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Distribution of Practice in Motor Skill Acquisition: Learning and Performance Effects Reconsidered

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Recent reviews about the effects of distribution of practice in motor learning have produced quite divergent conclusions. While there is agreement that massed practice depresses performance, the effect on learning has no firm consensus. One position is that massed practice depresses learning, although there are many that argue for no learning effect. In the present paper we review this literature. When distribution is considered in terms of the length of the inter-trial interval, there is strong evidence that massed practice depresses performance and learning (when learning is assessed by absolute retention measures). This conclusion was confirmed by the results of a meta-analysis. This finding is discussed relative to other literature on distribution of practice as well as some recent issues in motor learning.

Pioneered by the work of Ebbinghaus (1885/1964) and Jost (see Youtz, 1941), the issue regarding the relative merits of distribution of practice has provided a formidable challenge to experimental psychologists. The largest proportion of this research was conducted in the 1940s and 1950s, when the theoretical positions of Snoddy (1935) and Hull (1943) provided frameworks that aptly suited the contrast of massed and distributed practice schedules. With the developments in cognitive psychology during the 1960s came a declining interest in distribution of practice effects (and learning issues in general). The empirical work that continued in this area was motivated less by theory than by the practical issues that massed versus distributed effects afforded. In particular, though the effects of distribution of practice on motor learning had received considerable attention throughout the century, it was in this domain that the empirical work persisted. In recent years, the research has virtually stopped. So why review it now?

We see four reasons why a review of distribution of practice effects in motor skill acquisition would be beneficial. The first, and most important reason, is the wide disparity in conclusions about this research that recent treatments of the topic have provided. Second, due to changing views on how learning should be assessed there is a need to reevaluate this research accordingly. Third, the recent advent of meta-analytic techniques provides a statistical method for conducting such a reevaluation. Finally, some new findings in motor skills research have paved the way for a return of the issue of distribution of practice effects to a position of theoretical interest. These reasons form the purpose of the present review.

Recent Conclusions About Distribution of Practice Effects

Due to the decline in empirical work since the 1950s there has not been a comprehensive review of the distribution of practice literature with regards to motor learning in over 25 years (see Bilodeau & Bilodeau, 1961; McGeoch & Irion, 1952, chapter V). Nevertheless, virtually every motor learning text in recent years has included a section on this topic. Of interest here is the wide disparity in conclusions that these recent treatments of the topic have provided. Without exception, every author acknowledges that massing of practice trials is detrimental to motor *performance* (at least for continuous tasks). However, the effect of massing on *learning* has received quite varied opinions.

Some authors argue that massing of practice trials affects only performance — learning is not affected (Magill, 1985; Schmidt, 1975; Singer, 1980). For instance, Magill states that:

> The massed versus distributed practice schedule controversy should not be a controversy at all. The evidence seems rather convincing that the superiority of distributed practice over massed practice occurs as

> a performance attribute. When the amount of learning is taken into consideration, there does not appear to be any superiority of one over the other schedule (p. 374).

A recent historical review of motor skills research has stated the case even more bluntly: "Massed practice influences how well you perform, not how well you learn" (Adams, 1987, p. 50).

Other authors, emphasizing the immense difference between performance and learning effects due to distribution conditions, present a more cautious interpretation of this literature (Sage, 1984; Schmidt, 1982). Schmidt, for example, who stated earlier that "massing does not affect the amount of learning in motor tasks" (1975, p. 77), now concludes that massed practice produces "slightly less learning" and that it "is a powerful performance variable and a relatively weak learning variable" (1982, p. 484).

In contrast to these conclusions, Oxendine (1984) argues that distribution of practice affects both motor performance and learning:

> During the latter stages of a massed practice, the performance of the individual appears to be poorer than the actual state of comprehension or the true learning of the task. However, I am persuaded that there remains a distinct *learning* advantage to the proper type of distributed practice (p. 270).

Although Oxendine is not entirely clear as to what constitutes a "proper" distributed practice schedule, he seems to be the only author who has taken a strong stand in favor of a learning effect.

Clearly, there is no firm consensus on how distribution of practice affects motor learning. We will argue that this is primarily due to the variety of ways in which the assessment of learning has been conducted.

