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Distribution of Practice in Motor Skill Acquisition: A Few Questions and Comments

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It has been a pleasure to read and prepare a comment on this excellent review paper by Lee and Genovese. As an editor, I am always impressed with good organization, perceptive selection of material, and crisp clear presentation. It is not my purpose to criticize but, hopefully, to present a few thought-provoking ideas. The amount of material included in this review is staggering. I therefore have chosen to confine myself to a few small pieces of relatively stable intellectual ground.

Relatively Specific Comments About the Review

1. The authors have clearly and restrictively defined their most important terms. The reader must remember that "performance" refers to scores at the end of the practice period, and that "learning" is measured as scores on the first trial or few trials after rest.

2. The authors were wise to restrict themselves to a task which can be carried out, and has significance. In preparing a "complete" bibliography on rotary pursuit and related tracking papers, we have found over 600 studies seemingly worth intensive study, of which we estimate 200 to 500 to contain information significant for better understanding of distribution-of-practice phenomena. The amount of information is overwhelming. With respect to rotary pursuit and distribution of practice I feel in the position described by John A. McGeoch in a seminar in 1940. He was just completing his book on human learning and must have approached burn-out. Very simply, he believed that he was the last scientist who would be able to study and master all literature in the field of human learning. I have seen no reason to question this, and am not at all sure that we can even get all the significant literature on distribution of practice with skills by the tail before it takes off at Warp 5 or 10. A whole new approach based on construction of the best theories we can produce seems in order.

3. The authors seem to reify the term "learning." Consider the phrase "true learning." That this language use may represent a significant point of view is suggested by their use of the phrase "absolute retention." Unfortunately, the adoption of terminology such as this may partially blind the user to some important research considerations. There really isn't one kind of learning that is true, and many that are false. Some readers will remember Tolman's paper discussing "the many sorts of learning." Even this formulation is defective, since we must recognize a variety of continua forming a space within which we design our experiments, collect our data, and construct our theories. Perhaps we should ban the concept of learning and confine ourselves to finding what variables at what levels lead to how much resistance to change after what prior experiences. Well-developed information of this sort might give us a sufficient basis for precise and comprehensive theory. This does seem radical. Can a task of this magnitude be carried out, and would it be worth the effort? A much more modest approach would certainly lead to a more interesting game!

4. We must separate the phenomena based on equipment, procedural, and experimental variables about which we already know a good deal in relation to "performance" and "learning." Nonsystematic variations in procedures are frequently mentioned in the review. Individual-differences and equipment variables are not. In defense of the authors, I believe inclusion of these variables in such a review is impossible from a practical point of view. However, in defense of future readers, I feel compelled to point out that such variables as size of target, rate and pattern of movement of target, and individual-difference variables interacting with sequences of experience have been shown to affect "performance" and "learning." Researchers often fail to report the simplest information about the physical char-

acteristics of their equipment. They didn't 30 years ago and they don't now.

5. Failure to utilize what is already known about response variables can lead to questionable results. In Figure 1 (Bourne-Archer curves) of the review, one can see that the shapes of the postrest curves are different, and therefore that the processes are different. What to do about the "learning" measure in the face of systematic differences in amount of warming up? It is also now well-known that the prerest decremental factor takes at least 10 to 20 min to disappear fairly completely. With a 5-min rest, you still have prerest decrement present, greater amounts with greater prerest massing of practice. So, we have an interaction of work decrement with "learning." How to measure "learning" uncomplicated by varying amounts of prerest work decrement still present?

6. Distribution-of-practice research has not "virtually stopped." Lots of work is being done with the pursuit rotor. In part, publication is slow or nonexistent because of the extreme complexity of the phenomena underlying those commonly studied in the past. A small example comes to mind from our own laboratory. One might think that, as the duration of trials is decreased, the shape of the given intratrial curve would simply be that of the corresponding portion of curves for trials of longer duration. This is simply not the case. In fact as we decreased trial durations below 20 s and correspondingly decreased rests, we found large and unpredictable variations in slope of intratrial curves. The complexity of the changes has puzzled us to distraction.

7. In (2.) I mentioned that rotary pursuit and closely related literature runs into the hundreds of items. It seems only fair to turn around and make suggestions as to specific research programs the interested reader might wish to look into. My first suggestion is to look up all publications by R. B. Payne and colleagues, and watch for new papers. Payne's work is solid and often quite original. Several papers are listed in the present review article. My second suggestion is to do one's best to run down every paper and book in which Hans Eysenck and colleagues have published work on rotary pursuit and related skills. There must be good and sufficient reasons for including none of Eysenck's papers in the present review, but readers will need to read these papers if they expect to understand the field. Although Eysenck and I have agreed about few aspects of distribution of practice, I feel impelled to call out, "Where are you Hans? The field would not be the same without your work!"

8. The meta-analyses of "performance" and of "learning" are well-presented and seem to have the usual strengths and weaknesses. Assuming that you wish to accept the assigned meanings of "performance" and "learning," it is well established by the analyses that performance level is inversely affected by degree of massing, as is learning. Further, the effect of distribution of practice on learning is much smaller than the effect on performance. Presumably, these findings should make writing of books and review articles less difficult and statements about effects of distribution of practice less controversial. It is quite useful to have such clear and clean examples of meta-analysis in the literature.

