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The Limitations of Generalization Based on Restricted Information

ROBERT W. CHRISTINA and JOHN B. SHEA
The Pennsylvania State University

Lee and Genovese (1988) provided yet another attempt to reduce the complexities of psychological phenomena to some restricted generalization (i. e., that massed practice depresses performance as well as learning), as well as single dependent measure (i. e., the absolute retention score). We argue that this generalization-seeking approach to research is inadequate from the perspective of knowledge representation being dynamic and multidimensional. Theoretical as well as empirical criticisms which we raise emphasize that this perspective is at odds with the underlying pretheoretical assumptions motivating Lee and Genovese. We point out (a) that generalization-seeking research is counter to understanding psychological processes underlying motor learning and suggest that result-centered research strategies (Greenwald, Pratkanis, Leippe, & Baumgardner, 1986) offer a more promising alternative; (b) that the use of a retention and/or transfer design to measure learning confounds an assessment of what was learned with forgetting across the retention interval; (c) that the scores evaluated by Lee and Genovese are informative measures, each of which provides an assessment of a different aspect of performance; and (d) that absolute retention measures are no less likely to reflect temporary performance factors, as well as ceiling and floor effects, than relative retention scores. Finally, we emphasize that it is the responsibility of the investigators to provide a knowledgeable evaluation and interpretation of the pattern of their findings.

Generalization Versus Understanding

Numerous researchers have voiced their concerns that the pursuit of parsimony in the behavioral sciences through generalization-seeking research strategies is a fruitless and counterproductive endeavor. In proclaiming the death of the "laws of learning," McKeachie (1974) pointed out that no simple answers concerning human behavior were likely to be forthcoming. Jenkins (1979) has proposed that at least four components in any learning experiment must be considered before any generalization from experimental findings can be made. These components are the subjects, the instructions, the task(s) practiced, and the criterion task on which the subject is tested. Because the relationship among any number of these components can be falsified or even reversed by changes in one or more of the remaining components, parsimonious conclusions concerning learning phenomena are unlikely. It is very likely that "unidentified interactions" (Cronbach, 1975) among the levels of the four components have led to the inconsistencies in the distribution of practice literature alluded to by Lee and Genovese. A cursory review of the literature is sufficient to realize that distribution of practice effects are modifiable by a range of variables such as length of rest period (i. e., the retention interval) and the amount of prerest practice (Adams & Reynolds, 1954). This point is implicitly acknowledged by Lee and Genovese when they explain their reasons for including only "motor" tasks in their meta-analysis. Yet decisions such as this when based on a surface, or structural, level of task analysis are insensitive to the deeper psychological processes that are controlling performance. These processes might be used across task boundaries and not be represented by the surface structure of a particular task. A similar point to this has recently been made by Frith and Eysenck (1981) when they argued that a valid test of underlying psychological constructs should use criterion tasks equally dependent on the same constructs for performance. They went on to contrast the pursuit rotor as a task not necessitating the learning of "visuomotor coordination" with mirror drawing as a task that does require this kind of learning. Yet both of these tasks were included in the meta-analysis by Lee and Genovese. The point of this is

that the limitation of admissible evidence in the inquiry process as necessitated by meta-analysis is not only counter to the generalization seeking approach, but also to one in which understanding psychological processes through systematic analysis is emphasized.

Two alternatives to the generalization-seeking approach have been proposed by Greenwald et al. (1986). The first of these methods is referred to as the "method of condition-seeking" and consists of deliberately reducing the generalizability of an existing finding. This method seeks to identify conditions under which an experimental finding occurs. The correspondence of conclusions based on this approach to those based on the generalization-seeking approach is the same as that between statistical main and interaction effects. The second method is referred to as the "design approach." It is used to specify conditions that can produce a presently unobtainable result, or to produce reversals of familiar theory-based findings. The condition-seeking approach reduces the generality of a finding while the design approach increases the generality of a finding. These methods constitute what Greenwald et al. (1986) call result-centered research strategies. The choice between the generalization seeking and result-centered research strategies could influence the nature of future motor learning/control research. The generalization-seeking approach will very likely lead to explanations based on unitary constructs and the use of a limited range of research paradigms, whereas result-centered research strategies will provide the basis for a more active research community with coexisting multiple hypotheses and the use of a larger range of research paradigms.