Assessing Skill Acquisition: The Learning versus Performance Distinction

There has been widespread agreement that the effects of distribution of practice on learning must be separated from the temporary effects that occur during the acquisition schedule. One way of providing for this distinction is to interject a rest period at the end of the practice period, followed by a retention test of one or two trials (e.g., Estes, 1950; Kimble, 1949b; Tsao, 1950). The retention test allows for a measurement of performance after the temporary effects of the distribution schedules have dissipated. Another method is to employ a transfer design whereby all distribution of practice groups are transferred to a common distribution condition following the rest period (e.g., Adams & Reynolds, 1954; Ammons, 1950; Cook & Hilgard, 1949). This method allows for the assessment of learning at the beginning of the transfer period (like a retention test) as well as an evaluation of how long that difference persists under common practice conditions. The best method, and least common, is the double transfer design, whereby the acquisition groups are divided in half and transferred to massed and distributed conditions following a rest period (e.g., Ammons & Willig, 1956; Denny, Frisbey, & Weaver, 1955). This method has all of the advantages of the previous method plus the added assessment of how the switch in practice conditions may have affected performance.

A number of measures, devised to assess learning, have accompanied these variations in designs. Each measure uses some score from the retention or transfer test to assess learning for the various distribution of practice conditions. We will evaluate four of these measures here.

Four Measures of Learning

1. Absolute Retention. The absolute retention score is the simplest measure of learning. This score is merely the performance on the retention test or the first trial (sometimes the first few trials) of the transfer test.

2. Relative Retention. The relative retention measure considers the performance on the retention test (or the first trial(s) of the transfer test) relative to the performance at the end of the acquisition trials. The measure is calculated by subtracting the final acquisition score from the retention score. This measure has also been termed the difference score or the reminiscence score.

3. Percent Relative Retention. Percent relative retention is an expression of the relative retention measure as a percent of the amount of original improvement during the acquisition trials. This measure is calculated using the relative retention score as the numerator and dividing by the amount of improvement from the beginning to the end of the acquisition practice trials.

4. Final Score. The final score is calculated from designs that use a transfer test. This measure of learning is merely the last score(s) during the transfer test.

An Example — Bourne and Archer (1956)

A study by Bourne and Archer (1956) provides a good example of the different interpretations that could be made about the effect of distribution of practice on motor learning based on different measures. Bourne and Archer examined performance of adult subjects on the rotary pursuit tracking task over a period of 21 acquisition trials and nine transfer trials. All trials were 30 s in length and the rest period between trials 21 and 22 was 5 min. Five distribution of practice groups were formed as defined by the length of the inter-trial interval during the acquisition phase: 0, 15, 30, 45, and 60 s (creating an empirical continuum from most "massed" to most "distributed" practice conditions). All groups performed under the common condition of massed trials (i.e. 0 s inter-trial intervals) during the transfer phase.

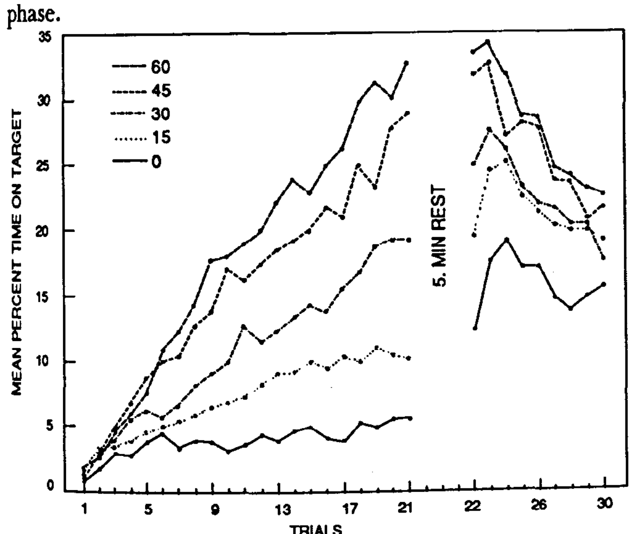

Figure 1—From "Time continuously on target as a function of distribution of practice" by L.E. Bourne Jr. and E.J. Archer, 1956, *Journal of Experimental Psychology, 51*, p. 27. Copyright 1956 by the APA. Reprinted by permission. See text for details.

The results of the Bourne and Archer (1956) study for mean percent time on target are illustrated in Figure 1. In their study, Bourne and Archer provided an analysis of their results in terms of three of the learning scores discussed above. They found a significant difference for absolute retention on the first post-rest trial ($p<.001$). As seen in Figure 1, longer inter-trial intervals resulted in better absolute retention scores. The relative retention scores resulted in an opposite effect ($p<.05$). The 0- and 15-s groups revealed significantly higher relative retention scores than the other three groups. The final score (trial 30) resulted in no significant differences ($F<1.00$).

