked group, all 24 trials of a particular symbol were performed consecutively. For the random group, the 24 trials of each symbol were pseudo-randomly intermixed within three blocks of 24 trials. More specifically, the order of presentation was constrained so that each of the symbols occurred eight times in a set of 24 trials but none was presented three times in succession.

The participants received verbal instructions describing how each of the symbols was to be drawn and explaining the respective scoring systems. They were also encouraged to attempt to obtain a perfect score for each of the symbols drawn. The encouragement was given to the blocked group at the beginning of each block when the new symbol was being introduced. The random group received the information for all three symbols at the beginning of the acquisition phase.

The 24 trials were divided into eight sets of 3 trials. Participants received error-correction information on 50% of the trial sets. That is, following a set of 3 trials, the experimenter looked at the trial set and gave feedback to the participants as to how to correct any aspect of the symbols that had been practiced. If there were no corrections to be made, the experimenter simply told the participants that the symbols were perfect. On alternating sets of trials, no information was provided.

Participants were tested in groups of 4, with one pair receiving the error-correction information on one set of trials and the other pair on the next set of trials. The feedback was provided individually, and we arranged participants' seating so that they could not hear or see the information provided to another participant in the experiment.

Interpolated phase. During the acquisition phase, participants had been removed from their classroom to participate in the experiment. During the interpolated phase, which lasted 30 min, participants returned to the classroom and engaged in whatever activity the regular schoolroom teacher was administering. Because of the different classroom settings and testing sessions, different tasks were encountered during that phase. Class 1 had engaged in an addition and subtraction task on the computer, Class 2 had been involved in story reading, and the final class performed math problems during the interval. Although each group performed different tasks, the important aspect was that all participants were involved in cognitively demanding activities that prevented mental rehearsal of the previous symbol handwriting task.

Retention test phase. The two acquisition groups (blocked and random) were further divided randomly into two groups, each of which transferred to blocked or random retention trials. The further division resulted in four retention groups: (a) blocked acquisition–blocked retention (B–B), (b) blocked acquisition–random retention (B–R), (c) random acquisition–blocked retention (R–B), and (d) random acquisition–random retention (R–R). A review of the scoring system for each of the three symbols was given. Participants in the blocked retention trials were to handwrite each of the symbols three consecutive times. Nine of the random retention trials were performed in an unsystematic order, such that three of each symbol were handwritten. No error-correction information was given during that phase.

Results and Discussion

We used a 3-point scoring system to score each handwritten symbol. All three lines were used in the evaluation of the symbol handwriting performance. For example, the scoring system was as follows for the *er* symbol: 1 point was given if the first large semicircular motion went from the top line to the bottom line; 1 point was given if the loop that followed intersected the middle line and crossed the point at which the first line had intersected; 1 point was given if the continued line touched the top line and finished with a small downward diagonal line. The dependent measure was the score attained for the handwriting of the symbols. The scores on each set of three trials per letter were averaged, creating eight trial set scores in the acquisition phase and only one score in the retention test phase.

The person responsible for scoring the symbols was blind to the experimental condition in which the child participated. Each symbol had a maximum score value of 3. The level of significance was set at $p < .05$. We used Tukey post hoc testing to determine significant differences among multiple means.

Acquisition Phase

We used a 2 × 3 × 8 (Group × Symbol × Trial Set) analysis of variance (ANOVA) with repeated measures on the last two variables to analyze the data. A main effect for symbol, $F(2, 84) = 3.4$, $MSE = 1.9$, was obtained. Tukey post hoc testing revealed that the children had the most difficulty with the *er* symbol ($M = 2.10$, $SD = 0.81$), followed by the invented symbol ($M = 2.30$, $SD = 0.72$), and the *th* symbol was the easiest of the three ($M = 2.50$, $SD = 0.95$). The symbol variable did not interact with any other variables, however, and for subsequent explanation, we combined and averaged the individual means.

A marginal main effect for group was attained at $p = .08$, $F(1, 42) = 3.2$, $MSE = 1.9$. The blocked group's acquisition scores were higher ($M = 2.42$, $SD = 0.95$) than were those of the random group ($M = 2.26$, $SD = 0.86$). The reason we obtained only a marginal effect is that a significant interaction of Group × Trial Set was also obtained, $F(7, 658) = 7.8$, $MSE = 0.1$. Moreover, that interaction did not reflect the typical learning curve interaction obtained for those two practice condition groups. That is, performance in the random condition did not approach that in the blocked condition; rather, the former group actually declined in performance. Post hoc testing showed that for the last four trial sets, the blocked group's performance was consistently superior to that of the random group (refer to Figure 1). That finding is not typical in contextual interference research, and the reasons for it are still unclear. There is the possibility that a practice condition that leads one to perform more poorly also leads to such outcomes as decreasing motivation or disengagement. Those negative learning characteristics may, in turn, have led to decreased performance in the later learning trials. That issue will be returned to later.