Generalization seeking should perhaps be confined to the theoretical level rather than to the empirical level. But it is the almost total neglect of any theoretical constructs that is the greatest shortcoming of the Lee and Genovese review. Aside from its important implications for application, the study of distribution of practice has had a theoretical importance in the study of reactive inhibition (Hull, 1943, 1952) and temporary work decrement (Ammons, 1947). These constructs are thought to temporarily depress performance. Reminiscence, or the spontaneous recovery from these temporary performance factors, offers a good explanation for the distribution of practice effect. While reminiscence is never explicitly mentioned by Lee and Genovese, it is still the subject of controversy among some researchers (Black & Payne, 1987; Frith & Eysenck, 1981). Lee and Genovese have chosen to present only one explanation for the distributed practice effect, and this is the "contextual interference effect." It is disconcerting to see the use of one experimental effect to explain another different effect, because such an explanation is concentrated at the empirical level. There is no discussion of possible underlying psychological constructs responsible for contextual interference effects that might provide an explanation for distribution of practice findings. At present the issue of whether a forgetting (Lee & Magill, 1985) or elaboration hypothesis (Shea & Zimny, 1983; 1988) provides the best account for the contextual interference effect has not been resolved. One might seriously question the wisdom of carrying unresolved theoretical issues in one area over to another area. At the very least, these issues should be resolved at the local level before they are generalized to other research areas. Cross topic generalization should be based on similarities rather than dissimilarities, and in the this respect Lee and Genovese report that the dissimilarities are more interesting than the similarities between contextual interference and distribution of practice effects. One important dissimilarity between these areas not mentioned by Lee and Genovese is that distribution of practice studies have involved the learning of a single task, while contextual interference studies have involved multiple task learning. From a multiple task learning perspective it is possible to view the random practice schedule as a massed practice condition. That is, the learner is exposed to all the tasks in close proximity. The blocked practice schedule can be viewed as a distributed practice condition in which practice on one task is completed before another task is first introduced, although the more traditional massing of practice condition is approximated for the individual tasks in the blocked practice schedule. From a multiple task perspective, then, massing of practice (i.e., the random practice schedule) clearly leads to better retention than distributed practice in the contextual interference experiments. This, of course, is just the opposite of the conclusion (generalization) made by Lee and Genovese.

Unconfounding Performance and Learning Variables

Lee and Genovese advocate the use of a retention test and transfer design to separate temporary performance variables from more permanent learning variables. The use of a retention test to assess learning confounds the amount learned during acquisition trials and forgetting across the retention interval. However, this procedure does not pose a problem for a definition that emphasizes learning as a *relatively permanent* change in the capability for responding (Magill, 1985, p. 24; Oxendine, 1984, p. 8; Sage, 1984, p. 16; Schmidt, 1988, p. 346; Singer, 1980, p. 9). This response-oriented definition represents a static view of memory in which response change is

emphasized as the dependent variable of interest. This perspective is quite different from more recent views which emphasize a dynamic view of memory incorporating changes in both the representational structure for a task and its closely associated strategic processing (Shea & Zimny, 1983; Zanone & Hauert, 1987).

Theorists who take a dynamic view of memory recognize representational structure as a complex integration of diverse types of knowledge (MacKenzie & Marteniuk, 1985; Shea, Hunt, & Zimny, 1985), some of which are more permanent than others. In fact, the more permanent components of a representational structure might be derived from temporary types of knowledge. The development of a comprehensive account of learning requires the study of less as well as more permanent types of knowledge comprising the memory representation for a task. The restrictions of not being able to study the course of change in the representational structure across acquisition trials (i. e., learning) or the status of this representation immediately following acquisition trials (i. e., immediate retention) imposed by the performance-learning paradigm described by Lee and Genovese (see also Sage, 1984, p. 37; Schmidt, 1988, pp. 359-362) are counterproductive from this perspective.

We think that researchers should be sensitive to variables that are unrelated to learning and which might influence their findings, but concern for temporary performance variables and the use of associated experimental paradigms should not influence the definition of hypothetical constructs. This is exemplified by the tendency of some researchers to refer to variables which influence performance on acquisition trials as "performance variables" and those which influence performance on retention trials as "learning variables." However, the processes responsible for learning occur during acquisition trials, and the retention test provides a measure of forgetting during the retention interval. Inferences concerning learning and forgetting processes should be made through an integrative description of performance across acquisition and retention trials. This point was recently emphasized by Burford, Lee, and Elliott (1987) when they concluded that "retention effects may not always be indicative of learning effects" (p. 54).

Do Absolute and Relative Retention Scores Lead to Divergent Conclusions?