An analysis of these learning measures reveals divergent conclusions. The absolute retention scores are interpreted to suggest that the more distributed the acquisition practice, the better the learning. The relative retention scores could be interpreted two ways. One interpretation would be that since the more massed groups showed more relative retention than the distributed groups then this resulted in better learning under massed practice conditions. However, an alternate (and more likely) interpretation would be that massed practice caused significant performance decrements that were alleviated by the rest interval. Thus, while massing depressed performance, the significant rise in performance following the rest is an indication that learning was not impaired. The final score can only be interpreted as no learning differences between the various distribution of practice groups.

The pattern of results in the Bourne and Archer study is typical of the findings in the distribution of practice literature with respect to motor skills. The different possible learning measures could often result in quite different conclusions about the effects of distribution of practice in the same experiment. When these different measures were then used to compare across studies it is understandable why such divergent conclusions were reached. We will argue however, that only one of these four measures is an adequate assessment of learning.

Evaluating the Measures

A comprehensive evaluation of the various measures of learning has been undertaken by Schmidt (1971, 1972, 1982; Salmoni, Schmidt, & Walter, 1984) and much of the discussion here is based on this work. The primary fault with the two relative retention measures is that they are "contaminated" by a performance score that does not reflect learning. The argument for the use of retention and transfer tests in the design of motor learning experiments is that some independent variables can have a temporary, depressing effect on performance. Thus, the assessment about learning is masked by the presence of the independent variable. If the independent variable does affect learning, then a performance effect will remain when the independent variable is no longer applied (i. e., on a retention or transfer test). This argument is the fundamental basis for the learning versus performance distinction. The case against the two relative retention scores then, is that they are calculated by using a value from the acquisition phase of the experiment, when the independent variable is still being applied. For a score to reflect a true measure of learning it must be calculated from performance data that are not contaminated by the temporary effects of the independent variable. Since relative retention is calculated as the retention score minus the last trial of acquisition, this measure of learning is determined, in part, by data that may not indicate anything about learning. The percent relative retention score is flawed even further since it is calculated by determining the amount of relative retention in proportion to the amount of original improvement over the acquisition trials.

Schmidt (1971) has also argued that in some cases, the relative retention scores are poor measures of learning for a statistical reason. His argument is that relative retention scores underestimate learning when the independent variable causes one (or more) condition(s) to perform near a ceiling or floor level. On statistical grounds, there is more room for improvement following the rest interval for conditions that resulted in moderate levels of performance during the acquisition phase. When relative retention scores are then calculated, the assessment of learning is determined to an extent by the levels of acquisition performance that had been reached by the last acquisition trial. This is clearly a problem in the interpretation of relative retention scores for distribution of practice experiments since longer inter-trial intervals promote performance that more closely approximate ceiling and floor levels.

On logical grounds, the final score is also a poor measure of learning. In the distribution of practice studies that use transfer designs all practice conditions are brought to a common level of inter-trial interval conditions for a series of transfer trials. Performance over the transfer trials indicates how original learning affects performance under like conditions. In some respects, the course of transfer performance is an indication of a resistance to the convergence of performance levels, which is an indication of learning. However, the choice of the final score as the measure of learning is an arbitrary data point, defined in terms of the experimental design. A better measure would be the number of trials for which a difference between original conditions (if one resulted) persisted under the common transfer conditions. To conclude that a learning effect was not apparent based on the final score would be to mask the potential differences due to a learning effect that could have appeared over one or more of the transfer trials.

Of the four measures commonly reported in the experiments on distribution of practice effects in motor learning, only the absolute retention score provides an adequate assessment of a true learning effect. The absolute retention score: (a) is not contaminated by data due to performance effects, (b) is not influenced by possible ceiling or floor performance effects, and (c) provides an initial measure of the potential learning effect before common transfer conditions produce the likely convergence in performance.

A Reevaluation of the Literature

The focus of the present review is an examination of the effects of distribution of practice on: (a) motor performance, at the end of acquisition; and (b) motor learning, as assessed by absolute retention. Before the literature is reevaluated, it is critical to define the limits of the review in terms of a number of variables.