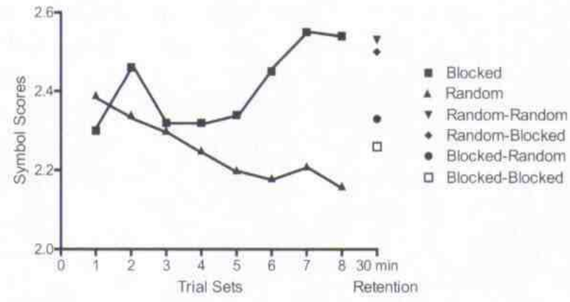

FIGURE 1. The symbol scores attained in the acquisition trial sets and in the 30-min retention test of Experiment 1.

Retention Phase

We used a 4 × 3 (Group × Symbol) ANOVA with repeated measures on the last variable to analyze the data. Once again, the ordering of the difficulty of the different symbols was the same as that seen in the acquisition phase. The *er* symbol was the most difficult ($M = 2.30$, $SD = 0.75$), and the *th* symbol was the easiest ($M = 2.60$, $SD = 0.84$), with the invented symbol falling in between the two ($M = 2.50$, $SD = 0.83$). That finding was supported by a main effect for symbol, $F(2, 84) = 6.5$, $MSE = 0.16$. That variable did not interact with group, however, and therefore means are collapsed in the subsequent section.

We also obtained a significant main effect for group, $F(1, 42) = 4.2$, $MSE = 1.2$. Tukey post hoc testing showed that the retention performance of the two groups that had practiced the symbols in the acquisition phase using a random schedule (R–R and R–B) was superior to that of the participants who had practiced the symbol in the blocked format (B–R and B–B), but within those two sets of groups there were no differences (see Figure 1 for the retention group means). Thus, no differences were obtained based on the relationship between the acquisition practice schedule and the retention test conditions. Those results are similar to those of Shea and Morgan's (1979) 10-min-delay groups in that the random acquisition condition groups (R–R and R–B) both outperformed either blocked condition group (B–R and B–B). No other findings were significant.

Our findings replicated the contextual interference effect: As compared with blocked acquisition trials, random practice in acquisition resulted in superior retention performance. Hence, higher levels of contextual interference were effective for the learning of fine motor skills by children. In addition, the match between encoding and retrieval conditions did not seem to influence the performance of participants in the retention phase.

EXPERIMENT 2

The first experiment served a number of purposes, the most important being that it enabled us to determine that high levels of contextual interference can have a positive impact on the motor skill of learning to handwrite. It also allowed us to ascertain that the general method used was appropriate and that the basic design could continue to be used. As such, continued experimentation on the contextual interference effect in handwriting was deemed useful. There were a few weaknesses that needed to be reconciled, however, and those were attended to in Experiment 2.

First, the use of different symbols served to introduce novelty into the task so that the children would not have had established motor patterns—an important criterion in motor-skill learning scenarios. At the same time, though, it removed the actual stimuli that are used when children are learning to write. Thus, in the next experiment, we used letters of the alphabet; specifically, the cursive lower-case letters *h*, *a*, and *y* were selected from the English alphabet for the participants to write. Within those three letters, the majority of the complex movement patterns required for the fine motor skill of handwriting are represented. As well, the three letters are all considered to be of moderate difficulty in terms of perceptual-motor complexity (Meulenbroek & Van Galen, 1990), thus eliminating that variable from the present research. We used the cursive form of the letters to maintain the novelty factor. Cursive writing is not introduced into the Ontario curriculum until Grade 3, and thus students in Grade 1 and the beginning of Grade 2 were recruited as participants.

For further ecological validity, we added a transfer test phase to the design. We introduced the transfer phase because we wanted to be able to argue that individual-letter writing in a random format is the best learning strategy for building toward the task of writing words. Thus, in the transfer test phase, the children were also required to write all three lower-case, cursive letters in an attached fashion to form the word *hay*. The introduction of the transfer task also made it appropriate for us to measure the length of time it took the children to write *hay*. Handwriting speed was thus introduced as a dependent measure because it has been described as an important indicator of handwriting proficiency (Bailey, 1988). Along with the transfer task, the children performed a retention test in which each letter was written individually two times in a blocked format. Only a blocked format was used for that part of the retention test because no differences based on the retention test format had been found in Experiment 1.