Using the Bourne and Archer (1956) study as being typical of the other studies reviewed, Lee and Genovese attempted to demonstrate how conflicting results can occur when different measures are used. Specifically, they show that longer intertrial intervals resulted in higher absolute retention scores and lower relative retention scores, and then argue that interpretations of these opposing results lead to divergent conclusions about the effect of practice distribution on learning. We argue that the results appear to be in conflict and lead to divergent conclusions because Lee and Genovese compared scores reflecting different aspects of performance without a thorough understanding of the nature of these differences. They compared scores that assess different aspects of performance in order to determine which practice distribution produced "better" learning. We can only guess what they mean by the term "better" learning because they did not define it, but it appears to refer to the highest level of performance retained immediately after the retention interval. Well, of the three scores compared, only the absolute retention score is capable of reflecting that aspect of performance. It is as unreasonable for Lee and Genovese to expect the two relative retention scores to reflect that aspect of performance as it is for them to expect the absolute retention score to reflect the aspects of the two relative retention scores. They simply failed to recognize that the three scores assess different aspects of performance. The problem here is not with these scores, but with Lee and Genovese's misinterpretation of what they actually represent.

First of all, they fail to recognize that neither the relative retention score nor the percent relative retention score qualify as direct measures of retention, that is, aspects of performance that are retained. The term "retention" refers to something held, kept, or retained and only the absolute retention score satisfies this definition because it reflects the amount of performance retained after the retention interval. What Lee and Genovese define as a measure of relative retention is a difference score that actually reflects the amount of performance change that occurred over the retention interval from the end of acquisition. It measures how much was lost or gained over the retention interval from the level of performance achieved at the end of acquisition. Thus, the relative retention score is not a direct measure of any aspect of performance that is retained. To directly assess the amount of performance attained, the absolute retention score should be used. What they define as the percent relative retention score is in fact a measure of how much the amount of performance change that occurred in acquisition was changed was over the retention interval. This score assesses how much of the amount of improvement in acquisition was lost or gained over the retention interval. Therefore, the percent relative retention score also is not a direct measure of any aspect of performance that is retained. To directly assess the amount of improvement in acquisition that was retained

over the retention interval, the absolute retention score should be used as the numerator rather than the relative retention score. When this expression is used, the percent relative retention increases with the absolute retention score when the amount of improvement in acquisition is held constant. A percent relative retention score below 100% indicates that not all of the improvement which occurred in acquisition was retained over the retention interval. A score above 100% reveals that more than the amount of improvement observed in acquisition was retained over the retention interval. Of course, a score of 100% indicates that the amount of improvement seen in acquisition was retained over the retention interval. When the expression recommended by Lee and Genovese is applied to the same data, the percent relative retention decreases as the absolute retention score increases up to 100% and then increases as the absolute retention score increases above 100%. One of these scores can only tell us how much of the amount of improvement which occurred in acquisition was lost or gained over the retention interval.

When the measures as defined by Lee and Genovese are interpreted for what they actually reflect, the results are not in conflict and do not lead to divergent conclusions. For instance, the Bourne and Archer (1956) data revealed that longer intertrial intervals in acquisition produce higher absolute retention scores and lower relative retention scores. Expressing these results in terms that describe what the measures actually represent we find that longer intertrial intervals in acquisition produced the greatest amount of performance retained after the retention interval, and the least amount of performance change (i.e., loss or gain) over the retention interval from the end of acquisition. If percent relative retention scores are used, then longer intertrial intervals produced the least change (i.e., loss or gain) over the retention interval in the amount of performance improvement that was observed in acquisition. The Bourne and Archer data also reveal that the shorter intertrial intervals produced higher relative retention scores and lower absolute retention scores. When these results are stated in terms that describe what the measures actually reflect, it becomes clear that the shortest intertrial intervals resulted in the least amount of performance retained after the retention interval and the greatest amount of performance change (i.e., lost or gained) over the retention interval from the end of acquisition. If percent relative retention scores are employed, then the shortest intertrial intervals produced the greatest change (i.e., loss or gain) over the retention interval in the amount of performance improvement that was seen in acquisition. Clearly, each of these scores provides us with specific information about a particular aspect of performance that the other two scores are incapable of providing. Each score tells us something different about performance from which a unique inference about learning can be made. However, when considered together, the different types of information provided by the three scores compliment each other and fit together like the pieces of a puzzle to provide a more complete picture about the effects of practice distribution on performance and learning. Selecting only one of these types of information on which to base an inference about learning as Lee and Genovese did by using the absolute retention score is similar to looking at one piece of the puzzle to determine what the entire picture looks like. There may be a danger in using this approach because the score selected reflects some but not all of the performance information subjects have produced. Thus, performance information is lost and the inference made about the effect of massed practice on learning is based on a limited amount of information. However, we do concede that selecting only one score to use would be appropriate if that score adequately reflects the particular aspect of performance on which we wish to base our inference about learning, and ignoring other types of performance information is justified.