Limits of the Review

The Literature Search. The present review is limited to only published research, retrieved in the following way. Initially, abstracts published since 1968 in Psychological Abstracts were searched. Prior to 1968, titles from *Baldwin's Dictionary* (1848-1893), *Psychological Index* (1894-1926), and *Psychological Abstracts* (1927-1967) were searched from the "Motor skills bibliographies" published by Ammons and Ammons in *Perceptual and Motor Skills* from 1949 until 1968. These bibliographies represent a thorough list of titles in motor skills research up to the commencement of the annotated versions of *Psychological Abstracts* in 1968. As well, the following journals were searched by hand from volume one to present: *Human Movement Science, Journal of Experimental Psychology, Journal of Human Movement Studies, Journal of Motor Behavior, Perceptual and Motor Skills,* and *Research Quarterly for Exercise and Sport.* The references cited in each retrieved article were then cross-checked for omissions. One important limitation of this review was the decision not to include technical reports. This decision was made based upon the fact that many of these reports were later published in journals, and also because many were not easily accessible.

Separating Motor and Verbal Learning Studies. The tasks used in the distribution of practice literature cover a very wide range. Our review is focused on motor, not verbal tasks. However, the distinction between motor and verbal skills is perhaps best classified in terms of a task continuum from high verbal, low motor components at one end to low verbal, high motor components at the other end (Underwood, 1949, pp. 398-407). Indeed, this distinction becomes even further clouded if one considers the arguments that learning motor skills involves a progression from a highly verbal, or cognitive stage to a more motor and autonomous stage with practice (e.g., Adams, 1971; Fitts, 1964). In this review we adopted a liberal interpretation of tasks involving the learning of a motor skill and included research that used "semi-verbal tasks" (Underwood, 1949) such as stylus mazes, inverted alphabet printing, and mirror tracing, as well as "pure motor tasks" (Underwood, 1949) such as pursuit tracking, balancing, and climbing tasks. Tasks that were excluded from the review were those where the motor component was deemed to serve only a perfunctory role, such as speaking or writing a word in a paired-associate learning experiment.

Studies Included in the Meta-Analysis. A total of 116 studies were retrieved that included a contrast of distribution of practice effects as a part of the experimental design. Not all of these studies could be used in the meta-analysis, however. In order to conduct the meta-analysis, certain criteria were established with the intent of providing as meaningful a contrast as possible, given the very wide range of experimental designs.

The first criterion was an empirical definition of "massed" and "distributed" practice. Our examination of current reviews of this literature revealed that the terms massed and distributed often were operationally defined in terms of the amount of rest during the inter-trial interval. Singer (1980), for instance, defined massed practice rather narrowly as practice "without any intermittent pauses" (p. 419). Schmidt (1982) provided a wider definition of massing where "the amount of practice time in a trial is greater than the amount of rest between trials" (p. 482). In contrast, distributed practice is defined in terms of rest intervals that are relatively longer than under massing conditions (e.g., Magill, 1985). Our review of the research revealed that in many studies, there were more than just two groups under comparison. For instance, in the Bourne and Archer (1956) study discussed previously there were five distribution groups, defined in terms of the length of the inter-trial interval (0, 15, 30, 45, and 60 s intervals). For Bourne and Archer, distribution of practice was treated operationally in terms of a massed to distributed continuum. Many other studies provided a similar dilemma for defining massed and distributed practice groups. However, our meta-analysis required that a comparison be drawn between one massed group and one distributed group in order to calculate an effect size. We therefore decided that given a study where three or more levels of distribution were contrasted, the massed group would be represented by the condition with the smallest inter-trial interval and the distributed group would be represented by the condition with the largest inter-trial interval.

The decision to define distribution conditions in terms of the length of the inter-trial interval provided an empirical solution for some but not all of the problems. What could not be reconciled under this definition were instances where the same distribution parameters were provided within a practice session, and time between sessions was the variable that separated distribution groups (e.g., Abrams & Grice, 1976). Another problem was encountered with studies that did not maintain constant inter-trial intervals (e.g., Doré & Hilgard, 1938). A final problem also existed with studies that defined massing in terms of length of the trial, keeping inter-trial interval constant (e.g., Marteniuk & Carron, 1970). Our decision was to calculate effect sizes for studies that provided massing conditions within a single practice session (some distributed conditions provided inter-trial intervals as long as 24 hrs). Further, both the massed and distributed groups were defined in terms of constant inter-trial intervals, with the "massed group" being the condition of shortest interval and the "distributed group" having the longest interval. A discussion of the literature that did not conform to these criteria will follow the presentation of the meta-analysis.