Another limitation of the first experiment concerned the short interpolated retention interval used—30 min. We were interested in determining whether the benefits of the random practice conditions would persist longer than that, perhaps extending to at least 24 hr. The 24-hr time interval seemed particularly appropriate given the typical scheduling of not practicing something learned on one day until the next school day. So, too, extending the interval of time between the acquisition phase and the retention and transfer phases ensured that any transient effects of practice would have had sufficient time to dissipate.

Method

Participants

Fifty children ranging in age from 6.0–7.5 years (M = 6.90 years, SD = 0.51 years) were recruited as participants from two schools. Data from 2 of the children were discarded because the children were not able to persist with the task. Therefore, data from 48 participants were included in the analysis, with 24 children in each experimental condition. Consent for participation was obtained from the parents or guardians of the children through a letter circulated in advance of the experiment.

Materials

Large 3-cm models of each individual letter were written on 10.2- × 15.2-cm cue cards and were displayed throughout the handwriting task. For the retention phase, a similar cue card displaying the word *hay* in lower-case, cursive writing was used. In addition, participants were given a sharp HB no. 2 pencil and writing paper (same style as in Experiment 1) with which to perform the writing task. Furthermore, we used a stopwatch to time handwriting speed during the transfer task.

Procedure

The same basic procedure was followed as that described in Experiment 1, with three exceptions. First, a transfer test was administered immediately following the retention test. That transfer task involved the children writing the three letters in lower-case, cursive script continuously to form the word *hay*. The model of the cursive form of the word *hay* on the cue card was first shown to the children and was then left on display above the writing paper provided for the transfer test. Children were then encouraged to write the word as accurately as possible. Although the children were aware that they were being timed for each trial of the word, speed was not emphasized in the instructions. Three trials of the word *hay* were written on a piece of paper, and each trial was timed from the point at which the children put the pencil to the paper until they lifted their pencil upon completion of the word. Another notable exception was that, for the retention test trials, all participants performed in a blocked order, with each letter written twice. We used that format because no differences had been found based on the test order of

Experiment 1. The final exception is that the first retention and transfer tests were performed 20 min after acquisition and were also repeated a day later (24-hr retention and transfer tests).

Dependent Measures

Similar to the scoring system used with the symbols, a point system was created surrounding characteristics of the letter's ascenders and descenders with respect to each of the three lines on the paper provided. Six aspects of each written letter were defined, creating a 6-point scoring system. A scorer who was blind to the condition in which the child participated assigned the points to each letter. In addition, the time taken to write the word *hay* was used as a dependent measure.

Results and Discussion

Acquisition Phase

We used a 2 × 3 × 8 (Group × Letter × Trial Set) ANOVA with repeated measures on the last two variables to analyze the data. A main effect for letter was obtained, $F(2, 92) = 14.6$, $MSE = 6.3$. Tukey post hoc testing showed that scores for the letter *a* ($M = 4.8$, $SD = 1.8$) were significantly higher than those for the letter *y* ($M = 4.0$, $SD = 1.3$), which, in turn, were higher than those for the letter *h* ($M = 3.8$, $SD = 1.1$); the scores for the letters *y* and *h* did not differ significantly from each other. The letter variable did not interact with any other variables, and, thus, for subsequent explanations the individual means of each letter were combined and averaged.

As can be seen in Table 1, the findings supported a significant Group × Block interaction, $F(7, 322) = 2.3$, $MSE = .67$. Contrary to the findings obtained in the first experiment, the random group performed significantly better than the blocked group during Trial Sets 2, 3, and 4, but no differences were evident in the remaining trial sets. The reverse pattern, in which the random group did better in some of the acquisition trial sets, is interesting; it suggests that the poorer performance of the random group in Experiment 1 as trials progressed was probably not caused by motivational or disengagement factors associated with the practice condition but instead may have been an artifact.

TABLE 1. Means and Standard Deviations (*SD*s) of the Acquisition Performance in Experiment 2

	Random		Blocked	
Trial set	*M*	*SD*	*M*	*SD*
1	3.8	.88	3.8	.95
2	4.2	.96	3.7	.97
3	4.4	.98	4.0	.91
4	4.5	.85	4.1	.86
5	4.3	.73	4.3	.96
6	4.3	.84	4.4	.89
7	4.3	.92	4.3	.97
8	4.2	1.00	4.4	.88

Note. Random and blocked refer to practice conditions.