Although we agree with Lee and Genovese that of the measures they evaluated the absolute retention score best reflects the feature of learning in which they were interested, we do not agree that the study of the effect of practice distribution on learning should be limited to this one feature. Certainly there are other features of learning worth assessing as a function of practice distribution besides the one assessed by the absolute retention score. But their position is that other features especially those assessed by the relative retention and percent relative retention scores should be ignored mainly for two reasons. First, the use of these two scores with the absolute retention score leads to divergent conclusions and second, the two relative retention scores are contaminated whereas the absolute retention score is not. We have just demonstrated that these three scores do not lead to divergent conclusions, and now we will show that the absolute retention score is not as clean as they would like us to believe.

Are Absolute Retention Scores Uncontaminated?

Lee and Genovese claim that for a performance score to reflect the relative permanence of the amount learned it should be calculated from data that were not contaminated with temporary effects produced by the independent variable which can mask learning effects. They argue

that the absolute retention score, unlike the two relative retention scores, is free of such contamination and hence, is the best measure for assessing learning effects. Their argument is based on the assumption that temporary performance effects caused by the independent variable are only present when the independent variable is being applied and this simply is not true. Temporary performance effects caused by the independent variable can persist for some time after that variable has been removed to contaminate absolute retention scores. For instance, how do Lee and Genovese know that the 5-min rest interval used in the Bourne and Archer (1956) study was long enough for all of the temporary performance effects caused by the different practice distributions to dissipate? Is it not reasonable to expect practice distributions with shorter intertrial intervals (i.e., massed practice conditions) to require longer rest or retention intervals for the temporary performance effects to dissipate than practice distributions with longer intertrial intervals (i. e., distributed practice conditions)? If so, and if the 5-min interval wasn't long enough, then the absolute retention scores in the Bourne and Archer study contained different amounts of temporary effect contamination depending upon the distribution practice used in acquisition. In other words, the absolute retention score resulting from their more distributed practice conditions had less contamination than the scores resulting from their more massed practice conditions. Moreover, when Lee and Genovese statistically analyzed these absolute retention scores, they were unknowingly comparing scores that could have been reflecting different amounts of temporary effect contamination rather than different amounts of learning.

Lee and Genovese could prove us wrong by providing evidence that the 5-min retention interval used by Bourne and Archer was long enough for all the temporary performance effects to dissipate. One way to obtain such evidence would be to include different retention intervals in the design of the experiment in order to study the time course of retention as a function of the distribution of practice in acquisition. However, care must be taken to control or account for other factors such as warm-up decrement and motivational changes that could differentially contaminate absolute retention scores with a temporary performance effect as a function of practice distribution when retention intervals are lengthened. For example, the longer retention intervals should allow for more of the massed practice effects that temporarily depress performance in acquisition to dissipate and hence, make absolute retention scores less contaminated by these effects. At the same time, however, the longer retention intervals could increase the decrement in performance caused by not warming up or increase the increment in performance produced by increased motivation and thus, contaminate absolute retention scores with these temporary effects. Therefore, a single retention interval such as the one used by Bourne and Archer could be justified if there is evidence that all the temporary effects due to different practice distributions have dissipated over that interval and temporary performance effects caused by other factors such as warm-up and motivation have been accounted for or controlled.

Up to this point, our discussion has focused on possible sources of temporary effect contamination of absolute retention scores within a single study. However, Lee and Genovese also compared absolute retention scores across studies in which different practice distributions (i. e., intertrial intervals) and retention intervals were used. In other words, the absolute retention scores they compared across studies were not equated on the basis of length of intertrial practice interval or retention interval. Consequently, the absolute retention scores they compared could have contained different amounts of temporary effect contamination depending on the distribution of practice and the length of the retention interval used. These scores also could have contained different amounts of temporary effect contamination due to the warm-up and motivation factors that were discussed previously. Moreover, we don't even know if Lee and Genovese checked to be certain that all groups within each of the studies used in the meta-analysis started out in acquisition at the same performance level. If they did not, and groups in some studies were at different levels at the beginning of acquisition, then differences in absolute retention scores become very difficult if not impossible to interpret. Unless Lee and Genovese can convince us that they controlled or accounted for these possible sources of temporary effect contamination of the absolute retention scores when they reevaluated the literature and conducted their meta-analysis, we have no alternative but to seriously doubt the validity of their conclusion about the effect of massed practice on learning. Contributing further to our doubt is the fact that their conclusion is based to a large extent on absolute retention scores which made use of time-on-target (TOT) scores. Apparently they were unaware of the potential problem of using TOT scores to assess the effects of an independent variable on tracking performance. (See Bahrick & Noble, 1966, for a more thorough discussion of this problem). Essentially, the problem is that TOT scores used alone are of limited value particularly if performance on different manipulations of the independent variable at different stages of acquisition varies