Meta-Analysis

Calculating Effect Sizes. Glass, McGraw, and Smith (1981) provide details of the minimal statistics required in order to calculate effect sizes from research articles. Of the 116 articles that we retrieved, 40 could not be used because the necessary information for the calculation of effect sizes was not reported. Effect sizes for a further 29 articles were also not calculated due to a failure to meet our design criteria discussed previously. From the remaining 47 articles effect sizes were calculated as the standard score differences between the masses and distributed groups at two points in the experiment (where possible): at the end of acquisition and either on the retention test or the first trial(s) of the transfer test. From these 47 articles a total of 52 effect sizes were calculated at the end of acquisition (some articles reported more than one experiment). Nearly half of the studies ($n = 21$) provided data for the calculation of effect sizes for retention (23 studies did not include a retention test and three studies did not provide the necessary statistics to calculate the retention effect sizes). A list of the studies that were used in this initial stage is presented in Appendix A. In parentheses after each reference is a letter that denotes which effect sizes were calculated (A = end of acquisition, R = retention).

Of the 52 effect sizes calculated at the end of acquisition the largest number were from studies that involved learning a tracking task ($n = 22$). Eight studies provided effect sizes for inverted alphabet printing tasks and another six studies involved mirror tracing tasks. The remaining 16 studies used the following tasks (n's in parentheses): Tsai-Partington numbers task (3), card sorting (1), rudder control (1), Bachman ladder (2), maze tracing (2), Minnesota rate of manipulation task (1), selective mathometer (2), novel basketball shooting (1), stabilometer (1), and the Mashburn apparatus (2).

Of the 21 effect sizes calculated for retention, 11 were tracking studies, and three used inverted printing tasks. The remaining seven effect sizes were from different tasks.

Results. Based on recent conclusions about the effect of distribution of practice on motor performance, the clear prediction was that a large, positive average effect size would be found at the end of acquisition. To assess this prediction we followed the steps regarding the statistical analysis of effect sizes suggested by Thomas and French (1986), which was based largely on the methods devised by Hedges (e.g., Hedges & Olkin, 1985). Effect sizes were calculated such that better performance for the distributed group resulted in a positive value (which was the case for every effect size in both acquisition and retention). A weighted estimate of the pooled standard deviation was used as the denominator in these calculations. The effect sizes were then corrected for sample size (hereafter these effect sizes are denoted as ES), and estimates of the variance for each ES were determined.

Using formula (5) in Thomas and French (1986), the weighted mean ES was found to be .96. According to Thomas and French (1985), an ES of this magnitude is considered to be "large." However, a plot of the distribution of these effect sizes revealed a slight negative skewness. A subsequent test for homogeneity resulted in a significant *H* statistic (χ^2 (51) = 136.13, p>.95), indicating that the group of ES was not homogeneous. An outlier test using the standardized residuals from the weighted mean identified four ES that were outside 95% of the distribution of ES [Estes, 1950 (ES = 3.08); Lorge, 1930 (ES = 9.69); Stelmach, 1969 (ES = 5.16 — Bachman Ladder expt.); Wild & Payne, 1983 (ES = 6.04)]. Removal of these outliers resulted in a less negatively skewed distribution as well as a nonsignificant *H* test [χ^2 (47) = 11.98, p<.95], indicating that the remaining ES were homogeneous. The weighted mean of these acquisition ES without the inclusion of the outliers remained large (.91). The standard deviation about this weighted mean was .50.

As discussed earlier, the conclusions about the effects of distribution of practice on learning are various, although many argue that there is no effect, or that the effect is quite small. Our analysis showed that there is a strong learning effect. The weighted mean ES for absolute retention on the initial calculation was found to be .53. This mean may be considered to fall in the "medium" ES range (Thomas & French, 1985). The test for homogeneity was not significant (χ^2 (20) = 5.46, p<.95). Once again though, a scatter plot of the individual ES indicated a slight negative skewness. An outlier test revealed two ES outside 95% of the distribution [Estes, 1950 (ES = 1.79); Pubols, 1960 (ES = 1.79)]. The resultant weighted mean ES based on the absolute retention scores was .49. The standard deviation was .35.