Twenty-min Retention Test and Transfer Test Phase

The obtained scores for each letter were submitted to a 2 × 3 (Group × Letter) ANOVA with repeated measures on the last variable. A main effect for letter was again obtained, $F(2, 92) = 4.7$, $MSE = .90$, and with a similar pattern of findings. That is, the scores for the letter *a* were significantly higher ($M = 4.5$, $SD = 1.8$) than were the scores of both the letters *h* ($M = 4.2$, $SD = 1.2$) and *y* ($M = 3.9$, $SD = 1.4$), and the scores of the latter two letters were not significantly different from each other. The letter variable significantly interacted with group, $F(2, 92) = 3.4$, $MSE = .90$; that interaction is presented in Figure 2. Specifically, we found that the random acquisition schedule yielded significant writing benefits for the letters *a* and *y*, whereas, in contrast, the blocked practice schedule significantly benefited the writing of the letter *h*.

A first thought to explain those findings is that the pattern of results was caused by the main effects obtained for letter, with the supposition that letter difficulty was connected to the effect. There was no direct support for that hypothesis, though, because the letters *h* and *y*, which were determined to be of similar difficulty, yielded different patterns of results. Similarly, the letters *a* and *y*, which were significantly different in both the acquisition and retention phases in terms of acquisition scores, followed the same pattern of results for retention. A particular methodological issue, however, was probably the basis for that finding.[1] We realized that participants in the blocked acquisition schedule always started their practice schedule with the letter *h*. Thus, there might have been a confound in terms of the order in which the letters were introduced. That issue was addressed in Experiment 3.

To analyze the transfer test, we conducted a one-way ANOVA for group, using the time scores for the writing of the word *hay*. The group that had learned under random conditions was able to write the word *hay* significantly faster ($M = 23.8$ s, $SD = 5.7$ s) than the group exposed to the blocked acquisition schedule ($M = 28.3$ s, $SD = 6.2$ s), $F(1, 46) = 4.4$, $MSE = 165.6$. That finding is particularly important because speed of writing has been a significant factor in the assessment of handwriting performance since the beginning of the century (Bailey, 1988). As well, the ability to write quickly is argued to be a fundamental educational skill that allows children to keep up with their peers in classroom settings (Phelps & Stempel, 1988; Ziviani & Elkins, 1984).

Twenty-Four-hr Retention Test and Transfer Test Phase

We conducted the same analyses as those done in the immediate retention and transfer test phase on both the letter and the time scores for the delayed retention and transfer

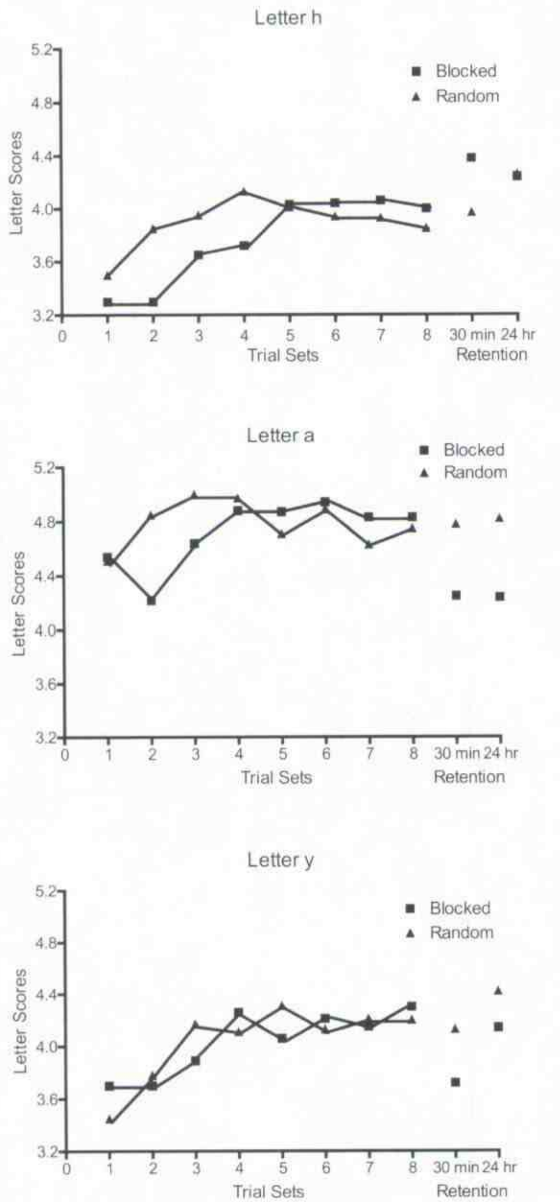

FIGURE 2. The letter scores attained for each letter in the acquisition trial sets and in the 20-min and 24-hr retention tests of Experiment 2.