over a wide range, and the scores are either very low or very high for some of the distributions to be evaluated. The study by Howland and Noble (1953) serves as an example to illustrate this problem. Howland and Noble attempted to evaluate the effects of control loading on tracking performance and used TOT scores as the dependent measure. The resulting performance curves in acquisition displayed an increasing separation with practice, which led them to conclude that the differential effects of control loading upon tracking performance increase as practice progresses. Bahrick, Fitts and Briggs (1957) replotted these curves for a larger scoring area and found that this increasing separation among the performance curves disappeared. Indeed, the increasing separation of the curves was an artifact of the gradually increasing sensitivity of the target area rather than the result of different control loadings. In other words, the arbitrary choice of target size by Howland and Noble produced scoring artifacts that they mistakenly attributed to the effects of the independent variable. We are concerned that Lee and Genovese may have made the same mistake by including TOT scores in their meta-analysis that were determined by scoring artifacts. Not only were the TOT scores they used the results of target sizes that were arbitrarily selected, but target size and consequently the sensitivity of the scoring area varied across studies. Unless Lee and Genovese can convince us that they controlled or accounted for this scoring artifact contamination of the TOT scores they used to assess absolute retention, once again we have no choice but to seriously doubt the validity of their conclusion about the effect of massed practice on learning.

We do not want to give the impression that we are opposed to the use of the absolute retention score, or that it is not as adequate as the two relative retention scores. What we are saying is that Lee and Genovese are naive to think that the absolute retention score is not susceptible to temporary effect contamination or possible ceiling and floor effects. All three scores are performance measures that are susceptible to these non-learning effects, but these effects are manifested somewhat differently in the three scores mainly because of the different way in which each score is calculated. Thus, although the absolute retention score unlike the two relative retention scores is not calculated from performance data in acquisition that is contaminated by temporary effects of the independent variable, it is still susceptible to contamination by temporary effects of the independent variable that persist over the retention interval and to other factors as well. The key to the successful use of these and other scores is (a) knowing what aspect of performance each of the scores reflects, (b) having a thorough understanding of the interrelations among the different scores, and (c) understanding the methodological and measurement problems inherent in each of the scores. Such understanding should lead to more purposive selection among the scores for a particular study and also provide a basis for interpreting the results obtained in various studies using different scores.

General Conclusions

We have presented four major criticisms of the Lee and Genovese paper. These criticisms emanate from our view that any generalization based on some limited amount of admissible evidence will provide an incomplete description of the psychological processes underlying motor skill learning. We have suggested a number of alternatives to the perspectives presented by Lee and Genovese. These alternatives have been summarized in the form of the following four general conclusions:

1. The use of result-centered research strategies should be considered as alternatives to the use of generalization-seeking procedures. These result-centered approaches could provide the impetus for a greater amount of research activity through the coexistence of multiple hypotheses.

2. The use of a retention test to assess learning confounds the amount learned and forgetting across the retention interval. This conclusion underscores the inadequacy of the more traditional definition of learning being "relatively permanent," and emphasizes the need to study learning from a perspective of memory as being dynamic and multidimensional in character. Inferences concerning learning and forgetting should be made through an integrative description of performance across acquisition and retention trials.

3. Each of the three retention scores evaluated by Lee and Genovese provides an assessment of a different aspect of performance, and in combination these measures provide a complimentary description of retention.

4. Absolute retention measures are as susceptible to the influence of temporary performance variables, and ceiling and floor effects, as relative retention measures. This conclusion, along with number three above, emphasizes the importance of the researcher having a thorough knowledge of the individual performance scores and the way they interrelate to provide a complete perspective of learning.

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Robert W. Christina is a professor and John B. Shea is an associate professor in the Department of Exercise and Sport Science at The Pennsylvania State University.

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Requests for reprints should be sent to Robert W. Christina, Motor Behavior Laboratory, 131 White Building, The Pennsylvania State University, University Park, PA 16803.

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