To summarize the results of the meta-analysis, the advantage of distributed over massed practice on performance at the end of practice is consistently large over experiments. A consistent advantage of distributed practice over massed practice was also found on performance after a retention interval, although the advantage was not quite as large as for the end of acquisition. Thus, our conclusion from the meta-analysis is that distributed practice is beneficial to both the performance and learning of motor skills, although the effect on performance is greater than the effect on learning.

Other Factors

Due to the limiting nature of meta-analyses we felt it necessary to review other aspects of the literature on distribution of practice that could not be reduced to effect sizes. The remainder of this section is a descriptive review of this literature.

Final Observations on the Meta-Analysis. Of the studies from which acquisition effect sizes were calculated there were three that also included retention trials, but for which effect sizes could not be determined. For each one of these studies a large ES at acquisition emerged. However, an examination of the data as plotted in their figures also reveals that a difference, albeit reduced, still remained after the retention interval (Archer, 1954, Figure 1; Stelmach, 1969, Figures 1 and 2; Wasserman, 1951, Figures 1 and 2). This observation is consistent with the results of the meta-analysis suggesting that the performance advantage for distributed practice conditions is reduced but still quite evident following a rest period.

Thomas and French (1986) suggested that a further examination of outlier studies helps to better understand the overall nature of the meta-analysis. However, a closer look at the five studies that resulted in outlier effect sizes here produced no remarkable observations. It should be noted however, that each effect size that was found to be an outlier shared the same characteristic — they were all at the same tail of the distribution. The four acquisition effect sizes and the two retention effect sizes that were found to be outliers were due to the difference between the massed and distributed groups being much larger than the average of the other studies included in the meta-analysis. Thus, our estimate of the average performance and learning effects was more conservative after removal of these outliers.

Studies for Which Effect Sizes Were Not Calculated. One obvious concern regarding meta-analyses is the possibility of a biased sample. Studies that were relevant to the meta-analysis but did not report the statistics needed to calculate effect sizes were further examined. In many studies there appeared to be rather large effects on absolute retention in favor of distributed practice conditions (e.g., Ammons, 1952; Drowatzky, 1970; Hagman & Rose, 1983; Schucker, Stevens, & Ellis, 1953; Whitley, 1970). However, one notable exception is an experiment by Reynolds (1952). In his study, Reynolds compared the acquisition of a gross motor balance task where the subject was required to learn to make periodic adjustments in posture while balancing on a platform. Both massed and distributed groups were required to make 80 adjustments of posture during the acquisition trials. The massed group performed all 80 during continuous practice. The distributed group received 1-min rests following adjustments 5 and 10, then 2-min rests following adjustments 20, 30, 40, 50, 60, and 70. Both groups were given a retention test following a 25-min retention interval. As expected, the distributed group performed better at the end of acquisition than the massed group. However, the absolute retention results (as displayed in their Figure 3) revealed a small but clear advantage in favor of the massed group. This study represents one of the few instances where massed practice resulted in better learning than distributed practice.

The Persistence of Learning Effects Under Common Transfer Conditions. Given the relatively strong evidence in favor of absolute retention (learning) effects, how long do distributed practice conditions continue to show better performance than massed conditions when the two perform under common transfer conditions? A study by Denny, Frisbey, and Weaver (1955) is important in this regard. In their study, Denny et al. had two groups of subjects practice for 20 trials (of 30 s lengths) separated by either 0 or 30 s of rest. A 5-min retention interval followed this acquisition phase. At the beginning of retention, each group was split in half and either continued to perform under the same practice schedule or under the other schedule (i. e., a double transfer design). Their findings are reproduced in Figure 2.

The results of Denny and colleagues show very strong performance and learning effects in favor of initially distributed practice conditions. Under common distributed transfer conditions, the initially distributed group ("D-D") showed a continued advantage for about 11 trials, compared to the initially massed group ("M-D"). Under common massed transfer conditions, the initially distributed group ("D-M") also showed a persistent advantage (cf. "M-M"), although for only about six trials. These findings have been replicated to about the same extent by Adams and Reynolds (1954) and Digman (1956) for distributed transfer conditions and by Ammons (1950) and Bourne and Archer (1956) under massed transfer conditions. The learning advantage then, appears to be subject to a convergence in performance under continued, common transfer trials.