data. No significant differences were found in terms of points awarded for the letters; however, a similar trend was seen whereby the random group obtained higher scores for the letters *a* and *y* than the blocked group did (refer to Figure 2). Significant differences were found for group on the time data, $F(1, 46) = 8.7$, $MSE = 91.5$. Once again, the random group was significantly faster ($M = 25.9$ s, $SD = 4.8$ s) than the blocked group ($M = 28.2$ s, $SD = 5.5$ s) at writing the word *hay*. Thus, the random acquisition schedule enhanced transfer performance.

EXPERIMENT 3

As noted in the discussion of Experiment 2, an important issue to address concerns the significant interaction obtained between the practice condition and the letter written during retention. Did that interaction result from issues such as perceptual-motor complexity or mainly from the procedure of having children always starting with the letter *h* in blocked acquisition trials? Because there was no direct link between our findings of the difficulty children had with the specific letters and the pattern of the interaction, we wanted to discount the former alternative and accept the latter. Thus, to address the issue further, we counterbalanced the order of letter introduction in the blocked practice condition of the acquisition phase. Given that ordering, if the interaction between group and letter failed to reach significance, then that finding would suggest that the significant interaction between group and letter in Experiment 2 was an artifact of the procedure. On the other hand, if the interaction persisted, then a deeper look into the variations of letter difficulty might be appropriate.

Another motivator for continued experimentation with the same procedures relates to the differences in the acquisition pattern for the groups between Experiment 1 and 2. Although both experiments showed the random retention performance to be better than blocked retention performance, issues concerning the acquisition results could still be important. For example, if a student is facing continual challenges because a random practice schedule was used and responds to the challenges by disengagement or reduced motivation, then the proposed random schedule might not be the best teaching technique. That question needed to be considered because, although it was possible for us in our experimental setting to request the child to persist with the task and to continue with the practice trials on the following day, that might not be possible in a classroom setting, and educators will be sensitive to that problem. That is, they might not want to implement a learning strategy that has too many negative consequences initially or leads the learner to initially reject the task at hand.

Method

Participants

Seventy-eight children ranging in age from 5.5–7.0 years ($M = 6.42$, $SD = 0.40$) were recruited as participants from five schools. More specifically, participants were drawn from classes at Montessori schools that were at a level equivalent to senior kindergarten,[2] as well as from Grade 1 classes in the public and Catholic school systems of the Ottawa-Carleton area. Consent for participation was obtained from the parents or guardians of the children through a letter circulated to them in advance of the experiment.

The data of only 68 of the children were used for analysis; data were eliminated for three main reasons. First, testing for 5 of the children was interrupted by a recess or lunch break that the child did not want to miss. Second, 3 of the children did not persist with the task and requested to return to their classroom although they had not performed all of the acquisition trials. Finally, 2 of the children showed near-excellent performance in the first and second trial blocks for all of the letters. Because we wanted this to be a motor-skill learning experiment, we did not include their data in the analysis.

Materials

The same materials were used as those described in Experiment 2. In addition, to further refine the scoring process, we made a transparent overlay in order to evaluate whether the appropriate lines in the handwriting space were touched. The overlay consisted of lines 0.5 mm in thickness and with distances corresponding to those on the writing paper.

Procedure

The same procedure was used as that described in Experiment 2, with three exceptions. First, a one-on-one experimenter–participant testing situation was used. Second, there was an agreement with the Ottawa-Carleton School Board that a child would not be tested alone with a single experimenter; thus, two experimenters were always present in the same room, each testing one child. The pairs were always placed a given distance away from each other, which ensured sufficient individual work space for each participant. Finally, for the blocked practice schedule, the letter order was counterbalanced across participants such that a letter was written first in a sequence, then second or third in the next sequence, and so forth.

Dependent Measures

The same scoring system used with the letters of Experiment 2 was used here. The handwriting was scored by three graduate students who were blind to the practice conditions under which the children participated. Interrater reliability was checked for 10% of the sample, and an $r = .99$ was obtained for the acquisition data. Same-day and next-day retention also had high reliabilities (rs = .89 and .87, respectively). Because of the high reliability, the remaining data were divided into three parts and scored by either one of the raters. In addition, the time taken to write the word *hay* was used as a dependent measure.