Personally, I have some misgivings about the use of meta-analyses in this review and in general. My misgivings are not logical but are to some extent feelings about research strategy. First, I would have bought the conclusions anyway, simply on the basis of published research. So what have the meta-analyses done for me? Second, the meta-analyses seem to have given me a false sense of security about the generality of the effects and their size. I have to shake off the belief that effects of distribution of practice can be found in performance and learning of any skill. Third, the meta-analyses have deflected my attention from the real research problems, which are determining what levels of which variables affect performance and learning in what ways. Fourth, I may forget that "performance" and "learning" have very special, restricted meanings in this situation. I could even end up applying my findings in gloriously general ways.

In fairness to meta-analysis, I must say that none of the above outcomes can logically be blamed on it. Unfortunately, like other exciting new techniques, meta-analysis seems to have the above effects on many who come into contact with it. Sadly, it is no substitute for large amounts of perceptively planned, systematically conceived, time-and-effort-consuming research.

9. Lee and Genovese include an excellent section toward the end of their paper. "Other Factors" presents a descriptive review and often a qualitative analysis of some results which don't reduce to "effect sizes." Such a section should appear in every meta-analysis paper, stretching the thinking of the authors, and reassuring us that they don't really believe everything can be reduced to a number and forced into a distribution or scale. I found this relatively short part of the paper as interesting as all the rest put together.

General Thoughts Stimulated By and Related To the Review

Related Phenomena. Although they are not necessarily associated with effects of distribution of practice, there are various phenomena with which the serious researcher may wish to become acquainted. They have been felt to have similarities to skills distribution phenomena that should not be neglected.

Stabilized retinal image. When images can be held in the same position on the retina by various devices, these images literally disappear as a function of duration of exposure and appear again later after "rest" away from the image and apparatus. The effect is startling. Something like this happens when one practices a task continuously. Even partial disappearance or distortion of the stimulus could produce a performance decrement. Distortion of response-produced cues could have a deleterious effect on the production of a skilled response. Sensory adaptation could have the same sorts of effects.

Reversible perspective. We included a self-administered lab task for home-study students on remote ranches. Students established some control over reversals of a Necker cube, then practiced 15 min a day for 5 days at speeding up the reversals. Typically within the first or second session reports came that the cube was breaking down into parts, that it took effort to keep it together as a unitary experience of a cube, as well as to speed up its reversals. Even partial disintegration of the cube was often accompanied by an experience of nausea. We felt we had clear evidence that organization and maintenance of the figure took energy (effort), and disintegration of a part of the individual's perceptual world led to actual physiological upset. Clearly, the perceptual organization of a patterned skill will take energy, will partially disintegrate with continuing performance, and may well be accompanied by physiological disturbance.

Where Can Distribution-of-Practice Research Go?

Hoping that the reader will indulge me, I would like finally to mention briefly a number of issues not falling clearly into the earlier sections, but seemingly of considerable importance to the conduct of distribution-of-practice research.

Note 1: Conceptualize variables as continuous. Several currently used variables are not satisfactory.

Note 2: Develop more nearly continuous micromeasures of important aspects of skilled behavior. Use high-resolution video cameras and display monitors with single-frame and slow-motion, accompanied by direct computer recording of data.

Note 3: Improve computer skills to make possible analysis of patterns in space and time. Unfortunately, we are faced with quite unfriendly computer systems and frequent computer changes while we are attempting to concentrate on intelligent design of studies and collection of data.

Note 4: Develop and maintain our mathematical skills. We must convince ourselves and our students that we need at least as much general proficiency in mathematics as do physicists.

Note 5: Address problems of computer search and retrieval. Working with closed data systems and with static procedures we have been spoiled rotten. Long-term bibliographic work demands rigorous general theories of behavior and measures we haven't even dreamed of. We are faced with analysis of constantly changing and redefined concepts in universes without boundaries.

Note 6: Learn to construct segments of rigorous theories for our own use. Remember, as do Lee and Genovese quite well, to define terms so that appropriate precision of identification and measurement are achieved. Don't let equipment determine the variables you wish to study!

Note 7: Seriously consider intensive study of the single case, i. e., an idiographic approach. In our laboratory we have found repeatedly that combination of data across subjects or across time obscures processes fatally or nearly so.

Note 8: Look for new design approaches, to open closed and even unimagined avenues of research. Our work[1] with "alternating" designs, with cross-limb transfer, and with graphic representations of interactions of complex processes has proved enormously stimulating of new ideas for us.

Note 9: Obtain subjective reports by trained subjects whenever possible. The experimenter should always be a subject, usually the first. At all stages of research, the experimenter must be empathetic, sensitive, and perceptive to obtain the most valuable information.

Note 10: Report your results, even though you are puzzled. Clearly indicate the uncertainties from your point of view.

Note 11: Learn to approach your science as a game. You don't have to win or lose a game to enjoy it thoroughly.

Footnote

[1]All through these comments, I mention work in progress or completed, but not published in full. Rather than try to set up an inappropriate reference section, let me invite anyone interested in a specific item to write or telephone me so I can supply appropriate information if it is available.

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