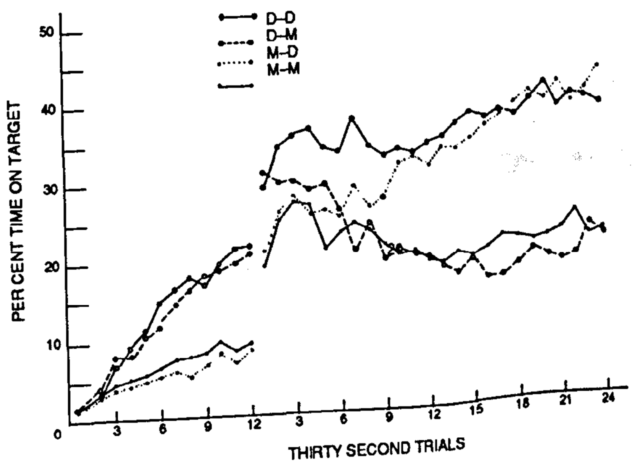

Figure 2—From "Rotary pursuit performance under alternate conditions of distributed and massed practice" by M. R. Denny, N. Frisbey and J. Weaver Jr., 1955, *Journal of Experimental Psychology, 49*, p. 49. Copyright 1955 by the APA. Reprinted by permission. See text for details.

Effects of Different Trial Lengths. In some studies of distributed practice effects the primary variable of interest was the trial length, given constant inter-trial intervals. For example, Kimble and Bilodeau (1949) compared two groups that receive 10-s inter-trial intervals with the "massed" group practicing the Minnesota Rate of Manipulation Task for 30 s per trial and the "distributed" group practicing for only 10 s per trial. They also compared two groups that received 30-s inter-trial intervals with massed and distributed conditions represented by 30- and 10-s trial lengths, respectively. Their results revealed that longer inter-trial intervals produced better acquisition performance. More importantly though, given constant intervals, shorter trial lengths also resulted in better acquisition performance. No retention tests were provided. These findings were replicated later by Barch (1959) and extended to tracking tasks by Hagan, Wilkerson and Noble (1980). A study by Marteniuk and Carron (1970) however, found opposite results for learning a tracking task. Marteniuk and Carron found a slight performance advantage for 40-s trial lengths compared to 20-s trial lengths (given constant 20-s inter-trial intervals) as well as a large learning effect one day later in favor of the "massed" group.

Effects of Constant Inter-trial Interval:Trial Length Ratios. Given 20 min of total time in practicing a tracking task, Plutchik and Petti (1964) found no differences at the end of acquisition for inter-trial interval : trial length ratios of .5. Similarly, large differences were also absent given ratios of 1 on a tracking task in a study by Noble, Salazar, Skelley, & Wilkerson (1979). They did find however, that a 20 s : 20 s (rest:work) ratio was poorer than a 60:60 ratio, although ratios of 90:90 and 120:120 produced intermediate results.

Effects of Increasing and Decreasing Rest Periods. Snoddy (1935) hypothesized that learning involved two opposed processes. Early in practice, learning should be facilitated by relatively long rest periods whereas later in practice, learning is most beneficial under short rest periods. Moderate support for the hypothesis was found by Renshaw and Schwarzbek (1938). Two groups performed acquisition trials on a tracking task under either increasing or decreasing periods of rest. Each group performed five blocks of seven trials. The increasing rest group received inter-trial intervals of 0, 1, 3, 5, and 9 min in each of the successive blocks. The decreasing rest group received the opposite pattern (9, 5, 3, 1, and 0 min). Their results favored the decreasing group early in practice (as expected) and the increasing group (although by a much smaller difference) at the end of practice. In contrast, Doré and Hilgard (1938) found that an increasing rest group produced a *larger* rather than a smaller difference at the end of practice relative to decreasing rest periods. Unfortunately, neither study provided a retention test, so the effects on learning are unknown. However, it appears that changing rest periods alters performance in a manner that favors longer inter-trial intervals, regardless of the schedule of change (McGeoch & Irion, 1952).