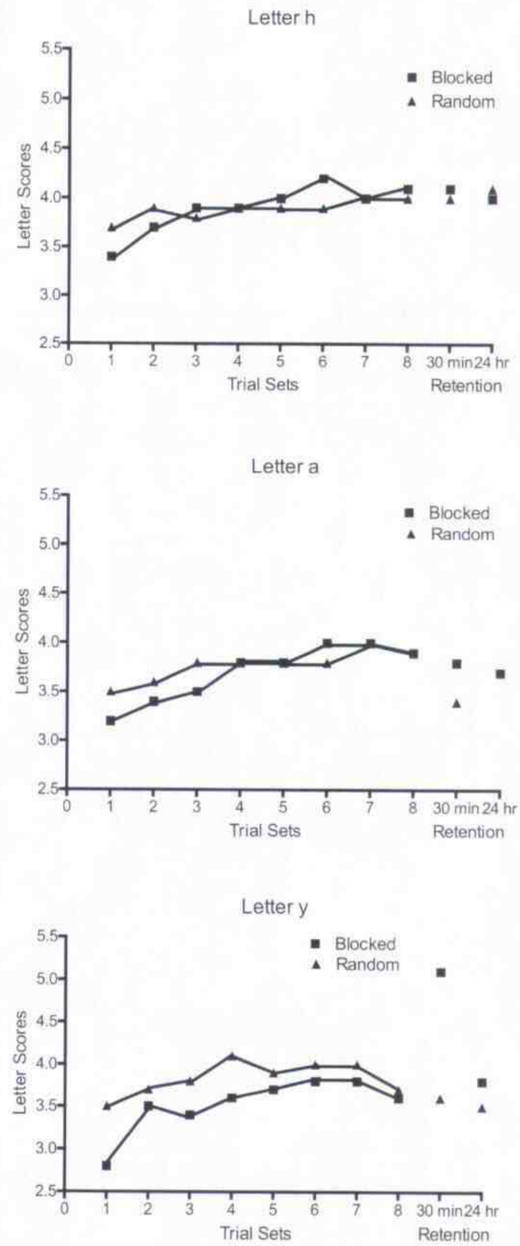

FIGURE 3. The letter scores attained for each letter in the acquisition trial sets and in the 20-min and 24-hr retention tests of Experiment 3.

Results and Discussion

Acquisition Phase

A 2 × 3 × 8 (Group × Letter × Trial Set) ANOVA with repeated measures on the last two variables was conducted on the scores. A main effect was found for trial set, $F(7, 462) = 5.3$, $p < .001$, indicating improvement in performance across the acquisition phase (see Figure 3). No other main effects or interactions were found. The absence of a main effect for acquisition indicates that the performance of the handwriting task in the acquisition phase did not vary as a function of the group in which the child participated (i.e., blocked or random). The absence of acquisition differences between the two practice conditions has also been evidenced in other studies (e.g., Lee & White, 1990, Wrisberg, 1991; Wulf & Lee, 1993). That the same acquisition pattern was found for the two groups is of interest in terms of the issues regarding the findings of Experiment 1. This final experiment showed no differences in acquisition, suggesting that the outcome of Experiment 1 might have been an artifact and that negative short-term consequences resulting from the adoption of a random practice schedule might not be a concern. There is also the possibility that the different stimuli used among the experiments might have contributed to the different findings. That is, the symbols used in Experiment 1 were likely to be unfamiliar to the children, whereas exposure to the cursive letters of Experiments 1 and 2 was more probable.