Distribution of Practice Effects for Discrete Tasks. Continuous tasks are typically classified in terms of prolonged time spent on the task, whereas discrete tasks are considered to be relatively rapid from initiation to completion (Schmidt, 1982). Virtually all of the literature considered to this point has examined the learning of a continuous motor skill. Indeed, our search of the literature has revealed only one study that examined distribution of practice effects using a discrete task. This study (Carron, 1969) involved the learning of a peg turn task. The task was to pick up a small dowel, turn it upside down, and reinsert the dowel into a small hole. The goal was to perform the task as fast as possible. One turn equalled one trial, so the trial length was approximately 1,300 to 1,700 ms. Carron defined distributed practice as 5 s between trials and massed practice as 300 ms between trials (or as close to 300 ms as possible). Although he did not report relevant statistics, a reexamination of Carron's data by Schmidt (1982, p. 485) revealed some interesting findings. In marked contrast to the literature, massed practice did not depress acquisition performance. Moreover, the massed condition resulted in moderately better learning, as measured on a retention test 2 days later. We (Lee & Genovese, 1988) have replicated and extended Carron's findings using a task of even shorter duration (500 ms). A tentative conclusion then, is that distributing practice on discrete tasks results in performance and learning effects that are quite different from the effects seen for continuous tasks.

Future Considerations

The present analysis revealed three important findings related to the effects of distribution of practice on motor skill acquisition. First, distributed practice was found to enhance performance (as measured at the end of acquisition). This finding is consistent with virtually every conclusion on the topic. Second, the effects of distributed practice were larger on performance than on the first trial(s) of retention. This finding is also consistent with most conclusions. Third, distributed practice conditions resulted in better learning than massed practice conditions (as measured by absolute retention). This finding is quite different than most conclusions that have been made recently (cf. Adams, 1987; Magill, 1985; Schmidt, 1975; Singer, 1980).

That retention is benefitted by distributed practice may not be too surprising. Other evidence in motor learning research has demonstrated effects that may reveal some commonalities. The most closely related practice schedule effect is the so-called "contextual interference" effect (Shea & Morgan, 1979). This practice schedule effect is observed when subjects are required to learn several variations of a motor task. For example, in Shea and Morgan's study, one group of subjects learned three different spatial patterns under a drill-type schedule ("blocked" practice), whereby all learning trials on one pattern were completed before practice on another pattern was undertaken. A second group of subjects learned all three patterns at once ("random" practice), in which trials on the task variations were conducted in an unsystematic order. In a sense, a blocked schedule is similar to a massed schedule since practice on any single variation of the task is conducted on consecutive trials. Further, a random schedule may be considered like a distributed schedule since practice trials on any one task do not occur in close proximity in time. The similarity is further extended when considered in terms of retention effects: random and distributed schedules facilitate retention relative to blocked and massed schedules, respectively.

What makes the comparison of contextual interference and distribution of practice effects more interesting however, are the *dissimilarities.* The most compelling dissimilarity is the effect that is seen during acquisition trials. Distributed practice conditions facilitate performance whereas random practice conditions are detrimental to performance. This difference in performance effects reveals a critical distinction for the role of "spacing" in motor skill learning as a function of the type of task to be learned. Recall that the majority of distribution of practice studies used continuous tasks. Most studies of contextual interference though, used discrete tasks. Indeed, the two distribution of practice studies that used discrete tasks revealed performance effects that were quite different than for continuous tasks (cf. Carron, 1969; Lee & Genovese, 1988). Similarly, two contextual interference experiments that used a continuous task (the pursuit rotor) failed to show the typical difference between blocked and random practice conditions (Lee & Magill, 1981; Whitehurst & Del Rey, 1983).

This apparent interaction between the spacing effect and task type on acquisition performance suggests that the similarity in retention effects between distribution of practice and contextual interference studies may only be a superficial similarity. We believe that this is due to the nature of how the information from these tasks is processed during acquisition trials. Since discrete tasks are usually very short in duration, information about performance is evaluated after the completion of the movement. The most important role of the inter-trial interval for discrete tasks appears to be in terms of this evaluation process (Salmoni, Schmidt, & Walter, 1984). However, since continuous tasks are much longer in duration, information is received and evaluated as an ongoing process during movement. It is likely that the inter-trial interval plays a less critical role in terms of evaluation processes for continuous tasks as compared to discrete tasks. Perhaps the role of the inter-trial interval for continuous tasks may be more related to various non-information processing type activities, such as those suggested by McGeoch and Irion (1952). Indeed, this hypothesis suggests that contextual interference and distribution of practice effects are types of phenomena where the role of spacing practice is particular to the nature of the information processing constraints of the task to be learned.

This emphasis on the nature of information processing activities as constrained by the nature of the task, and as affected by manipulations of the inter-trial interval, provides a fruitful basis for future research. Perhaps the wealth of knowledge on distribution of practice effects, which has been all but forgotten in recent years, will serve an important role in this future.

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Appendix A — Effect Size Studies

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