Twenty-min Retention and Transfer Test Phase

We used a 2 × 3 (Group × Letter) ANOVA with repeated measures on the last variable to test for differences in the retention data. The main effect for group approached significance at $p = .10$; the random group's scores ($M = 4.30$, $SD = 0.83$) were better than the blocked group's ($M = 3.60$, $SD = 0.97$). Figure 3 demonstrates how that effect did not interact with letter. The score was consistently higher for the random group than for the blocked group, regardless of the letter. That consistent pattern suggests that the interaction found in Experiment 2 was related to the fact that the blocked group always started the writing session with the letter *h*. We used a one-way ANOVA with group as the variable to analyze the mean times for the transfer task of writing the word *hay*. A main effect for group, $F(1, 66) = 7.2$, $MSE = 69.9$, revealed that the participants who practiced under random conditions in acquisition were able to write all three letters to spell the word *hay* more quickly than did the participants who practiced under blocked conditions (see Table 2). Of possible concern, however, is that the increased speed of writing the word *hay* was done at the cost of accuracy. That is, the classic speed–accuracy tradeoff might have been in effect here. To test that possibility, we performed the same scoring procedures on the letters in the word form of *hay* as that used for the letters written individually. In addition, the scores of each letter were subjected to the same data analysis as that of the retention phase. That analysis revealed a main effect for letter, $F(2, 128) = 14.0$, $p > .001$. A Tukey post hoc test showed that the letter *h* ($M = 3.80$, $SD = 0.86$) obtained significantly higher scores than both the letters *a* ($M = 3.20$, $SD = 0.79$) and *y* ($M = 3.30$, $SD = 0.83$), which were not different from each other. No differences were obtained for group, $F > 1.0$. The random group's ($M = 3.50$, $SD = 0.76$) and the blocked group's ($M = 3.30$, $SD = 0.96$) scores were similar. The similarity in scores indicates that the speed advantage was not obtained to the detriment of writing accuracy.

TABLE 2. Means and Standard Deviations (*SD*s) of the the Time Scores for the 20-min and 24-hr Transfer Tasks of Experiment 2

Practice condition	Time score (s) *M*	*SD*
20-min retention		
Blocked	28.41	9.46
Random	22.91	7.22
24-hour retention		
Blocked	24.76	9.38
Random	20.13	6.43

Twenty-Four-hr Retention and Transfer Test Phase

Data from the delayed retention and the transfer phases were submitted to the same analyses as were the data of the previous phase. For the retention scoring data, no differences emerged for group. Analysis of the time to complete the transfer task, however, again revealed a superior performance on the part of the random group; that group wrote the word *hay* significantly faster than did the group that had a blocked acquisition schedule, $F(1, 66) = 5.6$, $MSE = 63.6$ (see Table 2). The scoring data of the transfer letters were also subjected to analysis. Again, a main effect for letter showed that the letter *h* ($M = 4.10$, $SD = 0.73$) was written significantly better than the letters *a* ($M = 4.10$, $SD = 0.73$) and *y* ($M = 4.10$, $SD = 0.73$); however, no main effect was obtained for group. The random ($M = 3.70$, $SD = 0.84$) and blocked ($M = 3.60$, $SD = 0.92$) groups attained similar scores. Thus, once again, the speed–accuracy tradeoff was not implicated.

GENERAL DISCUSSION

In this investigation, we were interested in determining whether random practice of the fine motor skill of handwriting would be better for learning than blocked practice. We hypothesized that random acquisition trials would result in poorer immediate performance, as measured in the acquisition phase, but would lead to superior retention and transfer performance than would blocked acquisition trials. As stated previously, that hypothesis has two components: One is the pattern to be found in the acquisition phase and the other is associated with the expected pattern in the retention and transfer phases.

With respect to the acquisition phase results, the findings were quite varied across the three experiments. First, with the symbols used in Experiment 1, only a marginal group effect was noted, but a significant interaction between group and trial set was obtained. The blocked group outperformed the random group, but only in the latter trial sets of acquisition. Moreover, the difference emerged as a result of the random group's decreasing performance across the latter trial sets. That pattern was not repeated in the next two experiments, however, and thus might have been just an artifact of the data. In Experiment 2, when the task was changed to writing actual letters of the alphabet, the random group had a significantly better performance than the blocked group early in acquisition, but both groups were performing the same by the end of the acquisition trials. In Experiment 3, no differences were found between the two varied levels of contextual interference. The different patterns of findings have also been reported in the contextual interference literature; sometimes the blocked group has been superior (e.g., Lee & Magill, 1983; Shea & Morgan, 1979) or no differences have emerged between the two groups (e.g., Lee & White, 1990, Wrisberg, 1991; Wulf & Lee, 1993), and in a more limited number of cases, the random group has shown superior acquisition performance (Jarus & Goverover, 1999). Research into why those different patterns have emerged in the various studies would be useful. Possible differences in the administration of the practice conditions, for example, might be one issue to investigate.

The varied findings of the acquisition phase, though, are not a large concern, because the main interest lies in the effects to be found in the retention and transfer phases. Those results are more interesting because they are presumed to be a better reflection of the characteristics of learning, that is, the permanence (retention test) and adaptability characteristics of learning (transfer test; Magill, 2001). It is important to consider at the pedagogical level the absence of differences between the two groups at acquisition, however, because that implies that a child can maintain the same level of performance even when exposed to what might seem to be a more difficult task.

The 20-min retention data yielded more consistent findings than those found in acquisition and followed the pattern predicted by the contextual interference paradigm. For the retention test of Experiment 1, the random group was better than the blocked group at producing the three symbols. Similarly, participants who had undergone a random practice schedule performed better than those in the blocked acquisition group on two of the three letters in Experiment 2. The blocked group, however, did perform better than the random group on the one letter that had been practiced first in the series of trials for the blocked group. For the third experiment in the series, the random group again outperformed the blocked group; however, significance at the $p <$ .05 level was not attained.

Unfortunately, those effects did not seem to persist over a 24-hr interval. Although the pattern of the random group being better than the blocked group was found in the second experiment, the difference did not reach significance. Moreover, no such pattern was seen in Experiment 3. That result leads one to question the long-term consequence of high levels of contextual interference. It should be noted, however, that Experiment 3 had only one acquisition session, and in practical settings where longer retention benefits have been obtained (e.g., Goode & Magill, 1986; Wrisberg, 1991) more practice sessions were carried out. In consideration of that finding, a longitudinal design would be appropriate in handwriting research of this nature. A final point of interest is that the scoring system measure was only one of two measures used in this research; the other measure showed longer-term benefits.

More specifically, the final results to consider are those from the transfer tests administered 30 or 20 min following acquisition and 24 hrs after the acquisition phase. The results from those tests produced the most consistent findings: Participants who followed the random practice schedule in acquisition wrote the word *hay* much faster than did those who had undergone blocked practice conditions. Analysis of the scoring measure in the transfer phase also showed that the speed advantage did not result in a poorer accuracy performance, as compared with the blocked group's. As mentioned, the handwriting speed results are very important to consider given the educational benefits that arise when children are proficient at handwriting (Berninger et al., 1997; Graham et al., 2000). Taken together, the retention and transfer results do support the prediction forwarded that higher levels of contextual interference (random practice) lead to enhanced transfer and short-term retention performance in motor-skill learning. Moreover, those effects are occurring in the population of children, suggesting that despite their more limited memory and attention capabilities (Gallagher & Thomas, 1984, 1986) the strategy of using high levels of contextual interference to enhance learning is viable.

Although we did not design the research presented to determine the basic mechanisms as to why the random practice schedule produced better results, it is reasonable to link the results to the mechanisms that have been suggested by others. For example, the two main theories for explaining the contextual interference effect (Lee & Magill, 1983; Shea & Zimny, 1983) both rely on the idea that higher levels of contextual interference establish more effortful processing. Indeed, cognitive effort has been presented as a key feature in motor-skill learning (Lee, Swinnen & Serrien, 1994). It could be argued in this research that the random schedule led to more effortful processing on the part of the children because the continual need to execute different motor patterning demands provided a more challenging learning environment, as compared with a blocked practice schedule that allows participants to repeatedly execute the same motor demands.

At an applied level, the finding that random practice enhances later performance of handwriting skills is an important one. Such findings indicate that the mere repetition of a letter is not sufficient for learning. Thus, current techniques (e.g., Roberts & Samuels, 1993; Sims & Weisberg, 1984) used to teach handwriting skill need to be re-evaluated because they often encourage a blocked practice schedule in which the child repeatedly produces the same letter over a large number of consecutive trials. The results presented here suggest that random presentation of different letters to write within a number of trials is better for the establishment of proficient handwriting. That is, high levels of contextual interference should be introduced in the task of learning cursive handwriting. Certainly, others have also called upon teachers to change current practices as a function of new research (e.g., Armitage & Ratzlaff, 1985), and we do the same here.

This type of research finding is timely because the recent tendency in handwriting research has been to de-emphasize or even eliminate handwriting instruction as part of the writing curriculum (e.g., Berninger, 1994). Our findings suggest that that approach is ill-directed and that students do in fact need explicit instruction on how to form and fluently write letters. The underlying reason for this recommendation is that fluency in handwriting has been linked to other educational skills such as composition (Berninger et al., 1997; Graham et al., 2000) and other written expression (Jones & Christensen, 1999). Thus, early handwriting instruction could avoid future cognitive or affective difficulties, or both, that may be encountered by children with inadequate handwriting skills. Given the findings in this study, teaching strategies for handwriting that incorporate the use of a random acquisition schedule are recommended.

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NOTES

1. Timothy D. Lee provided valuable input concerning this possible variable.

2. Children were not recruited in Grade 1 at Montessori schools because those schools introduce cursive writing skills into their Grade 1 curriculum.

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